# Essays in Empirical Corporate Finance

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# Essays in Empirical Corporate Finance

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***To Iris, Léo, and Paul***

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# Executive Summary

This dissertation is made of three distinct chapters. The first chapter presents a joint work with Adrien Matray. We show that managers overreact to salient risks. We study how managers respond to the occurrence of a hurricane event when their firms are located in the neighborhood of the disaster area. We find that the sudden shock to the perceived liquidity risk leads managers to increase the amount of corporate cash holdings, even though the real liquidity risk remains unchanged. Such an increase in cash holdings is only temporary. Over time, the perceived risk decreases, and the bias disappears.

The second chapter presents a joint work with Romain Boulland. We examine earnings announcements by US firms, and how far in advance notice of the event is given (the "advance notice period"). We find that such advance notice period varies within firm and that its variation affects how much investors pay attention to earnings news. This variation in investors' attention affects short-run and long-run stock prices, thereby creating incentives for firms to strategically reduce the advance notice period when they plan to disclose bad news. Consistent with this idea, we find that within-firm variations in the advance notice period predict the earnings surprise.

The third chapter presents a joint work with François Derrien. We study M&A league tables, which provide rankings of investment banks. We find that they have a significant influence on M&A advisory activities of banks. The rank of a bank in the league table predicts its future deal flow. This creates strong incentives for banks to manage their ranks in the league table. League table management tools include selling fairness opinions and reducing fees. Banks use such tools mostly when their incentive to do so is higher: when a transaction is likely to imply substantial changes in their league table position or when they lost ranks in recent league tables.

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# Résumé de la Thèse en Français

La finance d'entreprise cherche à expliquer les choix de politique financière qui émergent de l'interaction entre les dirigeants de l'entreprise et leurs investisseurs. Pour expliquer ces choix, il est nécessaire de bien comprendre comment ces deux types d'agents prennent leurs décisions. Les modèles théoriques standard en finance d'entreprise supposent que tous les agents prennent leurs décisions de manière rationnelle en considérant toute l'information pertinente disponible. Mais suite aux progrès réalisés dans les technologies de la communication, l'information pertinente à analyser est désormais disponible plus rapidement et en plus grande quantité, ce qui augmente son coût de traitement lors de la prise de décision. L'information est aussi diffusée plus largement auprès d'agents qui, à la marge, sont de moins en moins sophistiqués, donc moins à même de l'analyser correctement. En pratique donc, des contraintes économiques ou cognitives limitent la capacité effective de traitement d'information. Si la capacité de traitement d'information des agents économiques est limitée, comment cela affecte-t-il les choix de politique financière? Cette thèse essaye d'apporter des réponses à ce vaste sujet de recherche en abordant trois questions distinctes.

Le premier chapitre porte sur l'évaluation des risques par les dirigeants d'entreprise. Les choix de politique d'entreprise sont pris dans l'incertitude et nécessitent d'évaluer la probabilité d'événements incertains. La théorie économique classique suppose que les dirigeants effectuent ce travail d'évaluation sur la base de toute l'information disponible indépendamment du contexte dans lequel l'évaluation du risque est réalisée. La littérature en psychologie en revanche suggère un autre *modus operandi*. Pour évaluer les risques, les individus utiliseraient des heuristiques, c'est à dire des raccourcis mentaux qui simplifient la tâche d'évaluer les probabilités en mettant l'accent sur « ce qui vient en premier à l'esprit » (Gennaioli et Shleifer, 2010). Lorsqu'ils ont recours à ces heuristiques, les agents ne

considèrent pas toute l'information disponible de la même façon. Seule l'information saillante qui vient facilement à l'esprit est prise en compte tandis que les autres éléments d'information sont négligés (Bordalo, Gennaioli et Shleifer, 2012b). Le premier chapitre de cette thèse examine si les dirigeants utilisent eux aussi des heuristiques pour évaluer la probabilité d'événements incertains. La période économique récente offre de nombreux exemples de défaillance coûteuse dans la gestion des risques. Ces exemples suggèrent que les dirigeants affichent peut-être une confiance excessive dans la permanence de certains états du monde et négligent la possibilité que des événements incertains puissent amener d'autres états de la nature. Démontrer que les dirigeants sur-réagissent à la saillance d'un événement et négligent d'autres sources de risque qui viennent moins facilement à l'esprit demeure toutefois difficile à établir de manière empirique. Le chapitre 1 cherche à établir cette preuve en observant comment les dirigeants réagissent dans une situation où un risque se matérialise et devient saillant, mais où aucune information nouvelle n'est communiquée sur sa probabilité réelle. Pour y parvenir, l'étude se concentre sur le risque de catastrophe naturelle, et plus particulièrement, sur le risque d'ouragans aux Etats-Unis. L'analyse révèle qu'après un ouragan, le choc produit par la catastrophe sur le risque de liquidité perçu par les dirigeants amène les entreprises situées dans le voisinage de la zone sinistrée à augmenter temporairement leur détention de liquidités alors que le risque réel n'a pas changé. Le reste du chapitre examine d'autres explications de ce résultat.

Le deuxième chapitre porte sur l'attention limitée des investisseurs et analyse comment les dirigeants tirent partie de cette attention limitée au moment d'annoncer leurs résultats. La littérature existante montre que l'attention plus ou moins forte portée par les investisseurs aux annonces de résultats affecte le cours de bourse de l'entreprise (Hirshleifer, Lim, et Teoh, 2009; DellaVigna et Pollet, 2009; Peress, 2008). Les dirigeants d'entreprise dont la fonction objective est de maximiser la valeur de leur cours de bourse devraient donc prendre en compte l'attention limitée des investisseurs dans leur stratégie de communication. Toutefois, très peu

d'études parviennent à établir ce résultat. Doyle et Magilke (2009) par exemple montrent que les entreprises ne cherchent pas de façon systématique à annoncer leurs mauvais résultats le vendredi ou en dehors des heures d'ouverture de la bourse qui sont pourtant des périodes pendant lesquelles l'attention des investisseurs est limitée. Le chapitre 2 étudie un autre moyen grâce auquel les entreprises peuvent exploiter l'attention des investisseurs pendant la saison des annonces de résultats. Aux Etats-Unis, les entreprises qui prévoient d'annoncer leurs résultats doivent prévenir en avance le marché de la date à laquelle l'annonce aura lieu (la «période de notification»). L'étude montre que pour une entreprise donnée, cette période de notification varie d'un exercice à l'autre et que ces variations affectent l'attention des investisseurs le jour de l'annonce. Le reste du chapitre démontre que les entreprises utilisent de façon stratégique cette période de préavis pour attirer (échapper à) l'attention des investisseurs lorsqu'ils s'apprêtent à annoncer de bonnes (mauvaises) nouvelles.

En raison de la quantité croissante d'information disponible dans l'environnement économique moderne, les fournisseurs de données ont mis au point de nouveaux outils qui facilitent le traitement de cette information. Le chapitre 3 étudie les effets de l'un de ces nouveaux outils: les "league tables". Les "league tables" sont des classements de banque principalement basés sur leur part de marché. Ces "league tables" existent pour de nombreuses activités pratiquées par les banques d'investissement - fusions et acquisitions (M&A), émission de titres, activité de crédit... Ces classements sont largement rapportés et commentés dans la presse financière. Ils sont donc des outils d'aide à la décision facilement disponibles pour les entreprises qui souhaitent choisir leur banque d'investissement. Les "league tables" sont souvent critiquées pour les critères de classement inappropriés qu'elles utilisent, pour les comportements de jeu excessif qu'elles induisent, et pour le temps que les banquiers leur consacrent au détriment du financement de l'économie réelle. Compte tenu des revenus générés par l'industrie de la banque d'investissement et du rôle de cette industrie dans l'économie, comprendre l'effet de ces classements sur les choix des banquiers et ceux des clients est primordial. C'est ce que fait

le chapitre 3, en se concentrant sur l'industrie des fusions et acquisitions. La conclusion de ce chapitre est que le rang d'une banque dans la "league table" prédit son volume d'affaire à venir. Les banques sont donc incitées à manipuler leur classement de manière à afficher le meilleur rang possible. Le reste du chapitre présente les effets de cette manipulation.

Chapitre 1: Les dirigeants d'entreprise réagissent-ils de façon excessive aux risques saillants? (avec Adrien Matray)

Ce chapitre démontre que les dirigeants d'entreprise commettent systématiquement des erreurs dans leur appréciation des risques. L'étude montre qu'ils répondent à des chocs de liquidité évités de justesse en augmentant *temporairement* la trésorerie de l'entreprise. Cette réaction est difficile à réconcilier avec la théorie du jugement bayésien dans l'incertain car dans le cadre de l'étude, le choc de liquidité est provoqué par un ouragan dont le risque est stationnaire (Elsner et Bossak, 2001; Pielke et al, 2008.). En revanche, cette réaction est conforme aux théories de la saillance (Tversky et Kahneman, 1973, 1974; Bordalo, Gennaioli et Shleifer, 2012a, 2012b, 2013) dont la prédiction centrale est que la saillance *temporaire* de l'événement conduit les dirigeants à réévaluer leur représentation du risque et à surestimer sa probabilité.

La plupart des décisions en matière de politique d'entreprise sont faites dans l'incertitude et requièrent un travail d'évaluation des risques par les dirigeants. Les modèles classiques en finance supposent que pour y parvenir, les dirigeants procèdent à des calculs de probabilité en suivant des règles purement et exclusivement statistiques. Dans cette approche, les croyances relatives au risque sont établies sur la base de toute l'information disponible et sont formées indépendamment des facteurs spécifiques au contexte dans lequel le travail

d'estimation a lieu. En pratique cependant, la littérature en psychologie observe que l'estimation de la probabilité d'un événement incertain est un travail complexe qui demande du temps. Cette littérature montre également que les individus ont des ressources cognitives qui ne sont pas illimitées. Les agents forment donc leur croyance relative au risque en s'appuyant sur des heuristiques, c'est à dire, des raccourcis mentaux qui simplifient la tâche d'évaluer les probabilités (Tversky et Kahneman, 1973 et 1974) en mettant l'accent sur «ce qui vient en premier à l'esprit » (Gennaioli et Shleifer, 2010). Dans cette approche, toutes les informations disponibles ne sont pas considérées avec le même degré d'importance, ce qui conduit à des erreurs dans l'estimation des risques qui peuvent avoir de graves conséquences. La question est donc de savoir si les dirigeants utilisent également ces règles heuristiques et si cette pratique affecte les politiques financières de l'entreprise.

L'étude se concentre sur l'heuristique de "disponibilité". Tversky et Kahneman (1973 et 1974) montrent que les gens ont tendance à inférer la fréquence d'un événement incertain à partir de sa disponibilité mentale, c'est-à-dire la facilité avec laquelle des exemples concrets d'une situation dans laquelle cet événement s'est produit viennent à l'esprit. L'inconvénient d'une telle règle est que la disponibilité mentale d'un événement peut également être affectée par la saillance de l'événement. Pour de nombreuses raisons (par exemple, une issue dramatique de l'événement ou des niveaux très élevés de couverture médiatique), certains événements ont des caractéristiques inhabituelles qui contrastent fortement avec le reste de l'environnement. Parce que ces événements apparaissent avec plus de saillance, ils viennent à l'esprit plus facilement. Les personnes qui utilisent l'heuristique de disponibilité auront alors tendance à surestimer la probabilité que le même événement se produise à nouveau. Comme le montrent Bordalo, Gennaioli et Shleifer (2012b), les individus qui ont recours à cette heuristique se comportent comme des "penseurs locaux" qui n'utilisent qu'une partie de l'ensemble des informations disponibles pour estimer les probabilités. Ils surpondèrent les événements

possibles dont les caractéristiques attirent leur attention et négligent les autres, ce qui les conduit à commettre des erreurs sur la véritable probabilité d'un événement.

Si les dirigeants d'entreprise utilisent également l'heuristique de disponibilité, les situations dans lesquelles le risque apparaît de façon saillante devraient les conduire à sur-réagir et à prendre des décisions inappropriées en matière de gestion des risques. Plus précisément, nous faisons l'hypothèse que les dirigeants surestiment alors la probabilité que le risque se concrétise à nouveau et prennent des mesures préventives excessives contre ce risque.

Tester cette hypothèse sur le terrain empirique soulève deux difficultés majeures. Tout d'abord, le risque perçu par le dirigeant n'est pas directement observable. Pour résoudre ce problème, l'étude se concentre sur la façon dont les dirigeants estiment le risque de liquidité à l'échelle de l'entreprise. Nous proposons alors d'utiliser les variations de la trésorerie de l'entreprise pour mesurer la façon dont la perception de ce risque évolue au cours du temps. Que la trésorerie de l'entreprise soit principalement une assurance contre le risque de choc de liquidité est un résultat largement établi par la littérature en finance. Dans ces conditions, les variations de la trésorerie de l'entreprise au cours du temps devraient donner une bonne indication de l'évolution du risque de liquidité qui est perçu par ses dirigeants.

Par ailleurs, tester cette hypothèse requiert aussi d'identifier un événement saillant dont la survenance ne véhicule aucune information nouvelle sur sa distribution de probabilité. Par exemple, la faillite de Lehman Brothers en 2008 a été un événement marquant qui a sans doute conduit les banquiers à réévaluer leur perception des risques. Cependant, cet événement a simultanément révélé des informations nouvelles auparavant ignorées sur la distribution objective de leurs risques. Il est donc impossible de savoir si leur réaction est due à un changement du risque perçu, ou à un changement du risque réel.

Nous proposons de résoudre ce problème en utilisant les ouragans comme source de choc de liquidité. Les ouragans sont des risques qui sont bien adaptés à notre recherche, pour trois

raisons. Tout d'abord, la fréquence des ouragans est stationnaire (Elsner et Bossak, 2001; Pielke et al, 2008.); par conséquent, l'apparition d'un ouragan ne véhicule aucune information nouvelle sur sa probabilité d'occurrence future. Deuxièmement, un ouragan est un événement marquant dont l'occurrence est exogène aux caractéristiques de l'entreprise et de ses dirigeants, et qui constitue une source crédible de choc de liquidité. Enfin troisièmement, l'effet de saillance de ces événements est susceptible de décroitre à mesure que la distance vis-à-vis de la zone sinistrée augmente. Ils permettent donc de mettre en œuvre une stratégie d'identification par différence en différences. Cette stratégie consiste à estimer l'effet de la saillance du risque sur le risque perçu en comparant comment un groupe d'entreprises traitées situées dans le voisinage de la zone sinistrée et un groupe de contrôle ajustent leur détention de trésorerie après l'ouragan.

Nous observons que les dirigeants répondent à la saillance soudaine du risque de liquidité causée par la proximité de l'ouragan en augmentant la détention de trésorerie, alors que rien n'indique que ce risque soit désormais plus grand qu'il ne l'était auparavant. Au cours des 12 mois qui suivent l'ouragan, les entreprises situées dans le périmètre voisin augmentent leur détention de cash par rapport au groupe de contrôle de près de 7% soit en moyenne 20 millions de dollars par entreprise. Cette augmentation de la trésorerie est temporaire. Le montant de la trésorerie augmente fortement au cours des trois trimestres qui suivent la catastrophe, puis revient progressivement au niveau observé avant l'ouragan au cours des quatre trimestres suivants. Avec le temps, l'effet de saillance diminue, les gens oublient l'événement, et le biais de perception disparaît. Ce biais augmente lorsque les dirigeants sont moins sophistiqués (par exemple, chez les dirigeants peu expérimentés en terme de gestion des catastrophes liées aux ouragans, les dirigeants de petites entreprises et les dirigeants d'entreprise dont l'activité est plus récente) et au contraire diminue chez les dirigeants d'entreprises peu contraintes financièrement qui sont a priori moins exposées au risque de liquidité.

Ce biais est coûteux pour les actionnaires. Tout d'abord, les dirigeants retiennent davantage les bénéfices de l'entreprise pour pouvoir accroître la trésorerie. Ensuite, en utilisant la méthodologie de Faulkender et Wang (2006), nous constatons que la valeur de marché de la trésorerie diminue lorsque les entreprises sont soumises à ce biais. Autrement dit, la trésorerie supplémentaire accumulée dans le bilan de l'entreprise ne se traduit pas par une variation positive de la capitalisation boursière, ce qui suggère que cette trésorerie aurait probablement été mieux employée différemment.

Nous discutons ensuite les autres hypothèses qui pourraient expliquer nos résultats, à savoir la possibilité d'un changement du risque d'ouragan, la possibilité que les dirigeants découvrent et apprennent ce risque, et enfin la possibilité d'externalités géographiques. Tout d'abord, la trésorerie pourrait augmenter si la probabilité réelle d'être touché par un ouragan augmente ou si les dirigeants ignorent ce risque et apprennent son existence lorsque l'ouragan se produit. Cependant, ces deux explications impliqueraient une augmentation permanente de la trésorerie, que nous ne trouvons pas dans nos résultats. Deuxièmement, la trésorerie pourrait augmenter de façon temporaire en raison d'externalités géographiques. Par exemple, l'ouragan pourrait créer temporairement de nouveaux débouchés pour les entreprises de la zone voisine. Ces entreprises feraient alors plus de profits et détiendraient plus de liquidités. Cependant, ce type d'effet de contagion impliquerait un changement positif dans la performance de l'entreprise (chiffre d'affaires, résultat d'exploitation), que nous ne trouvons pas dans nos résultats. L'ouragan pourrait aussi augmenter localement l'incertitude économique. Les entreprises locales pourraient alors choisir de reporter l'investissement et accumuler ainsi des liquidités. Cependant, cette incertitude supplémentaire devrait engendrer une plus grande variance des revenus ou une augmentation de la volatilité des rendements des actions, que nous ne constatons pas non plus. Pour alléger encore la crainte que ces effets (ou toute autre forme d'externalité régionale) soient à l'origine de nos résultats, nous effectuons deux tests supplémentaires. Tout d'abord, nous nous concentrons sur les entreprises qui sont vulnérables

au risque d'ouragan, mais dont le siège social est situé en dehors de la zone sinistrée et de ses environs. Ensuite, nous nous concentrons sur les entreprises américaines exposées au risque de tremblement de terre et examinons comment leurs dirigeants réagissent à l'annonce de violents séismes qui se produisent en dehors des États-Unis. Dans les deux cas, la distance vis-à-vis de la zone sinistrée rend peu probable la possibilité d'externalités géographiques. Pourtant, notre premier résultat tient toujours. Dans les deux cas, la détention de trésorerie augmente après la catastrophe.

Chapitre 2: Annoncer l'annonce (avec Romain Boulland)

Ce chapitre examine l'effet de l'attention limitée des investisseurs sur la stratégie de communication financière des dirigeants d'entreprise. La question que nous posons est de savoir si les dirigeants prennent en compte l'inattention des investisseurs lorsqu'ils communiquent leurs résultats. Pour y répondre, nous étudions le processus de notification par lequel les entreprises informent les investisseurs de la date et de l'heure de leur annonce. L'étude s'appuie sur un nouvel ensemble de données constitué de presque 53 000 communiqués de presse émis par des entreprises américaines au cours de la période 2007-2012, dans lesquels ces entreprises annoncent la date, l'heure, le numéro de conférence téléphonique et d'autres détails sur l'organisation de la publication de leurs résultats (la «notification») . En moyenne, ces éléments sont communiqués dix jours avant l'événement (la «période de notification»). Notre étude montre que le choix de cette période de notification affecte l'attention des investisseurs le jour de l'annonce, et que les entreprises utilisent cette période de notification pour communiquer de façon stratégique de moins bons résultats lorsque les investisseurs font moins attention.

Au moins deux raisons permettent d'expliquer pourquoi la période de notification est susceptible d'affecter l'attention des investisseurs. Premièrement, les investisseurs dont le calendrier est surchargé doivent choisir sur quelle annonce ils vont se concentrer. En l'absence de préférences ex-ante, une règle simple de hiérarchisation est de suivre l'ordre dans lequel les "notifications" sont envoyées. Cette règle du «premier arrivé, premier servi» implique qu'une période de notification plus longue augmente la probabilité que l'entreprise arrive la première dans l'agenda des investisseurs. Deuxièmement, l'émission d'une notification peut coïncider avec d'autres informations pertinentes, telles que des annonces de résultats par d'autres entreprises. Dans ce cas, cette notification pourra être négligée par les investisseurs. En effet, les investisseurs dont l'attention est limitée concentrent en principe leur attention sur les communiqués de presse dont la valeur informationnelle est la plus forte (annonces de résultats par d'autres entreprises) et ignorent les autres. Comme les annonces de résultats sont saisonnières, ce problème de chevauchement est plus susceptible de se produire lorsque la période de notification est courte. En revanche, une période de notification plus longue réduit le risque que l'émission d'une notification entre en concurrence avec des annonces de résultats par d'autres entreprises, ce qui augmente la probabilité que les investisseurs y fassent attention et la prennent en compte.

Conformément à ces prévisions, l'étude montre qu'un allongement de la période de notification amène plus d'attention de la part des investisseurs le jour de l'annonce. Par exemple, nous observons que pour la même firme, le nombre de participants à la conférence téléphonique augmente lorsque la date et l'heure de la publication des résultats sont communiquées plus tôt. Nous observons aussi que le volume anormal des transactions qui est traditionnellement utilisé comme mesure d'attention des investisseurs (par exemple Gervais, Kaniel, et Mingelgrin 2001; Barber et Odean 2008; Hou, Xiong, et Peng 2009) est plus élevé le jour de l'annonce lorsque les détails de la publication des résultats sont communiqués bien à l'avance.

Nous examinons ensuite si la variation de la période de notification affecte la vitesse à laquelle l'information nouvelle s'incorpore dans le cours de bourse. DellaVigna et Pollet (2009) et Hirshleifer et Teoh (2003) montrent que plus d'inattention affaiblit la réaction immédiate du cours de bourse à l'annonce des résultats et augmente le drift après l'annonce. Conformément à ces prévisions, nous constatons que plus une période de notification est grande, plus la réaction immédiate à l'annonce des résultats est forte et plus le drift post-annonce est faible.

Nous procédons ensuite à la question centrale de l'étude. Si la fonction objective des dirigeants est de maximiser la valeur présente du cours de bourse, alors leur intérêt est de réduire la période de notification lorsqu'ils s'apprêtent à communiquer de mauvais résultats pour minimiser l'impact négatif de la nouvelle sur le cours de bourse. Nous observons effectivement que pour la même entreprise, le BPA au-delà du BPA attendu par les analystes diminue en moyenne de près d'un cent lorsque la notification est envoyée une semaine plus tard. En d'autres termes, pour une même entreprise, les variations de la période de notification prédisent la surprise à venir. Ce résultat est robuste dans de très nombreuses spécifications, y compris sur le sous-ensemble des entreprises qui annoncent systématiquement leurs résultats à la même date.

Nous examinons alors comment cette corrélation entre période de notification et surprise sur les résultats à venir varie selon les entreprises. Premièrement, tous les dirigeants ne peuvent pas exploiter l'inattention des investisseurs. En particulier, les dirigeants d'entreprises très visibles dont le cours de bourse est systématiquement regardé par le marché n'ont pas cette possibilité. Nous constatons en effet que la variation de la période de notification est beaucoup plus informative sur la surprise à venir lorsque les entreprises sont moins visibles par le marché. Deuxièmement, certains dirigeants  se soucient sans doute plus de la valeur à court terme du cours de bourse de leur entreprise. Par exemple, les dirigeants qui ont

l'intention d'émettre de nouvelles actions devraient se concentrer davantage sur la maximisation du prix actuel de l'action ainsi que les chefs d'entreprise dont les actionnaires sont plus orientés sur leur rendement de court terme. Conformément à cette intuition, nous observons que la corrélation entre période de notification et la surprise est plus forte lorsque les entreprises s'apprêtent à émettre des actions dans le trimestre qui suit, ou lorsque la rotation du titre était particulièrement élevée à la fin du trimestre précédent. Dans l'ensemble, cette deuxième série de résultats suggère que les dirigeants répondent de façon stratégique à l'attention limitée des investisseurs en ajustant à la hausse ou à la baisse la période de notification quand c'est dans leur intérêt de le faire.

L'étude se termine par l'examen de la réaction des investisseurs à la date de notification. Les investisseurs devraient en théorie détecter le comportement stratégique des entreprises concernant le choix de la période de notification. Dans ce cas, ces derniers devraient réagir positivement à des notifications émises très tôt et négativement à des notifications très tardives. La réaction du marché à l'émission de la notification devrait donc être positivement corrélée avec la période de notification. Or cette corrélation positive ne ressort pas de nos analyses, ce qui suggère que la majorité des investisseurs ne perçoivent pas les conséquences d'un changement de la période de notification.

Comme l'information contenue dans le choix de la période de notification ne semble pas incorporée dans les prix, il est possible de construire une stratégie de trading qui en tire parti. Cette stratégie consiste (i) à acheter l'action lorsque la notification est émise plus tôt que la notification émise au même trimestre de l'année antérieure, et (ii) à vendre l'action lorsque la notification est émise plus tard. Cette stratégie dégage un rendement anormal élevé de 8 points de base par jour (environ 1,7% par mois) hors coûts de transaction ($t = 4,54$).

## Chapitre 3: Les effets des classements de banques d'investissement ("League Tables") (avec François Derrien)

Les "league tables" sont des classements basés sur les parts de marché des banques. Ces "league tables" existent pour de nombreuses activités pratiquées par les banques d'investissement - fusions et acquisitions (M&A), émission de titres, activité de crédit... Ces classements sont largement rapportés et commentés dans la presse financière. Ils sont donc des outils d'aide à la décision facilement disponibles pour les entreprises qui souhaitent choisir leur banque d'investissement. Les "league tables" sont souvent critiquées pour les critères de classement inappropriés qu'elles utilisent, pour les comportements de jeu excessifs qu'elles induisent, et pour le temps que les banquiers leur consacrent au détriment du financement de l'économie réelle. Compte tenu des revenus générés par l'industrie de la banque d'investissement et du rôle de cette industrie dans l'économie, comprendre l'effet de ces classements sur les choix des banquiers et ceux des clients est primordial. C'est ce que fait l'étude proposée dans ce troisième chapitre, en se concentrant sur l'industrie des fusions et acquisitions.

De nombreux articles de presse révèlent que les banques d'investissement se préoccupent de leur classement dans les league tables, peut être parce que ces classements sont l'une des rares sources d'information publique à la disposition des clients qui sont à la recherche d'une banque conseil. Si c'est le cas, alors les classements pourraient ne pas seulement refléter la performance passée mais aussi influencer la performance avenir. L'étude confirme cette intuition et montre que les changements de classement récents expliquent les variations du volume d'affaires futur de la banque.

Pour établir sans ambiguïtés un lien de cause à effet entre le rang de la banque dans le classement et son volume d'affaires futur, et notamment vérifier que cette relation n'est pas

expliquée par des variables non observées (par exemple, des changements dans la qualité de la banque), l'étude utilise deux spécifications supplémentaires. Tout d'abord, l'étude exploite le fait que les classements n'intègrent que les 25 plus grandes banques, même si les banques classées juste en dessous du rang 25 sont très similaires en termes de part de marché aux banques classées juste au-dessus. Bien que les banques classées autour de ce seuil soient très similaires, notre étude montre que le simple fait d'entrer (sortir) dans la league table a un impact positif (négatif) très significatif sur le volume d'affaires futur de la banque. Dans un autre test, l'étude exploite le fait que, lorsqu'une banque est acquise ou disparaît, les banques classées en-dessous d'elle gagnent mécaniquement des rangs, tandis que les banques classées au-dessus ne sont pas affectées. Notre analyse montre que les banques qui subissent cette hausse exogène de leur rang bénéficient par la suite d'une hausse de leur volume d'affaires qui est supérieur à celui des banques non affectées par l'événement. Cet impact du classement sur les volumes d'affaires futurs suggère que ces classements contribuent à la réputation des banques. Dans la mesure où les league tables sont l'une des seules mesures indépendantes disponibles de la performance des banques, il se pourrait que les entreprises les utilisent en dépit de leurs limites parce qu'elles ont peu d'informations sur la qualité des banquiers ou peu d'expérience en matière de fusions et acquisitions. Conformément à cette intuition, nous observons que le classement a moins d'importance sur la capacité de la banque à originer de nouveaux mandats lorsque les clients sont plus expérimentés. Dans l'ensemble, ces résultats indiquent que les changements dans le classement affectent la perception de la qualité de la banque, et ont des conséquences réelles pour les banques. Les league tables incitent donc fortement les banques à piloter au mieux leur position dans ces classements.

Dans ce contexte, nous émettons et étudions l'hypothèse que les banques sont prêtes à piloter leur rang dans la league table tant que le coût de ce pilotage en termes de revenus actuels, d'effort d'exécution, et de risque de réputation ne dépasse pas les bénéfices futurs attendus. Tester cette hypothèse suppose d'abord d'identifier des situations dans lesquelles les banques

sont incitées à piloter leur rang. Si toutes les banques pilotent leur rang en permanence avec la même intensité, alors cette activité de pilotage peut ne pas avoir d'incidence sur le classement, et sera même inobservable pour les chercheurs comme l'indique la littérature sur les tournois. Toutefois, les incitations à piloter ces classements varient selon les banques en fonction de leur rang. Tout d'abord, l'incitation à piloter son rang est plus forte lorsque le gain marginal du pilotage est plus élevé, c'est-à-dire lorsque la banque est très proche des autres concurrents dans le classement. L'incitation est aussi plus grande pour les banques qui ont récemment perdu des places et cherchent à récupérer leur rang.

Par ailleurs, tester notre hypothèse selon laquelle les banques pilotent les classements suppose aussi d'identifier les moyens qui leur permettent de piloter leur rang. Pour ce faire, les banques peuvent exploiter les règles de construction des league tables. Ces règles sont telles que, dans la plupart des cas, toutes les banques qui participent à une même transaction obtiennent le même crédit quel que soit leur rôle sur l'opération. Des mandats tels que des avis d'équité ("fairness opinion" ou "FO") qui demandent peu d'efforts en termes d'exécution (avec de très faibles commissions) mais qui donnent droit à 100% du crédit dans la league table sont des outils de pilotage potentiels du classement. Un avis d'équité (FO) est une évaluation par un tiers de l'équité du prix proposé dans le cadre d'une transaction. Les commissions facturées pour une FO sont généralement très faibles, ce qui les rend peu attrayantes d'un point de vue financier. Cependant, les FO sont bénéfiques en termes de league table car un fournisseur de FO obtiendra le même crédit dans la league table que celui obtenu par la banque conseil en titre. Une autre possibilité pour les banques qui souhaitent maintenir ou améliorer leur position dans le classement est d'abaisser leurs commissions. Ce faisant, elles réduisent leur niveau de revenus actuel, mais elles augmentent leur probabilité d'obtenir le mandat, ce qui augmente du même coup leurs chances de gagner des rangs dans le classement et d'accroître leurs revenus dans le futur.

Conformément à notre hypothèse de pilotage des classements, nous observons que les banques sont davantage enclines à piloter leur classement (c'est-à-dire à émettre des FO et à réduire leurs commissions) lorsque leurs incitations à le faire sont plus fortes. En particulier, notre étude montre que lorsque plusieurs banques conseillent le même client sur la même transaction, la banque qui bénéficie le plus de l'opération en termes de classement est celle qui émet la FO et facture les commissions les plus faibles.

Pour finir, l'étude se concentre sur les effets de ce pilotage des classements par les banques d'investissement. Nous montrons notamment que, lorsqu'elles pilotent leur rang, les banques offrent des services de moindre qualité à leurs clients. En particulier, les attestations d'équité potentiellement émises pour gagner des places dans la league table correspondent à des opérations pour lesquelles l'incertitude sur le «juste» prix de la transaction est plus forte, la probabilité d'échec est plus élevée, et les synergies sont plus faibles.

# Introduction

Corporate finance aims to explain investment and financing decisions that emerge from the interaction of managers and investors. A complete explanation of corporate policy choices therefore requires a correct understanding of how these two sets of agents make their decisions. Standard theoretical models in corporate finance assume that both managers and investors make their decisions using all relevant information. In our modern economies, this amount of information can be vast. Advances in communication technology have made public information available faster and in greater quantity, thereby considerably increasing the cost of processing all relevant information. Information is also disseminated more broadly. As such dissemination increases, the marginal recipient of information might be less sophisticated and less able to process it correctly. So in reality, economic or cognitive constraints limit the effective information processing capacity. If managers or investors have limited ability to process all information, how does it affect corporate decision making? This dissertation contributes to this broad research topic by addressing three separate and important questions.

The first chapter focuses on managers and studies how they process information when it comes to assessing risk. Corporate policy decisions are made under uncertainty and require to assess the likelihood of uncertain events. Under standard assumptions, beliefs about risky outcomes are based on all available information and are formed regardless of context-specific factors. Evidence from the psychology literature suggests, however, that individuals use heuristics, i.e., mental shortcuts that simplify the task of assessing probabilities by focusing on "what first comes to mind" (Gennaioli and Shleifer, 2010). Under this alternative manner of assessing risk, all information is not given equal importance. Salient information that grabs attention is overemphasized while non-salient information is neglected (Bordalo, Gennaioli and Shleifer, 2012b). Chapter 1 of this dissertation asks whether managers overreact to salient

risks. The recent economic period provides abundant anecdotes of costly risk management failures maybe caused by overreaction and risk neglect.[1] Yet, convincing evidence that managers overreact to salient risk is hard to establish empirically. Chapter 1 considers a situation in which a risk materializes and becomes salient but where there is no new information about its probability, namely the occurrence of hurricanes. We find that managers respond to the hurricane event when their firms are located in the neighborhood of the disaster area. Consistent with salience theories, the sudden shock to the perceived liquidity risk leads managers to temporarily increase the amount of corporate cash holdings, even though the real liquidity risk remains unchanged. The rest of the chapter examines various alternative explanations for this finding.

The second chapter focuses on investors' ability to process information and examines how managers may take advantage of it during earnings announcement seasons. Existing literature provides extensive evidence that investors' attention to earnings news affects stock prices (e.g., Hirshleifer, Lim, and Teoh, 2009; DellaVigna and Pollet, 2009; Peress, 2008). Therefore, managers who care about the value of their firm's stocks should take into account investor limited attention in their decisions. Surprisingly, however, Doyle and Magilke (2009) find no evidence that managers exploit investors' inattention when announcing their earnings. They show that firms do not opportunistically release bad earnings on Friday or outside the market hours. Chapter 2 considers an alternative and more subtle channel through which firms may exploit investors' attention during earnings announcement season. Firms that plan to disclose their earnings must notify in advance the date and time of the announcement to market participants (the "advance notice period"). We show that such advance notice period varies within firm and that its variation affects how much investors pay attention to earnings

[1] Examples of recent risk management failures include the huge trading losses reported by investments banks during the financial crisis of 2008 (e.g., Société Générale, JP Morgan, Morgan Stanley, ...) or industrial disasters like British Petroleum oil spill on April 2010, and whose cost may arise at $42.2 billion.

news. The rest of the chapter provides evidence that firms strategically use this advance notice period to attract (escape) investors' attention when they plan to release good (bad) news.

As a result of the increasing amount of information available in the environment, data providers have been developing new tools to facilitate raw information processing. Chapter 3 studies the effects of one of these new tools: the league tables. League tables are rankings based on banks' market shares. They cover many investment banking activities -- Mergers and Acquisitions (M&As), security underwriting, lending... They are widely reported and commented in the financial press, and are thus commonly available information to firms willing to select their investment bank. These rankings are frequently criticized for using inappropriate criteria, for inducing excessive gaming behaviors, and for distracting bankers away from what should be their real function in the economy. Given the revenues generated by the investment banking industry and the role of this industry in the economy, understanding the effect of these rankings on both clients' choices and bankers' behavior is key. This is what chapter 3 does, focusing on the M&A industry. Our main finding is that the rank of a bank in the league table predicts its future deal flow. As a result, banks manage their rank in the league table. The rest of the chapter discusses the effects of such league table management.

# Chapter 1

# Do Managers Overreact to Salient Risks?

# Evidence from Hurricane Strikes

Joint work with **Adrien Matray**

## Abstract

Consistent with salience theories of choice, we find that managers overreact to salient risks. We study how managers respond to the occurrence of a hurricane event when their firms are located in the neighborhood of the disaster area. We find that the sudden shock to the perceived liquidity risk leads managers to increase the amount of corporate cash holdings, even though the real liquidity risk remains unchanged. Such an increase in cash holdings is only temporary. Over time, the perceived risk decreases, and the bias disappears. This bias is costly for shareholders because it leads to higher retained earnings and negatively impacts firm value by reducing the value of cash. We examine alternative explanations for our findings. In particular, we find only weak evidence that the possibility of risk learning or regional spillover effects may influence our results.

*"It is a common experience that the subjective probability of traffic accidents rises temporarily when one sees a car overturned by the side of the road."*

A. Tversky and D. Kahneman (1974)

## 1.1. Introduction

In this paper, we provide empirical evidence that managers exhibit biases when assessing risk. Specifically, we show that managers systematically respond to near-miss liquidity shocks by *temporarily* increasing the amount of corporate cash holdings. Such a reaction cannot be explained by the standard Bayesian theory of judgment under uncertainty because the liquidity shock stems from a hurricane landfall whose distribution is stationary (Elsner and Bossak, 2001; Pielke et al., 2008). Instead, this reaction is consistent with salience theories of choice (Tversky and Kahneman, 1973, 1974; Bordalo, Gennaioli and Shleifer, 2012a, 2012b, 2013) that predict that the *temporary* salience of a disaster leads managers to reevaluate their representation of risk and put excessive weight on its probability.

Most corporate policy decisions are made under uncertainty and require managers to estimate risk. Standard corporate finance models assume that managers do so by estimating probabilities through a pure statistical approach. Under this assumption, beliefs about risky outcomes are based on all available information and are formed regardless of context-specific factors. In practice, however, assessing risk is complicated and time-consuming. Because individuals have limited cognitive resources, psychologists argue that they may rely on heuristics, i.e., mental shortcuts that simplify the task of assessing probabilities (Tversky and Kahneman, 1973 and 1974) by focusing on "what first comes to mind" (Gennaioli and Shleifer, 2010). Under this alternative manner of assessing risk, all information is not given equal importance, which may lead people to make mistakes in their estimation that can have important consequences. In this paper, we ask whether managers also use such heuristic rules and investigate whether this practice affects corporate policies.

We focus on the "availability heuristic" rule. Tversky and Kahneman (1973 and 1974) show that people have a tendency to infer the frequency of an event from its availability, namely the ease with which concrete examples of a situation in which this event occurred come to mind. As the quote above suggests, the drawback of such a heuristic rule is that availability may also be affected by the salience of the event. For many reasons (e.g., a dramatic outcome or high levels of media coverage), certain events have unusual characteristics that stand in stark contrast with the rest of the environment. Because such events are more salient, they come to mind more easily. People using the availability heuristic will then overestimate the probability that these events will occur again. As shown by Bordalo, Gennaioli and Shleifer (2012b), such people behave as "local thinkers" who use only partial (i.e., salient) information to estimate probabilities. They overweight possible outcomes whose features draw their attention while neglecting others and thereby make incorrect inferences about the true probability of an event.

If corporate managers also use the availability heuristic, salient risk situations should lead them to overreact and make inappropriate decisions in terms of risk management. Specifically, we hypothesize that managers then overestimate the probability that the risk will materialize again and take excessive precautionary measures against it.

Testing this hypothesis empirically gives rise to two major difficulties. First, the risk perceived by the manager cannot be directly observed. To address this problem, we focus on how managers estimate the risk of liquidity shock at the firm level and use the variations in corporate cash holdings to measure how their perception of this risk changes. [1] Given the overwhelming evidence that corporate cash holdings are primarily used as a buffer against the risk of liquidity shortage, variations in cash holdings should provide a good indication of the

[1] Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used in imperfect financial markets as an insurance mechanism against the risk of liquidity shock. Empirically, several papers document a positive correlation among various possible sources of cash shortfall in the future and the current amount of cash holdings; these studies thus confirm that precautionary motives are central to accumulating cash reserves (e.g., Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009, Acharya et al., 2012).

changes in liquidity risk that are perceived by firm decision makers.

Second, testing this hypothesis also requires the identification of a salient event whose occurrence does not convey any new information about the real distribution of its probability. For instance, the bankruptcy of Lehman Brothers in 2008 was a salient event that might have led bankers to reevaluate their *subjective* estimation of their risk exposure. However, this event is also likely to have affected the *objective* distribution of their risks.[2] It is therefore impossible to disentangle the part of their reactions caused by the increase in *subjective* risks from that caused by the increase in *objective* risks.

We address this problem here by using hurricanes as the source of liquidity shocks. Hurricanes are risks that are well suited for our purpose for three reasons. First, hurricane frequency is stationary (Elsner and Bossak, 2001; Pielke et al., 2008); thus, the occurrence of hurricane does not convey any information about the probability of a similar event occurring again in the future. Second, their occurrence is a salient event that is exogenous to firm or manager characteristics and represents a credible source of liquidity shock. Finally, hurricane events permit a difference-in-differences identification strategy because their salience is likely to decline as the distance from the disaster zone increases. This feature allows us to estimate the *causal* effect of risk saliency on the perceived risk by comparing how a treatment group of firms located in the neighborhood of the disaster zone and a control group of distant firms adjust their cash holdings after a disaster.

We find that managers respond to the sudden salience of liquidity risk caused by the proximity of a hurricane by increasing the amount of their firm cash holdings, although there is nothing to indicate that this risk is now bigger than it was. On average, during the 12-month period following the hurricane, firms located in the neighborhood area increase their cash holdings by 0.84 percentage points of total assets relative to firms farther away. We also find

[2] See Shleifer and Vishny (2011) for an analysis of how Lehman Brothers bankruptcy affected banks' balance sheets and increased the risk of fires sales.

that this cash increase is temporary. The amount of cash increases sharply during the first two quarters following the disaster and then progressively returns to pre-hurricane levels over the next four quarters. Thus, as time passes, salience decreases, people forget the event, and the bias vanishes. This bias increases when managers are likely to be less sophisticated (i.e., managers of firms without previous experience of hurricane strikes in their neighborhood area, managers of small firms, and managers of young firms) and decreases when they have good reasons to care less about liquidity risks because their firms are not financially constrained.

We also find that this bias is costly for shareholders. First, we find that managers institute higher earnings retention to increase cash holdings. Second, using the methodology of Faulkender and Wang (2006), we find that the market value of cash decreases when firms are subject to this bias. The additional cash accrued in the balance sheet does not lead to a positive change in market capitalization, which suggests that it would most likely have been better employed otherwise.

We then discuss alternative non-behavioral explanations to our findings, such as the possibility of changes in risk, risk learning, and regional spillover. First, cash holdings could increase if the real probability of being hit by a hurricane increases or if managers ignore the risk and learn of its existence only when the hurricane occurs. However, both of these explanations would imply a permanent increase in cash holdings, which we do not find. Second, cash might increase temporarily because of regional externalities. For instance, the hurricane may temporarily create new business opportunities for firms in the neighborhood area. These firms would then make more profits and hold more cash. However, this type of spillover effect would imply a positive change in operating performance (sales, income), which we do not find. The hurricane might also locally increase business uncertainty for firms in the neighborhood area. These firms may then postpone investment and accumulate cash.

However, this additional uncertainty should generate greater variance in revenues or increased volatility in stock returns, which we also do not find. To further alleviate the concern that these effects (or any other form of regional spillover effect) are driving our results, we perform two additional tests. First, we focus on firms that are vulnerable to a hurricane risk but whose headquarters are located outside the disaster zone and its neighborhood. Second, we focus on US firms exposed to earthquake risk and examine how they react to violent earthquakes that occur outside the US. In both situations, the distance to the disaster zone makes the possibility of regional spillover irrelevant. Nevertheless, our primary finding still holds. In both cases, cash holdings increase after the disaster.

Finally, we verify that holding more cash protects firm revenues better in the case of a hurricane. Indeed, if managers respond to the salience of hurricane risk by increasing cash holdings, then we would expect that holding cash helps to reduce firm losses when this risk materializes. We test this prediction and examine how firms located in disaster areas perform in terms of revenue after the disaster depending on the level of their cash holdings before the hurricane. We find that firms that hold more cash perform better and recover much faster than other firms. This finding explains why managers are willing to increase cash holdings when they perceive that the risk of a hurricane strike is higher.

Our paper shows that managers are prone to use the availability heuristic to assess risk, which affects firm value by reducing the value of cash. As such, this study contributes first to the literature on behavioral corporate finance. Baker and Wurgler (2012) organize this literature around two sets of contributions: "irrational investors" and "irrational managers." Our paper is related to the "irrational managers" strand of the literature, which primarily focuses on how overconfidence and optimism can affect both investment and financing decisions (Malmendier and Tate, 2005; Hirshleifer, Low and Teoh, 2012; Landier and Thesmar, 2009). More recently, this literature has begun to study the effects of bounded

rationality (Brav et al., 2005), such as reference point thinking (Baker, Pan and Wurgler, 2012; Baker and Xuan, 2011; Loughram and Ritter, 2002; Ljungqvist and Wilhelm, 2005; Dougal et al., 2011).

Next, our results are related to the growing literature that focuses on the effects of individual traits and past experiences on investors' decisions (Malmendier and Nagel, 2011; Malmendier and Nagel, 2013; Kaustia and Knüpfer, 2008; Choi et al., 2009; Greenwood and Nagel, 2009). Because saliency is experienced-based, our paper complements this literature and shows that irrelevant contextual factors also influence firm decision makers.[3]

Finally and more generally, our paper contributes to the vast literature on the effects of behavioral biases "in the field."[4] *A priori*, managers may act rationally because they are neither unsophisticated agents nor students in a lab with no real economic environment. Therefore, as noted by Levitt and List (2007), we should expect managers not to be affected by behavioral biases. Whether they rely on the availability heuristic to make financial decisions is thus an open question and to the best of our knowledge, this paper is the first to empirically show that managers use the availability heuristic to assess risk and the first to study its effects.

The rest of the paper is organized as follows. Section 1.2 briefly summarizes what is known about hurricane risk. Section 1.3 proposes hypotheses based on the availability heuristic phenomenon and reviews the related scientific and anecdotal evidence. Section 1.4 presents our empirical design. Section 1.5 provides evidence about whether managers overreact to salient risks. Section 1.6 investigates whether this overreaction is costly. Section 1.7 discusses the possibility of alternative non-behavioral explanations. Section 1.8 examines the effects of cash holdings on post-hurricane performance. Section 1.9 concludes.

[3] Another strand of research examines how salience affects individuals' attention. This literature shows that investors pay more attention to salient news (Barber and Odean 2008), which affects stock prices (Ho and Michaely, 1988; Klibanoff, Lamont, and Wizman, 1998; Huberman and Regev, 2001).
[4] DellaVigna (2009) provides a detailed survey of the real effects of behavioral economics.

## 1.2. Hurricane activity on the US mainland

Hurricanes are tropical cyclones that form in the waters of the Atlantic and eastern Pacific oceans with winds that exceed 32 m per second (approximately 72 miles per hour). In this section, we briefly summarize what is known about the risk of hurricanes in the US and why it is justified to use such a risk for our experiment. We highlight that hurricane risk can randomly affect an extensive number of firms throughout the US territory, has not changed over time and should remain unchanged in the coming decades in terms of both volume (frequency) and value (normalized economic cost).

### *1.2.1. Event location*

Hurricanes can randomly affect a large fraction of the US territory. Coastal regions from Texas to Maine are the main areas at risk. An extensive inland area can also be affected, either by floods resulting from the heavy rainfalls accompanying hurricanes or by the high winds produced by the hurricane as it moves across land. In the SHELDUS database (the main database for natural disasters in the US), 1,341 distinct counties (approximately 44% of the total counties in the US) are reported to have been affected at least once by a major hurricane. Figures 1.1 through 1.4 show on a map examples of disaster areas for hurricanes Fran, Floyd, Allison, and Katrina.

[INSERT FIGURES 1.1 TO 1.4 AROUND HERE]

### *1.2.2. Event frequency*

Hurricanes are regular events in the US. Since 1850, an average of 17 hurricanes strike the US mainland during any ten-year period.

[INSERT FIGURE 1.5 AROUND HERE]

Figure 1.5 suggests no particular increasing or decreasing trend in this frequency. This absence of a trend is supported by the climatology literature. Overall, the distribution of hurricane strikes in the US is found to have been stationary since early industrial times for all hurricanes and major hurricanes as well as for regional activity (Elsner and Bossak, 2001; Pielke et al, 2008; Blake et al., 2011). Regarding possible future changes in storm frequencies, Pielke et al. (2008) conclude in their survey that given "the state of current understanding (…) we should expect hurricane frequencies (…) to have a great deal of year-to-year and decade-to-decade variation as has been observed over the past decades and longer."

### *1.2.3. Event cost*

The total cost of hurricane strikes in terms of economic damages is now much larger than it was at the beginning of the past century (Blake, Landsea and Gibney, 2011). However, after normalizing hurricane-related damage for inflation, coastal population and wealth, no trend of increasing damage appears in the data. For instance, Pielke et al. (2008) find that had the great 1926 Miami hurricane occurred in 2005, it would have been almost twice as costly as Hurricane Katrina; thus, they stress that "*Hurricane Katrina is not outside the range of normalized estimates for past storms.*" Overall, their results indicate that the normalized economic cost of hurricane events has not changed over time, consistent with the absence of trends in hurricane frequency and intensity observed over the last century.

## 1.3. The psychological mechanisms for probability evaluation and risk assessment

### *1.3.1. The availability heuristic*

Because assessing the likelihood of uncertain events is a complex and time-consuming task, people naturally tend to use their own experiences for developing simple mental rules to rapidly adjust their beliefs and adapt to their environment. Tversky and Kahneman (1973, 1974) describe such heuristic rules and show that, although useful in general, they sometimes lead people to make mistakes. One such rule is the "availability heuristic," which derives from the common experience that "frequent events are much easier to recall or imagine than infrequent ones." Therefore, when judging the probability of an event, most people assess how easy it is to imagine an example of a situation in which this event actually occurred. For example, people may assess the probability of a traffic accident by recalling examples of such occurrences among their acquaintances.

Tversky and Kahneman (1973, 1974) show that the use of this rule is problematic because availability may also be affected by factors that are not related to actual frequency. In particular, they argue that factors such as familiarity with the event, the salience of the event, the time proximity of the event and/or the preoccupation for the event's outcome can affect its availability and generate a discrepancy between subjective probability and actual likelihood. The availability of a car accident, for instance, will be higher when the person involved in the accident is famous (familiarity), if the accident was observed in real time (salience), if the accident occurred recently (time proximity), or if the physical pain caused by the injuries resulting from traffic accidents has been recently "vividly portrayed" (preoccupation with the outcome). In all these cases described above, the subjective probability of a car accident will then be temporarily higher than its actual likelihood.

### *1.3.2. Scientific and anecdotal evidence*

The availability heuristic theory is consistent with anecdotal and scientific evidence. In a series of studies by Lichtenstein et al. (1978), people were asked to estimate the frequency of

several dozen causes of death in the United States. The results from this study show that salient causes that killed many people during a single occurrence were overestimated, whereas less salient causes were systematically underestimated. In a survey conducted to understand how people insure themselves against natural hazards, Kunreuther et al. (1978) observe a strong increase in the number of people willing to buy insurance at a premium immediately after an earthquake. Conversely, people were found to be reluctant to buy such insurance even at a subsidized rate in the absence of a recent major earthquake. Johnson et al. (1993) also find that people are willing to pay more than two times the amount for the same insurance product in situations in which the risk is salient compared to situations in which it is not, confirming that saliency increases perceived risk.[5]

To account for such empirical findings, Bordalo, Gennaioli, and Shleifer (2012b, 2013b) develop a theoretical framework of choice under risk in which salient attributes grab individuals' attention. In their model, individuals do not equally consider the full set of possible states of the world when it comes to assessing risk. They neglect non-salient states, and over-emphasize the salient ones. Because the salience of a state depends on contextual factors, individuals then make context-dependent risk estimations. When a good state is salient, they over-estimate the likelihood of a positive outcome and take too much risk. When a bad state is salient, they over-estimate the probability of a negative outcome and are excessively risk averse. In both cases, individuals overreact to salient risks.[6]

### *1.3.3. Implications and hypothesis development*

[5] Other similar results can be found in the housing literature, in which changes in housing prices can be used to infer changes in perceived risk. This literature shows that the occurrence of a salient event (e.g., floods, earthquakes, nuclear accidents, etc.) systematically results in a decrease in property prices that is larger than the value of the insurance premium (see, for instance, MacDonald et al., 1990; Bin et al., 2004, 2008; Kousky, 2010)

[6] Other models based on the mechanism of salience include Bordalo, Gennaioli and Shleifer (2012a, 2013a), Gabaix (2011), Gennaioli and Shleifer (2010), Köszegi and Szeidl (2013), and Schwartzstein (2009). These models share the common assumption that individuals do not consider the whole set of available information before making a decision and neglect part of it. Significant judgment errors then occur when the neglected data are relevant for decision making.

In this paper, we focus on decision makers in firms. We ask whether they rely on the availability heuristic to assess risk and examine whether they overreact to salient risks (hereinafter, the *availability heuristic* hypothesis). Firm decision makers are neither uninformed, unsophisticated agents (such as home owners or property insurance retail buyers), nor are they undergraduate students in an experiment conducted outside of a real economic environment.[7] Whether managers will make incorrect financial decisions in the real world because of the availability heuristic therefore largely remains an open question.

One challenge is that we cannot directly observe the risk perceived by firm managers. To address this difficulty, we assume that changes in risk perception can be inferred from variations in corporate cash holdings. There is indeed strong theoretical and empirical evidence in the corporate finance literature that the main driver of policies regarding cash holdings is risk management. Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used as an insurance mechanism against the risk of a liquidity shock in imperfect financial markets because firms have limited access to external financing. In this context, cash holdings offer a buffer against any risk of cash shortage that would prevent firms from financing positive Net Present Value (NPV) projects. Consistent with this argument, several empirical papers document a positive correlation among various possible sources of cash shortfalls for future and current levels of cash holdings (Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009; Ramirez and Altay, 2011; Acharya et al., 2012). Surveys of CFOs also confirm this link. For instance, Lins et al. (2010) find that a sizeable majority of CFOs indicate that they use cash holdings for general insurance purposes.

If managers rely on the availability heuristic to assess the risk of an event that would trigger a cash shortage, cash holdings should then vary in response to the salience of this

[7] Levitt and List (2007) discuss the limitations of lab experiments and explain why economic agents may evolve toward more rational behaviors when placed in a familiar environment.

event. Under the *availability heuristic* hypothesis, we thus argue that corporate cash holdings will increase (decrease) in those situations in which the risk of cash shortage becomes more (less) salient.

Because firms are not identical to one another, the effect of event saliency on corporate cash holdings may vary in the cross section of the population. A primary source of heterogeneity is the level of managerial sophistication; sophisticated agents are expected to be less affected by behavioral biases. Therefore, changes in cash holdings for firms with sophisticated managers should be less sensitive to event saliency. Another source of heterogeneity is the level of financial constraints. Managers of less financially constrained firms should be less concerned about potential liquidity shocks. Therefore, changes in cash holdings for unconstrained firms should be less sensitive to event saliency. Another source of heterogeneity consists of firms' vulnerability to hurricane disasters. Indeed, not all industries are similarly affected by hurricane events. Certain industries may suffer higher losses, perhaps because they are more difficult to insure or because they are more dependent on the local economy. Changes in cash holdings should be more sensitive to event saliency for firms that operate in such vulnerable industries.

## 1.4. Empirical design

### *1.4.1. Identification strategy*

In this paper, we use both the occurrence of hurricanes and the proximity of the firm headquarters to the disaster area to identify situations in which the risk of liquidity shocks becomes salient. Our motivation for the use of hurricanes relies on the following arguments.

First, hurricanes can trigger liquidity shocks because of the heavy damage they can inflict.[8] Although firms might buy insurance to cover this risk, direct insurance is unlikely to cover all type of indirect losses. In addition, Froot (2001) shows that hurricane insurance is overpriced.[9] Thus, firms should prefer to self-insure by accumulating cash reserves instead of directly insuring this liquidity risk. Second, the occurrence of hurricanes is a salient event because hurricanes draw people's attention and leave their marks on observers' minds. Third, this saliency effect is likely to vary with the proximity of the landfall. Indeed, we expect the event to be salient for firms located in the disaster area and the environs nearby (referred to herein as the neighborhood) but not for more distant firms. In particular, it might be almost completely ignored by those located in areas far from the disaster area. Fourth, the occurrence of a hurricane makes hurricane risk more available but does not imply a change in the risk itself. The distribution of hurricanes is stationary; therefore, there is no reason to believe that the real risk of hurricane landfall changes after its occurrence. Finally, hurricanes are exogenous events that can randomly affect a large number of firms. A firm's distance from hurricane landfalls thus offers an ideal natural experiment framework to test for the presence of a causal link between event saliency and managers' risk perception through changes in corporate cash holdings.

### *1.4.2. Data*

We obtain the names, dates and locations of the main hurricane landfalls in the US from the SHELDUS (Spatial Hazard and Loss Database for the United States) database at the University of South Carolina. This database provides the location for each disaster at the

[8] Cash shortages can come in many ways, including reinvestment needs caused by the partial destruction of operating assets (headquarters, plants, equipment, etc.), a drop in earnings because of a drop in local demand, or new investment financing needs caused by unexpected growth opportunities (reconstruction opportunities, acquisition of a local competitor, etc.).

[9] Froot (2001) shows that hurricane insurance is in short supply because of the market power enjoyed by the small number of catastrophe reinsurers. As a result, insurance premiums are much higher than the value of expected losses. Garmaise and Moskowitz (2009) provide evidence that such inefficiencies in the hurricane insurance market lead to partial coverage of this risk at the firm level, which hurts bank financing and firm investment.

county level for all major hurricanes since the early 1960s. To ensure that the event is sufficiently salient, we focus on hurricanes with total direct damages (adjusted for CPI) above five billion dollars. We also restrict the list to hurricanes that occurred after 1985 because there are no financial data available from Compustat Quarterly before that date. This selection procedure leaves us with 15 hurricanes between 1989 and 2008. We obtain detailed information about their characteristics (start date, end date, date of landfall, direct number of deaths, total damage, and category) from the tropical storm reports available in the archive section of the National Hurricane Center website and from the 2011 National Oceanic and Atmospheric Administration (NOAA) Technical Memorandum. Table 1.1 presents summary statistics for these 15 hurricanes.

[INSERT TABLE 1.1 AROUND HERE]

We obtain financial data and information about firm headquarters location from Compustat's North America Fundamentals Quarterly database.[10] Quarterly data rather than annual data are used to identify changes in cash holdings in firms near hurricane landfalls with the highest possible precision.[11] We restrict our sample to non-financial and non-utility firms whose headquarters are located in the US over the 1987-2011 period. If the county location of a firm's headquarters is missing or if the fiscal year-end month is not a calendar quarter-end month (i.e., March, June, September or December), the firm is removed from the sample. This selection procedure leaves us with a firm-quarter panel dataset of 11,948 firms and 411,490 observations. In Panel A of Table 1.2, we present summary statistics for the main firm-level variables we use. All variables are winsorized at the first and 99th percentile and are defined in Appendix.

[INSERT TABLE 1.2 AROUND HERE]

[10] One possible concern with location data is that Compustat only reports the current county of firms' headquarters. However, Pirinsky and Wang (2006) show that in the period 1992-1997, less than 3% of firms in Compustat changed their headquarter locations.

[11] We obtain the same results with annual financial data.

### *1.4.3. Assignment to treatment and control groups*

We measure the degree of salience of each hurricane event according to the distance between the firm's headquarters and the landfall area. For this purpose, we define three different geographic perimeters that correspond to various distances from the landfall area: the *disaster zone*, the *neighborhood* area, and the *rest of the US mainland*. The *disaster zone* includes all counties affected by the hurricane according to the SHELDUS database. The *neighborhood* area is obtained through a matching procedure between affected counties and non-affected counties according to geographical distance. Under this procedure, we first assign a latitude and longitude to each county using the average latitude and average longitude of all the cities located in the county. For each affected county, we next compute the distance in miles to every non-affected county using the Haversine formula.[12] We then match with replacement each affected county with its five nearest neighbors among the non-affected counties.[13] This procedure leaves us with a set of matched counties that constitute our neighborhood area and a set of non-matched counties that form the *rest of the US mainland* area. Figures 1.1 to 1.4 present the results of this identification procedure on a map for hurricanes Fran, Floyd, Allison and Katrina.

[INSERT FIGURES 1.1 TO 1.4 AROUND HERE]

Firms located in the *neighborhood* area (represented by the light blue zone on the map) are assigned to the treatment group because the hurricane landfall should be a salient event for the managers of such firms. Given their proximity to the disaster zone, the hurricane is indeed a near-miss event, meaning that they could have been affected by the hurricane but were not by chance. For that reason, we expect the event to raise firm managers' attention. Firms located

[12] The Harversine formula gives the distance between two points on a sphere from their longitudes and latitudes.
[13] We find that on average, a county has approximately five adjacent counties. Our results remain the same when we use three or four rather than five nearest non-affected counties.

in the *rest of the US mainland* (the blank zone on the map) are assigned to the control group. Given their distance from the landfall area, the hurricane should not be a salient event for the managers of these firms. Some of these managers may even completely ignore the event if they are located in an area in which the risk of a hurricane strike is not of concern. Firms located in the *disaster zone* (the dark blue zone on the map) are separated in our analysis because of the direct effects of the hurricane on their cash levels. Given their location, these firms are affected by the disaster. The event is not only obviously salient for their managers but is also a potential source of direct cash outflow (e.g., replacement costs of destroyed operating assets) or cash inflow (e.g., receipt of the proceeds of insurance claims). The variation of cash holdings surrounding the hurricane event is thus more likely to reflect the direct effects of the disaster rather than the change in managerial perceived risk. In practice, we do not remove these firms from our sample.[14] Instead, we control to ensure that the variation of cash holdings that we observe when these firms are affected by the hurricane does not influence our results. Panel B of Table 1.2 presents summary statistics for each group of firms.

[INSERT TABLE 1.2 AROUND HERE]

The statistics are mean values computed one quarter before a hurricane's occurrence. The last column shows the t-statistic from a two-sample test for equality of means across treated and control firms. Treatment firms and control firms appear to be similar along various dimensions, including the amount of cash holdings.

### *1.4.4. Methodology*

[14] In fact, we cannot exclude these firms because these firms can also be in the neighborhood of another hurricane at another point in time. Because we are considering various hurricane strikes over time, it is possible that the same firm may be in each of the three groups defined in our experiment (*disaster zone*, *neighborhood*, and *the rest of the US mainland*).

We examine the effect of the hurricane saliency on managers' risk perception through changes in the levels of corporate cash holdings using a difference-in-differences estimation. We follow the specification proposed by Bertrand and Mullainathan (2003) to handle situations with multiple time periods and multiple treatment groups. The basic regression we estimate is

$$Cash_{itc} = \alpha_i + \delta_t + \gamma X_{itc} + \beta Neighbor_{tc} + \varepsilon_{itc}$$

where *i* indexes firm, *t* indexes time, *c* indexes county location, $Cash_{itc}$ is the amount of cash as a percentage of total assets at the end of the quarter, $\alpha_i$ are firm fixed effects, $\delta_t$ are time fixed effects, $X_{itc}$ are control variables, $Neighbor_{tc}$ is a dummy variable that equals one if the county location of the firm is in the neighborhood of an area hit by a hurricane over the last 12 months and zero if not, and $\varepsilon_{itc}$ is the error term that we cluster at the county level to account for potential serial correlations (Bertrand, Duflo and Mullainathan, 2004).[15]

Firm fixed effects control for time invariant differences among firms (which include fixed differences between treatment and control firms). Time (year-quarter) fixed effects control for differences between time periods, such as aggregate shocks and common trends. The other variables, $X_{itc}$, systematically include a dummy variable $Disaster\_zone_{tc}$ to capture the effect of the hurricane strike when the firm is located in the disaster zone. This $Disaster\_zone_{tc}$ variable enables the comparison of firms in the neighborhood area with firms farther away (the rest of the US mainland) by isolating the changes in cash holdings observed when firms are located in the disaster zone from the rest of our estimation.[16] Our estimate of the effect of hurricane landfall proximity is $\beta$, which is our main coefficient of interest. It measures the change in the level of cash holdings after a hurricane event for firms in the neighborhood of the disaster area relative to a control group of more distant firms.

[15] Allowing for correlated error terms at the state level or firm level leads to similar inferences in the statistical significance of regression coefficients.

[16] When firms are located in the disaster area, changes in cash holdings are likely to be caused by the direct effects of the hurricane.

## 1.5. Do managers overreact to salient risks?

### *1.5.1. Main results*

We examine the effect of the event availability on the risk perceived by firm managers through differences in corporate cash holdings after a hurricane landfall. Tables 1.3 and 1.4 present our main results.

[INSERT TABLE 1.3 AROUND HERE]

Table 1.3 reports the effects of being in the neighborhood of a disaster area in the 12 months after a hurricane. Column 1 shows that, on average, firms located in the neighborhood of a disaster zone increase their cash holdings (as % of total assets) by 0.84 percentage points during the four quarters following the hurricane event. This effect represents an average increase in cash holdings of 16 million dollars in absolute terms and accounts for 8% of the within-firm standard deviation of cash holdings.

We investigate the robustness of this effect in the rest of Table 1.3. First, our results may capture within-year seasonality. Because hurricane activity is seasonal, firms in the neighborhood area might anticipate the possibility of hurricane strikes and hold more cash at the end of the third quarter of the year. We control for this possibility by using firm-quarter fixed effects rather than firm fixed effects. Second, our result might be driven by industry-specific shocks. Thus, we use year-quarter-SIC3 fixed effects rather than year-quarter fixed effects to control for differences between time periods by industry (SIC3). Column 3 shows that the inclusion of these two high-dimension fixed effects does not alter our estimation. In fact, the magnitude of the effect of hurricane proximity on cash holdings remains exactly the same. In column 3, we show that this effect is robust to the inclusion of firm-specific controls: age, size and market-to-book. Because such controls might be endogenous to the proximity of

a hurricane disaster, we do not include them in our basic specification.[17] Similar to Bertrand and Mullanaithan (2003), we prefer to verify that our findings are not modified by their inclusion.[18] Overall, the effect is extremely robust to the different specifications, and the magnitude of the coefficient is always the same. Consistent with the *availability heuristic* hypothesis, managers respond to the sudden salience of danger by increasing their firm cash holdings, although there is no indication that the danger is bigger now than it was.

[INSERT TABLE 1.4 AROUND HERE]

In Table 1.4, we examine how the effect of hurricane proximity on cash holdings changes over time. Specifically, we study the difference in the level of cash holdings between treated and control firms at different points in time before and after hurricane landfall. To do so, we replace the *Neighbor* variable with a set of dummy variables, *Neighbor_q(i)*, that captures the effect of the saliency of the event at the end of every quarter surrounding the hurricane. For each quarter *i* (-*i*) after (before) the hurricane, we create a variable, *Neighbor_q+i*, that is equal to one if the county location of the firm headquarters at the end of the quarter was in the neighborhood of an area hit by a hurricane during quarter *q0* and zero otherwise. The regression coefficient estimated for this dummy variable then measures the difference-in-differences in the level of cash holdings i (-i) quarters after (before) the disaster. We undertake the same procedure for the *Disaster_zone* variable. This approach allows us to identify when the effect starts and how long it lasts. Column 1 of Table 4 shows that no statistically significant change in cash holdings appears before the hurricane event for firms located in the neighborhood area. However, consistent with a causal interpretation of our result, we do find that the amount of cash begins to increase following the occurrence of the

[17] See Roberts and Whited (2012) for a discussion about the effect of including covariates as controls when they are potentially affected by the treatment.
[18] Similarly, this result does not change when other control variables frequently associated in the literature with the level of cash holdings are added, such as capital structure, working capital requirements, capital expenditures, or R&D expenses.

hurricane.[19] This effect increases during the subsequent three quarters, and the increases in cash holdings reach their maximum during *q+2* and *q+3*. The coefficient for the *Neighbor_q+2* and *Neighbor_q+3* variables show that, on average, firms located in the neighborhood area respond to the saliency of the disaster by increasing their cash levels by 1.15 and 1.13 percentage points of their total assets (approximately 20 million dollars and approximately 11% of the within-firm standard deviation of *cash*) at the end of the second and third quarters after the hurricane, respectively. The level of cash holdings then begins to decrease, and the effect progressively vanishes over the next three quarters. The coefficient for the *Neighbor_q+8* variable shows that the average difference in cash holdings between firms in the neighborhood area and control firms is not statistically different from zero two years after the hurricane landfall.

This drop in the amount of cash holdings is consistent with our behavioral interpretation. As time goes by, memories fade, the salience of the event decreases, and the subjective probability of risk retreats to its initial value. Managers then reduce the level of corporate cash holdings.

[INSERT FIGURE 1.6 AROUND HERE]

We plot the result of this analysis in a graph in which we also display the evolution of the difference in corporate cash holdings between firms located in the *disaster zone* and control firms. This graph is presented in Figure 1.6. While firms in the neighborhood area experience a temporary increase in cash holdings, firms hit by the hurricane display a symmetric decrease. This "reversed mirror" trend is notable for two reasons. First, it confirms that the occurrence of a hurricane can trigger a liquidity shock, as firms hit by a hurricane experience a significant drop of 0.6 percentage points in their cash holdings. Second, it offers an indication of the magnitude of the increase in cash observed when firms are located in the

[19] The positive and statistically significant effect for *Neighbor_q0* does not contradict our interpretation. Indeed, *q0* is the first balance sheet published *after* the event and therefore shows the change in cash that occurs *in reaction to* the hurricane.

neighborhood area. Indeed, the graph demonstrates that the additional amount of cash accrued in the balance sheet (+1.1 percentage points of total assets), presumably for insurance purposes against the risk of cash shortages after a hurricane strike, exceeds the actual loss of cash (-0.6 percentage points) that firms experience when this risk materializes. Thus, even if the increase in cash holdings observed for firms in the neighborhood area was justified, the magnitude of this increase would be excessive compared to the real loss of cash at risk. However, we do recognize that the loss of cash (-0.6%) we observe here may not correspond to the real economic cost of the hurricane. We address this issue in Section 7 when we examine market reaction at the time of landfall. We find that the present value of losses caused by the disaster represents 1.03% of the total assets of the firm, on average, which remains lower than the increase in cash observed in firms located in the neighborhood area (+1.1%).

### *1.5.2. Cross sectional variation in managers' responses*

Because firms have different characteristics, they may not respond in the same way to the salience of hurricane risk. We first investigate whether this response changes with the degree of sophistication of firm decision makers. Our primary proxy for sophistication is the experience of a firm's managers in terms of hurricane proximity. Indeed, we expect managers to learn from past experiences and to be less sensitive to danger saliency if they have previously been "fooled." In practice, we count the number of instances in which a firm has been located in the neighborhood area during previous hurricane events. We then split our sample into three categories of sophistication (low, medium, and high). Firms are assigned to the low (medium or high) sophistication category if their headquarters were never (once or more than once, respectively) located in the neighborhood area during a prior hurricane event.

To complement this analysis, we also use two more indirect proxies for sophistication: firm size and age. We use firm's size because we expect large firms to be run by sophisticated CEOs and CFOs (e.g. Krueger Landier and Thesmar, 2011). We use the age of the firm because various studies in the behavioral literature show that young age is more associated with behavioral biases (Greenwood and Nagel, 2009; or Malmendier and Nagel, 2011). Each period, we split our sample into terciles of firm size and terciles of firm age, and we assign firms to the high, medium, or low sophistication category if they belong to the high, medium, or low tercile of the distribution, respectively.

For each criterion (experience, size, and age), we define three dummy variables corresponding to each sophistication category (e.g., *Low Sophistication*, *Medium Sophistication*, *High Sophistication*). We then interact each dummy variable with the *Neighbor* variable to investigate how the response to the salience of hurricane risk varies with the degree of managerial sophistication.

[INSERT TABLE 1.5 AROUND HERE]

Columns 1 to 3 of Table 1.5 indicate that a low degree of sophistication systematically leads to a strong increase in the amount of cash holdings. Conversely, we find no statistically significant change in cash holdings for firms whose managers are likely to be more sophisticated. In all three cases, an F-test indicates that the difference between the two coefficients (high vs. low) is statistically significant at the 1% or 5% level.

In the Internet Appendix, we further investigate how this response varies in the cross section of the population. First we find that managers of firms located in the neighborhood area have a stronger response to the salience of liquidity risk when their firms are more financially constrained. Second, we show that firms in the neighborhood area also respond more strongly when their firm is more vulnerable to a hurricane disaster. Specifically, the amount of corporate cash holdings increases more when a firm operates in an industry that

suffers higher losses in the case of hurricane disaster, when firms operate a business that is more difficult to insure, and when firms are less diversified geographically.

### *1.5.3. Robustness and validity check*

Our main source of concern is the slight heterogeneity between treated firms and control firms. Although these firms are fairly comparable along various dimensions, Table 1.2 indicates that some differences exist in terms of age and dividends. To ensure that our results are not driven by this heterogeneity, we combine our difference-in-differences approach with a matching approach. We match on SIC3 industry, size, age, market-to-book, financial leverage, working capital requirements, capital expenditures, and dividends. The results of this analysis as well as a detailed description of our matching procedure are presented in the Appendix. Overall, this analysis leads to the same conclusion as the one obtained with the simple difference-in-differences approach: firms located in the neighborhood area temporarily increase their level of cash holdings after the hurricane.

To ensure that this result is both valid and robust, we also conduct a series of additional tests that are described and reported in the Appendix. In particular, we run a placebo test in which we randomly change the dates of hurricanes to ensure that our results are driven by hurricane landfalls. We also re-run our main regression in many different ways to verify that our effect is robust to alternative specifications. Finally, we verify that our effect is not driven by the manner in which we scale corporate cash holdings. Thus, we re-run the main regression using firm size (total assets) as the dependent variable and find nothing.

## 1.6. Is the overreaction costly?

Because the liquidity risk remains unchanged, managers' decisions to temporarily increase cash holdings after a hurricane event are likely to be suboptimal in terms of resource

allocation. In this section, we examine whether this temporary increase in cash is costly for shareholders. We begin by analyzing the counterparts to this cash increase. Next, we study whether this response to risk saliency negatively impacts firm value by reducing the value of cash.

### *1.6.1. Source of cash*

The cash increase observed after the hurricane landfall may come from a variety of sources: an increase in revenues (*Sales Growth* variable) and operating profits (*EBIT Margin* variable), a drop in net working capital requirements (*NWC* variable), a drop in investments (*Net_investment* variable), a decrease in repurchases (*Repurchases* variable), a reduction of dividends (*Dividend* variable), or an increase in new financing (debt or equity) (*New_financing* variable). Because total assets include the amount of cash holdings, we do not normalize these items by total assets and instead use the amount of sales (unless the literature suggests another more relevant normalization method). Next, we replicate our difference-in-differences analysis and apply our basic specification to each item separately.[20] The results of this analysis are reported in Table 1.6.

[INSERT TABLE 1.6 AROUND HERE]

We begin by examining whether hurricanes affect operating activity. Column 1 shows that, on average, the occurrence of a hurricane has no significant effect on revenues for firms located in the neighborhood area of the disaster zone. While sales growth decreases by 2.4 percentage points relative to the control group for firms hit by the hurricane, we find no evidence that the relative sales growth for neighborhood firms is affected by the proximity of the disaster. Column 2 confirms that neighborhood firms are truly unaffected in terms of operating activity. Unlike firms in the disaster zone, firms located in the neighborhood area

[20] We include firm-quarter fixed effects rather than firm fixed effects in the specification to adjust for within-year seasonality. Using firm fixed effects leads to the same results.

suffer no significant decrease in operating margin (the coefficient on the *Neighbor* variable is not statistically different from zero).

In the rest of Table 6, we examine other possible channels through which the change in cash holdings may occur. We find no evidence that the proximity of the hurricane modifies either the investment activity (columns 3 and 4) or the financing activity (column 7). However, we find that the proximity of the disaster changes payout policies. Indeed, column 6 indicates that firms in the neighborhood area tend to pay lower dividends and retain more earnings after the hurricane (the coefficient on the Neighbor variable is negative and statistically significant at the 5% level). We find no evidence that neighborhood firms reduce the amount of repurchases after the hurricane (column 5).

In columns 8, 9 and 10, we further investigate whether hurricanes affect the payout policy or the financing policy. We use a linear probability model to assess whether hurricane landfalls affect the likelihood of stock repurchases, dividend payment, and new financing issues. In column 8, we find that the likelihood of a stock repurchase is lower in the case of hurricane proximity. Similarly, column 9 indicates a decrease in the probability of dividend payment. However, we find no change in the probability of new security issues in column 10.

Overall, these results suggest that, when located in the neighborhood area of a disaster zone, firm managers increase the amount of cash holdings by increasing earnings retention.

### *1.6.2. Value of cash*

We next investigate whether this change in cash holdings is an efficient decision or a source of value destruction for shareholders. If it is an efficient decision, the increase in cash holdings should translate into a similar increase in value for firm shareholders. If by contrast, cash would have been better employed otherwise, the additional cash accrued in the balance

sheet should be discounted and will not result in a similar increase in terms of market capitalization.

In our tests, we follow the literature on the value of cash (Faulkender and Wang, 2006; Dittmar and Mahrt-Smith, 2007; Denis and Sibilkov, 2010). We examine how a change in cash holdings leads to a change in market valuation for firms in the neighborhood relative to control firms over different time periods surrounding the hurricane event. We estimate the additional market value that results from a change in a firm's cash position by regressing the abnormal stock return of the firm on its change in cash holdings and various control variables. The coefficient for the change in cash holdings is then interpreted as a measure of the value of a marginal dollar of cash. Next, we interact this coefficient with a dummy variable, *Neighbor_q0*, that is equal to 1 if the firm is in the neighborhood area at time *q0*. This allows us to assess whether being in the neighborhood area of a hurricane marginally deteriorates or improves the value of a marginal dollar of cash. The abnormal return we use is the stock return in excess of the Fama and French (1993) size and book-to-market portfolio return. All control variables are those used in the cash value literature. We exclude from our analysis those observations that correspond to firms located in the disaster zone and to stocks that are not sufficiently liquid.[21] Finally, we perform this analysis for different time windows around the date of the hurricane strike to examine how the effect varies over time. The results of this analysis are reported in Table 1.7.

[INSERT TABLE 1.7 AROUND HERE]

In columns 1 and 2 of Table 1.7, we estimate the value of cash during two time periods that end before the occurrence of the hurricane. We find that being located in the neighborhood area at time *q0* does not change the value of cash before the occurrence of the hurricane. This result is reassuring as cash variations for these firms (Neighborhood area) are not yet

[21] Stocks not sufficiently liquid are defined as stocks with more than 50% of zero daily returns during the time window considered in the analysis (see Lesmond et al. (1999) for a discussion about the relationship between illiquidity and zero returns).

statistically different from those of other firms in the rest of the US mainland. However, when the time window begins to capture the hurricane event, the same analysis shows that the value of cash decreases for firms that are in the neighborhood area. In column 3, for instance, the interaction term between *Neighbor_q0* and *Change in cash* is negative and statistically significant. This result indicates that over a 6-month period surrounding the hurricane landfall, the value of a marginal dollar of cash decreases on average by 22 cents when the firm is located in the neighborhood area compared to an average value of 88 cents otherwise. In columns 4 and 5, we use larger time windows around the event, and we obtain similar results. Unsurprisingly, the effect disappears when the time window becomes too large (column 6) because firms located in the neighborhood area increase their level of cash holdings only temporarily.

Overall, these results suggest that the managerial decision to increase the amount of corporate cash holdings temporarily after hurricanes negatively impacts firm value by reducing the value of cash.

## 1.7. Are there any other alternative explanations?

In this section, we discuss alternative explanations to our results, namely, the possibility of "regional spillover," "change in risk," and/or "risk learning." We first examine and test the implications of each alternative interpretation. Next, we propose and perform another experiment based on earthquake risk whose design alleviates the concern that such alternative explanations are driving our findings.

### *1.7.1. The possibility of "regional spillover"*

First, cash might increase temporarily because of geographical externalities. Indeed, firms located in the neighborhood area could be indirectly affected by the hurricane. Such indirect

effects may then explain why the amount of cash holdings temporarily increases. We review the main possible regional spillover effects and test whether they are likely to drive our results.

*Higher business and / or investment opportunities*

A first spillover effect might arise if the hurricane creates new business or investment opportunities for firms in the neighborhood area. In this case, neighborhood firms may temporarily hold more cash because they make more profits or because they plan to invest in the disaster zone.[22] Under this possible interpretation of our results, firms located in the neighborhood area should thus perform better and invest more after the disaster. However, none of our findings in Table 6 are consistent with such predictions. Indeed, we find no evidence that the proximity of the hurricane positively impacts either growth in terms of revenue or operating income. In addition, we do not find that neighborhood firms invest more after the hurricane. In the Appendix, we further investigate how the hurricane affects the growth of sales for neighborhood firms relative to the control group at every quarter surrounding the disaster. The graph in Figure 1.7 illustrates the main outcome of this analysis.

[INSERT FIGURE 1.7 AROUND HERE]

This graph shows that growth in revenues for neighborhood firms does not increase significantly relative to the control group after the hurricane. Therefore, and unlike firms located in the disaster zone, firms located in the neighborhood area are on average truly unaffected. This conclusion is also supported by the analysis of the market reaction at the time of the hurricane landfall.

[INSERT TABLE 1.8 AROUND HERE]

[22] For instance, a firm operating in the building materials industry and located in the neighborhood area may face a significant increase in demand caused by new housing and reconstruction needs in the disaster zone. This firm may then temporarily have more revenues and hold more cash. Alternatively, this firm might take advantage of the difficulties faced by local competitors to invest in the disaster zone. In this case, such a firm could accumulate cash temporarily to seize new investment opportunities and would ultimately generate higher revenues.

In Table 1.8, we report the results of a simple event study analysis. For each group of firms (disaster area, neighborhood area, and the rest of the US mainland), we estimate the average Cumulated Abnormal Return (CAR) of the stock price over the hurricane event period. The methodology used to perform this event study is described in the Appendix. Unsurprisingly, we find a negative abnormal return for firms located in the disaster zone. However, we find no significant reaction for firms located in the neighborhood area, which suggests that investors perceive that there are no benefits (new business and/or investment opportunities) from the proximity of the natural disaster.[23]

*Higher business uncertainty*

A second form of spillover effect might arise if the hurricane creates locally higher business uncertainty. In this case, managers may decide to stop and/or postpone their investment projects. Neighborhood firms would then temporarily hold more cash. However, this explanation would imply a negative reaction at the announcement of the hurricane, which we do not find. We also do not find that firms in the neighborhood area reduce their investments in Table 1.6 (Column 4). We also explicitly test whether the proximity of the hurricane creates higher uncertainty.

We begin by examining whether the proximity of the hurricane affects the volatility of firm revenues.

[INSERT TABLE 1.9 AROUND HERE]

We use two different approaches to conduct this examination. In Panel A of Table 1.9, we estimate revenue volatility at the firm level using the standard deviation of sales growth in a time series. We estimate the standard deviation of the growth in revenues before and after the

[23] We also note that at the time of the event study, the change in cash holdings is not yet observable by market participants. Thus, finding no market reaction here is not inconsistent with the decrease in the value of cash observed afterwards in Table 10

hurricane for each firm over a four-quarter period.[24] We then test whether this standard deviation is higher for firms in the neighborhood are after the hurricane. In panel B of Table 1.9, we estimate revenue volatility at the county level using the standard deviation of sales growth in cross section. We estimate the standard deviation of the growth in revenues across all firms from the same county at every quarter surrounding the hurricane event. We then test whether this standard deviation at the county level is affected by the hurricane. Under both approaches, we find that the proximity of the hurricane strike does not significantly affect the variance in revenues.

[INSERT TABLE 1.10 AROUND HERE]

Our analysis of stock return volatility in Table 1.10 also provides evidence that the hurricane does not create higher uncertainty for firms in the neighborhood area. In Panel A, we follow a methodology proposed by Kalay and Loewenstein (1985) and use an F-test to assess whether a hurricane event affects stock return variances. We find that an F-test cannot reject at the 5% level the null hypothesis that the pre-hurricane and post-hurricane stock return variances are equal for the majority of firms in the neighborhood area (64.8%). We next compute stock return volatility at each quarter and test in Panel B whether this volatility changes for firms in the neighborhood area using our baseline specification; we again find that the proximity of the hurricane does not affect stock return volatility. Overall, these results suggest that investors do not perceive higher uncertainty after the hurricane.

*Higher financing constraints*

Other regional spillover effects include the possibility that the hurricane hurts the lending capacity of banks. If bank customers withdraw their deposits after the hurricane, banks located in the disaster zone and/or the neighborhood area may no longer be able to effectively

[24] Estimating the standard deviation over a longer time window leads to the same results.

finance the local economy. Firms in the neighborhood might anticipate that banks will be constrained after the shock and may decide to hold more cash as a precaution. Under this explanation, the amount of new credits at the bank level should decrease after the hurricane. We test this prediction in the Appendix and find the opposite result. In fact, the amount of new commercial and industrial loans increases after the hurricane event for banks located in the disaster zone and for banks located in the neighborhood area relative to other banks. This result casts doubts on the possibility that the hurricane damages the entire local bank lending capacity. It is also consistent with our findings in Table 1.6 that the proximity of the hurricane does not negatively affect the probability of issuing new financing (Column 10).

*Other forms of regional spillover effects*

Because a variety of other forms of regional spillover effects might affect our results, we conduct another series of tests in which we focus on firms operating outside of the disaster zone and outside of the neighborhood area.[25] To the extent that these firms are less dependent on the local economy, any increase in corporate cash holdings should be less likely to be driven by a regional spillover effect. The results of these tests are reported in Table 1.11.

[INSERT TABLE 1.11 AROUND HERE]

In the first column, we re-run our main test and focus on firms operating in "tradable good" industries only.[26] Firms producing tradable goods (i.e., goods that can be sold in locations distant from where they are produced) can sell their products throughout the US market. Therefore, they should be less dependent on local market demand. Column 1 indicates that neighborhood firms operating in tradable-good sectors also increase the amount of their corporate cash holdings after a disaster.

[25] One such type of spillover might be related to hurricane insurance. Hurricanes may increase the costs of local insurance companies that must compensate their clients for hurricane-related damages. Local insurance companies react by increasing insurance premia after hurricanes, and local companies may react to such increased insurance costs by reducing their level of insurance and by increasing their level of cash instead. After some time, insurance premia return to normal levels, and firms insure again and decrease their cash holdings accordingly.

[26] We follow the classification proposed by Mian and Sufi (2012).

In the second column, we define two groups of neighbors according to geographical distance. Specifically, we define a fourth category of firms that correspond to firms located in the neighborhood of the disaster zone but not in its close neighborhood (hereafter, a "Remote Neighbor"). To identify these firms, we match with replacement each affected county with its ten nearest neighbors among the non-affected counties. Firms are then assigned to the Remote Neighbor group if their headquarters are located in the ten nearest non-affected counties but not in the five closest. The regression in Column 2 indicates that these firms also respond to the occurrence of the hurricane by increasing the amount of corporate cash holdings.

In the third column, we focus on firms that are neither in the disaster area nor in the neighborhood area but that are sensitive to the risk of hurricane strike because they have been strongly affected once by a hurricane during the sample period.[27] We create a dummy variable *Vulnerable* that is equal to one if the firm is identified as sensitive to the risk of hurricane disaster and zero if not. The regression in Column 3 indicates that the managers of these firms also increase cash holdings after the hurricane.

Overall, these results suggest that while some regional spillover effects may possibly affect firms in the neighborhood area, these effects cannot be the key explanation of our primary finding.

*1.7.2. The possibility of a "change in risk"*

Cash holdings might also increase if the real probability of being struck by a hurricane increases. However, this explanation would imply a permanent increase in cash, which we do not find in our results. To be consistent with a "change in risk" interpretation, the increase in risk must be temporary.

[27] To detect these firms, we first compare the growth in revenues observed in the data after each disaster with the prediction from the regression specified in Table D and reported in the Appendix. Next, we exclude firms whose actual sales growth is higher than predicted. A firm is then defined as vulnerable if the difference between its actual and predicted sales growth is lower than the median of the distribution.

Such a temporary increase in risk might occur if hurricane strikes cluster in certain geographic areas during a one-year or two-year period. In this case, being a neighbor could indicate that the probability of being hit by a hurricane in the coming year is now higher than it used to be. We are not aware of any evidence of such a clustering phenomenon in the climate literature (see section 2). Nevertheless, we assess this possibility by testing whether the probability of being hit by a hurricane depends on the geographical location of past hurricane strikes. In other words, we test whether being in the neighborhood of an area hit by a hurricane affects the probability of being hit by a hurricane in the next two years following a hurricane event. The results of this test are reported in the Appendix. We find no evidence that being in a county located in the neighborhood of an area affected by a hurricane over the past two years affects the probability of being hit by a hurricane.

#### *1.7.3. The possibility of "risk learning"*

Finally, cash holdings might increase if managers ignore or underestimate the risk before the occurrence of the hurricane and learn the true probability of a disaster after the hurricane's landfall. However, this explanation would again imply a permanent increase in cash, which we do not find.

It is also difficult to reconcile such a risk-learning hypothesis with our results regarding the value of cash. If managers learn the true probability of suffering a liquidity shock and increase their cash holdings accordingly, investors should value this decision positively and should not discount the additional cash in the balance sheet.

#### *1.7.4. Reaction to extreme earthquakes outside the US*

To further alleviate the concern that our results are driven by a non-behavioral explanation, we perform one final experiment based on earthquake risk rather than hurricane risk. We test the validity of the *availability heuristic* hypothesis by looking at US firms whose headquarters are located in urban communities in which earthquakes are frequently felt. We then focus on the announcement of extremely violent (and therefore salient) earthquakes outside the US and examine whether these firms respond to such announcements by changing the amount of their cash holdings. Finding an increase in cash holdings would then be consistent with the *availability heuristic* hypothesis while allowing us to rule out other possible explanations. Indeed, it would neither be consistent with the *change in risk* hypothesis nor with the *risk-learning* hypothesis because the occurrence of an earthquake outside the US (for instance, in Pakistan) provides no information about the likelihood of experiencing an earthquake in US territory.[28] It would also not be consistent with the *geographical spillover* hypothesis because of the distance to the disaster area. We obtain information about the level of intensity felt by zip code address for each earthquake from the "*Did you feel it?*" surveys performed under the Earthquake Hazard Program by the USGS. For each zip code, we compute the average earthquake intensity felt over the past 20 years. We assign the average earthquake intensity felt to each firm in Compustat using the zip code from the headquarters' address. We then focus on firms within the top 10% of the average intensity felt distribution and assign them to a seismic zone group (treatment group). All other firms are assigned to a non-seismic zone group (control group). Next, we focus on the strongest earthquakes that have occurred outside the US in the past 30 years according to descriptions of magnitude, total deaths, and total damage. We obtain all this information from the Significant Earthquake Database.[29] These selection criteria lead to the list of major non-US earthquakes described in the Appendix. We

[28] In addition, this test focuses on US firms whose managers frequently feel earthquakes. Thus, they cannot ignore this risk. This also casts doubts on the possibility of a learning reaction.

[29]National Geophysical Data Center/World Data Center (NGDC/WDC) Significant Earthquake Database, Boulder, CO, USA. (Available at http://www.ngdc.noaa.gov/nndc/struts/form?t=101650&s=1&d=1)

then estimate the average change in cash holdings for the seismic zone group around the announcement of the earthquake outside the US using exactly the same matching methodology as the one previously used and described above for hurricanes. The results of this analysis are depicted in the graph of Figure 1.8.[30]

[INSERT FIGURE 1.8 AROUND HERE]

Figure 1.8 shows qualitatively the same pattern as that previously observed. Firm managers located in seismic areas respond to the sudden salience of earthquake risk by temporarily increasing the level of cash holdings compared to firms located outside a seismic zone. This analysis confirms that firm managers are subject to the availability bias while rejecting other non-behavioral explanations.

## 1.8. The effects of cash holdings on post-hurricane performance

If managers respond to the salience of hurricane risk by increasing corporate cash holdings, and if this reaction is motivated by seeking insurance against such risk, then we should expect cash holdings to protect firm revenues and reduce losses when this risk materializes. We run this falsification test in this section. We focus on firms affected by a hurricane event and examine how the level of cash holdings before the disaster affects firm performance in terms of sales growth after the disaster.

To perform this test, we again use a difference-in-differences methodology. We use an approach identical to that used to estimate the effect of a hurricane on cash holdings except that (i) firms in the treatment group are firms whose headquarters are located in the disaster area, (ii) firms assigned to the control group are all other firms, and (iii) the outcome variable we are interested in is growth in revenues. We estimate how firms that are directly affected by the hurricane perform in terms of sales growth relative to the control group after the disaster

[30] More details about our methodology and the detailed results are provided in the Appendix.

conditional on their level of cash holdings (low, medium or high) before the hurricane. The graph depicted in Figure 1.9 illustrates the main outcome of this analysis.[31]

[INSERT FIGURE 1.9 AROUND HERE]

This graph compares three categories of firms defined according to the level of their cash holdings before the hurricane (high, medium, or low) and shows how each category performs in terms of sales growth relative to the control group over time. All categories of firms appear to be negatively affected by the hurricane during the first two quarters following the hurricane event. On average, sales growth is approximately 9% lower for treated firms than for control firms during the second quarter following the disaster, and the economic magnitude of this revenue loss is similar across the three categories of firms. However, performance in terms of sales growth in subsequent quarters is different. Firms in the high cash tercile before the disaster rapidly catch up with firms in the control group in terms of sales growth. These high cash firms even temporarily outperform control firms and recover their loss of revenues within the year following the shock. By contrast, it takes approximately two years for firms in the low cash tercile to catch up with firms in the control group in terms of sales growth, and these low cash firms never recover their losses.

Overall, these results confirm that holding cash contributes to insuring against the effects of hurricane risk. They are consistent with our primary finding and help to explain why managers may be willing to increase the amount of corporate cash holdings when they perceive that the risk of a hurricane strike is higher.

## 1.9. Conclusions

In their seminal paper, Tversky and Kahneman (1973, 1974) observe that people have a tendency to develop heuristic rules to reduce the complex task of estimating probabilities.

[31] More details about our methodology and the detailed results are provided in the appendix.

They show that, although useful in general, relying on these rules can also produce mistakes. This paper provides direct evidence that firm managers rely on one such rule to assess risk: the availability heuristic. Using cash holdings as a proxy for risk management, we find that managers located in the neighborhood area of a hurricane landfall temporarily perceive more risk after the event even though the real risk remains unchanged. We show that this mistake, which is caused by the temporary salience of the danger, is costly and inefficient. It leads to reduce shareholders compensation and destroys firm value by reducing the value of cash. Over our sample period and across all firms, the total amount of cash temporarily immobilized because of this assessment bias is almost 65 billion dollars. Given the large and increasing diversity of risks that must be assessed every day by firm managers, our results suggest that the total real economic cost of this bias is likely to be considerable.

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## 1.11. Appendix

### *1.11.1. Complementary results and additional robustness tests*

In this appendix, we present some complementary results and a series of robustness tests mentioned in the chapter but not reported there for brevity. We also provide further details about the methodology used to implement our matching approach in section 1.5.3 and to perform the event study presented in section 1.7.1. The structure of this appendix follows the structure of chapter 1.

*Additional cross sectional tests (Mentioned in Section 1.5.2)*

We further investigate how managers' response to hurricane risk saliency varies in the cross section of the population. We examine how this response varies according to firm financial constraints, and to the vulnerability of the firm to hurricane disaster

First, we examine how this response changes according to the degree of financial constraints of the firm. Firms with low constraints should care less about the risk of hurricane since they can easily raise new funds in case of cash shortage. Conversely, firms more vulnerable to capital market imperfections should be more precautionary and more sensitive to this risk. We follow the literature and create a dummy variable FC which is equal to one if the firm is considered as financially constrained according to the following criteria: the lack of debt rating, the absence of dividend payment, and the firm dependence to external finance.[33] Columns 1 to 3 of Table 1.12 show that the cash holdings policy is no longer sensitive to the hurricane saliency when firms are not financially constrained. However, the interaction term between Neighbor and the FC dummy indicates that on average the level of cash holdings increases substantially when the firm is financially constrained. For firms which depend strongly on external finance for instance, having been in the neighborhood of a

[33] A firm does not have debt rating if the S&P Long-term Senior Debt Rating is missing or is reported as Default (D), Selective Default (S.D.), or Not Meaningful (N.M.)). A firm pays no dividend if the Dividend variable is equal to zero. A firm is dependent on external finance if it belongs to an industry whose average dependence ratio over the sample period is above the median of the distribution.

hurricane in the last twelve month entails an increase in cash by 1.61 percentage points of total assets (column 3).

Second, this response may also vary depending on a how vulnerable a firm is to a hurricane disaster. We use three criteria to assess this degree of vulnerability: the importance of economic losses by industry, the dependence on intangible assets rather than tangible assets which are easier to insure, and the dependence on the domestic US market. First, some industries are likely to be more vulnerable than others. To assess such vulnerability, we calculate the CAR of each firm affected by a hurricane at the time of the impact. Next, we compute the average CAR by industry (SIC3) and use this average CAR to determine whether an industry is likely to suffer strong economic losses in case of disaster. An industry is defined as vulnerable if its average CAR is in the lowest part of the distribution (first tercile). Second, we use the share of total assets which are intangible assets to assess whether the business of the firm is more difficult to insure. Firms are identified as vulnerable if this share is in the top tercile of the distribution. And finally, a firm is identified as vulnerable if it is exposed to the US market only. We follow Frésard and Valta (2012) and consider that a firm is mainly exposed to the US market if less than 20% of its revenues are generated outside the US. For each criterion (Most affected SIC3, Intangible assets, and Domestic), we define a dummy variable Vulnerable that is equal to one if the firm is considered as vulnerable and zero if not. We then interact this variable with the Neighbor variable to study how the response to the salience of hurricane risk varies with the degree of firm vulnerability. The results of this analysis are reported in Table 1.13. Column 1 to 3 show that the interaction term between Neighbor and Vulnerable is always positive and statistically significant, meaning that corporate cash holdings increases more when a firm is more vulnerable to hurricane disaster.

*Matching approach (Mentioned in Section 1.5.3)*

We use a kernel matching approach similar to the one proposed by Heckmann, Ichimura and Todd (1998) where the matched outcome for each treated firm is a weighted average of the effects observed on several non treated firms. In this approach, the weights are chosen so that the observations closer in terms of distance receive greater weight. In practice, we match each treated firm (neighborhood area) with all the control firms (rest of US mainland) from the same industry (SIC3) 6 months before the occurrence of the hurricane (ie. time q-3) . For each treated firm, we then compute the Mahalanobis distance to all matched firms along seven dimensions : size, age, market-to-book, financial leverage, working capital requirement, dividend and capital expenditures. The weight assigned to each matched firm is then given by

$$w_{i,j} = \frac{K\left(\frac{d_{i,j}}{h}\right)}{\sum_{k=1}^{k=n} K\left(\frac{d_{i,k}}{h}\right)}$$

where i indexes the treated firm, j indexes the matched firm, ni is the number of firms matched to i, di,j is the Mahalanobis distance between i and j, K(.) is the Gaussian density function and h is a bandwidth parameter. For each treated firm i, we follow Todd (1999) and simply set the bandwidth equal to the distance to the nearest matched j. This methodology allows to use a smaller bandwidth when the treated firm has more matched firms in its local neighborhood. The matched outcome is then the weighted average of the change in cash observed for all matched firms (ie. control firms from the same SIC3 industry).

The results of this analysis are presented in Table 1.14 and Figure 1.8. Table 1.14 and the graph from figure 1.8 show the same kind of pattern as the one already observed with the simple diff-in-diff approach. Firms located in the neighborhood area temporarily increase their level of cash holdings after the hurricane.

*Additional robustness tests (Mentioned in Section 1.5.3)*

To make sure that our primary finding is valid and robust, we also conduct a series of additional tests which are reported in Table 1.15.

First, we run a placebo test. We randomly change the dates of the hurricanes and find nothing (column 1). Then we re-run our main regression and find that our effect is robust to different specifications: we add as controls the main determinants of cash (size, age, market-to-book, debt, net working capital, capex and R&D) (column 2), we use all 23 major hurricanes reported in the SHELDUS database (and not only the ones with total damages above 5 billion dollars) (column 3), we change the definition of neighbor counties and use the three closer (column 4) or the seven closer (column 5) and finally we use annual data and find again a temporary increase in cash (column 6).

*Effects of hurricane proximity on revenues (mentioned in section 1.7.1)*

We examine how the effect of hurricane proximity on sales growth changes over time. Specifically, we study the difference in sales growth between treated and control firms at different points in time before and after the hurricane landfall. To do so, we use exactly the same specification used in Table 1.4, except that we use the Sales Growth rather Cash as our dependent variable. The results of this analysis are presented in Table 1.16. We find that the growth in revenues for neighbor firms is never statistically different from the growth in revenues of control firms. By contrast, firms located in the disaster zone and affected by the hurricane experience a significant drop in revenues compare to the control group during the year following the hurricane.

*Event Study Methodology (mentioned in section 1.7.1)*

The event window is defined as $[BOH_{c,h-1}$ ; $EOH_{c,h+1}]$, where *c* indexes county and *h* hurricane, and where *BOH (EOH)* is the beginning (end) of hazard date reported in the SHELDUS database. By definition, firms assigned to Treatment group or Control group are

not located in a county reported by SHELDUS. In this case, the event window is defined as [$Min(BOH_h)$-1 ; $Max(EOH_h)$+1], where $Min(BOH_h)$ ($Max(EOH_h)$) is the minimum (maximum) of the beginning (end) of hazard dates reported in the SHELDUS database for hurricane h. Because the events we are looking at overlap in time, we cannot assume the independence between the variances of security abnormal returns. To address this issue, we form an equally-weighted portfolio whenever the event windows perfectly overlap. For firms assigned to the neighbor group and control group, we obtain 15 portfolios because there are 15 hurricanes (and thus 15 different event windows). We obtain 74 portfolios for firms assigned to the disaster zone category (instead of 15) because all affected counties are not affected at the same time by the same hurricane. While some are affected on Monday, other can be affected on Tuesday and Wednesday as the hurricane moves across land.

For each portfolio *p*, the average abnormal return over the event window is then estimated as the parameter $AR_p$ in the equally-weighted market model (see Betton, Eckbo, Thorburn (2008))

$$\mathrm{r_{p,t}} = \alpha_\mathrm{j} + \beta_\mathrm{p}\mathrm{rm_t} + \mathrm{AR_p w_t} + \epsilon_\mathrm{p,t}, \qquad \text{with } \mathrm{t} = \mathrm{day}\{\mathrm{BOH_p} - 201; \mathrm{EOH_p} + 1\}$$

where $r_{jt}$ is the return to portfolio *p* over day *t*, $r_{mt}$ is the crsp equally-weighted market return, and $w_t$ is a dummy variable that takes a value of one if day *t* is in the event window and zero otherwise. This conditional event parameter approach allows us to easily incorporate variable-length event windows across portfolios and directly produces an estimate of the standard error of the Abnormal Return *AR*. To be included in the portfolio, a security must have at least 150 non missing and non zero returns over the estimation period (200 days), and no missing return over the event window (See Savickas (2003)). The cumulative abnormal return (CAR) to portfolio *p* over event window w is

$$\mathrm{CAR_p} = \mathrm{w_p AR_p}$$

where $w_p$ is the number of trading days in the event window. For each group, the average CAR is

$$\mathrm{ACAR} = \left(\frac{1}{\mathrm{N}}\right)\sum_{\mathrm{p}=1}^{\mathrm{T}} \mathrm{n_p CAR_p}$$

where *N* is the total number of securities, np is the total number of securities in portfolio p, and *T* is the total number of equally-weighted portfolios. Since the event windows do not overlap between portfolios, we can assume that the variances of the portfolio abnormal returns are independent. For each category, the variance of the average abnormal return is

$$\mathrm{V(ACAR)} = \left(\frac{1}{\mathrm{N^2}}\right)\sum_{\mathrm{p}=1}^{\mathrm{T}} \mathrm{n_p^2 w_p^{\,2} \sigma^2_{AR_p}}$$

where $\sigma_{ARp}$ is the estimated standard error of $AR_p$. The z-values are determined as

$$\mathrm{z} = \frac{\mathrm{ACAR}}{\sqrt{\mathrm{V(ACAR)}}}$$

*Effects of hurricane proximity on Bank Loans (mentioned in section 1.7.1)*

We examine the effect of hurricane proximity on Commercial and Industrial Loans (C&I Loans) at the bank level using the data from the FDIC database. This database provides "Reports of Income and Condition" (Call Reports) that include detailed quarterly financial and regulatory bank data for all commercial and domestic banks in the U.S. We include all banks in our sample provided that standard viability conditions of the bank are respected. [34] The outcome variable we are interested in is the amount of new commercial loans at the bank level. This variable corresponds to the change in commercial and industrial loans (RCON1766) relative to the previous quarter scaled by total assets (RCFD2170). We then use the same difference-in-differences methodology as the one used to measure how the proximity of the hurricane affects cash holdings over time. The results of this analysis are reported in Table 1.17. We find that on average the amount of new C&I loans increases after

[34] To be included in our sample, banks must have non zero or negative equity, total assets above 25 million dollars, consumer loans representing less than 50% of total assets, more than two years of existence, and non missing values on the commercial and industrial loans variable

the hurricane relative to control group for both, banks located in the disaster zone and banks located in the neighborhood area.

*Determinants of Disaster Likelihood (Mentioned in Section 1.7.2)*

We test whether the probability to be hit by a hurricane depends on the geographical location of past hurricane strikes. We use a linear probability model where the dependent variable is a dummy equal to 1 if the county is hit by the hurricane during a given quarter and where the main explanatory variable is a dummy equal to 1 if that county was in the neighborhood of a disaster area over the past 12 months. We add county fixed effects to account for the fact that the risk of a hurricane strike varies geographically. We also add quarter dummies to account for hurricane seasonality and cluster the observations at the state level. The results of this analysis are reported in Table 1.18. Column 1 shows that being in a county located in the neighborhood of an area affected by a hurricane over the past 12 months conveys no particular information about the probability to be hit during a given quarter. Similarly, column 2 shows that being in a county located in the neighborhood of an area affected by a hurricane over the past two years does not convey either any particular information about this risk. Column 3 and 4 show similar results when taking into account all hurricanes from the SHELDUS database (and not only the 15 biggest). Overall, these results go against the hypothesis of a change in risk.

*Reaction to extreme earthquakes outside the US (mentioned in section 1.7.4)*

We focus on the biggest earthquakes occurred during the past 30 years according to magnitude, total deaths, and total damages description. These selection criteria leads to the list of major non US earthquakes described in Table 1.19. We then estimate the average change in cash holdings for the seismic zone group around the announcement of the earthquake outside

the US using exactly the same matching methodology as the one already used for the hurricanes. The results of this analysis are reported in Table 1.20

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*Effects of cash holdings on post-hurricane performance (mentioned in section 1.8)*

We estimate the effects of cash holdings on post-hurricane performance using a difference-in-differences methodology. Our approach is the following: (i) firms assigned to the treatment group are firms whose headquarter is located in the disaster area, (ii) firms assigned to the control group are all the other firms, and (iii) the dependent variable is the growth in revenues. Next we split our sample into tercile of cash at every quarter. We create a dummy variable High (Medium) (Low) Cash Tercile that is equal to one if the firm belongs to the third (second) (first) tercile and zero if not. Then we examine at every quarter i around the hurricane whether a firm was in a high, medium or low tercile before the occurrence of the hurricane. For each quarter i, we then create a dummy variable Disaster_zone_q+i x High (Medium) (Low) Tercile if the firm was located in the disaster area at quarter q0 and was in the high (Medium) (Low) cash tercile at quarter q-1. We then estimate for each category of firm, how the occurrence of the hurricane affects their performance in terms of sales growth relative to the control group at every quarter around the event. We include Year x Quarter x Cash Tercile fixed effects to control for differences across time by category of firms in terms of cash holdings. The results of this analysis are reported in Table 1.21.

### *1.11.2. Variables definition*

Variables used in difference-in-difference estimations (in alphabetical order)

| | |
|---|---|
| *Age* | Number of years between the date of the current quarterly financial accounts and the date of the first quarterly financial accounts reported in Compustat |
| *Assets* | Total assets |
| *Cash* | Cash and cash equivalents scaled by total assets |
| *Debt* | Total debt: short term debt + long term debt scaled by total assets |
| *Dependence on External Finance* | SIC3 average ratio of capital expenditures minus operating cash flow over capital expenditures. Operating cash flow: income before extraordinary items + depreciation and amortization - change in working capital - capital expenditures |
| *Disaster zone* | Dummy equal to 1 if the county location of the firm headquarter is in an area hit by a hurricane over the past 12 months |
| *Dividend* | Total dividends over last year net income |
| *FC* | Dummy variable equal to 1 if the firm is financially constrained and zero otherwise |
| *High Sophistication* | Dummy variable equal to 1 if the sophistication of the manager is high and zero otherwise |
| *Intangible assets* | Long term assets - Net property, plants, and equipment scaled by long term assets |
| *Low Sophistication* | Dummy variable equal to 1 if the sophistication of the managers is low and zero otherwise |
| *Market-to-Book* | Market to book ratio. Equity market value over total equity |
| *Medium Sophistication* | Dummy variable equal to 1 if both High Sophistication and Low Sophistication are equal to zero |
| *Neighbor* | Dummy variable equal to 1 if the county location of the firm headquarter is in the neighborhood of an area hit by a hurricane over the past 12 months |
| *Ebit Margin* | Income before interests and taxes over total revenues |
| *Net Investments* | Total net cash flow from investing activities (capital expenditures + acquisition expenditures + R&D expenses - disposals) scaled by net property, plant and equipment |
| *Net Working Capital* | Inventories + receivables - payables scaled by total revenues |
| *New Financing* | Issuance of long term debt + sale of new stocks scaled by equity market value |
| *Sales growth* | Growth in total revenues relative to the same quarter of the previous year |
| *Repurchases* | Purchase of common and preferred stocks over last year net income |
| *R&D* | R&D expenses over total assets |
| *Size* | Log of total assets |
| *Vulnerable* | Dummy variable equal to 1 if the firm is vulnerable to hurricane disaster and zero otherwise |
| *ΔC&I Loans / Assets* | Change in Commercial and Industrial Loans relative to the previous quarter scaled by total assets at the bank level |

Variables used in the test on the value of cash reported in Table 1.7 (in alphabetical order)

| | |
|---|---|
| *Cash* | Cash and cash equivalents |
| *Earnings* | Net income before extraordinary items |
| *Dividends* | Common dividends |
| *Interest Expenses* | Interests expenses |
| *Non Cash Assets* | Total assets minus all cash and cash equivalents |
| *R&D* | R&D expenses (set to zero if missing) |
| *Leverage* | Total debt (long term debt + short term debt) over total debt + equity market value |
| *Lagged Cash* | Cash and cash equivalents at time q-2 over equity market value at time q-2 |
| *Net Financing* | New financing = Net new equity issue + Net new debt issue |

**Figure 1.1**

***Identification of Neighbors: Illustration for Hurricane Fran (1996)***

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Fran in 1996. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

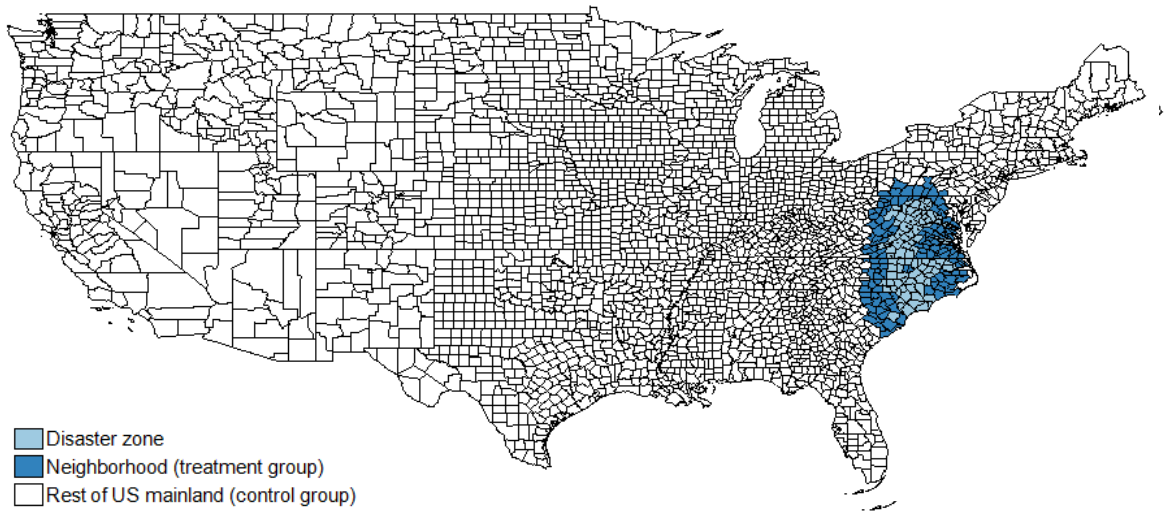

**Figure 1.2**

***Identification of Neighbors: Illustration for Hurricane Floyd (1999)***

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Floyd in 1999. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

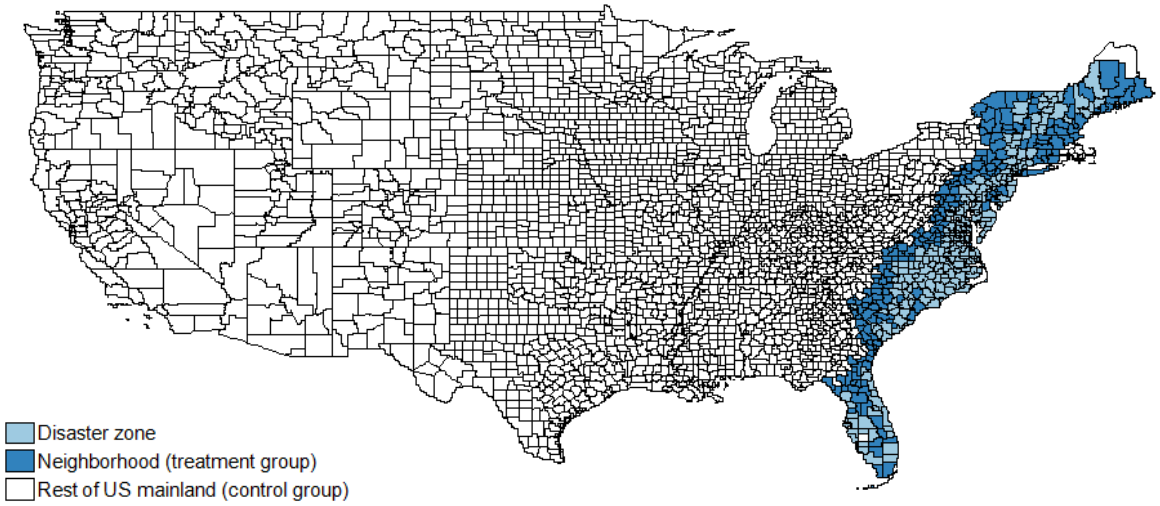

**Figure 1.3**

***Identification of Neighbors: Illustration for Hurricane Allison (2001)***

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Allison in 2001. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

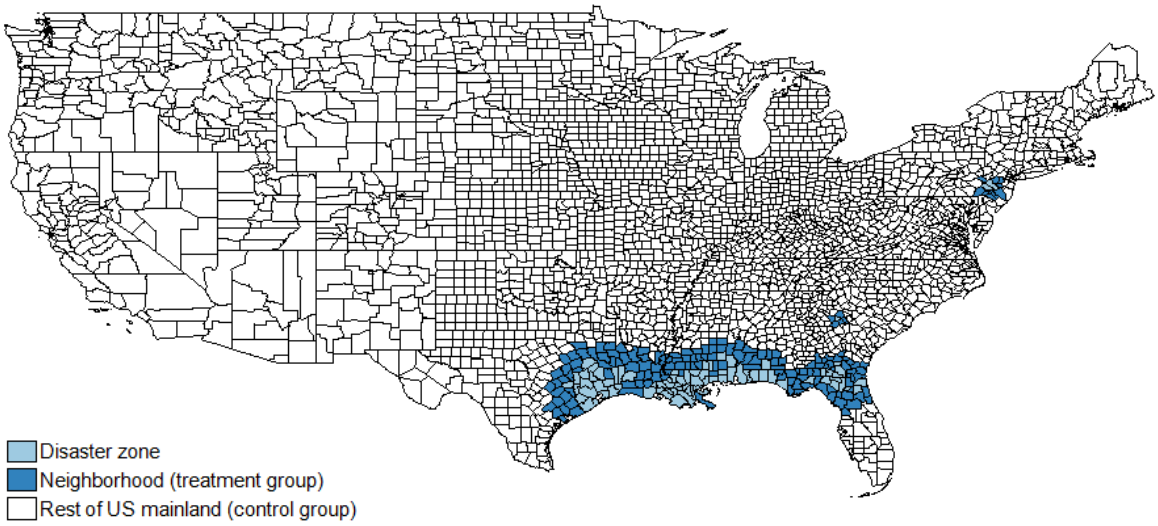

**Figure 1.4**

***Identification of Neighbors: Illustration for Hurricane Katrina (2005)***

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Katrina in 2005. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

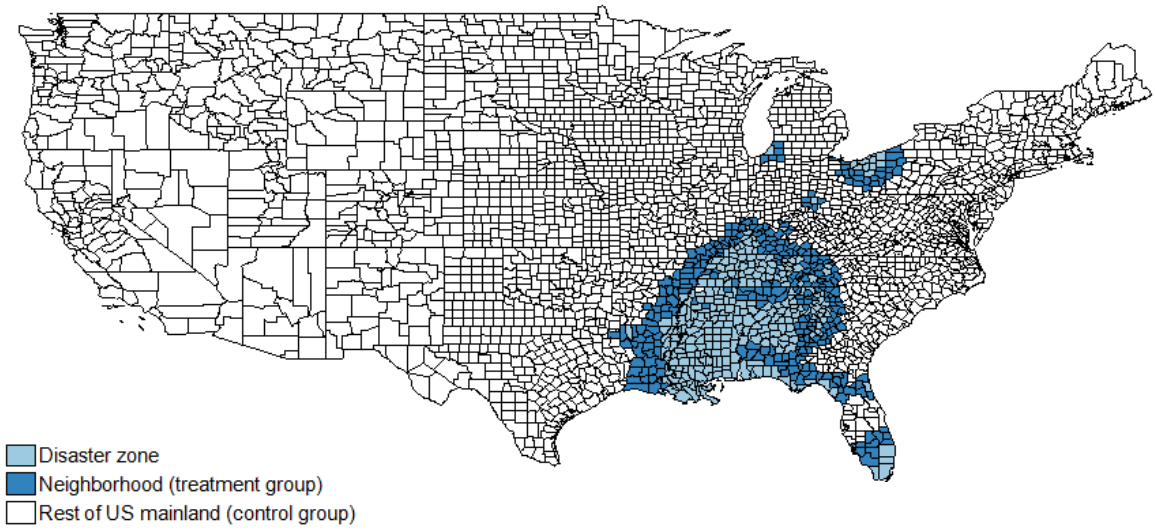

**Figure 1.5**

***Number of Hurricanes by Decade since 1850***

This graph presents the total number of hurricanes with landfall in the US mainland by decade since 1850. The source of the information is the NOAA Technical Memorandum (2011)

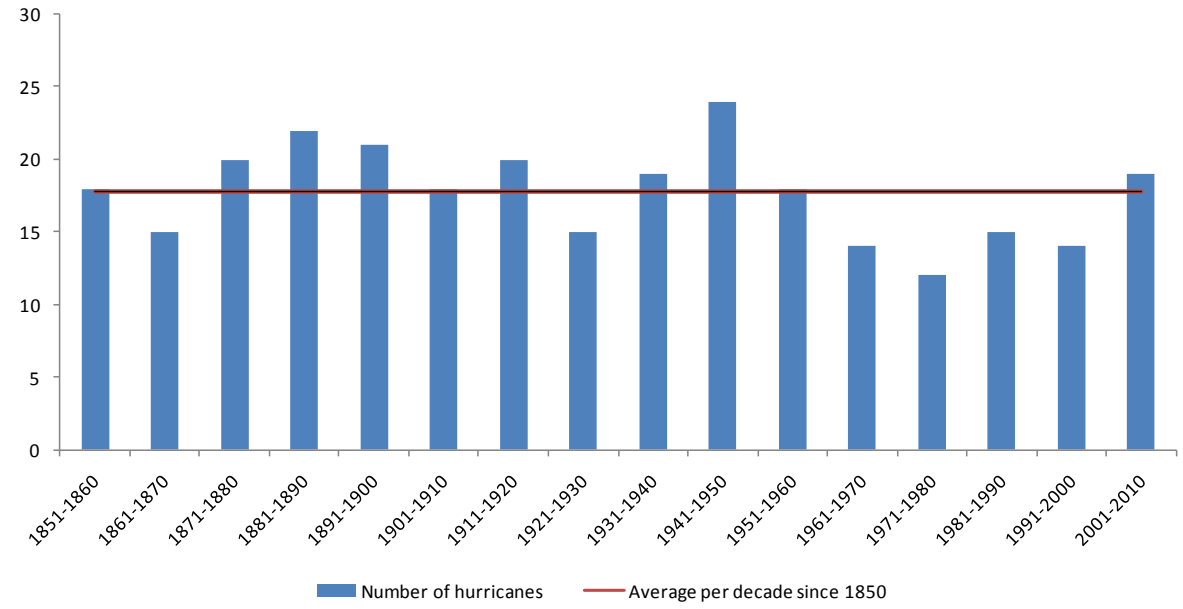

**Figure 1.6**

***Effects of Hurricane Proximity on Corporate Cash holdings***

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the hurricane event (quarter *q0).* The blue line plots the difference-in-differences in the level of corporate cash holdings for firms located in the neighborhood area. The red line plots the difference-in-differences in the level of corporate cash holdings for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table 4. ***, **, and * denote significance at the 1%, 5% and 10% levels.

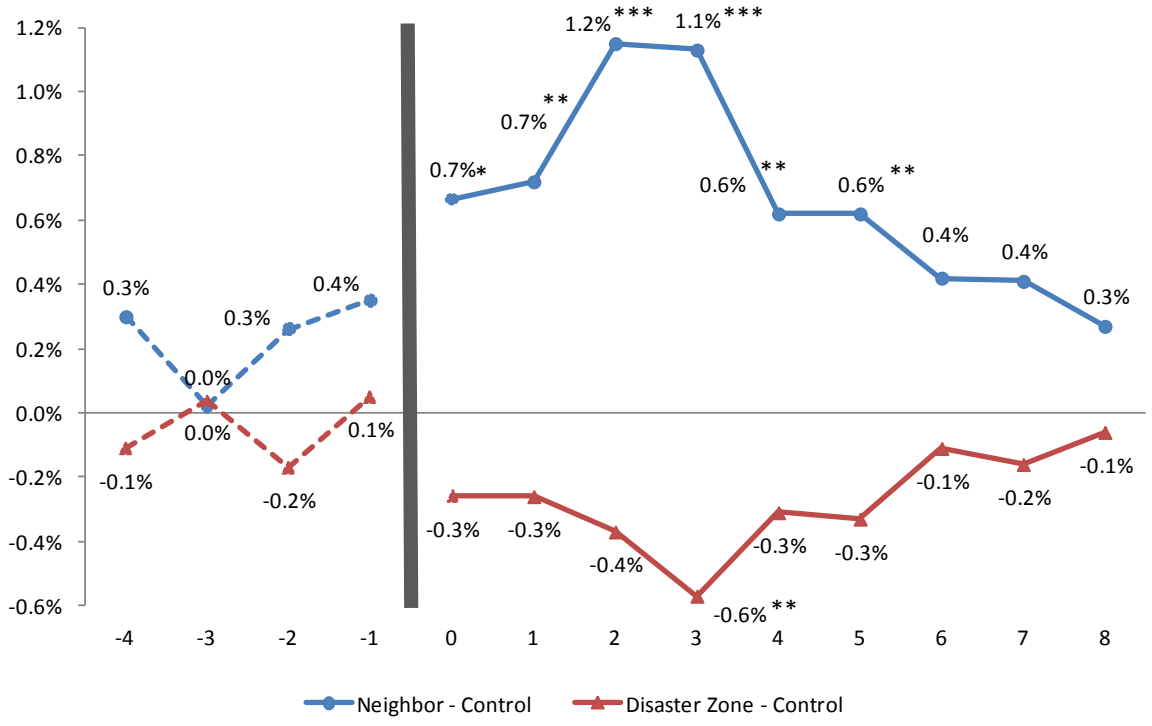

**Figure 1.7**

***Effects of Hurricane Proximity on Sales Growth***

This graph presents difference-in-differences in sales growth at different quarters surrounding the hurricane event (quarter *q0).* The growth in sales is the growth in total revenues relative to the same quarter of the previous year. The blue line plots the difference-in-differences in sales growth for firms located in the neighborhood area. The red line plots the difference-in-differences in sales growth for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table D reported in Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

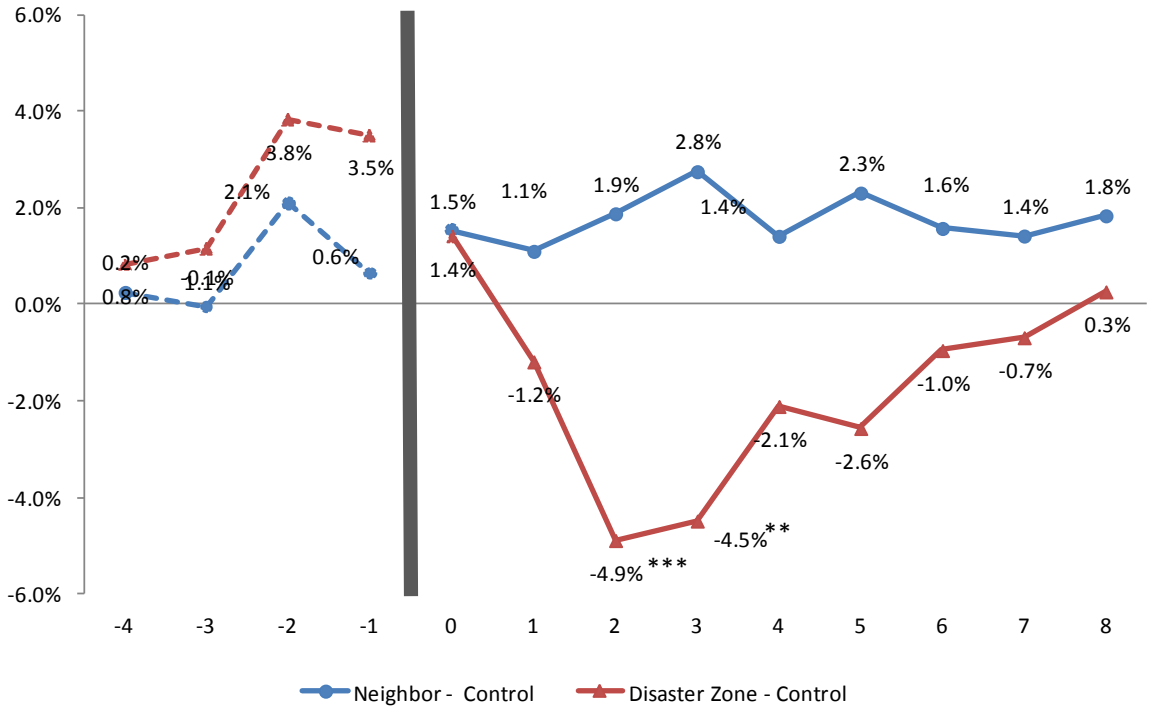

**Figure 1.8**

***Effects of Earthquakes outside the US on Corporate Cash holdings of US Firms***

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the announcement of a violent earthquake outside the US (quarter *q0)* for a sample of US firms located in a seismic area. This sample comprises 1,191 treated firms whose headquarters are located in a urban community where an earthquake is frequently felt according to the U.S. Geological surveys ("Seismic zone firms"). For each treated firm, the counterfactual outcome is the weighted average of the change in the level of cash holdings relative to q-2 over all control firms with the same SIC 3 code ("Matched firm"). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed at quarter *q-2* (ie. three months before the earthquake occurrence) along four dimensions: size, age, market-to-book, and financial leverage. ***, **, and * denote significance at the 1%, 5% and 10% levels.

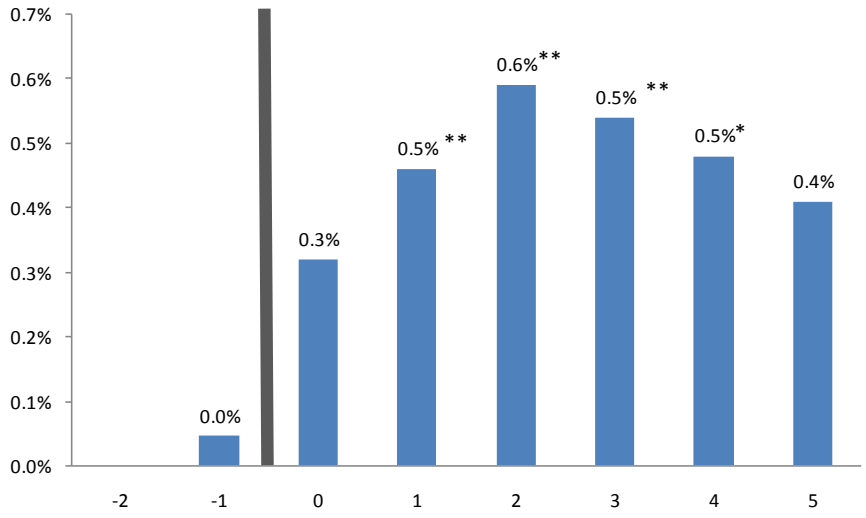

**Figure 1.9**

***Effects of Cash Holdings on Revenues of Firms Located in the Disaster Area***

This graph presents difference-in-differences in sales growth between firms located inside and outside the disaster area at different quarters surrounding the hurricane event (quarter *q0)* conditional on the level of corporate cash holdings before the occurrence of the disaster. The growth in sales is the growth in total revenues of the firm relative to the same quarter of the previous year. The blue (respectively, red, green) line plots the difference-in-differences in sales growth for the sub-sample of firms with a level of cash holdings in the top (respectively, middle, bottom) tercile of the distribution at the end of the quarter before the occurrence of the hurricane. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table I reported in Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

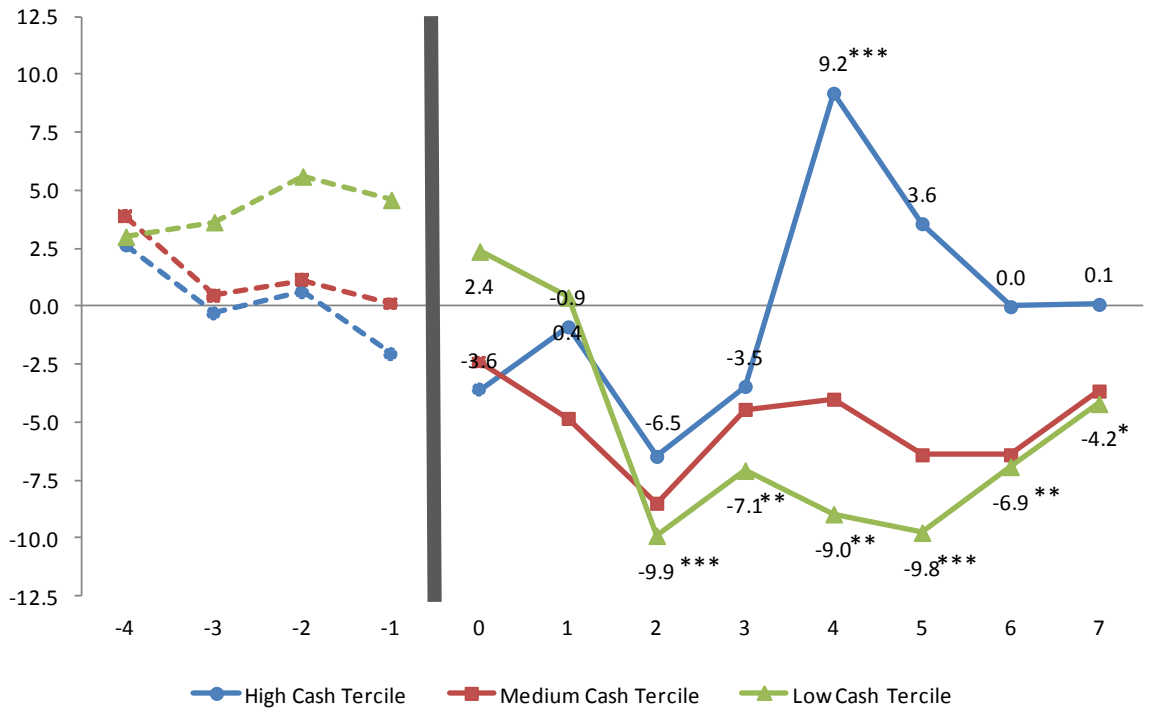

**Figure 1.10**

***Effects of Hurricane Proximity on Corporate Cash holdings over time (Matching)***

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters around the occurrence of the hurricane (quarter *q0)*. The sample comprises 2,060 treated firms whose headquarter is located in the neighborhood of an area hit by a hurricane during quarter *q0* ("Neighbor firms"). For each treated firm, the counterfactual outcome is the weighted average of the change in cash over all control firms with the same SIC 3 code ("Matched firm"). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed six months before the hurricane landfall at quarter q-3 along seven dimensions : size, age, market-to-book, financial leverage, dividend, capital expenditures and net working capital. ***, **, and * denote significance at the 1%, 5% and 10% levels.

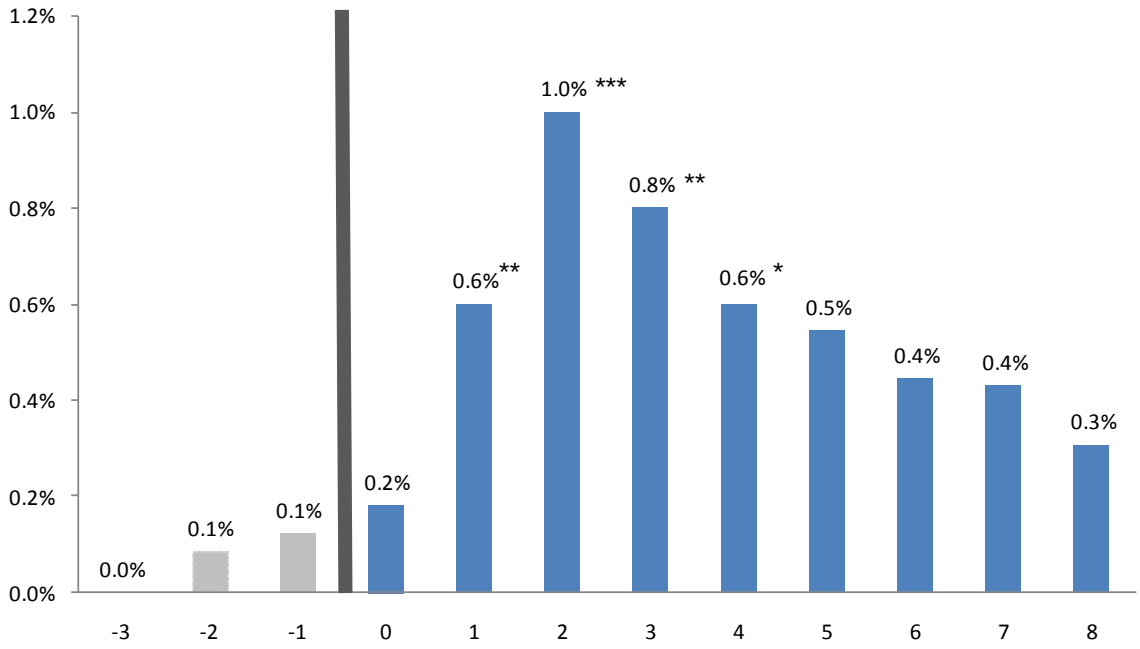

**Table 1.1**

***Major Hurricanes Landfall in the US Mainland over the 1987-2011 Period***

This table describes the 15 major hurricanes according to total damages (adjusted for inflation) that occurred in the US mainland over the 1987-2011 period. *Fatalities* is the estimated total number of direct deaths in the US mainland due to the hurricane. *Damages* is the estimated value of total direct damages due to tropical storms in the US mainland expressed in billion dollars. *Damages (CPI adjusted)* is the estimated value of total damages expressed in billion dollars adjusted for the Consumption Price Index as of 2010. *Category* measures the wind intensity according to the Saffir and Simpson Hurricane Wind Scale which ranges from 1 (lowest intensity) to 5 (highest intensity). Primary source of information is the SHELDUS database. Information about *Start date*, *End date*, *Landfall date*, *Damages* and *Fatalities* comes from the tropical storm reports available in the archive section of the National Hurricane Center website. Information about Category comes from the NOAA Technical Memorandum (2011).

| Name | Year | Start date | End date | Landfall date | Fatalities | Damages | Damages (CPI adjusted) | Category |
|---|---|---|---|---|---|---|---|---|
| Hugo | 1989 | 9/10/1989 | 9/22/1989 | 9/22/1989 | 21 | 7.0 | 12.3 | 4 |
| Andrew | 1992 | 8/16/1992 | 8/28/1992 | 8/24/1992 | 26 | 26.5 | 41.2 | 5 |
| Opal | 1995 | 9/27/1995 | 10/5/1995 | 10/4/1995 | 9 | 5.1 | 7.4 | 3 |
| Fran | 1996 | 8/23/1996 | 9/8/1996 | 9/6/1996 | 26 | 4.2 | 5.8 | 3 |
| Floyd | 1999 | 9/7/1999 | 9/17/1999 | 9/14/1999 | 56 | 6.9 | 9.0 | 2 |
| Alison | 2001 | 6/5/2001 | 6/17/2001 | 6/5/2001 | 41 | 9.0 | 11.1 | TS* |
| Isabel | 2003 | 9/6/2003 | 9/19/2003 | 9/18/2003 | 16 | 5.4 | 6.4 | 2 |
| Charley | 2004 | 8/9/2004 | 8/14/2004 | 8/13/2004 | 10 | 15.1 | 17.4 | 4 |
| Frances | 2004 | 8/25/2004 | 9/8/2004 | 9/5/2004 | 7 | 9.5 | 11.0 | 2 |
| Ivan | 2004 | 9/2/2004 | 9/24/2004 | 9/16/2004 | 25 | 18.8 | 21.7 | 3 |
| Jeanne | 2004 | 9/13/2004 | 9/28/2004 | 9/26/2004 | 4 | 7.7 | 8.8 | 3 |
| Katrina | 2005 | 8/23/2005 | 8/30/2005 | 8/25/2005 | 1,500 | 108.0 | 120.6 | 3 |
| Rita | 2005 | 9/18/2005 | 9/26/2005 | 9/24/2005 | 7 | 12.0 | 13.4 | 3 |
| Wilma | 2005 | 10/15/2005 | 10/25/2005 | 10/24/2005 | 5 | 21.0 | 23.5 | 3 |
| Ike | 2008 | 9/1/2008 | 9/14/2008 | 9/13/2008 | 20 | 29.5 | 29.9 | 2 |

(*) "TS" : Tropical Storm

**Table 1.2**

***Descriptive Statistics***

This table reports firm-level summary statistics. Panel A reports statistics of the main firm-level variables over the 1987-2011 period. Panel B presents average values of the variables for treated and control firms one quarter before the hurricane strike. Treated and control firms are defined according to their headquarter locations. The last column shows the t-statistic from a two-sample test for equality of mean across treated and control firms. All variables are from Compustat Quarterly, excluding financial, utilities and non US firms. All variables are winsorized at the 1st and 99th percentiles. The variables are defined in Appendix 1.

Panel A: Firm Level Statistics

| | N | Mean | SD | P25 | Median | P75 |
|---|---|---|---|---|---|---|
| Age | 411,490 | 10.0 | 7.8 | 3.8 | 8.0 | 14.5 |
| Assets | 411,490 | 1,156 | 3,716 | 19 | 95 | 510 |
| Cash | 411,490 | 18.0% | 22.4% | 2.0% | 7.8% | 26.0% |
| Debt | 409,801 | 29.8% | 34.8% | 3.8% | 21.8% | 41.9% |
| Dividend | 210,680 | 11.0% | 20.7% | 0.0% | 0.0% | 14.4% |
| EBIT Margin | 397,098 | -54.8% | 246.6% | -9.1% | 4.5% | 11.5% |
| Market-to-Book | 359,449 | 2.8 | 6.7 | 1.0 | 1.9 | 3.5 |
| Net Investments | 382,576 | 41.9% | 120.5% | 2.9% | 8.1% | 28.9% |
| Net Working Capital | 408,392 | 13.8% | 47.6% | 5.8% | 16.0% | 27.1% |
| Repurchases | 209,049 | 25.7% | 88.8% | 0.0% | 0.0% | 0.4% |
| Sales Growth | 371,703 | 23.8% | 73.6% | -6.2% | 8.2% | 28.2% |

Panel B: Descriptive Statistics for Treated and Control Firms

| Firm Headquarter Location Group Assignement | Disaster Zone Excluded | Neighborhood Treatment | Rest of US Control | *t* -statistic |
|---|---|---|---|---|
| Age | 10.9 | 11.2 | 10.2 | 2.14** |
| Assets | 1,316 | 1,308 | 1,135 | 1.15 |
| Cash | 14.5% | 18.1% | 18.7% | -0.41 |
| Debt | 33.0% | 30.0% | 29.0% | 0.96 |
| Dividend | 8.4% | 8.9% | 10.4% | -1.95* |
| EBIT Margin | -62.2% | -59.4% | -55.3% | -0.55 |
| Market-to-Book | 2.90 | 3.08 | 2.85 | 1.34 |
| Net Working Capital | 10.2% | 12.2% | 13.5% | -1.02 |
| Net Investments | 38.3% | 44.5% | 44.7% | -0.05 |
| Repurchases | 28.7% | 23.8% | 23.6% | 0.09 |
| Sales Growth | 28.8% | 23.7% | 24.5% | -0.45 |
| N | 2,941 | 3,102 | 40,087 | |
| N distinct firms | 1,959 | 2,201 | 9,801 | |

**Table 1.3**

***Impact of Hurricane Proximity on Corporate Cash Holdings***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the level of corporate cash holdings. *Cash* is the total amount of cash and cash equivalents scaled by the total assets of the firm at the end of the quarter. *Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy variable equal to 1 if the county of the firm headquarters is in an area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent Variable: Cash / Assets (in percentage points) | | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| Neighbor | 0.84*** | 0.83*** | 0.81*** |
| | (0.23) | (0.24) | (0.28) |
| Disaster zone | -0.30 | -0.22 | -0.28 |
| | (0.22) | (0.22) | (0.26) |
| Size | | | -1.06*** |
| | | | (0.18) |
| Age | | | -1.01 |
| | | | (12.41) |
| Market-to-Book | | | 0.12*** |
| | | | (0.01) |
| Firm Fixed Effects | Yes | - | - |
| Year-Quarter Fixed Effects | Yes | - | - |
| Firm-Quarter Fixed Effects | - | Yes | Yes |
| Year-Quarter-SIC3 Fixed Effects | - | Yes | Yes |
| N | 411,490 | 411,490 | 359,449 |

**Table 1.4**

***Impact of Hurricane Proximity on Corporate Cash Holdings over Time***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the level of corporate cash holdings at different quarters surrounding the hurricane event. *Cash* is the total amount of cash and cash equivalents scaled by the total assets of the firm at the end of the quarter. *Neighbor_q+i* is a dummy equal to 1 if the county of the firm headquarters at quarter *q+i* is in the neighborhood of an area hit by a hurricane during quarter *q0*. *Disaster_zone_q+i* is a dummy variable equal to 1 if the county of the firm headquarters at quarter *q+i* is in the area hit by a hurricane during quarter *q0*. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Cash / Assets (in percentage points) | | |
|---|---|---|
| | Coefficient | Standard Error |
| Neighbor_q-4 | 0.30 | (0.26) |
| Neighbor_q-3 | 0.02 | (0.29) |
| Neighbor_q-2 | 0.26 | (0.28) |
| Neighbor_q-1 | 0.41 | (0.34) |
| Neighbor_q0 | 0.65* | (0.33) |
| Neighbor_q+1 | 0.73** | (0.31) |
| Neighbor_q+2 | 1.15*** | (0.28) |
| Neighbor_q+3 | 1.13*** | (0.27) |
| Neighbor_q+4 | 0.61** | (0.31) |
| Neighbor_q+5 | 0.63** | (0.29) |
| Neighbor_q+6 | 0.42* | (0.25) |
| Neighbor_q+7 | 0.41 | (0.28) |
| Neighbor_q+8 | 0.27 | (0.28) |
| Disaster zone_q-4 | -0.12 | (0.24) |
| Disaster zone_q-3 | 0.05 | (0.25) |
| Disaster zone_q-2 | -0.17 | (0.26) |
| Disaster zone_q-1 | 0.04 | (0.28) |
| Disaster zone_q0 | -0.26 | (0.29) |
| Disaster zone_q+1 | -0.25 | (0.26) |
| Disaster zone_q+2 | -0.37 | (0.28) |
| Disaster zone_q+3 | -0.57** | (0.24) |
| Disaster zone_q+4 | -0.31 | (0.25) |
| Disaster zone_q+5 | -0.32 | (0.27) |
| Disaster zone_q+6 | -0.11 | (0.30) |
| Disaster zone_q+7 | -0.17 | (0.32) |
| Disaster zone_q+8 | -0.07 | (0.28) |
| Firm Fixed Effects | Yes | |
| Year-Quarter Fixed Effects | Yes | |
| N | 411,490 | |

**Table 1.5**

***Cross Sectional Effects According to Managerial Sophistication***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the level of corporate cash holdings conditional on various measures for the level of managerial sophistication. *Cash* is the total amount of cash and cash equivalents scaled by the total assets of the firm at the end of the quarter. The sophistication of managers is measured according to three criteria: Experience (number of cases in which the firm is located in the neighborhood area), the size of the firm (total assets), and the age of the firm (number of years in Compustat). For each measure of sophistication, *High (respectively, Medium, Low) sophistication* is a dummy variable equal to 1 if the degree of sophistication of managers of the company is identified as high (respectively, medium, low). *Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy variable equal to 1 if the county of the firm headquarters is in the area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Cash / Assets (in percentage points) | | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| Sophistication criteria | Experience | Size | Age |
| Neighbor x High Sophistication | -0.89 | 0.31 | 0.26 |
| | (0.66) | (0.27) | (0.36) |
| Neighbor x Medium Sophistication | 0.64 | 0.63* | 0.62 |
| | (0.52) | (0.37) | (0.39) |
| Neighbor x Low Sophistication | 1.18*** | 1.68*** | 1.91*** |
| | (0.34) | (0.52) | (0.54) |
| High Sophistication | 0.38 | -3.12*** | 1.54*** |
| | (0.47) | (0.37) | (0.37) |
| Low Sophistication | -0.11 | 0.42 | 4.73*** |
| | (0.37) | (0.49) | (0.30) |
| Disaster zone | -0.26 | -0.28 | -0.26 |
| | (0.22) | (0.23) | (0.25) |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes |
| N | 411,490 | 411,490 | 411,490 |
| High - Low sophistication | 2.07*** | 1.37** | 1.65*** |
| *F* -test | (7.95) | (4.53) | (6.63) |

**Table 1.6**

***Source of Change in Cash due to Hurricane Landfall Proximity***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on various outcome variables that affect the level of corporate cash holdings. *Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy variable equal to 1 if the county of the firm headquarters is in the area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. All dependent variables in columns 1 to 7 are expressed in percentage points. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable | Sales growth (%) | EBIT Margin (%) | NWC (% Assets) | Net investment (% PPE) | Repurchase (% Earnings) | Dividend (% Earnings) | New financing (% Mark. Cap.) | Repurchase dummy | Dividend dummy | New financing dummy |
|---|---|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | | | Linear Probability Model | | |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] | [10] |
| Neighbor | 1.42 | -2.90 | -0.42 | -1.02 | -0.24 | -0.54** | 0.29 | -0.01** | -0.01* | 0.01 |
| | (1.42) | (2.32) | (0.42) | (1.65) | (1.53) | (0.27) | (0.24) | (0.01) | (0.00) | (0.00) |
| Disaster zone | -2.35** | -6.30** | -0.64 | -3.80** | 0.10 | -0.61** | -0.71** | 0.00 | 0.00 | 0.00 |
| | (1.20) | (3.16) | (0.70) | (1.85) | (1.64) | (0.27) | (0.30) | (0.01) | (0.01) | (0.00) |
| Firm-Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 371,703 | 397,098 | 408,392 | 382,576 | 209,049 | 210,680 | 352,257 | 357,831 | 386,532 | 389,921 |

**Table 1.7**

***Change in the Value of Cash after the Hurricane Landfall***

This table presents changes in the value of corporate cash holdings over different time windows surrounding the hurricane event. The dependent variable is the excess return of the firm relative to the Fama and French (1993) 25 size and book-to-market portfolios over the specified time window. Hurricane landfall occurs at quarter *q0*. *Neighbor_q0* is a dummy variable equal to 1 if the county of the firm headquarters was in the neighborhood of the area hit by the hurricane at quarter *q0*. *Change in X* indicates a change in *X* from quarter *q-2* to quarter *q+i*. Variables *X* are defined in Appendix 1. All independent continuous variables are scaled by the market value of equity of the firm at the beginning of the time window (q-2). Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Excess Stock Return Relative to the Fama & French (1993) 25 Portfolios | | | | | | |
|---|---|---|---|---|---|---|
| Time Window | [q-2 ; q-1] | [q-2 ; q0] | [q-2 ; q+1] | [q-2 ; q+2] | [q-2 ; q+3] | [q-2 ; q+4] |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Change in Cash * Neighbor_q0 | -0.23 | -0.15 | -0.22** | -0.27** | -0.24* | -0.12 |
| | (0.14) | (0.13) | (0.11) | (0.13) | (0.15) | (0.14) |
| Change in Cash | 0.32*** | 0.59*** | 0.88*** | 1.08*** | 1.27*** | 1.29*** |
| | (0.06) | (0.08) | (0.08) | (0.10) | (0.08) | (0.10) |
| Change in Earnings | 0.04 | 0.17*** | 0.22*** | 0.33*** | 0.60*** | 0.89*** |
| | (0.03) | (0.03) | (0.04) | (0.09) | (0.07) | (0.10) |
| Change in Dividends | -0.26 | 1.56 | 2.73** | 6.15*** | 1.31 | 4.24** |
| | (0.95) | (1.32) | (1.08) | (1.79) | (1.61) | (1.69) |
| Change in Interest Expenses | 0.46 | -0.65 | -2.67*** | -3.86*** | -4.08*** | -0.23 |
| | (0.45) | (0.54) | (0.54) | (0.85) | (0.80) | (0.32) |
| Change in Non Cash Assets | 0.06*** | 0.08*** | 0.13*** | 0.14*** | 0.15*** | 0.12*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Change in R&D | -0.5 | -0.43 | 0.80*** | 0.51 | -0.67 | -0.51 |
| | (0.32) | (0.36) | (0.29) | (0.67) | (0.56) | (0.54) |
| Lagged Cash | 0.05*** | 0.12*** | 0.21*** | 0.34*** | 0.37*** | 0.33*** |
| | (0.01) | (0.02) | (0.02) | (0.03) | (0.03) | (0.03) |
| Change in Cash x Lagged Cash | -0.03 | -0.24** | -0.22** | -0.25** | -0.41*** | -0.36*** |
| | (0.08) | (0.11) | (0.11) | (0.12) | (0.08) | (0.09) |
| Leverage | -0.13 | -0.13*** | -0.20*** | -0.25*** | -0.30*** | -0.32*** |
| | (0.12) | (0.01) | (0.02) | (0.02) | (0.02) | (0.03) |
| Change in Cash x Leverage | -0.07*** | -0.36*** | -0.63*** | -0.74*** | -0.94*** | -1.31*** |
| | (0.01) | (0.12) | (0.13) | (0.16) | (0.16) | (0.18) |
| Net Financing | 0.00 | -0.04* | -0.08*** | -0.05** | -0.02 | -0.06*** |
| | (0.03) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Neighbor_q0 | 0.01** | 0.01 | 0.00 | 0.00 | -0.01 | -0.03 |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.02) | (0.02) |
| Constant | -0.01 | -0.02*** | -0.04*** | -0.04*** | -0.07*** | -0.08*** |
| | (0.00) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| N | 12,196 | 11,808 | 11,466 | 10,894 | 10,359 | 10,136 |

**Table 1.8**

***Market Reaction at Hurricane Landfall***

This table presents the Average Cumulative Abnormal stock Return (ACAR) over the hurricane landfall period (hereafter the "event window") depending on the proximity of the firm headquarters to the disaster area. For each hurricane, firms are assigned to the *Disaster zone* group, the *Neighbor* group, or the *Control* group depending on the location of their headquarters. The event windows start one day before the beginning of the hurricane strike and end one day after the end of the hurricane strike. For each group of firms, ACAR and *z* statistics are estimated using equally weighted portfolios of firms with similar event windows. See Appendix for the details of the abnormal return estimation. The economic gain is the implicit average change in market value corresponding to the ACAR expressed as a percentage of total assets. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Group | N (firms) | N (portfolios) | ACAR (%) | *Z* | Economic gain (% of assets) |
|---|---|---|---|---|---|
| Neighbor | 2,583 | 15 | -0.04% | (-0.16) | -0.10% |
| Disaster zone | 1,991 | 74 | -0.82%** | (-2.23) | -1.03% |
| Control (Rest of US) | 30,350 | 15 | -0.08% | (-0.56) | -0.11% |

**Table 1.9**

***Change in Sales Growth Volatility after the Hurricane Landfall***

This table presents difference-in-differences estimates of the effect of the hurricane proximity on sales growth volatility. In panel A, we estimate the volatility of the growth in revenues at the firm level after (before) the hurricane by measuring the standard deviation of sales growth over the four quarters following (preceding) the occurrence of the disaster. In panel B, we estimate the volatility of the growth in revenues at the county level using the standard deviation of sales growth across firms for each quarter around the hurricane. The specification in panel B is weighted by the average number of firms in the county. ***, **, and * denote significance at the 1%, 5% and 10% levels.

Panel A: Impact of Hurricane Proximity on Sales Growth Variance at the Firm level

Dependent variable: Sales Growth Standard Deviation (in percentage points)

| | Coefficient | Standard Error |
|---|---|---|
| Neighbor | 0.21 | (0.56) |
| Disaster zone | -1.24* | (0.67) |
| Firm Fixed Effects | Yes | |
| Time Fixed Effects | Yes | |
| N | 89,990 | |

Panel B: Impact of Hurricane Proximity on Sales Growth Variance at the County level

Dependent variable: Sales Growth Standard Deviation at the County Level (in percentage points)

| | Coefficient | Standard Error |
|---|---|---|
| Neighbor_q-1 | -0.95 | (1.78) |
| Neighbor_q0 | 0.98 | (2.65) |
| Neighbor_q+1 | 1.24 | (2.47) |
| Neighbor_q+2 | 2.94 | (2.79) |
| Neighbor_q+3 | 3.10 | (3.03) |
| Neighbor_q+4 | -1.70 | (2.42) |
| Neighbor_q+5 | -1.85 | (2.11) |
| Neighbor_q+6 | -1.84 | (2.41) |
| Neighbor_q+7 | -2.26 | (2.20) |
| Disaster zone_q-1 | 0.70 | (2.60) |
| Disaster zone_q0 | -2.83* | (1.49) |
| Disaster zone_q+1 | -2.97* | (1.62) |
| Disaster zone_q+2 | -4.26 | (3.27) |
| Disaster zone_q+3 | -3.88 | (2.72) |
| Disaster zone_q+4 | -0.85 | (3.31) |
| Disaster zone_q+5 | 0.21 | (3.14) |
| Disaster zone_q+6 | 0.75 | (1.89) |
| Disaster zone_q+7 | -1.18 | (2.18) |
| County Fixed Effects | Yes | |
| Year-Quarter Fixed Effects | Yes | |
| N | 42,540 | |

**Table 1.10**

***Change in Stock Returns Volatility after the Hurricane Landfall***

This table presents results of two tests examining the effect of the hurricane proximity on stock returns volatility. Panel A presents results of an F-test of the equality of stock return variances around the hurricane period for each group of firms (Neighbor, Disaster Zone, and Control). Stock return variances are estimated over two 30-days periods, one before the start of the hurricane period and the other after the end of the hurricane period. Column 1 (2) reports the percentage of firms experiencing a decrease (increase) in stock return variance that is statistically significant at the 5% level. Column 3 reports the percentage of firms for which the F-test cannot reject the null hypothesis of stock variances equality between the two periods at the 5% level. In Panel B, we presents difference-in-differences estimates of the effect of the hurricane proximity on stock returns volatility. The dependent variable is the (annualized) stock returns volatility measured by the standard deviation of daily stock returns over the quarter. *Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy variable equal to 1 if the county of the firm headquarters is in the area hit by a hurricane over the past 12 months. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

Panel A: F-test of the Equality of Stock Returns Variances

| | | Change in Stock Returns Variance | | |
|---|---|---|---|---|
| Group | # Firms | % Down | % Up | % No change |
| | | [1] | [2] | [3] |
| Neighbor | 1,773 | 16.6% | 18.6% | 64.8% |
| Disaster zone | 2,299 | 16.7% | 19.7% | 63.5% |
| Control | 27,539 | 16.4% | 17.9% | 65.8% |

Panel B: Impact of Hurricane Proximity on Stock Returns Volatility

| Dependent variable: Stock Returns Volatility (in percentage points) | |
|---|---|
| Neighbor | 0.95 |
| | (1.01) |
| Disaster zone | 1.33** |
| | (0.60) |
| Firm-Quarter Fixed Effects | Yes |
| Year-Quarter Fixed Effects | Yes |
| N | 317,949 |

**Table 1.11**

***Hurricane Strike and Firms Operating Outside the Neighborhood Area***

This table presents difference-in-differences estimates of the effect of the occurrence of a hurricane strike on the level of corporate cash holdings for firms whose operations are less dependent on the local economy affected by the hurricane. *Cash* is the total amount of cash and cash equivalents expressed in percentage points of the total assets of the firm at the end of the quarter. *Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy variable equal to 1 if the county of the firm headquarters is in the area hit by a hurricane over the past 12 months. In column 1, we restrict the sample to firms operating in "tradable goods" industries following the classification proposed by Mian and Sufi (2012). In column 2, *Remote Neighbor* is a dummy variable equal to 1 if the county of the firm headquarters is in the remote neighborhood of an area hit by a hurricane over the past 12 months. In column 3, *Vulnerable* is a dummy variable equal to 1 if a hurricane occurred during the past 12 months, if the firm is vulnerable to the risk of hurricane disaster, and if the headquarters of the firm are located outside the disaster area and its neighborhood. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Cash / Assets (in percentage points) | | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| | Tradable Industries | Remote Neighbors | Vulnerable Firms Outside the Neighborhood area |
| Neighbor | 0.99*** | 0.71*** | 0.89*** |
| | (0.39) | (0.26) | (0.23) |
| Remote Neighbor | | 0.48* | |
| | | (0.26) | |
| Vulnerable | | | 0.66** |
| | | | (0.31) |
| Disaster zone | -0.40 | -0.40* | -0.20 |
| | (0.34) | (0.23) | (0.24) |
| | | (0.34) | (3.58) |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes |
| N | 233,065 | 411,490 | 411,490 |

**Table 1.12**

***Cross Sectional Effects According to Financial Constraints***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the level of corporate cash holdings conditional on the presence of financial constraints. *Cash* is the total amount of cash and cash equivalents expressed in percentage points of the total assets of the firm at the end of the quarter. Firms financially constrained are identified according three criteria : the presence of a bond rating, the payout policy, and the dependence on external financing. *FC* is a dummy variable equal to 1 if the company is identified as financially constrained by the respective criterion. *Neighbor* is a dummy equal to 1 if the county location of the firm headquarter is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy equal to 1 if the county location of the firm headquarter is in the area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. Standard errors corrected for clustering of the observations at the county level are reported between parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Cash / Assets (in percentage points) | | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| Financial Constraints criteria | Bond Ratings | Payout ratio | Dependence on External Financing |
| Neighbor x FC | 1.07*** | 0.90** | 1.66*** |
| | (0.40) | (0.41) | (0.40) |
| Neighbor | 0.13 | 0.2 | -0.01 |
| | (0.28) | (0.34) | (0.24) |
| Disaster zone | -0.16 | -0.33 | -0.28 |
| | (0.27) | (0.24) | (0.24) |
| FC | 1.05*** | -0.66*** | |
| | (0.35) | (0.16) | |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes |
| N | 354,473 | 390,097 | 411,490 |

**Table 1.13**

***Cross Sectional Effects According to Vulnerability***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the level of corporate cash holdings conditional on the vulnerability of the firm to a hurricane disaster. *Cash* is the total amount of cash and cash equivalents scaled by the total assets of the firm at the end of the quarter. The vulnerability to a hurricane disaster is measured using three criteria. First, we measure the CAR observed for firms located in the disaster zone at the time of the impact and use the average CAR by industry (SIC3) to determine the most affected industries in case of hurricane disaster. Firms are then identified as vulnerable if they operate in such an industry (*Most affected SIC3*). Second, we use the share of total assets which are intangible assets to assess whether the business of the firm is more difficult to insure. Firms are identified as vulnerable if this share is in the top tercile of the distribution (*Intangible assets*). Third, firms are identified as vulnerable if less than 20% of their revenues is generated outside the US market (*Domestic*). *Vulnerable* is a dummy variable equal to 1 if the company is identified as being more vulnerable to a hurricane disaster by the respective criterion. *Neighbor* is a dummy equal to 1 if the county location of the firm headquarter is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy equal to 1 if the county location of the firm headquarter is in the area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. Standard errors corrected for clustering of the observations at the county level are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Cash / Assets (in percentage points) | | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| Vulnerability criteria | Most affected SIC3 | Intangible assets | Domestic |
| Neighbor x Vulnerable | 1.47*** | 1.13** | 1.16*** |
| | (0.54) | (0.54) | (0.37) |
| Neighbor | 0.04 | 0.46** | -0.12 |
| | (0.41) | (0.22) | (0.32) |
| Disaster zone | -0.26 | -0.40* | -0.29 |
| | (0.27) | (0.23) | (0.22) |
| Vulnerable | | 9.85*** | 0.90*** |
| | | (0.34) | (0.25) |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes |
| N | 295,096 | 411,490 | 411,047 |

**Table 1.14**

***Effects of Hurricane Proximity on Corporate Cash holdings over time (Matching)***

This table presents changes in corporate cash holdings over time caused by the proximity of a hurricane occurred at quarter *q0*. The sample comprises 2,060 treated firms whose headquarter is located in the neighborhood of an area hit by a hurricane during quarter *q0* ("Neighbor firms"). For each treated firm, the counterfactual outcome is the weighted average of the change in cash over all control firms with the same SIC 3 code ("Matched firm"). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed six months before the hurricane landfall at quarter q-3 along seven dimensions: size, age, market-to-book, financial leverage, capital expenditures and net working capital. *t*-statistics are reported in the last column. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| **Average change in cash from q-3 to** | **Neighbor firms** | **Matched firms** | **Diff-in-diffs** | ***t*-statistic** |
|---|---|---|---|---|
| q-2 | -0.5% | -0.6% | 0.1% | 0.51 |
| q-1 | -0.7% | -0.8% | 0.1% | 0.55 |
| q0 | -0.6% | -0.8% | 0.2% | 0.69 |
| q+1 | 0.0% | -0.6% | 0.6%** | 1.96 |
| q+2 | 0.4% | -0.6% | 1.0%*** | 2.97 |
| q+3 | 0.1% | -0.7% | 0.8%** | 2.38 |
| q+4 | -0.3% | -0.9% | 0.6%* | 1.71 |
| q+5 | -0.1% | -0.6% | 0.5% | 1.47 |
| q+6 | -0.5% | -0.9% | 0.4% | 1.18 |
| q+7 | -0.7% | -1.1% | 0.4% | 1.12 |
| q+8 | -0.9% | -1.2% | 0.3% | 0.79 |

**Table 1.15**

***Impact of Hurricane Proximity on Corporate Cash Holdings (Robustness Tests)***

This table presents results of robustness tests examining the effect of the proximity of a hurricane strike on the level of corporate cash holdings. *Cash* is the total amount of cash and cash equivalents scaled by the total assets of the firm at the end of the quarter. *Neighbor* is a dummy equal to 1 if the county location of the firm headquarter is in the neighborhood of an area hit by a hurricane over the past 12 months. *Disaster_zone* is a dummy equal to 1 if the county location of the firm headquarter is in an area hit by a hurricane over the past 12 months. All other variables are defined in Appendix 1. Standard errors corrected for clustering of the observations at the county level are reported between parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent Variable: Cash / Assets (in percentage points) | | | | | | |
|---|---|---|---|---|---|---|
| | Placebo | Additional Controls | All Hurricanes | Less adjacent counties | More adjacent counties | Compustat Annual |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Neighbor | 0.05 | 0.65*** | 0.70*** | 0.71*** | 0.83*** | 0.54** |
| | (0.22) | (0.20) | (0.21) | (0.25) | (0.20) | (2.11) |
| Disaster Zone | 0.11 | -0.23 | -0.22 | -0.31 | -0.27 | -0.17 |
| | (0.18) | (0.21) | (0.20) | (0.22) | (0.22) | (-0.84) |
| Size | | -0.92*** | | | | |
| | | (0.14) | | | | |
| Age | | -0.11*** | | | | |
| | | (0.02) | | | | |
| Market-to-Book | | 0.84*** | | | | |
| | | (0.04) | | | | |
| Debt | | -14.72*** | | | | |
| | | (0.44) | | | | |
| Net Working Capital | | -29.25*** | | | | |
| | | (1.62) | | | | |
| Capex | | -29.15*** | | | | |
| | | (3.14) | | | | |
| R&D | | -44.98*** | | | | |
| | | (4.57) | | | | |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 411,490 | 373,576 | 411,490 | 411,490 | 411,490 | 134,483 |

**Table 1.16**

***Impact of Hurricane Proximity on Sales Growth***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the growth in revenues at different quarters around the occurrence of the hurricane. *Sales growth* is the growth in revenues of the firm during the quarter relative to the same quarter of the previous year. *Neighbor_q+i* is a dummy equal to 1 if the county location of the firm headquarter at quarter *q+i* is in the neighborhood of an area hit by a hurricane during quarter *q0*. *Disaster_zone_q+i* is a dummy equal to 1 if the county location of the firm headquarter at quarter *q+i* is in the area hit by a hurricane during quarter *q0*. Standard errors corrected for clustering of the observations at the county level are reported between parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: Sales Growth (in percentage points) | | |
|---|---|---|
| | Coefficient | Standard Error |
| Neighbor_q-4 | 0.25 | (1.42) |
| Neighbor_q-3 | -0.06 | (1.41) |
| Neighbor_q-2 | 2.09 | (1.45) |
| Neighbor_q-1 | 0.64 | (1.78) |
| Neighbor_q0 | 1.52 | (1.53) |
| Neighbor_q+1 | 1.10 | (1.85) |
| Neighbor_q+2 | 1.88 | (1.93) |
| Neighbor_q+3 | 2.75 | (1.69) |
| Neighbor_q+4 | 1.41 | (1.60) |
| Neighbor_q+5 | 2.31 | (1.83) |
| Neighbor_q+6 | 1.57 | (1.26) |
| Neighbor_q+7 | 1.42 | (1.80) |
| Neighbor_q+8 | 1.84 | (1.66) |
| Disaster zone_q-4 | 0.84 | (1.65) |
| Disaster zone_q-3 | 1.13 | (1.92) |
| Disaster zone_q-2 | 3.82 | (2.48) |
| Disaster zone_q-1 | 3.49 | (2.60) |
| Disaster zone_q0 | 1.39 | (2.48) |
| Disaster zone_q+1 | -1.19 | (1.79) |
| Disaster zone_q+2 | -4.89*** | (1.91) |
| Disaster zone_q+3 | -4.49** | (1.93) |
| Disaster zone_q+4 | -2.11 | (2.24) |
| Disaster zone_q+5 | -2.57 | (1.82) |
| Disaster zone_q+6 | -0.96 | (1.59) |
| Disaster zone_q+7 | -0.69 | (1.47) |
| Disaster zone_q+8 | 0.26 | (1.68) |
| Firm Fixed Effects | Yes | |
| Year-Quarter Fixed Effects | Yes | |
| N | 371,703 | |

**Table 1.17**

***Impact of Hurricane Proximity on Bank Loans***

This table presents difference-in-differences estimates of the effect of the proximity of a hurricane strike on the amount of new commercial and industrial loans of the bank at different quarters around the occurrence of the hurricane. *ΔC&I Loans* is the amount of new commercial and industrial loans granted during the quarter at the bank level expressed in percentage points of the total assets at the end of the quarter. *Neighbor_q+i* is a dummy equal to 1 if the county location of the bank headquarter at quarter *q+i* is in the neighborhood of an area hit by a hurricane during quarter *q0*. *Disaster_zone_q+i* is a dummy equal to 1 if the county location of the bank headquarter at quarter *q+i* is in the area hit by a hurricane during quarter *q0*. Standard errors corrected for clustering of the observations at the county level are reported between parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent variable: ΔC&I Loans / Assets (in percentage points) | | |
|---|---|---|
| | Coefficient | Standard Error |
| Neighbor_q-4 | -0.02 | (0.26) |
| Neighbor_q-3 | 0.00 | (0.02) |
| Neighbor_q-2 | 0.02 | (0.02) |
| Neighbor_q-1 | 0.00 | (0.02) |
| Neighbor_q0 | -0.01 | (0.02) |
| Neighbor_q+1 | 0.00 | (0.02) |
| Neighbor_q+2 | 0.04* | (0.02) |
| Neighbor_q+3 | 0.00 | (0.02) |
| Neighbor_q+4 | -0.01 | (0.02) |
| Neighbor_q+5 | 0.00 | (0.02) |
| Neighbor_q+6 | 0.00 | (0.02) |
| Neighbor_q+7 | -0.01 | (0.02) |
| Neighbor_q+8 | -0.04 | (0.03) |
| Disaster zone_q-4 | 0.00 | (0.24) |
| Disaster zone_q-3 | -0.01 | (0.03) |
| Disaster zone_q-2 | 0.02 | (0.02) |
| Disaster zone_q-1 | -0.01 | (0.03) |
| Disaster zone_q0 | 0.02 | (0.03) |
| Disaster zone_q+1 | 0.03 | (0.03) |
| Disaster zone_q+2 | 0.03 | (0.03) |
| Disaster zone_q+3 | 0.06** | (0.03) |
| Disaster zone_q+4 | 0.09*** | (0.03) |
| Disaster zone_q+5 | 0.05** | (0.03) |
| Disaster zone_q+6 | 0.01 | (0.03) |
| Disaster zone_q+7 | 0.03 | (0.03) |
| Disaster zone_q+8 | 0.00 | (0.03) |
| Bank Fixed Effects | Yes | |
| Year-Quarter Fixed Effects | Yes | |
| N | 411,490 | |

**Table 1.18**

***Determinants of Disaster Likelihood***

This table presents panel regressions examining the effects of the proximity to a county where a hurricane made landfall during the past 2 years on the likelihood to be affected by a hurricane during a given quarter. The analysis is performed at the county level. The dependent variable is a dummy equal to 1 if the county is hit by a hurricane (Only one of the 15 major hurricanes in column 1 and 2, and any hurricane in column 3 and 4). *Neighbor* is a dummy equal to 1 if the county is in the neighborhood of an area hit by a hurricane over the past 12 months. *Neighbor_last_24* is a dummy equal to 1 if the county is in the neighborhood of an area hit by a hurricane over the past 24 months. Standard errors are clustered at the state level and reported between parentheses. All specifications include quarter dummy variables. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent Variable | Major Hurricane [1] | Major Hurricane [2] | Any Hurricane [3] | Any Hurricane [4] |
|---|---|---|---|---|
| Neighbor | 0.01 | | 0.00 | |
| | (0.007) | | (0.009) | |
| Neighbor_last_24 | | -0.01* | | -0.01 |
| | | (0.006) | | (0.007) |
| County Fixed Effects | Yes | Yes | Yes | Yes |
| Quarter Fixed Effects | Yes | Yes | Yes | Yes |
| N | 65,604 | 65,604 | 94,600 | 94,600 |

**Table 1.19**

***Major Earthquakes outside the US since 1980***

This table describes the 11 major earthquakes occurred outside the US since 1980. See the text for the details of the selection criteria. *Magnitude* measures the energy contained in an earthquake according to the Richter scale, *Tsunami* is a dummy equal to one if the earthquake generated a Tsunami, *Fatalities* is the total number of deaths, and *Damages* is the estimated value of total damages expressed in billion dollar. *Damages(CPI adjusted)* is the estimated value of total damages expressed in billion dollar adjusted for the Consumption Price Index as of 2011. Primary source of information is the Significant Earthquake Database from the National Geophysical Data Center.

| Country | Year | Date | Magnitude | Tsunami | Fatalities | Damages | Damages (CPI adjusted) |
|---|---|---|---|---|---|---|---|
| Mexico | 1985 | 9/19/1985 | 7.5 | Yes | 9,500 | 4,000 | 8,362 |
| Iran | 1990 | 6/20/1990 | 7.1 | No | 40,000 | 8,000 | 13,768 |
| Turkey | 1999 | 8/17/1999 | 7.2 | Yes | 17,118 | 20,000 | 27,003 |
| Taiwan | 1999 | 9/20/1999 | 7.3 | No | 2,297 | 14,000 | 18,902 |
| India | 2001 | 1/26/2001 | 7.5 | No | 20,005 | 2,623 | 3,332 |
| Indonesia | 2004 | 12/26/2004 | 8.3 | Yes | 227,898 | 10,000 | 11,908 |
| Pakistan | 2005 | 10/8/2005 | 7.4 | No | 80,361 | 5,200 | 5,989 |
| China | 2008 | 5/12/2008 | 7.6 | Yes | 87,652 | 121,000 | 126,415 |
| Indonesia | 2009 | 9/30/2009 | 7.3 | Yes | 1,117 | 2,200 | 2,307 |
| Haiti | 2010 | 1/12/2010 | 7.0 | Yes | 222,570 | 8,000 | 8,253 |
| Japan | 2011 | 3/11/2011 | 8.2 | Yes | 15,854 | 210,000 | 210,000 |

**Table 1.20**

***Effects of Earthquakes outside the US on Corporate Cash holdings of US Firms***

This table presents changes in corporate cash holdings over time for US firms located in a seismic area after the occurrence of a major earthquake outside the US at quarter *q0*. The sample comprises 3,668 treated firms whose headquarter is located in an urban community where an earthquake is frequently felt according to the U.S. Geological surveys ("Seismic zone firms"). For each treated firm, the counterfactual outcome is the weighted average of the change in cash over all control firms with the same SIC 3 code ("Matched firm"). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed at quarter *q-2* (ie. three months before the earthquake occurrence) along four dimensions: size, age, market-to-book, and financial leverage. *t*-statistics are reported in the last column. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| **Average change in cash from q-2 to** | **Seismic zone firms** | **Matched firms** | **Diff-in-diffs** | ***t* -statistic** |
|---|---|---|---|---|
| q-1 | -0.63% | -0.68% | 0.05% | 0.30 |
| q0 | -0.73% | -1.05% | 0.32% | 1.62 |
| q+1 | -0.74% | -1.20% | 0.46%** | 2.03 |
| q+2 | -0.49% | -1.09% | 0.59%** | 2.35 |
| q+3 | -0.70% | -1.24% | 0.54%** | 1.97 |
| q+4 | -0.77% | -1.25% | 0.48%* | 1.68 |
| q+5 | -0.83% | -1.22% | 0.39% | 1.36 |

**Table 1.21**

***Hurricane Effects on Sales Growth Conditional on Corporate Cash Holdings***

This table presents difference-in-differences estimates of the effect of a hurricane strike on the growth in revenues conditional on the level of corporate cash holdings before the occurrence of the disaster. *Sales growth* is the growth in revenues of the firm during the quarter relative to the same quarter of the previous year. *High (Medium) (Low) Cash Tercile* is a dummy variable equal to 1 if the firm is in the top (middle) (bottom) tercile of the sample in terms of corporate cash holdings before the occurrence of the disaster. *Disaster_zone_q+i* is a dummy equal to 1 if the county location of the firm headquarter at quarter *q+i* is in the area hit by a hurricane during quarter *q0*. Standard errors are corrected for clustering of the observations at the county level and reported between parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels.

| Dependent Variable: Sales Growth (in percentage points) | | |
|---|---|---|
| | Coefficient | Standard Error |
| Disaster Zone_q-4 x High Cash Tercile | 2.66 | (5.48) |
| Disaster Zone_q-3 x High Cash Tercile | -0.31 | (5.89) |
| Disaster Zone_q-2 x High Cash Tercile | 0.61 | (5.62) |
| Disaster Zone_q-1 x High Cash Tercile | -2.06 | (4.16) |
| Disaster Zone_q0 x High Cash Tercile | -3.61 | (5.74) |
| Disaster Zone_q+1 x High Cash Tercile | -0.88 | (6.38) |
| Disaster Zone_q+2 x High Cash Tercile | -6.47 | (6.21) |
| Disaster Zone_q+3 x High Cash Tercile | -3.45 | (5.02) |
| Disaster Zone_q+4 x High Cash Tercile | 9.20*** | (3.62) |
| Disaster Zone_q+5 x High Cash Tercile | 3.57 | (5.19) |
| Disaster Zone_q+6 x High Cash Tercile | -0.01 | (7.80) |
| Disaster Zone_q+7 x High Cash Tercile | 0.09 | (5.70) |
| Disaster Zone_q-4 x Medium Cash Tercile | 3.87 | (2.99) |
| Disaster Zone_q-3 x Medium Cash Tercile | 0.45 | (2.82) |
| Disaster Zone_q-2 x Medium Cash Tercile | 1.13 | (2.66) |
| Disaster Zone_q-1 x Medium Cash Tercile | 0.10 | (2.85) |
| Disaster Zone_q0 x Medium Cash Tercile | -2.41 | (3.65) |
| Disaster Zone_q+1 x Medium Cash Tercile | -4.83* | (2.63) |
| Disaster Zone_q+2 x Medium Cash Tercile | -8.49*** | (3.38) |
| Disaster Zone_q+3 x Medium Cash Tercile | -4.44 | (2.88) |
| Disaster Zone_q+4 x Medium Cash Tercile | -3.70 | (2.28) |
| Disaster Zone_q+5 x Medium Cash Tercile | -6.93*** | (2.71) |
| Disaster Zone_q+6 x Medium Cash Tercile | -6.64** | (2.77) |
| Disaster Zone_q+7 x Medium Cash Tercile | -3.65 | (2.39) |
| Disaster Zone_q-4 x Low Cash Tercile | 3.01 | (4.08) |
| Disaster Zone_q-3 x Low Cash Tercile | 3.64 | (5.02) |
| Disaster Zone_q-2 x Low Cash Tercile | 5.61 | (3.61) |
| Disaster Zone_q-1 x Low Cash Tercile | 4.60 | (3.19) |
| Disaster Zone_q0 x Low Cash Tercile | 2.37 | (2.98) |
| Disaster Zone_q+1 x Low Cash Tercile | 0.39 | (2.65) |
| Disaster Zone_q+2 x Low Cash Tercile | -9.89*** | (3.26) |
| Disaster Zone_q+3 x Low Cash Tercile | -7.08** | (3.58) |
| Disaster Zone_q+4 x Low Cash Tercile | -8.96*** | (3.17) |
| Disaster Zone_q+5 x Low Cash Tercile | -9.76*** | (3.31) |
| Disaster Zone_q+6 x Low Cash Tercile | -6.92** | (3.26) |
| Disaster Zone_q+7 x Low Cash Tercile | -4.20* | (2.26) |
| Firm Fixed Effects | Yes | |
| Year-Quarter-Cash Tercile Fixed Effects | Yes | |
| N | 368,094 | |

# Chapter 2

# Announcing the Announcement

Joint work with **Romain Boulland**

**Abstract**

Firms must notify the date and time of earnings announcements to market participants before the event (the "advance notice period"). We find that such advance notice period varies within firm and that its variation affects how much investors pay attention to earnings news. Using various measures of investors' attention – including attendance to earnings conference calls and trading volume – we find that investors are more attentive when the date and time of earnings disclosure is scheduled far in advance. This variation in investors' attention affects short-run and long-run stock prices, thereby creating incentives for firms to strategically reduce the advance notice period when they plan to disclose bad news. Consistent with this idea, we find that within-firm variations in the advance notice period predict the earnings surprise. A trading strategy that exploits such variations yields abnormal returns of 1.7% per month.

*"The fundamental scarcity in the modern world is scarcity of attention."*

Herbert A. Simon

## 2.1. Introduction

During earnings announcement seasons, investors need to digest news from numerous companies in a very short period of time. At the peak of the season, about 250 US firms announce their earnings on the same day. While prior research shows that investors' limited attention is central to explain how stock prices incorporate earnings news during this period, little evidence exists that managers who care about the value of their firm's stocks take advantage of investors' attention constraint.[1]

This paper examines the effects of investors' limited attention on corporate managers' decisions. The question we ask is whether managers take investors' inattention into account when preparing their audience to the forthcoming earnings announcement. To answer this question, we study the notification process by which managers make investors aware of the date and time of earnings announcement events. We use a new dataset of circa 53,000 press releases by US firms over the 2007-2012 period which announce the date, time, conference call number and other details about the organization of their earnings release to market participants (the "notice of earnings"). On average, such details are released ten days before the event (the "advance notice period").[2] We argue that the choice of this advance notice period affects investors' attention to earnings news and that firm managers use this notification period to strategically manage investors' attention.

[1] Prior literature finds that stock prices under-react to earning news when multiple firms announce their earnings on the same day (Hirshleifer, Lim, and Teoh 2009), on Friday (DellaVigna and Pollet 2009), or when media coverage is low (Peress 2008). However, little evidence exists that managers exploit investors' inattention when announcing their earnings. Doyle and Magilke (2009) for instance find no evidence that firms opportunistically release bad earnings on Friday or outside the market hours.

[2] For the sake of clarity, we call the action of communicating the date, time and any other organizational detail about earnings releases "notice of earnings". We call the action of disclosing quarterly earnings information "earnings announcement" or "earnings release". As illustrated in Figure 2.3, the "advance notice period" is the number of days between the date of the first notice of earnings and the earnings announcement date.

There are two reasons why the advance notice period can cause higher or lower attention. First, investors with a crowded agenda should decide which earnings announcement they will focus on. Therefore, a simple rule of thumb is to follow the order in which investors receive the notices of earnings. This "first-come, first-served" rule implies that a longer advance notice period increases the probability that the announcing firm is first on investors' agenda. Second, a notice of earnings for a given firm can coincide with other relevant information, such as earnings announcements by other firms. In this case, such a notice of earnings could be overlooked by investors. Indeed, investors whose attention is limited will then focus on news with the most valuable content (earnings announcements by other firms) and ignore the notice of earnings. Since earnings announcements are seasonal, this overlap problem is more likely to occur when the advance notice period is short.[3] By contrast, a longer advance notice period reduces the risk that a notice of earnings competes with simultaneous earnings announcements for investors' attention, thereby increasing the probability that investors include the event in their agenda.

Consistent with those predictions, we find that an increase in the advance notice period leads to higher attention to earnings news. We measure investors' attention using attendance to earnings conference calls. Controlling for known determinants of investors' attention as well as firm fixed-effects, we find that the number of conference call participants increases when the date and time of the earnings release are communicated earlier. To complement this analysis, we use the abnormal trading volume as an alternative measure of investors' attention (e.g. Gervais, Kaniel, and Mingelgrin 2001; Barber and Odean 2008; Hou, Xiong, and Peng 2009). Again, we find that investors are more attentive to earnings announcements when earnings release details are communicated well ahead of time. Specifically, we compare firms

[3] A short advance notice period for a given firm implies that the date of its notice of earnings is very close to its earnings announcement date. Since earnings announcements are seasonal and thus occur around the same dates, it is then very likely that the date of this notice of earnings coincide with the date of earnings release by other firms. Figure 1 illustrates very clearly this possibility of overlapping dates between both type of news.

that announce their earnings within the same day (day of announcement fixed effects). Controlling for the magnitude of the earning surprise and firm heterogeneity (firm fixed-effects), we find that firms with longer advance notice periods have higher abnormal trading volumes.

Next, we investigate whether the variation in the advance notice period affects the speed of earnings news incorporation into stock prices. DellaVigna and Pollet (2009) and Hirshleifer and Teoh (2003) predict that higher inattention leads to lower immediate stock price reaction and higher post-earnings announcement drift. Consistent with their predictions, we find that a longer advance notice period increases the immediate reaction to earnings announcements and decreases the post-earnings announcement drift. Overall, these results indicate that a short notice period hurts investors' attention on earnings announcement day, which reduces stock price immediate reaction to earnings news.

We then proceed to the central question of this paper and examine whether managers strategically use investors' limited attention by making shorter notice period when they are about to disclose bad news. Consistent with this idea, we find that for a given firm, the earnings surprise decreases on average by almost one cent when the notice of earnings is sent one week later. In other words, within-firm variations in the advance notice period predict the earnings surprise. This finding holds after controlling for delays in earnings releases or when focusing on the subsample of firms that consistently report their earnings on the same date. Therefore, our effect is not driven by the well documented behavior that managers tend to announce good news early and bad news late (Kross and Schroeder, 1984, Begley and Fischer, 1998, and Bagnoli, Kross, and Watts, 2002). While consistent with the "good news early / bad news late" practice documented by the accounting literature, our finding differs from this strand of research by showing that managers' communication ahead of the earnings

announcement day also conveys information about earnings news that is not contained in the choice of the announcement date.

Next, we investigate how managers' response to investors' inattention varies across firms. First, not all managers can exploit investors' inattention. In particular, managers of highly visible firms whose stock is consistently scrutinized by the market do not have this possibility. Consistent with this idea, we find that a change in the advance notice period is much more informative about the earnings surprise for less visible firms, i.e. firms with the same fiscal year-end as their industry peers, which thus report earnings at the same moment as their competitors, firms with low analysts coverage, and small-cap companies. Second, some managers may care more about the short-term value of their firm's stock. For instance, managers who plan to issue new equity should focus more on maximizing their current stock price as well as managers of firms with short-term oriented shareholders. Consistent with this intuition, we find that a change in the advance notice period is more informative about the earnings surprise when firms issue equity in the subsequent quarter or when their share turnover was high at the end of the previous quarter. Overall, this second set of results suggests that firm managers respond strategically to investors' limited attention by making shorter or longer advance notice period when it is in their interest to do so.

Finally, we investigate whether investors anticipate the implication of earnings notification on future earnings surprise. Indeed, investors may detect firm's strategic behavior regarding the choice of the advance notice period. In this case, they may react positively to early notices and negatively to late notices. If so, the market reaction to the notice of earnings would be positively correlated with the advance notice period. We fail to find such a correlation, which suggests that a majority of investors do not perceive the implications of a change in the advance notice period.

Consistent with this interpretation, we show that it is possible to build a trading strategy that takes advantage of the predictive power of the within-firm variation in the advance notice period. Such a strategy consists in (i) buying stocks when the notice of earnings is issued earlier than the notice of earnings issued one-year ago for the same fiscal quarter, and (ii) selling stocks when this notice of earnings is issued later. This strategy yields substantial abnormal returns of eight basis points per day (circa 1.7% per month) before transaction costs ($t = 4.54$).[4]

Our paper builds on two streams of research. First, we contribute to the literature on investors' limited attention.[5] Several studies in this field examine the effects of investors' attention on stock prices (DellaVigna and Pollet 2009, Hirshleifer, Lim, and Teoh 2009, Peress 2008). They find that stock prices incorporate earnings news less rapidly when investors are less attentive. However, manifestations of investors' inattention are difficult to identify empirically, and recent studies argue that some of these results are not due to investors' inattention but to heterogeneity between firms (Michaely, Rubin, and Vedrashko 2013). Our paper contributes to this debate. We identify a new channel affecting investors' attention and provide additional evidence that a lack of attention affects stock prices. We control for firm fixed-effects in all our tests, which alleviates the concern that our estimated differences in stock price reaction are due to permanent differences between firms. Other studies in this field investigate how investors consciously allocate their attention to particular information. While many studies highlight the influence of external factors (Peng and Xiong 2006, Corwin and Coughenor 2008, Chakrabarty and Moulton 2009), very few studies underline the influence of firm communication policy on the way investors allocate their

[4] Frazzini and Lamont (2007) also identify significant mispricing during earnings announcement period. They find that buying stocks of announcing firms and selling stocks of non-announcing firms every month yield substantial abnormal returns. They suggest that earnings announcements grab the attention of individual investors who rarely short sale and thus push up prices too high, thus creating temporary overpricing. While such overpricing around the earnings announcement date might influence our finding that buying stocks in case of early notice of earnings yield positive abnormal returns, it cannot explain why selling stocks of announcing firms in case of late notice of earnings also yield positive abnormal returns.

[5] See Lim and Teoh in Baker and Nofisnger (2010) for a comprehensive review.

amount of attention. Finally, some studies investigate whether managers strategically exploit investors' inattention during the earnings release process. There are evidence that managers use the display of financial information to influence investors' perception of earnings results, for instance through the issuance of pro forma earnings (Hirshleifer and Teoh 2003, Bradshaw and Sloan 2002). Regarding the strategic timing of earnings release, studies reach mixed conclusions. Some papers highlight that earnings released after the market closes or on Friday are more likely to contain bad news (Patell and Wolfson 1982, Penman 1987, Damodaran 1989, DellaVigna and Pollet 2009, Bagnoli et al. 2005). However, recent papers find no evidence that this empirical regularity is due to managers trying to exploit investors' inattention. In particular, Doyle and Magilke (2009) find no evidence that firms opportunistically report worse news after the market closes or on Fridays. In that respect, our paper provides the first evidence that managers strategically "time" the release of bad or good news as a response to investors' limited attention.

Second, our paper is related to the literature on the timing of earnings announcement. Previous research has consistently identified that managers release bad news late (Kross 1982; Givoly and Palmon 1982; Kross and Schroeder 1984), and that the market reaction to earnings news is extremely negative when such a delay occurs (Begley and Fischer 1998; Bagnoli, Kross, and Watts 2002). In this paper, we control for delays in earnings releases and also verify that our results systematically hold when focusing on the subsample of firms that consistently report their earnings on the same date. Therefore, our results are not driven by this "good news early / bad news late" practice. We contribute to this literature by pointing out to a new dimension of the timing of earnings announcement. We show that, in addition to the choice of the earnings announcement date, managers use the notice of earnings to influence the reaction of investors to good versus bad news.

The rest of the paper is organized as follows. Section 2.2 provides a background description on the earnings release process in the US and develops our hypothesis. Section 2.3 describes the data. Section 2.4 provides evidence that the advance notice period influences investors' attention to earnings news. Section 2.5 examines whether such variation in investors' attention affects stock prices. Section 2.6 provides evidence that firm managers strategically time the notice of earnings. Section 2.7 tests whether investors infer the relation between the advance notice period and the subsequent earnings surprise. Section 2.8 concludes.

## 2.2. Notice of Earnings Disclosure Background and Hypothesis Development

### *2.2.1. Legal requirements and practices*

Pursuant to the 2002 Sarbanes-Oxley Act and the 2004 Regulation Fair Disclosure (*Reg FD*), public companies' quarterly earnings announcements are highly regulated activities, under strict control of the SEC. In particular, the SEC mandates that quarterly earnings releases disclosed by means of a press release trigger the filing of an 8-K form, and the conference call of earnings (if any) should be held shortly hereafter and be easily available to investors (e.g. through a real-time webcast). However, the SEC displays very few requirements regarding the notice of earnings disclosure: consistent with *Reg FD*, detail on when and how to access the conference must be made widely available to all investors, but there are virtually no constraints on when to notify this information to investors.

In the absence of any guidelines, legal advisors recommend the notification to be made at least one week before the earnings announcement.[6] Anecdotal evidence suggests that earnings schedules are known late in the process, and that such a short notice period can be an issue for market participants. The NASDAQ website reports an earnings schedule calendar for firms

[6]"The Earnings Release : Legal Requirements and Best Practices", Insights, March 2008, Aspen Publishers

listed on the NASDAQ[7] based on an "expected date" for earnings release -i.e. an estimation derived from past years' release date, rather than the true date of earnings release. In a letter to the SEC,[8] the CFA institute complains about the notification process (or the lack thereof) and asks the SEC to issue "additional statements […] that encourage companies to announce reasonably ahead of time when earnings will be released". It also expresses its concern that a short notice period may disadvantage some market participants in accessing information related to earnings announcements.[9]

In practice, notices of earnings release are communicated to investors through a specific press release similar to the one reproduced in Appendix A. It shows that, on November 2, 2009, Agilent Technologies issued a press release titled "Agilent Technologies to Host Webcast of Fourth-Quarter Fiscal Year 2009 Financial Results Conference Call" in which the company states that it will release its fourth-quarter earnings result on November 13, 2009. In that case, the earnings announcement date is thus known to potential participants eleven days in advance. We systematically identify those press releases (details on this data step are provided in the Data section) to recover when investors are first notified about the date of earnings release.

### *2.2.2. Hypothesis development*

There are two channels by which early notices of earnings can influence the degree of investors' attention to earning news. First, in the spirit of Kahneman (1973), we consider investors as individuals with a limited amount of attention that they can allocate to the stocks they wish to follow during the earnings announcement season. Constrained by the amount of

[7]Available on http://www.nasdaq.com/earnings/earnings-calendar.aspx?
[8]Available on http://www.sec.gov/comments/s7-23-08/s72308-9.pdf
[9] "We would welcome additional statements by the SEC that encourage companies to announce reasonably ahead of time when earnings will be released. While some companies already engage in this practice, others continue to release earnings statements without any prior notice, which may disadvantage those without the dedicated means to consistently track this information"

information that they can process at the same time, individuals tend to adopt simple rules of thumb to facilitate the decision process. We argue that investors with a crowded agenda during the earnings announcement season adopt a "first-come, first-served" rule by which they follow the order in which they receive the notices of earnings to fill their agenda. This rule implies that a longer advance notice period increases the probability that the announcing firm is first on investors' agenda.

[Insert Figure 2.1 here.]

The second channel by which the advance notice period can affect investors' attention is also a consequence of investors' busy schedule during the earnings announcement season and is better understood visually. Figure 2.1 depicts the average number of earnings announcement by day of the year along with the number of notices of earnings. It is clear from this graph, that during each earnings announcement season, investors potentially face attention constraints, the number of announcements being as high as 250 in a single day (the blue line). And while investors are fully focused on earnings announcement, many notices of earnings (the red line) are also issued during that period. As a consequence, those notices of earnings are more likely to be overlooked by investors. We argue that a longer advance notice period thus reduces the risk that a notice of earnings competes with simultaneous earnings disclosures for investors' attention, thereby increasing the probability that investors include the event in their agenda.

Both views lead to the following two predictions. First, the length of the advance notice period should be positively related to investors' attention to earnings news (*H1*). Second, given limits to arbitrage in the form of risk aversion, lower investors' attention caused by a short notice period should lead to slower information incorporation into stock prices (Hirshleifer and Teoh 2003, DellaVigna and Pollet 2009). Therefore, a short notice period

should generate lower immediate stock price reaction to earnings news and higher post-earnings announcement drift (*H2*).

If changes in advance notice period affect short term stock prices, firm managers may be willing to behave strategically. Several studies document that firm managers care about the value of their firm's stock, for instance for career motives (Healy and Palepu 2001; Graham, Harvey, and Rajgopal 2005). Such motivations lead managers to take actions that maximize stock prices at earnings announcement, such as the timing of news disclosure (Begley and Fischer 1998, Bagnoli, Kross, and Watts 2002). Similarly, career motives and reputation concerns could lead managers to maximize the immediate stock price reaction to earnings news by reducing (increasing) the advance notice period when they plan to announce bad (good) news. In this case, a longer advance notice period would predict better earnings surprise (H3).

## 2.3. Data Collection and Descriptive Statistics

### *2.3.1. Notice of Earnings Release Data*

We obtain corporate earnings schedule for U.S. companies from the Thomson Reuters Archives website[10] which gives unlimited access to all articles published on the Reuters newswire over the 2007-2012 period. A significant part of Reuters' news flow consists of press releases directly written by the companies, in which case Reuters does not alter the original companies' press releases and accepts no responsibility for their content. We focus on such firm-initiated press releases that explicitly schedule an earnings announcement (see Appendix A for an example). We systematically identify those press releases by writing a PERL script that matches string patterns expressing the future action of releasing or

[10] http://www.reuters.com/resources/archive/us/

announcing an earnings such as *[to announce/to report/ to release/to host/ to webcast] [conference call]*.[11] To match those press releases with firm-level data, we also require the press release to include a valid company ticker, i.e., any characters of the press release that match the patterns *(NYSE: )* or *(NASDAQ: )*.

We obtain data on earnings announcement dates from Compustat and I/B/E/S. We start with all quarterly earnings announcements from Compustat with a corresponding record in I/B/E/S, and when the earnings announcement dates between the two sources differ, we apply the procedure described in DellaVigna and Pollet (2009) and take the earlier date as the correct one. Finally, we match each press release of notice with the corresponding notified earnings announcement. The detail of this data step is provided in Appendix B.1. The final sample includes 52,872 notices of earnings that could be matched with their corresponding earnings announcement. This accounts for 3,897 distinct firms.

### *2.3.2. Notice of Earnings Release Descriptive Statistics*

An important issue for us is to check whether the earnings release date notified in the press release is actually met by the firm. In other words, we want to explore whether firms consider the date communicated to the market as binding, or whether they systematically delay or advance the release of earnings. We tackle this question by checking whether the date announced in the press release of notice effectively matches with the actual date of announcement. We perform this step through a specific algorithm and a random sampling.[12] Specifically, we recover from the press release of notification the date at which the announcement is supposed to take place and we compare this date with the announcement date recorded in either Compustat or I/B/E/S. We find that for about 9% of our observations

[11] Other significant string patterns include *[announces] [webcast/conference call]* or *[schedules/will announce] [earnings results]*

[12] Details on this dataset is provided in Appendix B.2.

the announced date does not match the recorded date. We further explore this issue by drawing a random sample of 1% of the unmatched observation and manually collect the actual date of announcement from Factiva. In all cases, firms respect the announcement date they announce but the recorded date in Compustat or I/B/E/S is wrong (often by a day or two). We conclude that all firms respect the date of earnings that they notify to the market. This is consistent with the findings by Duarte-Silva et al. (2010) who show that over the 1995-2006 period, a maximum of 791 earnings announcements were explicitly delayed by the firms.

We define the advance notice period as the difference in days between the earnings announcement date and the first time this date is notified to investors. In Figure 2.2, we graph the distribution of the advance notice period where, for visual purpose, the difference is taken in *calendar* days.[13]

[Insert Figure 2.2 here.]

The distribution exhibits five modes: the first one corresponds to notices made approximately one week (six days) before the earnings announcement; the second one two weeks (14 days) before; the third one three weeks before (21 days) etc... A simple and intuitive explanation for this pattern is that it reflects the efficiency of the internal reporting process of the firm, where some firms with an intrinsic better organizational process would prepare market participants way ahead of the earnings announcement, while poorly organized firms do last minute notifications. We rule out this interpretation by looking at the variation *within* each firm of the advance notice period. In Table 2.1, we define *Advance Notice Period* as the difference in *trading* days between the earnings announcement date and the notification date and we compute the standard deviation of the raw variable (*Overall*); of the average of

[13] In the remainder of the paper, for the sake of homogeneity with the analysis of stock price reaction, we will consider this difference in *trading* days. Results are unaffected if we consider *calendar* days instead.

the variable for each firm across time (*Between*); and of the demeaned variable (*Within*).[14] While part of the total standard deviation (5.95) is driven by cross-sectional differences (3.38), the *within-firm* distribution shows that there exists considerable variation for each firm across time (5.08). This enables us to consider the notification of earnings announcement as a choice of the manager of the firm that varies across time, rather than the output of the firm's internal organizational.

[Insert Table 2.1 here.]

### *2.3.3. Other data sources and sample description*

Analyst EPS estimates and actual earnings are taken from the I/B/E/S files. We use quarterly data and define the earnings surprise as the difference between actual earnings and the consensus analyst forecast from the I/B/E/S consensus file. Denoting $e_{k,t}$ the earnings per share for firm *k* at time *t*, $c_{k,t}$ the corresponding consensus, and $P_{k,t}$ the price of the share at the end of the quarter, the earnings surprise $s_{k,t}$ is defined as:

$$s_{k,t} = \frac{e_{k,t} - c_{k,t}}{P_{k,t}}$$

To mitigate the effects of outliers we remove observations for which the earnings surprise is superior (in absolute term) to one and we trim observations with earnings surprise in the top and bottom 1% of the distribution. Finally, we collect stock return and trading volume from the CRSP dataset, and accounting data from the Compustat dataset. All variables are winsorized at the 1% in each tail. In Table 2.1, we present sample description of the main variables used. Firms, on average send the notices of earnings 10 trading days before the earnings announcement occur. To reflect the sharp categorization of *Advance Notice Period*, evident in Figure 1, we divide this variable into five quintiles. We were able to recover 52,872

[14] Intuitively, the *Between* standard deviation indicates how much firms are on average different from each other, while the *Within* standard deviation gives the variation within each firm of the *Advance Notice Period* across time

earnings announcement with data on the *Advance Notice Period*. By comparison, over the 2007-2012 period, there are 67,253 earnings announcements available in I/B/E/S with valid data on earnings surprise and subject to minimal data requirements. Our sample of firms is thus representative of the universe of announcing firms.

## 2.4. Advance notice period and investors' attention to earnings news

This section examines whether the advance notice period influences investors' attention to earnings news using various proxies for investors' attention.

### *2.4.1. Attendance to earnings conference call*

Existing literature on investors' attention proposes various proxies for investors' attention such as extreme returns (Barber and Odean 2008), trading volume (Gervais, Kaniel, and Mingelgrin 2001; Barber and Odean 2008; Hou, Xiong, and Peng 2009), news and headlines (Barber and Odean 2008; Yuan, 2012), advertising expense (Lou 2009; Chemmanur and Yan 2009), or google searches (Da, Engelberg, and Ga 2011).

In this paper we propose to use a novel and direct measure of investors' attention to earnings news: the number of participants to earnings conference call. We obtain this information from earnings conference call transcripts which report the names of all persons who participate to the conference call, including executives (such as the CEO), and sell-side and buy-side equity analysts.[15] Ideally, we would like the information on all the people who *listen* to the conference call –not only those who *speak,* but since the former measure is not available, we adopt the latter.

[15] Earnings transcripts are collected from the website "Seeking Alpha" (http://seekingalpha.com/)

Our test is a regression of the number of conference call participants (excluding executives) on *Advance Notice Period* divided into five quintiles, controlling for known determinants of investors' attention.[16] We include firm fixed-effects so that coefficient estimates are driven by changes of the variables within each firm. The results of this analysis are shown in Table 2.2.

[Insert Table 2.2 here.]

In the first column of Table 2.2, we find that an increase in the advance notice period increases the number of conference call participants. In term of economic impact, a one-quintile increase of *Advance Notice Period (ANP)* translates into having 0.064 more participants to the conference call. This means that for a firm with an average audience to the conference calls of six people, a 10 week longer advance notice period translates into having one extra person following the call. An additional analysis (not reported) confirms that the effect is driven by firms that switch from the lowest to the highest quintile of *ANP* from one quarter to another. Consistent with the findings in DellaVigna and Pollet (2009) and Hirshleifer, Lim, and Teoh (2009), we also find that the number of market participants attending the conference call is lower on Friday or when there are multiple firms announcing their earnings during the same day, reassuring us on the validity of earnings call attendance as a measure of investors' attention. We next investigate the robustness of this result. In column 2, we examine whether our findings are robust to the inclusion of additional control variables. To the extent that the earnings call participants mentioned in the transcript are only those who ask questions during the call, one possible issue is that our result may be driven by a spurious correlation between the advance notice period and the complexity of the earnings conference call. We control for this aspect by adding a variable measuring the number of words in the CEO's introductory speech, which presumably varies with the degree of complexity of the

[16] Alternatively, we consider a Poisson regression with fixed effects. Inferences are similar.

information released. We also add standard controls such as firm size and market-to-book. The results are virtually unchanged: higher advance notice period increases attendance to the conference call. Last, in column 3, we add earnings announcement date fixed-effects to study the effect of the advance notice period on attendance for firms who announce their earnings on the same day. This specification does not affect our results.

*2.4.2. Trading volume*

Trading volume has often been used as a proxy for investors' inattention (e.g. Barber and Odean, 2008). Consequently, if investors pay lower attention to earnings news when the date and time of the earnings disclosure are notified later, we should observe lower trading volume on the day of earnings announcement. We examine whether this is the case by regressing the abnormal trading volume for company *k* on day *t* on the advance notice period, controlling for variables known to affect the trading volume as well as firm fixed-effects, year-week fixed-effects, fiscal quarter fixed-effects and day of the week fixed-effects. The abnormal trading volume is calculated in the following way:

$$AbVol_{k,t} = \log(vol_{k,t}) - \left( \frac{\sum_{i=21}^{40} \log(vol_{k,t-i})}{20} \right)$$

where $vol_{k,t}$ is the number of shares traded on day *t* for company *k*, and where the "normal" trading volume is the average number of shares traded over a [-40, -20] trading days preceding the earnings announcement. We compute the abnormal trading volume around earnings announcement as the mean abnormal trading volume over a [-1;+1] window surrounding earnings announcement, and denote it *VOL[-1;1]*

[Insert Table 2.3 here.]

In the first column of Table 2.3, we find that the advance notice period is positively correlated with the abnormal trading volume. Controlling for firm fixed effects, later notices of earnings lead to lower trading volume, in line with hypothesis *H1* that investors' attention varies with the advance notice period. This relation is robust to the addition of standard control in the regression (column 2) as well as earnings announcement date fixed-effects (column 3).

### 2.5. Advance notice period and stock price reaction to earnings news

Immediate reaction to earnings announcement and the post-earnings announcement drift has often been associated with plausible proxies for investor attention (DellaVigna and Pollet 2009; Hirshleifer, Lim, and Teoh 2009). We thus predict that the length of the advance notice period is associated with a similar pattern.

We compute earnings return and cumulative abnormal return for different windows at the date of earnings announcement. Specifically, denoting $r_{k,t}$ the return of the share of a company *k* on day *t*, we compute the cumulative abnormal return $CAR[\tau, T]$ over a $[\tau, T]$ window as the buy-and-hold return $\prod_{t=\tau}^{T}(1 + r_{k,t}) - \prod_{t=\tau}^{T}(1 + r_{p,t})$, where $r_{p,t}$ is the characteristics-adjusted portfolio return based on a monthly matching of stocks that belong to the same size/book-to-market/momentum quintiles in the spirit of Daniel, Grinblatt, Titman, and Wermers (1997).

We estimate the following equation:

$$CAR[\tau, T]_{k,t} = \alpha_0 + \beta\, DS_{k,t} + \gamma\, Advance\ Notice\ Period_{k,t} +$$

$$\delta\, DS_{k,t} \times Advance\ Notice\ Period_{k,t} + \zeta\, Controls_{k,t} + \varepsilon_{k,t}$$

where $DS_{k,t}$ is the earnings surprise for firm *k* at quarter *t* divided into ten deciles of surprise, and where *Advance Notice Period* is divided into five quintiles. The coefficient $\beta$ is the stock price response to the level of earnings surprise. Our interest is in the coefficient $\delta$ of the interaction between *DS* and *Advance Notice Period* which gives the sensitivity of the stock price response to earning surprise conditional on the level of the advance notice period. In other words, it gives the magnitude of the response to earnings surprise when managers vary the length of the notification. To control for known determinants of the post-earnings announcement drift, we include a set of dummies for the day of the week (DellaVigna and Pollet 2009), as well as the number of earnings announcement made on the same day (Hirshleifer, Lim, and Teoh 2009). We include the standard set of controls and each time we include a control, we also interact it with the level of earnings surprise. We compute robust standard errors clustered by the date of announcement.

[Insert Table 2.4 here.]

Table 2.4 presents our results where all specifications include firm fixed effects. In the first three columns, we display the immediate reaction to earnings announcement. On average, a one-quintile increase in the advance notice period translates into a three percentage point increase in the immediate reaction to earnings announcement. This result is robust to the inclusion of standard controls and date fixed effects. In the last three columns, we display results of the analysis of the post-earnings announcement drift. We compute the cumulative abnormal return over a 40 trading day window in the two days following the announcement. On average, over this period, a one-quintile increase in the advance notice period is associated with a five percentage point decrease in the post-earnings announcement drift.

Overall, across all specifications, the immediate reaction is larger and the post-earnings announcement is weaker for firms with higher value of *Advance Notice Period*. This suggests that an early notification of earnings release increases the speed of incorporation of earnings

information by market participants. The length of the notification has real effects on firms' stock price.

## 2.6. Do firms strategically notify the date and time of earnings disclosure?

This section examines whether firms strategically choose the date at which they send the notice of earnings to attract (escape) investors' attention when they plan to issue good (bad) news.

### *2.6.1. Advance notice period and earnings surprise*

If managers behave strategically, then a change in the advance notice period should predict the forthcoming earnings surprise. Specifically, we expect to find under this strategic behavior assumption that good earnings surprise are notified earlier than bad earnings surprise. We thus expect a positive correlation between the earnings surprise and the advance notice period at the firm level. We test this prediction by regressing the earnings surprise on the advance notice period, controlling for firm fixed-effects in all specifications

[Insert Table 2.5 here.]

In the first column of Table 2.5, we regress the earnings surprise on the advance notice period with no time-varying control variables, but controlling for firm fixed effects, fiscal quarter fixed effects, and time fixed effects. Consistent with the predictions of the strategic behavior hypothesis, we find that the advance notice period predicts the earnings surprise. In terms of economic magnitude, one within-firm standard deviation of the advance notice period divided into quintiles (1.23 in Table 2.1) explains about 5% of the within-firm standard deviation of the earnings surprise (1.23*0.053/1.343=4.8%). In a non-reported regression, we

use the earnings surprise before normalization by the stock price as a dependent variable, and find that a six trading day longer advance notice period leads to an increase in the earnings surprise by about one penny.

We investigate the robustness of this result in the rest of the table. A first concern is the possibility that the advance notice period is correlated with the date surprise, i.e. the difference between the date of earnings announcement expected by investors, and the earnings announcement date. If so, our result may be driven by the well documented fact that firms change their earnings announcement date and choose later-than-expected date of earnings announcement when they plan to disclose bad news (Kross and Schroeder, 1984, Begley and Fischer, 1998, and Bagnoli, Kross, and Watts, 2002). To control that this effect does not drive the correlation between earnings surprise and advance notice period, we add two well-known controls associated with the timing of earnings release: (i) the date surprise (i.e. the difference between the earnings announcement date and the 'expected announcement date', defined as the same day of the week as the earnings four quarter previous), and (ii) the reporting lag (i.e. the difference between the date of announcement and the quarter-end date). In column (2), the addition of these two control variables does not change our result. In column (3), we further add traditional control variables such as size, age, and market-to-book and still find unchanged results. To rule out the possibility that our result is driven by firms announcing bad results late, we focus on the subset of firms that consistently announce their earnings at the same date. We define this subset as the firms that do not change their earnings announcement date in more than 80% of the case.[17] Again we still find a positive correlation between the advance notice period and earnings surprise (column 4). Finally, we check for the robustness of this correlation by using alternative definition of earnings surprise such as a dummy

[17] We choose the 80% threshold because a higher threshold dramatically reduces the size of the sample.

variable equal to one if the surprise is positive (column 5), and the quarterly net income growth on a year-on-year basis (column 6).

### *2.6.2. Cross-sectional results on firm visibility*

Next, we check that the magnitude of the correlation between the advance notice period and the earnings surprise varies according to the degree of firm visibility. Indeed, being able to attract or escape investors' attention should be less of an issue for more visible firms which are consistently scrutinized by the market and much more of an issue for less visible firms.

We use three proxies to measure firm visibility: (i) the difference between the fiscal year-end of the company and the average of its industry, (ii) the analyst coverage of the firm, and (iii) the market capitalization of the firm. We use the absolute difference in number of days between the fiscal year-end of the firm and the average fiscal year-end of its peers from the same three-digit SIC code as a measure of visibility because earnings are mechanically less likely to be announced at the same time as the earnings of industry peers when this difference is large. If so, investors are less likely to be distracted by simultaneous announcements made by firms from the same industry and the firm should then be more visible. We also follow the literature and use both the number of sell side analysts (e.g., Baker, Nofsinger, and Weaver 2002) and the size of the firm as measured by the natural logarithm of its market capitalization to assess the degree of firm visibility. For each criterion, we split our sample into three categories of visibility (low, medium, and high), and then define three dummy variables corresponding to each degree of firm visibility. With respect to the fiscal year-end criterion, the high (low) dummy variable is equal to one if the absolute difference between the fiscal year-end of the firm and the industry average is in the top (bottom) 25 centiles of the distribution during the quarter, and zero otherwise. The medium dummy variable is equal to one if both the high and low dummy variables are equal to zero. We follow the same

methodology to define the high, medium and low dummy variables for the other two criterions. Finally, we perform a regression of earning surprise on advance notice period similar to the specification in Table 2.5 column (3) where we add an interaction term between advance notice period and the three dummy variable measuring the degree of firm visibility.

[Insert Table 2.6 here.]

Column 1 to 3 of Table 2.6 show that the advance notice period is much more predictive of the earnings surprise when the visibility of the firm is low. By contrast, we find that when the visibility of the firm is high, the magnitude of the correlation between advance notice period and earnings surprise is low and even sometimes not statistically different from zero. On average, for a *High Visibility* firm, going to the *Low Visibility* subgroup implies that a one-quintile increase in the advance notice period will be from now on associated with a 10 percentage point increase of the normalized earnings surprise. In all three cases, an F-test indicates that the difference between the two coefficients (Advance Notice Period x High Visibility vs. Advance Notice Period x Low Visibility) is statistically significant at the 1% or 5% level.

### *2.6.3. Cross-sectional results on managerial horizon*

Last, we investigate how the strategic use of the advance notice period varies according to the horizon of the managers. We use two measures for managerial horizon. First, we use the amount of new equity to be issued one quarter forward. We assume that managers who plan to issue equity in the next quarter has greater incentives to maximize short-term stock prices than managers who do not intend to raise funding on the equity market. Second, we measure managerial horizon by the share turnover during the last month of the previous quarter. We assume that high share turnover signals short-term oriented shareholders. Managers who

maximize shareholders' value exhibit shorter horizons when share turnover is high. Our strategy is to regress earnings surprise on advance notice period, a proxy for managerial horizon (equity issuance or share turnover), and an interaction term between horizon and advance notice period.

[Insert Table 2.7 here.]

Table 2.7 presents our results. For each proxy of managerial horizon, the interaction term is positive statistically significant. This indicates that a change in the advance notice period is more informative about the earnings surprise when managers have shorter horizons. This suggests that firm managers respond strategically to investors' limited attention by making shorter or longer advance notice period when it is in their interest to do so.

## 2.7. The effects of investors' attention management and trading strategy

So far, we found that firms use the advance notice period to strategically time the disclosure of bad versus good news. A natural question is whether investors are aware of this "attention management" strategy by firms. We are thus interested in whether investors perceive that an early notice implies that firms will disclose good news while a late notice is indicative of bad news. To explore this question, we run two different types of analyses: first, we look at the stock price reaction at the date of the notice of earnings; second, we build a trading strategy that takes advantage of the predictive power of the advance notice period on earnings surprise.

### *2.7.1. Stock price reaction to notices of earnings*

If investors are aware of the strategic behavior of the firm regarding the advance notice period, then we should observe a stock price reaction at the date of notice that reflects the new information received by investors. Specifically, investors should react positively to a longer

advance notice period (indicative of a future positive earnings surprise) and negatively to a shorter advance notice period. To answer this question, we compute the immediate reaction at the date of notification as the cumulative abnormal return over a [-1;+1] window centered around the date of notification (*CAR[-1;1]).* We then regress the immediate stock price reaction on *Advance Notice Period* and a set of control variables. Table 8 displays the results and show that the coefficient on *Advance Notice Period* is not statistically different from zero. This suggests that market participants fail to understand the implication of an early notice on the subsequent earnings. Interestingly, the coefficient on *Date Surprise* is statistically and economically significant, suggesting that investors react to the information content of the press release and interpret negatively any earnings release date that falls after the usual announcing date, consistent with the findings by (Bagnoli, Kross, and Watts 2002). However, they fail to fully integrate the information conveyed by the date at which this press release is issued.

[Insert Table 2.8 here.]

*2.7.2. Trading strategy*

The possibility still exists that investors react to early or late notice between the date of notification and the date of earnings announcement. A more comprehensive way to test whether investors integrate the information conveyed by early versus late notice is to build a trading strategy that takes advantage of the predictive power of within-firm variations in the advance notice period on earnings surprise. We follow a strategy similar in spirit to Barber, Lehavy, McNichols and Trueman (2007) that take advantage of analysts' upgrades and downgrades. By analogy with their strategy, we 'upgrade' a stock when the notice of earnings is made earlier than one year before and we 'downgrade' the stock otherwise. We thus form two distinct "buy" and "sell" portfolio. Our strategy consists in (i) buying stocks when the notice of earnings is issued earlier than the notice of earnings one-year ago for the same fiscal

quarter, and (ii) selling stocks otherwise. It is important to stress that this strategy only exploits information that are easily known by any investors. In fact, it only requires keeping track of last year's notices of earnings for each firm in the portfolio.

We create calendar-time portfolios that invest one dollar each time an earnings release is notified. Let $x_{k,t}$ denote the compounded daily return of stock *k* from the date of notification through date *t*. The equally-weighted portfolio return on date *t* is given by:

$$\frac{\sum_{k=1}^{n_t} x_{k,t-1} R_{k,t}}{\sum_{k=1}^{n_t} x_{k,t-1}}$$

where $n_t$ is the number of stocks (or notifications) held in the portfolio at date *t* and $R_{k,t}$is the total return of stock *k* on calendar date *t*. Similarly, we define the value-weighted portfolio return on date *t* as:

$$\frac{\sum_{k=1}^{n_t} x_{k,t-1} R_{k,t}}{n_t}$$

The buy portfolio consists in buying a stock when the notice of earnings is issued earlier than four quarter previous, and the position is held until five days have passed after the earnings announcement. That way, an investor takes advantage of the positive stock price reaction that follows a positive earnings surprise. The five day cutoff is here to ensure that an investor benefit from the position even if the market reacts to the earnings surprise with a delay. The sell portfolio is constructed in a similar way.

We compute the risk-adjusted return of each portfolio *p* using the Carhart 4-factor model:

$$R_{p,t} - R_{t,f} = \alpha_p + \beta_p\left(R_{m,t} - R_{t,f}\right) + s_p SMB_t + h_p HML_t + m_p WML_t + \varepsilon_{pt}$$

where $R_{pt}$is the portfolio return on date *t*, $R_{m,t}$ is the market return on date *t*, $R_{t,f}$ is the risk-free rate on date *t*, and $SMB_t$, $HML_t$, $WML_t$ are the size, book-to-market, and momentum

factors taken from Kenneth French's website. We compute robust standard errors using the Newey-West estimator with six lag.

[Insert Table 2.9 here.]

Table 2.9 presents the results. In column (1) to (3), we present results for value-weighted portfolios where the first line (*Constant*) denotes the excess return. A long-short portfolio that buys early notifications and sells late notifications generates an excess return of 8.4 basis points per day. In column (4) to (6) we present results for the equally-weighted portfolio. We find an excess return of the same order of magnitude. In both cases, the long-short portfolio generates an excess return of around 1.7% per month.

## 2.8. Conclusion

While investors' inattention to earnings announcement has been consistently shown as an explanation for several market inefficiencies, there have been mixed evidence of managers trying to benefit from this bias by timely disclosing bad news when investors are inattentive. We contribute to this literature by looking at the preparation by firms of earnings announcements through the notification of earnings disclosure. We show that the length of the advance notice period affects investors' attention to earnings news and stock price reaction at announcement. Firm managers make use of the advance notice period to time the release of good versus bad news. We find that the length of the advance notice period is predictive of earnings surprise, with longer notices being associated with more positive earnings surprise, and that this strategic behavior is more pronounced for firms that face visibility issues. Investors fail to fully understand the implication of early versus late notice on the level of earnings surprise. A long-short portfolio that buys stocks with early notices and sells stocks with late notice generates an excess return of 1.7% per month.

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## 2.10. Appendix

### *2.10.1. Notice of earnings example: Agilent Technologies*

# Agilent Technologies to Host Webcast of Fourth-Quarter Fiscal Year 2009 Financial Results Conference Call

* Reuters is not responsible for the content in this press release.

Mon Nov 2, 2009 11:00am EST

SANTA CLARA, Calif.--(Business Wire)--
Agilent Technologies Inc. (NYSE:A) will release its fourth-quarter fiscal year 2009 financial results before the stock market opens on Nov. 13. The company will host a live webcast of its investor conference call in listen-only mode.

Date: Friday, Nov. 13

Time: 5:30 a.m. (PT)

Web access: www.investor.agilent.com

Listeners may log on and select "Q4 2009 Agilent Technologies Inc. Earnings Conference Call" in the "News & Events -- Calendar of Events" section. The webcast will remain on the company site for 90 days.

In addition to the online broadcast, a telephone replay of the conference call will be available at 8:30 a.m. (PT) after the call on Nov. 13 through Nov. 20 dialing +1 888 286 8010 (for international, dial +1 617 801 6888) and entering pass code 96035796.

About Agilent Technologies

Agilent Technologies Inc. (NYSE:A) is the world`s premier measurement company and a technology leader in communications, electronics, life sciences and chemical analysis. The company`s 18,000 employees serve customers in more than 110 countries. Agilent had net revenues of $5.8 billion in fiscal 2008. Information about Agilent is available on the Web at www.agilent.com.

NOTE TO EDITORS: Further technology, corporate citizenship and executive news available on the Agilent news site at www.agilent.com/go/news.

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### *2.10.2. Data processing*

This appendix provides details on two important data steps. First, we show how to match each press release of notification with the corresponding earnings announcement. Second, we describe how to identify whether the release date set forth in the press release of notification has been respected by the company.

*Matching press releases with the corresponding earnings announcement*

We match each press release of notification with the corresponding earnings on the basis of the fiscal quarter-year reported in the press releases. To recover the fiscal quarter in the press releases, we search for string pattern such as *[first/second/third/fourth] [quarter]*. It is more challenging to recover the fiscal year as several years can be mentioned in a press release (not only the fiscal year but also the year at which the announcement actually occurs). We opt for the following approach: for each press release in our dataset, identified by a company ticker and a fiscal quarter, we look forward to identify the next earnings announcement made by the firm in the fiscal quarter mentioned in the press release. When there are several press releases that notify the same earnings announcement, we take the earlier one to identify the *first* time the earnings announcement date was made public to investors. Finally, we remove press releases where the notification is published on the same day of the earnings announcement.

*Checking that the notified release date has been respected by the firm*

For each press release of notification, we search for string patterns that match a date i.e. any strings of the form *[Month Day, Year]* such as May 9, 2012. A press release of notification can mention several other dates than the date of earnings announcement (e.g. the date until which the conference webcast will be available). We then check whether at least one of those dates reported in the press release match with the actual date of announcement. If this is the

case, we consider the company to respect its notification. Of the 54,570 notices of earnings in the initial dataset, we were able to identify 49,441 earnings announcements where the release date announced in the press release matches either the Compustat or the I/B/E/S reporting date. We are thus left with 5,129 notices of earnings (about 9.4% of the dataset) where the earnings release date is potentially not respected by the firm. Due to the difficulty of extracting the date of announcement from the text of the press release, this figure represents an upper bond of the number of non-respected earnings notification. To further examine this question, we draw a random sample of 1% of the unmatched observations (52 press releases) and manually check whether the date of announcement has been respected by the firm. We find that in 71% of the case (37 observations), firms actually respect their earnings schedule date, but our procedure fail to identify it[1]. In the remaining 29% of the cases (15 observations), firms respect their notifications, but the earnings announcement date recorded in either I/B/E/S or Compustat is wrong (often by a day or two) and no match can thus be found. On this random sample, we thus find no firms that do not respect the date of earnings that they announce in advance to market participants.

[1] For instance the date "May 9, 2012" can be displayed in the press release under the form "Wednesday, May 9". The latter expression is not matched by our procedure

### *2.10.3. Variables definition*

| | |
|---|---|
| *Advance Notice Period* | The number of days between the release of the notice detailing the date and time of the forthcoming quarterly earnings disclosure, and the earnings announcement day, divided into five quintile. |
| *EPS Surprise* | The difference between the announced earnings per share and the consensus earnings per share, normalized by the stock price at the end of the corresponding quarter. |
| *DS* | The *Earnings Surprise* divided into ten deciles |
| *Absolute Earnings Surprise Decile* | The absolute value of *Earnings Surprise* divided into ten deciles |
| *Date Surprise* | The difference in calendar days between the earnings announcement date and the expected announcement date, defined as the same day of the week as the earnings four quarter previous. |
| *Reporting Lag* | The difference in calendar days between the date of announcement and the quarter-end date |
| *Number of Announcements* | The number of earnings announcements that occur on the same day. |
| *Friday* | Dummy variable for announcements made on Friday. |
| *Number of Analysts* | The numbers of analysts who attend the conference call. |
| *Number of Words* | The number of words in the CEO's introductory speech. |
| *Size* | The natural logarithm of market capitalization. |
| *Market-to-Book* | Market to book ratio |
| *Age* | The number of year elapsed since a firm's inception |
| *RoA* | Net Earnings over Assets at the end of the period |

**Figure 2.1**

***Number of Events Related to Quarterly Earnings Disclosure***

This figure plots the average number of events related to quarterly earnings disclosure by day within a trading year, based on (i) a sample of 4,875 US firms (90,870 observations) from the Compustat Quarterly database and (ii) a sample of 3,897 US firms (52,872 observations) from Reuters Archive over the 2007-2012 period. It figures the number of earnings announcements (the blue line), and the number of notices of earnings (the red line).

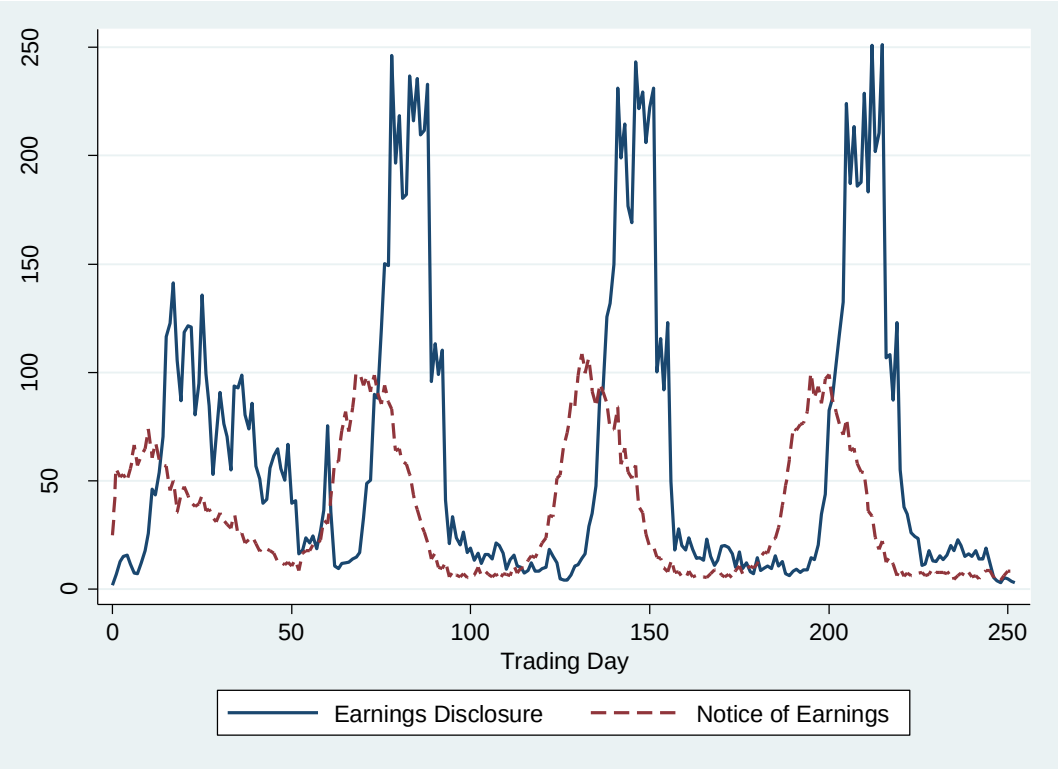
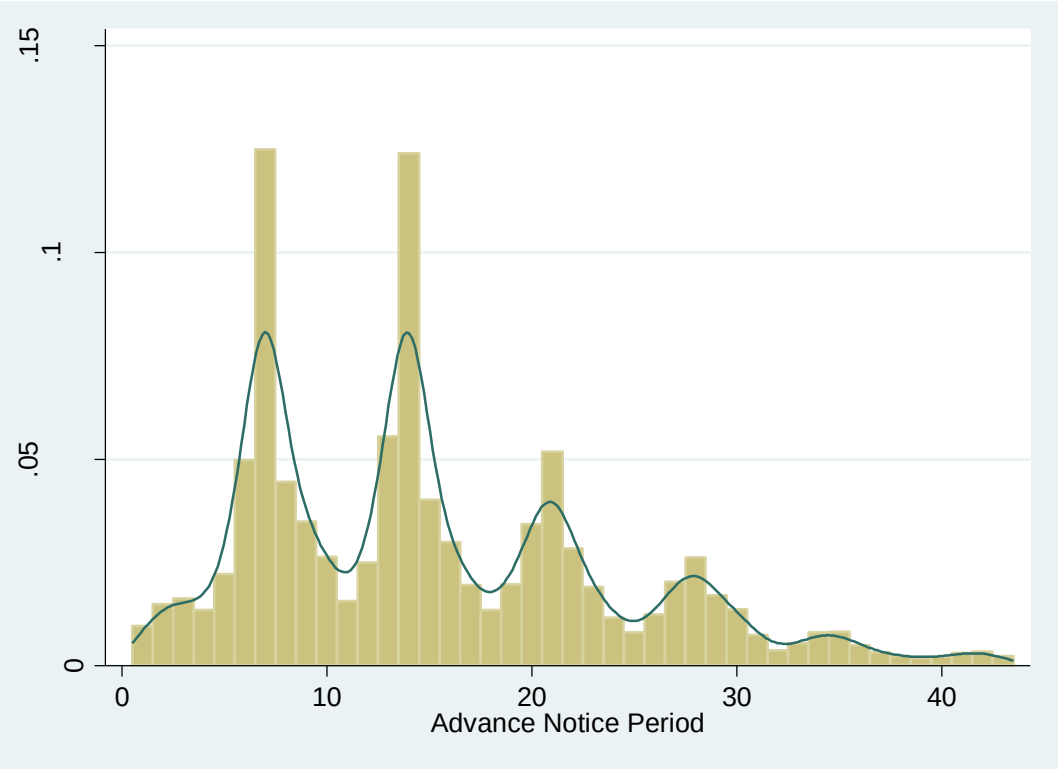


**Figure 2.2**

***Advance Notice Period Distribution***

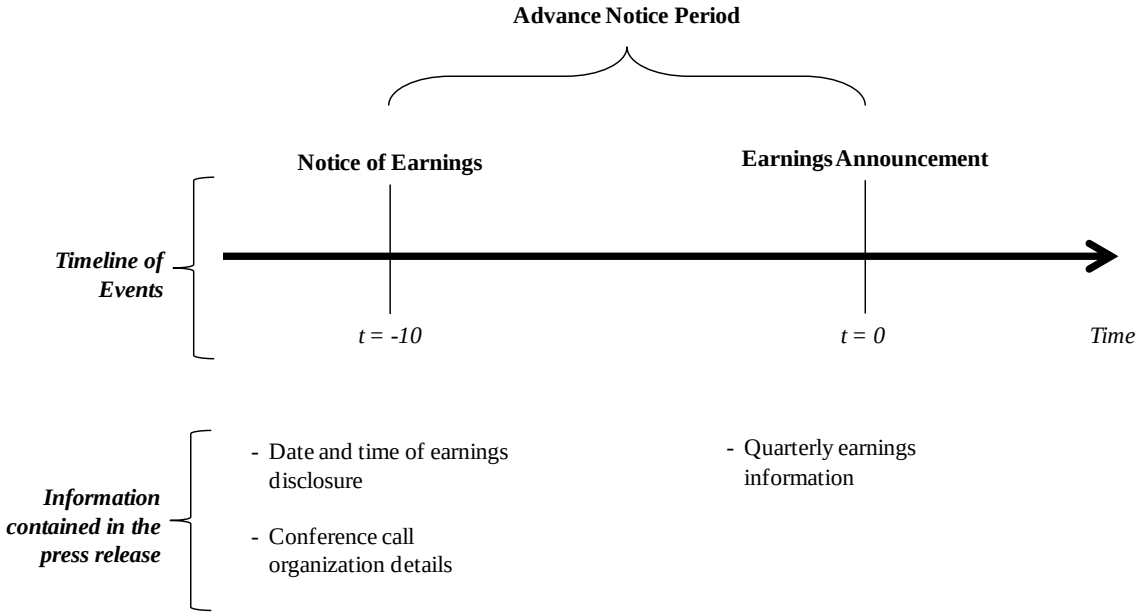
This figure displays the density function of the advance notice period, the number of calendar days between the notice detailing the date and time of the forthcoming quarterly earnings disclosure, and the earnings announcement day. The sample includes 52,872 observations, corresponding to any notice detailing the date and time of the forthcoming quarterly earnings disclosure which we are able to identify in the Reuters press release database, and which we are able to match with 3,897 US firms from the Compustat Quarterly database over the 2007-2012 period



## Figure 2.3

### *Timeline of Events*

This figure presents the standard timeline of the earnings release process in the US. Information about the organization of the forthcoming earnings release is sent on average ten days before the event. Such information typically includes the date and time of earnings release as well as the earnings conference call number. We call the action of sending this information to market participants "Notice of Earnings". We call the action of disclosing quarterly earnings information to market participants "Earnings Announcement". The "Advance Notice Period" is the number of days between the date of the first notice of earnings and the earnings announcement date.



## Table 2.1

### *Descriptive Statistics*

This table presents summary statistics for our main variables. The sample includes 90,870 firm-quarter observations over the 2007-2012 period corresponding to 4,875 US firms from the Compustat Quarterly database and the I/B/E/S database. Of these 90,870 earnings announcements, 52,872 observations (3,897 firms) could be matched with the corresponding notice of earnings date from Thomson Reuters Archive. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date. *EPS Surprise* is the difference between the announced earnings per share and the consensus earnings per share, normalized by the stock price at the end of the corresponding quarter. All other variables are defined in Appendix C. All continuous variables are winsorized at the 1% level in each tail.

| | Count | Mean | p10 | p50 | p90 | Standard Deviation | | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | Overall | Between | Within |
| Advance Notice Period (Continuous) | 52 872 | 10.58 | 4.00 | 10.00 | 19.00 | 5.95 | 3.38 | 5.08 |
| Advance Notice Period (Quintile) | 52 872 | 2.87 | 1.00 | 3.00 | 5.00 | 1.46 | 0.84 | 1.23 |
| EPS Surprise | 67 253 | -0.121 | -0.929 | 0.054 | 0.866 | 1.838 | 1.646 | 1.343 |
| Size | 89 285 | 6.63 | 4.02 | 6.65 | 9.18 | 1.98 | 0.31 | 1.95 |
| RoA | 89 171 | -3.98% | -5.64% | 0.55% | 3.36% | 10.81 | 10.11 | 5.79 |
| Leverage | 88 708 | 0.23 | 0.00 | 0.16 | 0.53 | 0.27 | 0.14 | 0.24 |
| Market-to-Book | 84 467 | 3.05 | 0.79 | 1.88 | 5.83 | 4.03 | 2.35 | 4.08 |
| # Analysts at Earnings Call | 14 675 | 7.40 | 3.00 | 7.00 | 12.00 | 3.50 | 1.85 | 2.94 |

## Table 2.2

### *Advance Notice Period and Attendance to Earnings Calls*

This table presents panel regressions examining the effect of the advance notice period on the number of participants to the earnings conference call. The dependent variable is the number of conference call participants. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. *Friday* is a dummy variable for announcements made on Friday. *Number of Announcements* is the number of contemporaneous announcements. *Number of Words* corresponds to the number of words in the CEO's introductory speech. *Date Surprise* is the difference between the earnings announcement date and the expected announcement date. *Reporting Lag* is the difference in calendar days between the announcement date and the quarter-end date. All other variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and clustered by firm. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Number of Conference Call Participants

| | (1) | (2) | (3) |
|---|---|---|---|
| Advance Notice Period | 0.064** | 0.064** | 0.080** |
| | (2.38) | (2.28) | (2.54) |
| Friday | -0.266** | -0.272** | - |
| | (-2.15) | (-2.18) | - |
| Number of Announcements | -0.001*** | -0.001*** | - |
| | (-2.86) | (-3.02) | - |
| Number of Words | | -0.018*** | -0.017*** |
| | | (-4.24) | (-3.51) |
| Date Surprise | | -0.011** | -0.006 |
| | | (-1.98) | (-0.91) |
| Reporting Lag | | -0.004 | 0.003 |
| | | (-1.13) | (0.44) |
| Size | | 0.481*** | 0.400** |
| | | (3.46) | (2.54) |
| Market-to-Book | | 0.009 | 0.013 |
| | | (0.55) | (0.70) |
| Age | | 0.017 | 0.032 |
| | | (0.03) | (0.04) |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | - |
| Earnings Announcement Date Fixed Effects | No | No | Yes |
| Adj. $R^2$ | 2.2% | 2.9% | 3.1% |
| N | 11,994 | 11,420 | 11,420 |

## Table 2.3

### *Advance Notice Period and Trading Volume Response to Earnings News*

This table presents panel regressions examining the effect of the advance notice period on trading volume response to earnings news. Abnormal trading volume on day *t* is defined as the log trading volume on that day minus the average log trading volume over a [-40,-21] trading day window preceding day *t*. The dependent variable VOL[-1,1] is the average abnormal trading volume over days [-1,1] surrounding the announcement date. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. All other variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and clustered by day of announcement. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Abnormal Volume at Earnings Announcement Date

| | (1)<br>VOL [-1,1] | (2)<br>VOL [-1,1] | (3)<br>VOL [-1,1] |
|---|---|---|---|
| Advance Notice Period | 0.0054** | 0.0070*** | 0.0075** |
| | (2.32) | (2.85) | (2.58) |
| Number of Announcements | 0.0000 | 0.0000 | - |
| | (0.40) | (0.17) | - |
| Date Surprise | | 0.0004 | 0.0004 |
| | | (0.58) | (0.63) |
| Reporting Lag | | -0.0013 | -0.0005 |
| | | (-1.29) | (-0.44) |
| Absolute Earnings Surprise Decile | | 0.0149*** | 0.0145*** |
| | | (14.98) | (13.64) |
| Market-to-book | | 0.0012 | 0.0008 |
| | | (1.22) | (0.77) |
| Size | | -0.011 | -0.0121 |
| | | (-1.17) | (-1.11) |
| Age | | -0.3131 | -0.0702 |
| | | (-1.09) | (-0.20) |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Week Fixed Effects | Yes | Yes | - |
| Day of Week Fixed Effects | Yes | Yes | - |
| Earnings Announcement Date Fixed Effects | No | No | Yes |
| Fiscal Quarter Fixed Effects | Yes | Yes | Yes |
| Adj. $R^2$ | 10.6% | 13.5% | 36.7% |
| N | 52,816 | 41,984 | 41,984 |

## Table 2.4

### *Advance Notice Period and Market Reactions to Earnings News*

This table presents panel regressions examining the effect of the advance notice period on the relation between announcement or post-announcement returns and earnings surprises. The dependent variable is indicated under each column heading. *DS* is earnings surprise deciles (DS=1: lowest, 10: highest). *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. Control variables include *Date Surprise*, *Reporting Lag*, *Number of Announcements*, *Size*, *Market-to-Book*, *Number of Analysts*, and indicator variables for each day of the week. All control variables are also interacted with the deciles of surprise *(DS)*. See Appendix C for variable definitions. Standard errors are adjusted for heteroskedasticity and clustered by the day of announcement. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Market Reaction to Earnings Announcement

| | (1) CAR [-1,1] | (2) CAR [-1,1] | (3) CAR [-1,1] | (4) CAR [2,42] | (5) CAR [2,42] | (6) CAR [2,42] |
|---|---|---|---|---|---|---|
| Advance Notice Period x DS | 0.030*** | 0.029** | 0.027** | -0.057*** | -0.057** | -0.045** |
| | (2.94) | (2.29) | (1.99) | (-2.97) | (-2.53) | (-1.97) |
| Advance Notice Period | -0.131* | -0.08 | -0.065 | 0.209 | 0.237 | 0.187 |
| | (-1.77) | (-0.88) | (-0.69) | (1.48) | (1.44) | (1.10) |
| DS | 0.994*** | 1.573*** | 1.811*** | 0.401*** | 0.610** | 0.603** |
| | (29.01) | (11.54) | (14.41) | (6.29) | (2.41) | (2.53) |
| Controls (Interacted) | No | Yes | Yes | No | Yes | Yes |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Week Fixed Effects | Yes | Yes | - | Yes | Yes | - |
| Fiscal Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Earnings Ann. Date Fixed effects | No | No | Yes | No | No | Yes |
| Adj. $R^2$ | 12.9% | 14.2% | 15.1% | 1.0% | 2.0% | 2.3% |
| N | 43,405 | 34,306 | 42,580 | 42,580 | 34,141 | 34,114 |

## Table 2.5

### *Advance Notice Period and Earnings Surprise*

This table presents panel regressions examining the effects of the advance notice period on the level of earnings surprise at the time of the earnings announcement. The dependent variable is the earnings surprise. In columns (1) to (4), the earnings surprise is the difference between the announced earnings per share and the consensus earnings per share, normalized by the stock price at the end of the corresponding quarter. In column (4), we restrict the sample to firms whose earnings announcement date is always the same. In column (5), the earnings surprise is a dummy equal to 1 if the surprise is positive and 0 if not. In column (6), the earnings surprise is the net income growth in the quarter relative to the same quarter of the previous year. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. All other variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and clustered by firm. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Earnings Surprise at the Earnings Announcement Date

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | (Actual EPS - Estimated EPS) / Price | | | | Positive EPS Surprise | Net Income Growth |
| Advance Notice Period | 0.053*** | 0.064*** | 0.066*** | 0.050** | 0.017*** | 12.073*** |
| | (4.75) | (5.51) | (5.60) | (2.33) | (5.66) | (4.13) |
| Date Surprise | | -0.008*** | -0.013*** | 0.001 | -0.004*** | -5.144*** |
| | | (-3.97) | (-4.58) | (0.13) | (-5.85) | (-6.09) |
| Reporting Lag | | -0.007*** | -0.006*** | 0.003 | -0.001 | -2.834*** |
| | | (-3.80) | (-2.93) | (1.42) | (-1.45) | (-5.53) |
| Size | | | -0.214*** | -0.256*** | -0.048*** | -162.084*** |
| | | | (-4.02) | (-2.66) | (-4.27) | (-10.38) |
| Market-to-book | | | 0.002 | -0.009 | 0 | 4.135*** |
| | | | (0.52) | (-1.12) | (-0.24) | (3.02) |
| Age | | | -3.420** | -19.541 | -0.531 | -996.153 |
| | | | (-2.14) | (-1.46) | (-0.70) | (-0.64) |
| RoA | | | -0.544* | 0.462 | -0.123* | -1575.286*** |
| | | | (-1.66) | (0.63) | (-1.90) | (-6.57) |
| Leverage | | | 0.430** | 0.469 | 0.116*** | 243.681*** |
| | | | (2.52) | (1.52) | (3.01) | (5.48) |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Fiscal Quarter Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample restriction | No | No | No | Yes | No | No |
| Adj. $R^2$ | 0.7% | 0.8% | 1.0% | 0.8% | 0.6% | 4.6% |
| N | 45,617 | 44,704 | 42,060 | 9,336 | 42,060 | 35,382 |
| # Firms | 3,731 | 3,663 | 3,536 | 653 | 3,536 | 3,248 |

## Table 2.6

### *Cross-sectional Effects According to Firm Visibility*

This table presents panel regressions examining the cross-sectional effects of the advance notice period on the level of earnings surprise according to firm visibility. The dependent variable is the earnings surprise defined as the difference between the announced earnings per share and the consensus earnings per share, normalized by the stock price at the end of the corresponding quarter. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. Firm visibility is measured using three criteria: the fiscal year-end month of the firm relative to the average of the industry (same three-digit SIC code), the number of analysts covering the firm, and its market capitalization. *High* (*Low*) *Visibility* is a dummy variable equal to one if the proxy for firm visibility falls in the top (bottom) 25 centiles of the distribution during the quarter and zero otherwise. *Medium* is a dummy variable equal to one if both L*ow* and *High* equal zero, and zero otherwise. Controls variables include *Date Surprise*, *Reporting Lag*, *Size*, *Market-to-Book*, *Leverage*, and *Age*. All control variables are interacted with *High Visibility, Medium Visibility*, and *Low visibility*. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and clustered by firm. t-statistics are in parentheses. The bottom of the table reports coefficients and f-statistics of an F-test that tests the equality of coefficient estimates for two variables: Advance Notice Period x High Visibility, and Advance Notice Period x Low Visibility. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Earnings Surprise at the Earnings Announcement Date

| Proxy for visibility | (1)<br>Fiscal Year-End | (2)<br># Analysts | (3)<br>Market Cap. |
|---|---|---|---|
| Advance Notice Period x High Visibility | 0.034 | 0.036*** | 0.014 |
| | (1.63) | (2.66) | (1.40) |
| Advance Notice Period x Medium Visibility | 0.063*** | 0.056*** | 0.071*** |
| | (4.11) | (3.55) | (4.85) |
| Advance Notice Period x Low Visibility | 0.105*** | 0.108*** | 0.184*** |
| | (4.41) | (3.52) | (3.33) |
| High Visibility | -0.21 | -0.781** | -2.351*** |
| | (-0.39) | (-2.01) | (-3.08) |
| Medium Visibility | -0.209 | -0.603* | -2.458*** |
| | (-0.68) | (-1.65) | (-3.60) |
| Controls (Interacted) | Yes | Yes | Yes |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes | Yes |
| Fiscal Quarter Fixed Effects | Yes | Yes | Yes |
| Adj. $R^2$ | 1.2% | 1.1% | 1.7% |
| N | 42,060 | 34,783 | 42,060 |
| # Firms | 3,536 | 3,488 | 3,536 |
| ANP x Low Visibility - ANP x High Visibility | 0.071** | 0.072** | 0.170*** |
| F-test | (4.66) | (9.26) | (5.04) |

## Table 2.7

### *Cross-sectional Effects according to Managerial Horizon*

This table presents panel regressions examining the cross-sectional effects of the advance notice period on the level of earnings surprise according to managers' horizon. The dependent variable is the earnings surprise defined as the difference between the announced earnings per share and the consensus earnings per share, normalized by the stock price at the end of the corresponding quarter. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. Managerial horizon is measured using two criteria: the amount of new equity that will be issued in the next quarter scaled by the firm market capitalization (*New Equity Issue*$_{q+1}$), and the share turnover during the last month of the previous quarter (*Share Turnover*$_{q-1}$). In column (1), *Short Horizon* is equal to *New Equity Issue*$_{q+1}$. In column (2), *Short Horizon* is equal to *Share Turnover*$_{q-1}$. Controls variables include *Date Surprise*, *Reporting Lag*, *Size*, *Market-to-Book*, *Leverage*, and *Age*. All control variables are interacted with *Short Horizon*. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and clustered by firm. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Earnings Surprise at the Earnings Announcement Date

| | (1) | (2) |
|---|---|---|
| Proxy for Short Horizon | New Equity Issue$_{q+1}$ | Share Turnover$_{q-1}$ |
| Advance Notice Period x Short Horizon | 0.903*** | 2.692* |
| | (2.70) | (1.87) |
| Advance Notice Period | 0.059*** | 0.036** |
| | (4.79) | (2.07) |
| Short Horizon | 0.7602 | 19.993 |
| | (0.20) | (1.39) |
| Standard Controls (Interacted) | Yes | Yes |
| Firm Fixed Effects | Yes | Yes |
| Year-Quarter Fixed Effects | Yes | Yes |
| Fiscal Quarter Fixed Effects | Yes | Yes |
| Adj. $R^2$ | 1.1% | 1.1% |
| N | 39,977 | 39,146 |
| # Firms | 3,414 | 3,392 |

## Table 2.8

### *Advance Notice Period and Market Reaction to Notices of Earnings*

This table presents panel regressions examining the effect of the advance notice period on the firm stock return at the notification of the date and time of the next quarterly earnings disclosure. The dependent variable *CAR[-1,+1]* is the cumulated abnormal return over days [-1,+1] around the date of the notice of earnings. *Advance Notice Period* is the number of days between the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) and the earnings announcement date, divided into five quintile. See Appendix C for other variable definitions. Standard errors are adjusted for heteroskedasticity and clustered by the day of announcement. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

| Dependent Variable: Market Reaction to Earnings Schedule Notification | |
|---|---|
| | CAR[-1;+1] |
| Advance Notice Period | 0.031 |
| | (1.36) |
| Date Surprise | -0.023*** |
| | (-3.05) |
| Friday | 0.069 |
| | (0.68) |
| Size | -0.201** |
| | (-2.15) |
| Market-to-Book | 0.00 |
| | (-1.03) |
| Firm Fixed Effects | Yes |
| Year-Week Fixed Effects | Yes |
| Fiscal Quarter Fixed Effects | Yes |
| Adj. $R^2$ | 0.2% |
| N | 43,363 |

## Table 2.9

### *Advance Notice Period (ANP) Portfolios Abnormal Returns*

This table presents daily abnormal return portfolios from January 2007 to December 2012. The portfolios of stocks are formed according to the date of the notice detailing the date and time of the forthcoming quarterly earnings disclosure. Stocks are added to the High (Low) ANP portfolio when the date of the notice of earnings (i.e. the press release announcing the date and time of the forthcoming quarterly earnings disclosure) comes earlier (later) than the date of the notice of earnings issued for the same quarter of the previous year. Stocks are removed from the High (Low) ANP portfolio five trading days after the earnings announcement date. In Column 1 through 3, all stocks are value weighted within a given portfolio. In Column 4 through 6, all stocks are equally weighted within a given portfolio. Column 1 through 6 report the coefficients of an OLS regressions of portfolios daily return in excess of the Treasury bill rate on daily factors. MktRf is the return on the CRSP value-weighted index minus the treasury rate. SMB and HML are the daily returns from the Fama and French (1993) factor-mimicking portfolios for size and book-to-market, respectively. UMD is the daily return from the Carhart (1997) factor-mimicking portfolio for momentum. The constant is the average daily risk-adjusted return expressed in basis points. Heteroskedasticity and autocorrelation consistent standard errors are calculated using the Newey-West estimator with six lags. t-statistics are in parentheses. ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Portfolio Daily Excess Return (in bp)

| | Value weights | | | Equal weights | | |
|---|---|---|---|---|---|---|
| Portfolio: | High ANP | Low ANP | Long / Short | High ANP | Low ANP | Long / Short |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Constant | 4.689*** | -3.680* | 8.369*** | 4.146*** | -3.775** | 7.921*** |
| | (3.07) | (-1.96) | (4.54) | (2.64) | (-2.20) | (4.18) |
| MktRF | 1.012*** | 1.003*** | 0.008 | 1.179*** | 1.187*** | -0.008 |
| | (47.71) | (57.23) | (0.37) | (42.13) | (43.62) | (-0.34) |
| SMB | 0.709*** | 0.767*** | -0.058** | 0.265*** | 0.328*** | -0.063* |
| | (19.62) | (19.91) | (-2.04) | (5.32) | (5.80) | (-1.93) |
| HML | 0.116*** | 0.115*** | 0.001 | 0.090** | 0.081 | 0.009 |
| | (3.35) | (3.38) | (0.02) | (2.17) | (1.62) | (0.19) |
| UMD | -0.002*** | -0.002*** | 0.000* | -0.000* | -0.001*** | 0.000* |
| | (-7.43) | (-8.78) | (1.73) | (-1.85) | (-3.22) | (1.65) |
| N | 1,257 | 1,257 | 1,257 | 1,257 | 1,257 | 1,257 |

# Chapter 3

# The Effects of Investment Bank Rankings: Evidence from M&A League Tables

Joint work with **François Derrien**

**Abstract**

League tables, which provide rankings of investment banks, have a significant influence on M&A advisory activities of banks. The rank of a bank in the league table predicts its future deal flow. This creates strong incentives for banks to manage their ranks in the league table. League table management tools include selling fairness opinions and reducing fees. Banks use such tools mostly when their incentive to do so is higher: when a transaction is likely to imply substantial changes in their league table position or when they lost ranks in recent league tables. League table management is effective and seems to affect the quality of service of banks. In particular, fairness opinions are associated with higher offer price uncertainty, lower probability of deal completion and lower deal synergies when they are more likely to be done for league table management purposes.

## 3.1. Introduction

League tables are rankings based on banks' market shares. They cover many investment banking activities -- Mergers and Acquisitions (M&As), security underwriting, lending... They are widely reported and commented in the financial press, and are thus commonly available information to firms willing to select their investment bank. These rankings are frequently criticized for using inappropriate criteria (Bao and Edmans (2011)), for inducing excessive gaming behaviors,[1] and for distracting bankers away from what should be their real function in the economy.[2] Given the revenues generated by the investment banking industry and the role of this industry in the economy, understanding the effect of these rankings on both clients' choices and bankers' behavior is key. This is what this paper does, focusing on the M&A industry.

[Insert Figures 3.1 and 3.2 here.]

Figure 3.1 confirms that banks take league table rankings very seriously. Using all the M&A transactions done in the U.S. between January 1999 and December 2010, it shows the weekly frequency of reporting of M&A advisory roles by banks to Thomson Financial, the main league tables provider in the US. The histogram shows a strong increase in the number of advisory roles reported during the last weeks of each quarter, followed by a sharp drop right after the end of the quarter. This suggests that banks carefully monitor the reporting of their transactions when it matters the most, i.e., right before the calculation of league table rankings.[3] To ensure that these peaks are motivated by league table concerns and do not merely reflect seasonality in M&A activity or announcements, we present in Figure 3.2 the

[1] See Rajan (2010): "A better explanation [for the attitude of bankers toward risk] is that they were vying among themselves by heading league tables for underwriting or lending, regardless of the longer-term risk involved". See also "M&A Rankings manipulated", Bloomberg News, 12/27/2004.
[2] "It's time to stop league table obsessions", Financial Times, 23/04/2007.
[3] Thomson Financial publishes league tables at the end of every quarter. Netter, Stegemoller and Wintoki (2001) report a strong clustering of announcements of deal completion at quarter ends and argue, as we do in this paper, that this behavior is consistent with banks monitoring closely the reporting of their deals for league table purposes.

weekly frequency of deal announcements. In Figure 3.2, we observe no clustering of announcement dates.

The active monitoring of reporting dates by banks suggests that league table rankings matter, perhaps because they are one of the few sources of public information at the disposal of clients looking for M&A advisors. If this is the case, then league tables could not only reflect M&A past market shares of banks, but they could also influence their future M&A market shares. We confirm this by showing that recent changes in the rank of a bank in the league table explain future changes in the number of M&A mandates of the bank, controlling for known determinants of market shares and bank fixed effects.

To establish causality between rankings and future deal flow unambiguously and ensure that this relationship is not driven by unobserved variables (e.g., changes in bank quality), we use two additional specifications. First, we use the fact that league tables report only the top 25 banks in the ranking, even though banks right below rank 25 are very similar in terms of M&A market share to banks right above that rank. Using a Regression Discontinuity Design (RDD) setting around rank 25 of the league table, we find that entering (exiting) the league table has a significant positive (negative) impact on future deal flow. In another test, we exploit the fact that when a bank is acquired, banks ranked below it in the league table mechanically gain ranks, while banks ranked above it are unaffected. We find that banks that benefit from such an exogenous gain of ranks increase their deal flow more than unaffected banks. This impact of league table rankings on future business volumes suggests that these rankings contribute to the reputation of banks. This could be because league tables provide one of the only independent measures of bank performance. So firms may use them despite their limitations because they have little information about the quality of M&A advisors or little experience of the M&A market. Consistent with this explanation, we find that league table rankings matter less for new business origination with more experienced M&A clients.

Overall, these results indicate that changes in the ranking affect the perception of bank quality, which has real consequences for banks. This creates strong incentives for banks to manage their positions in these rankings. We hypothesize that bankers are willing to engage in such "league table management" as long as its cost in terms of current earnings, execution efforts, and reputation risk do not exceed its expected benefits. To test this hypothesis, we need first to identify variations in incentives to do league table management. If all banks constantly manage their rankings, then league table management may not affect rankings, and may not even be observable to researchers, as the tournament literature shows.[4] However, incentives to manage league table rankings vary across banks and within bank over time. First, incentives to manage are larger for banks that are closer to their competitors in terms of the total deal value they have accumulated in the league table since the beginning of the year. Second, incentives to manage are stronger for banks that lost ranks in recent league tables than for banks that just gained ranks. Indeed, a bank that has gained ranks recently faces higher demand than a bank that has lost ranks recently, and has therefore more opportunities to generate fees. Both banks can allocate part of their resources to league table management to increase their future deal flow. However, doing so is less costly for the bank that has just lost ranks and has excess capacity.[5] Moreover, league tables provide an independent measure of the performance of banks and their employees, and league table ranks can be used as benchmarks to evaluate this performance. Simple measures like recent changes in the ranking can affect the reputation of bankers, which can in turn affect their market values and their bonuses. Hence the incentives for bankers to manage the rankings of their banks, and in particular to ensure that they do not fall short of recent benchmarks.

[4] See Lazear and Rosen (1981), Green and Stokey (1983) and Nalebuff and Stiglitz (1983).
[5] We assume that banks cannot fully adjust their capacity in real time to respond to shifts in demand caused by rank changes.

To test our hypothesis that banks manage their league table rankings, we also need to identify potential league table management tools. To manage their position in the ranking, banks can exploit the construction rules of league tables. These rules are such that, in most cases, all the banks that participate in a transaction obtain the same league table credit regardless of their role in the transaction.[6] Thus, mandates associated with low effort (and low fees) but with full league table credit, like fairness opinions (FOs), are potential league table management tools. A fairness opinion is a third-party assessment of the fairness of the pricing of a proposed transaction.[7] The fees charged for an FO are usually very low, which makes them unattractive from a financial point of view.[8] However, FOs are beneficial in terms of league table credit because a FO provider obtains the same league table credit as a regular advisor. Another possibility for banks willing to maintain or improve their position in the league table is to cut their fees. By doing so, they reduce their current level of earnings but they increase their probability of obtaining mandates, thereby increasing their chances of gaining ranks in the league table and their future deal flow.

Consistent with our league table management hypothesis, we find that banks are more likely to manage their league table ranking (i.e., to provide FOs and to reduce their fees) when their incentives to do so are greater. In particular, we show that when there are *multiple* advisors for the *same* deal and the *same* client, the bank that benefits the most from the deal in terms of ranking (because the deal leads to a larger reduction in the gap with its competitors in the league table) is more likely to provide a FO and to charge lower fees, as is the bank with the worst relative performance in recent league table rankings.

[6] The term "league table credit" that we use throughout the paper is equivalent to the term "rank value". Both terms refer to the score credited to banks that participate in an M&A transaction. Cumulated league table credits are used to rank banks in the league table, as we explain in detail in Section 2.
[7] For a complete description of M&A fairness opinions, see Davidoff (2006) and Kisgen, Qian and Song (2009).
[8] In our sample, the median fee is 14bp (circa 500 thousand dollars) for FOs, vs. 66bp (circa 2.75 million dollars) for regular advisory mandates.

Next, we investigate the implications of this strategic response to league table rankings. We first show that league table management allows banks to improve their league table rankings. We also provide some evidence that, when engaged in league table management, banks deliver services of lower quality to their client. In particular, fairness opinions for which the suspicion of league table management is high are associated with higher uncertainty about the "fair" price of the transaction, lower probability of deal completion, and lower deal synergies.

To our knowledge, no existing paper studies league table rankings specifically. However, several studies analyze the determinants and the consequences of investment banks' reputation. Because league tables are designed to measure bank performance, our study is related to this literature, which reaches mixed conclusions. Bowers and Miller (1990) and Allen, Michel, and Shaked (1991) do not find any relationship between the reputation of the advisor and the acquirer's return in the transaction. Servaes and Zenner (1996) find that the acquirer's return is lower when the acquirer uses a bank as an advisor than for "in-house" deals, but partially explain this finding by differences in deal characteristics of the two sub-groups of transactions. Rau (2000) finds a negative relationship between the investment bank's market share and the acquirer's wealth gain. Bao and Edmans (2011) identify a significant bank fixed effect on acquirers' returns. In other words, some banks are better than others at creating value for their M&A clients. However, there is no relation between a bank's quality, measured by acquirer's returns, and its future market share. In fact, the only variable that explains a bank's future market share is its current market share.[9] Our paper contributes

[9] Other studies reach different conclusions by using alternative approaches to differentiate banks in terms of quality or by focusing on subsets of transactions. Kale, Kini and Ryan (2003) find that the most reputable advisors are associated with larger wealth gains for their clients. Golubov, Petmezas and Travlos (2011) find that, in public transactions, the reputation of the bank measured by its market share positively influences the return of the acquirer. McConnell and Sibilkov (2011) show that acquirers are more likely to retain their M&A advisors following higher wealth gains in their previous deals with these advisors.

to this literature by showing that league table rankings do affect the perception of bank quality, which affects the behavior of banks.

Several papers also analyze conflicts of interest in the M&A industry. McLaughlin (1992) reports that the compensation of advisors in M&A transactions depends mostly on deal completion rather than the quality of the transaction, and argues that this can create conflicts of interest for advisors. Bodnaruk, Massa and Simonov (2009) analyze transactions in which one of the advisors holds a stake in the target before the deal is completed. They show that these deals are more likely to be completed, but create less value for the acquirer because the target tends to be overvalued. By providing evidence of league table management, our paper enriches our understanding of the incentives of banks and how they respond to them.[10]

The rest of the paper is organized as follows. Section 3.2 discusses the construction of M&A league tables and presents the data. Section 3.3 analyzes the relation between league table rankings and future market share. Section 3.4 examines the strategic response of bankers to league table rankings. Section 3.5 examines the implications of league table management. Section 3.6 concludes.

### 3.2. League table construction and data

M&A league tables appeared in the U.S. in the early 1970s and are now a standard practice using fixed and well-documented criteria. M&A league table providers include Thomson Financial, Bloomberg, Dealogic and Mergermarket. We focus on M&A league tables provided by Thomson Financial because our data source for M&A transaction is Securities Data Company (SDC), also provided by Thomson Financial. Thomson Financial publishes M&A league tables, which report the top 25 banks in terms of M&A activity at the end of

[10] See Hong and Kubik (2003) and Ljungqvist, Marston, and Wilhelm (2006) for evidence on other conflicts of interest in the investment banking industry.

each quarter. Appendix 1 presents one such league table (for the fourth quarter of 2006). The rules used to construct league tables are detailed in the official League Table Criteria document issued by Thomson. They can be summarized as follows:

- The ranking in a given quarter is based on the sum of "Rank Values" of transactions announced since the beginning of the calendar year. "Rank Value" is the value of the transaction ("Deal Value" item in SDC), plus the net debt of the target company if 100% of the economic interest of the target is acquired from an initial holding of less than 50%.
- Eligible deals include all deals resulting in a change of economic ownership. Rumored and withdrawn deals at the time of the league table construction are not eligible.
- Eligible mandates include all mandates with any involvement in the deal, either as the advisor of the target company (sell-side mandate), as the advisor of the acquiring company (buy-side mandate), or as the advisor of the ultimate parent company on either side of the transaction.
- The definition of eligible advisory roles is relatively large and includes in particular the case where the financial advisor only issues a fairness opinion.[11]
- Each financial advisor eligible for league table purposes receives almost systematically the full rank value of the deal.[12]

Participation in the league table is free. Thomson Financial automatically ranks any advisory role it is aware of provided that it obtains confirmation from an external source such as a press release, a press release announcing the transaction or an extract of the engagement

[11] "5.13 : The following financial advisory roles are eligible for league table credit: initiation of the transaction, negations of terms and conditions, formal advice to board on fairness, public position on fairness, management of other advisors/process, coordination/review of due diligence, formal advice on the commercial merit of the transaction and valuation analysis". (Source: M&A League Table Criteria Q3 2010, Thomson Reuters.)
[12] Exceptions to this rule include the case in which the financial advisor advises a minority shareholder of either the acquirer or the target.

letter. Since 2005, each bank has the right to challenge anonymously any role of any competing bank in any deal. The challenged bank has to respond to the challenge by providing documentation proving its involvement in the deal. The challenge process is possible because each bank can follow its position in the ranking (as well as that of other banks) in real time through league tables that are available on Thomson One Banker's website.

We use Thomson Financial's Securities Data Company (SDC) data for mergers and acquisitions announced between January 1999 and December 2010. Thomson provided league table rankings before 1999 but some important items, like the date at which the advisor obtains credit for a deal ("Date Advisor Added") are often missing before 1999. We retain "Any U.S. involvement" deals eligible for league table purposes with at least one financial advisor reported by Thomson SDC. This yields an initial sample of 37,349 deals corresponding to 55,760 deal-bank observations or mandates. We follow Bao and Edmans (2011) and exclude banks with a number of mandates per year smaller than two over our sample period. We also exclude banks that never appear in the league table in our sample period, i.e., banks that are never in the top 25 banks using Thomson's criteria. This leads to a final sample of 39,690 deal-bank observations and 80 unique banks. For each deal, Thomson provides information on the number of financial advisors of the target and the acquiring firm as well as their names, assignments and fees.[13] In particular, Thomson reports whether the financial advisor provides advisory services, a fairness opinion, or both. To calculate cumulative abnormal returns around announcement dates, we use stock price data from Datastream because our sample includes cross-border deals involving non-U.S. targets or acquirers.

[13] This information is available for most of the deals, with the exception of fees, which are observable for only 3,052 observations out of 39,690.

An important variable for our study is the rank of each bank in the league table at any point in time. League table ranks are publicly available through two sources: Thomson's web interface, and historical league tables published in the press. In our tests, we instead use league table ranks that we compute using the same criteria as Thomson Financial. A description of the procedure we use to estimate league table ranks appears in Appendix 2. We use these estimated ranks rather than those provided by Thomson Financial through its web interface because the latter are based on the information currently available and not on the information available at the time of the publication of the league table.[14] We also use our estimated ranks rather than those that appear in Thomson's historical press releases because some of our tests require the use of weekly ranks, while historical league tables are published quarterly. Moreover, for some tests we need the ranks of banks outside the league table, that is, banks with ranks 26 and higher. These are not available in either past league tables currently available from Thomson's website or in historical press releases.

To check the accuracy of our procedure, we compare our estimated rankings with those in Thomson's historical press releases published between December 2000 and December 2010. Appendix 3 shows the level of matching between our estimated league tables and the published ones. On average, 76% of the banks in our estimated rankings exactly match their rank in the published rankings. The average difference between estimated ranks and published ranks is 0.35 (1.3 when we calculate the difference on the subsample of banks with an estimated rank different from their published rank). This suggests that the procedure we use to construct league tables is quite accurate.

[14] Thomson Financial's website rewrites history using the most recent information available. This leads to substantial discrepancies compared to the historical league tables published in the press. For instance, some transactions that are now reported as withdrawn by Thomson SDC were known as pending at the time of the league table publication. Based on past information, these pending transactions were eligible for league table credit, but based on present information they are not. Moreover, many bank mergers occurred during our sample period. Based on past information (before merger), league table credits are attributed to each bank separately, but based on present information (after merger), all the league table credit is given to the surviving entity. For example, Lehman Brothers does not appear in the pre-2008 league tables produced today by Thomson's web interface. All the league table credit it obtained prior to 2008 is attributed to Nomura and Barclays.

We use two variables to capture the incentives of banks to engage in league table management. The first variable aims at capturing the effect of a given deal on the league table position of the bank. This effect should be assessed considering both the absolute impact of the deal in terms of league table credit (i.e., the size of the deal), and its relative impact, which also depends on the credit the bank needs to gain ranks (or to avoid losing ranks). Deal *d* has a strong impact on bank *i*'s ranking relative to bank *j* if the credit associated with participation in the deal ($rank\_value_d$) is large relative to the difference between the two banks in terms of total league table credits accumulated by banks *i* and *j* since the beginning of the year ($LT\_credit_i$ and $LT\_credit_j$) , i.e., if the following ratio is large:

$$\log\left(\frac{\text{rank\_value}_d}{\left|\text{LT\_credit}_\text{i} - \text{LT\_credit}_\text{j}\right|}\right)$$

The larger this ratio, the more beneficial the deal is for bank *i* in terms of closing (or enlarging) the gap with its competitor. To the extent that each bank is competing with all other banks in the table, we average this ratio across all competitors.[15] Our *LT_contribution* variable therefore measures the average impact of deal *d* on the gap between bank *i* and its competitors in terms of league table credit.

$$\text{LT\_contribution}_{i,d} = \frac{1}{24} \times \sum_{\substack{j=1 \\ j \neq i}}^{25} \log\left(\frac{\text{rank\_value}_d}{\left|\text{LT\_credit}_\text{i} - \text{LT\_credit}_\text{j}\right|}\right)$$

Under our league table management hypothesis, the incentives for a bank to manage its league table ranking are larger when the *LT_contribution* variable is larger.

The second variable we use as a proxy of a bank's incentives to manage its league table rank, *deviation*, measures the recent performance of the bank in the ranking. This

[15] All our subsequent results are unchanged if we consider only the two closest competitors of the bank instead of the 24 banks in the league table.

variable is equal to the difference between a bank's rank at the end of the previous calendar year and the most recent rank (calculated at end of the previous quarter in bank-quarter level tests, at the end of the previous week in deal-level tests). Our league table management hypothesis implies that the propensity of a bank to engage in league table management is larger when this variable is smaller.

[Insert Table 3.1 here.]

Appendix presents the other variables used in our tests, and Table 3.1 presents summary statistics of these variables. All the continuous variables are winsorized at 1% in each tail.

### 3.3. League table ranking and future deal flow

First, we explore the relation between league table rankings and future deal flow. Anecdotal evidence suggests that banks take their league table rankings very seriously. This could be because M&A clients rely on these rankings to choose their advisors. If this is the case, then current rankings could affect future activity of banks. To test this, we explore the link between current league table ranks and future M&A activity in panel regressions at the quarter-bank level. Because both the rank of the bank and its quarterly volume of mandates have a strong stationary component, we do not examine the effect of the bank's rank on its M&A activity in levels. Rather, we explore the effect of a change in the bank's rank on the change in its number of M&A mandates in the following quarter. We take a *long* difference approach and focus on year-on-year rather than quarter-on-quarter variations in the number of mandates. The advantage of this long-difference approach is that it fits well the design of league tables, which are yearly cumulative rankings. This approach also neutralizes any within-year seasonality in M&A activity. We thus regress the growth in the number of mandates in a given quarter relative to the same quarter of the previous year on the change in ranks of the

bank at the end of the previous quarter relative to the same quarter of the previous year. In this test, we focus on "published rankings", i.e., ranks between 1 and 25, and we assign rank 26 to any bank that does not appear in the league table.[16]

[Insert Table 3.2 here.]

The first column of Table 3.2 presents the first specification with no controls but with year-quarter fixed effects that capture changes in M&A activity over time. We do not add bank fixed effects because our dependent variable is calculated as a difference, so any fixed effect related to the level of business volume is already differenced out. Changes in deal flow are positively and significantly related to past changes in league table rankings *(ΔLT_rank*). The coefficient of 1.7 means that a gain of one rank in the league table corresponds to a growth in the number of mandates of 1.7%, or that a one within-bank standard deviation increase of *ΔLT_rank* leads to an increase in *ΔMandates* of about 7.31% on average, which represents 6.5% of the within-bank standard deviation of this variable.

We investigate the robustness of this result in the next columns of Table 3.2. Our first concern is that this result may vary with the rank of the bank. So we add the rank of the bank as a control variable in column 2. As in all subsequent tests, we multiply this variable by -1, so that it is larger for better-ranked banks. This variable is negatively related to growth in M&A activity meaning that the relative effect of a gain in ranks on the number of mandates is attenuated for better-ranked banks. A second concern arises from the fact that league tables are based on deal value market shares. The literature (e.g., Rau (2000), Bao and Edmans (2011)) finds that the main determinant of a bank's current market share is its past market share. Perhaps changes in deal value market shares explain future changes in deal volume. To ensure that this is not the case, we control for past changes in deal value market share in column 3. We define deal value market share as the total value of the deals advised by the

[16] We obtain the same results when we ignore the banks ranked 26.

bank during a given period divided by the total value of M&A transactions during the same period. This definition is similar to that used in Bao and Edmans (2011) and most of the literature. Adding this control variable to the regression does not affect our main finding that a change in rank positively affects the deal flow of the bank in the subsequent quarter.

Our last concern is the possibility that our dependent variable is serially correlated. A bank experiencing a significant increase in business volume in a given quarter could also have been experiencing a similar increase in the previous quarter. In this case, the growth in the volume of mandates would not come from a change in ranks, but from the fact that the business volume of the bank was already growing in the previous period. We try to alleviate this concern in columns 4 and 5 of Table 3.2. In column 4, we add a lagged transformation of the left-hand side variable and estimate a dynamic panel regression to isolate the effect of past changes in the bank's deal flow on future changes of this variable. As in the previous columns, the coefficient on $\Delta LT\_rank$ is positive and statistically significant. A possible concern with this specification is that the OLS estimation is biased if there is any time invariant component in the error term of the regression. If we suspect the presence of any bank fixed effects in the *change* in (and not the *level* of) the M&A activity of the bank, then the explanatory variable $\Delta mandates_{q-1}$ is indeed positively correlated with the error term at period *q*. In this case, the coefficient on the lagged variable is biased upward (see Bond (2002)). We address this issue in column 5 of Table 2 by adding bank fixed effects to remove any time invariant component related to the *change* in business volume. This within-transformation helps mitigate the OLS estimation problem if the time dimension of the panel is sufficiently large.[17] In this last specification, the coefficient on $\Delta LT\_rank$ is still positive

[17] Dynamic panel estimations using individual fixed effects also create a bias, but its magnitude is inversely related to the panel length (T) (See Nickell (1981)). Since we are using quarterly data, our panel spans over 48 time periods, which should significantly reduce the bias. Jason and Owen (1999) find that the fixed effects model performs as well or better than any other dynamic panel estimation techniques starting from T=30. Flannery and

and significant at the 10% level, and its magnitude is little affected by the inclusion of bank fixed effects.

These results suggest that current league table rankings affect future M&A activity. However, changes in league table rankings could be correlated with omitted variables (e.g., bank quality) that affect future deal flow. To rule out this alternative explanation of our previous results and show unambiguously a *causal* league table effect, we use two alternative specifications.

First, we use the fact that only the top 25 banks appear in the published league tables. Thus, if the effect we document in Table 3.2 above is linked to the visibility offered by the league table, entering or leaving the ranking should have a significant impact on a bank's future M&A activity. In Table 3.3, we test this in a Regression Discontinuity Design (RDD) setting. We consider the full ranking of banks and not only the top 25 banks that appear in the published league table. We divide banks into two groups according to the variable $Full\_rank_{q-1}$, equal to the rank of the bank in the full ranking at the end of the previous quarter.[18] Banks are assigned to the treatment group when they are ranked between ranks 1 and 25, and therefore appear in the published league table. Banks below the rank-25 threshold are assigned to the control group. The dummy variable $Above25_{q-1}$ is equal to one if the bank is in the treatment group at the end of the previous quarter, and 0 otherwise. Our goal is to estimate the effect of this variable on the future deal flow of the bank. Our methodology derives from Roberts and Withed (2012) who propose to estimate the following equation in the vicinity of rank 25:

$$Mandates_{i,q} = \alpha + \beta\, Above25_{i,q-1} + \gamma\,(Full\_rank_{i,q-1} - 25) + \delta\, Above25_{i,q-1} \times (Full\_rank_{i,q-1} - 25) + \varepsilon_{i,q}$$

Hankins (2013) also find that the fixed effect estimator may perform as well and even better than alternative techniques in the presence of endogenous variables using a length of panel T=12.

[18] This variable is often referred to as the forcing variable in the RDD literature.

To be consistent with the specification of Table 3.2, we use a differentiated version of this equation, which allows us to explore how moving in or out of the published league table in a given quarter affects the subsequent M&A deal flow of a bank:

$$\Delta Mandates_{i,q} = \beta\, \Delta Above25_{i,q-1} + \gamma\, \Delta Full\_rank_{i,q-1} + \delta\, \Delta(Above25 \times Full\_rank)_{i,q-1} + \mu_{i,q}$$ [19]

The left-hand side variable is again $\Delta Mandates_q$, the growth in the quarterly number of mandates observed at the bank level. Our main variable of interest is $\Delta above25_{q-1}$. It is equal to -1 if the bank left the league table during the previous year (i.e., between the ranking published one year ago and the ranking published at the end of the previous quarter), +1 if the bank entered the league table, and 0 otherwise. The additional control variables in our specification ensure that $\Delta above25_{q-1}$ only captures the effect of a switch in (or out of) the published league table. $\Delta Full\_rank_{q-1}$ controls for the effect of the change in rank that occurs simultaneously, and $\Delta(Full\_rank \times above25)_{q-1}$ controls for the number of ranks gained or lost specifically inside the published league table because the effect of a rank variation may be different on the two sides of the threshold.

[Insert Table 3.3 here.]

In the regression of Table 3.3, Panel A, we restrict our sample to banks that are ranked between 21 and 30 at the beginning of the quarter. The assumption we are making in this test is that banks in the vicinity of rank 25 are very similar, except that some appear in the league table whereas others do not. If this assumption is correct, and if the relation between the league table ranking of a bank and its future deal flow is causal, then the variable that captures movements of banks in and out of the league table should significantly explain their changes

[19] Differentiating $Above25_{i,q-1}$ yields $\Delta Above25_{i,q-1} = Above25_{i,q-1,y} - Above25_{i,q-1,y-1}$, a variable equal to 1 if the rank of the bank increased above the threshold, -1 if it decreased below the threshold, and 0 otherwise. Similarly, differentiating the other two variables yields $\Delta Full\_rank_{i,q-1} = (Full\_rank_{i,q-1,y} - 25) - (Full\_rank_{i,q-1,y-1} - 25)$, a variable equal to the overall variation of the rank, and $\Delta(Full\_rank \times Above25)_{i,q-1} = ((Full\_rank_{i,q-1,y} - 25) \times Above25_{i,q-1,y}) - ((Full\_rank_{i,q-1,y-1} - 25) \times Above25_{i,q-1,y-1})$, a variable equal to the number of ranks gained / lost inside the published league table.

in business volume. The table shows that entering or leaving the league table has a significant effect on the deal flow of the bank in the subsequent quarter. The test indicates that entering (leaving) the published league table results in an increase (decrease) in the growth of the number of mandates of about 20%.

To provide further evidence that it is the presence in the league table that explains changes in M&A activity, we run falsification tests in which we repeat the previous regression focusing on different thresholds of the forcing variable (from rank 21 to rank 29), rather than the real threshold (rank 25). We also vary the number of ranks around the threshold for which we run our test. The results, in Panel B of Table 3.3, present the coefficient on the main variable *Δ above d*, where *d* takes values between 21 and 29. Both the magnitude and the significance of these coefficients confirm that the only relevant threshold is rank 25, whatever number of ranks around the threshold we use. In other words, a bank that is in the vicinity of rank 25 and switches from below rank 25 to above rank 25 (or vice versa) has a significant change in M&A activity. This is not the case for banks in the vicinity of other ranks.

An important assumption of RDD tests is that the forcing variable (in our case, the rank of the bank) cannot be manipulated. Our claim that banks manage their league table ranks seems to contradict this assumption. However, Lee (2008) shows that RDD is still valid in the presence of manipulation of the forcing variable as long as there remains some uncertainty regarding the outcome of the manipulation. In our case, even though banks manage their rankings, they cannot monitor the amount of league table management of their competitors. Since the outcome of a bank's league table management depends on the behavior of its competitors, it is necessarily imprecise. If this is the case, then ex post, we should observe total league table credits that are very similar on both sides of rank 25, and our RDD tests around rank 25 will be valid. If, on the other hand, league table management creates a discontinuity in the distribution of observed league table credits above the rank-25 threshold,

then league table management is a source of heterogeneity between the two groups of banks (those below the threshold and those above), and the RDD tests are not valid. In other words, even if banks manage their rankings, our RDD tests are valid as long as banks below the rank-25 threshold are not too different in terms of M&A activity from banks above that threshold.

[Insert Figure 3.3 here.]

Figure 3.3 suggests that this is indeed the case. For each rank in the vicinity of rank 25 (from ranks 22 to 28), it shows the average accumulated league table credit in US$m at the end of the calendar year over the 1999-2010 period. This graph shows no discontinuity on the right hand side of rank 25 (i.e., no sudden drop at rank 26), confirming that banks on both sides of the rank-25 threshold are quite similar in terms of their M&A activity.

The second method we use to establish that league table rankings have a direct effect on future market share uses bank mergers as a shock to rankings that is unrelated to bank characteristics. When two banks merge, one of them disappears from the league table. Banks ranked below the lower ranked of the two banks that merge lose a competitor in the ranking and, all else equal, they gain a rank in the next league table. We identify 11 bank mergers with such an effect on league table rankings between 1999 and 2010. The list of these mergers appears in Appendix. We run a difference-in-differences test in which the dependent variable is the number of mandates done by the bank in the quarter. Treated banks are those that mechanically gain a rank following the merger that occurred in the previous 12-month period. The *Exit* variable takes the value 1 for these banks, while it is equal to 0 for banks in the control group, which are all the banks whose rank was unaffected by the bank merger. We

include bank and time fixed effects to control for differences across banks and differences over time. [20]

[Insert Table 3.4 here.]

Table 3.4 presents the results. The coefficient on the *Exit* variable is significantly positive in column 1 of the table. On average, following a bank merger, banks that benefit from an artificial gain in ranks increase their number of mandates by four relative to banks that do not benefit from such a gain in ranks. This number is large compared with the average quarterly number of mandates in our sample (about 13). In column 2, we control for the rank of the bank at the end of the previous quarter, ignoring the ranks gained artificially following bank mergers. To ensure that the result of the first column is capturing a causal relation between the exogenous shock to the bank's rank and its future deal flow, we also introduce two placebo variables in column 3, $Exit_{+1}$ and $Exit_{-1}$, which are respectively the 1-year lead and 1-year lag of the *Exit* variable. None of the placebo variables is related to deal flow.

Overall, these results show that the rank of a bank in the league table influences its future deal flow. This suggests that league table rankings affect clients' perceptions of bank quality although they may not be a good proxy for quality. One explanation for this result is that league tables are one of the only independent and public measures of bank performance. So M&A clients may be more inclined to use them when they have less experience of the M&A market. We find empirical evidence consistent with this view in Table 3.5.

[Insert Table 3.5 here.]

In this table, we run deal-level tests in which we study the effect of a bank's league table rank on its probability of being hired by M&A clients. This approach allows us to

[20] Our specification follows Bertrand and Mullainathan (2003) to handle situations with multiple shocks and multiple treatment groups.

interact the rank variable with other deal-level variables that are known to influence this probability, in particular the experience of M&A clients. We can then examine when the rank of the bank in the league table matters the most for new business origination. We use OLS regressions with bank-level and deal-client-level fixed effects, in which the dependent variable is an indicator variable equal to one if the bank is selected as a financial advisor by the client for the deal, and 0 otherwise. In line with our previous results, the regression reported in column 1 of Table 3.5 shows that the probability of obtaining a mandate increases with the rank of the bank, controlling for the past market share of the bank.

In column 2, we test our hypothesis that the link between a bank's league table rank and its probability of being hired by a client decreases with the M&A experience of the client. We measure experience with two variables. *Prev_M&A* is the number of M&A transactions of the client in the past five years. It measures the overall M&A experience of the client. The second variable, *Prev_deals*, is equal to the number of deals of the client in which the bank was involved in the past five years. It measures the intensity of the relationship between the client and the bank. We predict that the league table rank of the bank should matter less in the client's decision when the client knows the M&A market better (i.e., if *Prev_M&A* is large), or when the client knows the bank better (i.e., if *Prev_deals* is large). The regression in column 2 of Table 5 confirms this hypothesis: The coefficients of the interaction variables *LT_rank* × *Prev_M&A* and *LT_rank* × *Prev_deals* are both negative and significant. In other words, the bank's rank is less likely to influence decisions of clients with more experience of the M&A market or stronger relationships with the bank. In column 3, we interact our two experience variables with past market share of the bank, to ensure that our results are driven by the rank of the bank, and not by its past market share. Our results are unchanged. In fact, in column three, past market share of the bank, in itself or interacted with the experience variables, comes out insignificant. Overall, these results confirm that the impact of league

table rankings on a bank's market share is stronger for inexperienced clients. This is in line with our conjecture that league tables affect M&A deal flows because they are one of the few sources of public information on the M&A market. As such, they influence inexperienced clients, while experienced clients rely more on their private knowledge of the M&A market.

## 3.4. Do banks manage their league table ranks?

### *3.4.1. Fairness opinions*

Given the relation between the position of a bank in the league table and its future M&A activity, banks have an incentive to inflate their rank artificially, thereby increasing their future M&A deal flow. In this section, we test this *league table management* hypothesis. We start this analysis by focusing on the first way for banks to inflate their rank at relatively low costs: Fairness opinions (FOs), which involve limited effort but generate the same league table credit as regular advisory roles. We hypothesize that incentives to do fairness opinions are stronger for banks in the two following situations: first, when the deal is likely to have a big impact on their ranks, and second, when the bank lost ranks in the most recent league tables.

When testing this hypothesis, we face several identification concerns. A first concern is the possibility that banks with strong incentives to manage their league table rank participate in deals that are more likely to include FOs. For example, if banks that lost ranks in recent league tables want to regain their lost ranks or face lower demand, they might be willing or forced to participate in deals with higher execution complexity, higher litigation risk for the managers, or lower probability of success. All these unobserved deal characteristics may also be associated with a higher probability of observing a FO. To address this issue, we use an identification strategy similar to that of Khwaja and Mian (2008): We focus on deals with

*multiple* banks for the *same* client and use deal-client fixed effects. This approach allows us to compare banks exposed to the *same* deal-client conditions and which obtain the *same* league table credit, but differ in their incentives to manage their league table positions. We can then estimate how these incentives affect the probability to be the bank that provides a FO among all the banks that work for the same client. To the extent this *within deal-client* comparison fully absorbs deal-specific and client-specific variables affecting the demand for FOs, the estimated difference in the probability to do a FO can be plausibly attributed to differences in bank incentives to improve their ranking position.

Another identification concern is that the way we measure banks' incentives to engage in league table management could be correlated with other bank characteristics that explain the supply of FOs. The within deal-client variation in *LT_contribution* reflects the variation in the average distance between the bank and its competitors in the ranking. Since this variation mainly stems from variations in the number and value of deals advised by the bank's competitors, it should be independent of the characteristics of the bank itself. However, recent league table performance, measured by the *deviation* variable, is correlated with the rank of the bank, which could affect the probability of providing a FO. Therefore, we control for the rank of the bank in the most recent league table, and we use bank fixed-effects to control for time invariant heterogeneity between banks.

[Insert Table 3.6 here.]

The results of this analysis are presented in Table 3.6. We estimate the probability to do a FO using a linear probability model, where the dependent variable is equal to 1 if the bank does a FO and 0 otherwise. [21] In column 1, the *LT_contribution* variable, which

[21] Kisgen, Qian and Song (2009) point out that in about one third of their sample of FOs, Thomson either indicates no fairness opinion when the financial advisor issued one in reality or does not mention the presence of an additional fairness opinion provider. In the summer 2010, however, Thomson started to provide additional

measures the impact of the deal on the gap between the bank and its competitors in the league table, has a positive and statistically significant coefficient. The bank that lost (gained) more ranks in recent league tables is also more (less) likely to be the one that provides a FO (the coefficient on the *deviation* variable is negative and significant). Consistent with our hypothesis, these results show that for a given deal/client, the bank that is more likely to be the bank that does the fairness opinion is the one that benefits the most from the transaction in terms of ranking improvement, or the one that had the worst recent league table performance. We investigate the robustness of these results in column 2 by adding additional time-varying controls at the bank level. The coefficient on *LT_contribution* is still positive and statistically significant at the 1% level, and its economic magnitude is almost the same as in the regression of the first column. Likewise, the coefficient on *deviation* is still negative and statistically significant.

Next, we test our league table management hypothesis at the bank-quarter level. Such a setting excludes the use the *LT_contribution* variable, which is deal specific. Instead, it allows us to focus on the *deviation* variable, which is equal, in this context, to the change in the bank's league table rank between the end of the previous year and the end of the previous quarter. Our hypothesis is that banks that lost (gained) ranks in the most recent quarterly ranking relative to the last annual ranking do more (less) FOs in the current quarter. In the first two columns, we focus on "published ranks", i.e., ranks between 1 and 25, and we assign rank 26 to any bank that does not appear in the league table. We run panel regressions including bank and time fixed effects, and controlling for the rank of the bank at the end of the previous quarter. The dependent variable is the number of fairness opinions done by the

data on fairness opinions (in particular the valuation materials contained in the fairness opinions letters) and reviewed all the information reported in the database about fairness opinions issued from 2000 onwards.

bank in the quarter as a fraction of its total number of deals (in column 1) or in absolute terms (in column 2).[22]

[Insert Table 3.7 here.]

The results of these tests, which appear in Table 3.7, confirm our hypothesis. A bank that has lost (gained) a rank between the last annual ranking and the last quarterly ranking increases (decreases) its number of FOs by 0.03 (about 4% of the within-bank standard deviation of the number of FOs) and its percentage of FOs by 0.2% (about 2% of the within-bank standard deviation of that variable) on average. To ensure that these results are not driven by demand (e.g., lower-ranked banks facing higher demand for FOs and lower demand for more lucrative mandates), we control for the rank of the bank. In fact, the regression of column 2 shows that better-ranked banks tend to do more, not fewer, FOs. The fact that banks that lost ranks, and thus face lower demand for FOs, increase their number of FOs is therefore consistent with a supply interpretation of our results, whereby such banks voluntarily provide more FOs.

In columns 3 and 4 of Table 3.7, we repeat this test using the rank of the bank in the full ranking instead of the rank from the published league. We include a dummy variable equal to 1 if the bank appears in the league table (i.e., in the top 25 banks) at the end of the previous quarter, and we interact this dummy variable with the *deviation* variable. These tests show that banks do more fairness opinions after losing a rank only when they appear in the league table: the coefficient on the *deviation* variable is small and statistically insignificant, while the coefficient on the interaction term *deviation* × *above25* is negative and statistically significant. This is consistent with our previous findings that gaining ranks matter for future

[22] In some cases, a bank that is the only advisor on one side of a deal does a fairness opinion. Such a fairness opinion is unlikely to be done to manage the bank's rank in the league table, since the bank already obtains league table credit for that transaction through its advisory role. In our tests, we ignore these fairness opinions and focus on FOs done in a co-mandate context, i.e., when there are other banks involved on the same side of the deal.

M&A activity only for banks that are in the league table. Banks that do not appear in the published ranking have less incentives to manage their ranks, and therefore they do not.

Overall, these results are consistent with our hypothesis that banks are more likely to do fairness opinions in transactions that have a bigger impact on their future ranking and when they lost more (or gained fewer) ranks in recent league tables.

### *3.4.2. Fees*

To increase its ranking, a bank can also reduce the amount of fees charged for a given transaction. By doing so, the bank increases its chances of obtaining the mandate and the corresponding deal credit in the league table. Our league table management hypothesis predicts that banks decrease their fees for deals that have a strong impact on their league table position and after poor recent league table performance. We test these predictions using mandate-level OLS regressions in which the dependent variable is the amount of fees as a percentage of the deal value (in basis points). We have information on fees in 3,052 mandates, which represent less than 10% of our total sample. This could bias our results. However, if the disclosure of fees in SDC is not random, then it is conceivable that banks do not disclose their fees precisely when they are willing to cut their fees in order to obtain a mandate. If this is the case, missing fees might bias our results in the direction of rejecting our hypothesis.

[Insert Table 8 here.]

In Table 3.8, we use a specification similar to the one we use in our earlier FO tests. We focus on co-mandate situations and include deal-client fixed effects, which allows us to compare directly different banks that participate in the *same* deal with the *same* client. The main variables of interest are *LT_contribution* and *deviation*, which proxy for the relative impact of the deal on the ranking of the bank and for the recent league table performance of

the bank, respectively. The coefficient on the *LT_contribution* variable is significantly negative. Consistent with our hypothesis, this suggests that for a given deal-client, the bank that has the most to gain from the deal in terms of league table credit (i.e., the bank with the larger *LT_contribution*) tend to be the bank that charges the lower fees.

The coefficient on *deviation* is positive and significant: This regression shows that in a co-mandate context, controlling for time-varying bank characteristics and bank fixed-effects, the bank with the lower fees is, on average, the bank that lost more (or gained less) ranks in recent league tables. This result does not seem to be driven by better-ranked banks charging higher fees. In fact, in the second column of Table 3.8, in which we add bank-specific controls, the rank of the bank in the league table has a significantly negative impact on fees. This seems to contradict the results of Walter et al. (2005) and Golubov et al. (2011), who find that more prestigious banks charge higher fees. This is because variables measuring past market share of the bank and previous relations between the bank and the client, as well as bank fixed effects, capture bank characteristics that are related to bank prestige and that explain a large fraction of the fees. When we eliminate bank fixed effects, we find a positive relation between a bank's rank and its fees. In terms of economic magnitude, a one-standard deviation change in *deviation* is associated with about 8 basis points, corresponding to about 210 thousand dollars for the median deal in our sample.

### 3.5. The effects of league table management

In this section, we examine the consequences of league table management. First, we ask whether the league table management actions we have identified in the previous section have an effect on a bank's position in the league table. It might be the case that league table management is not effective, or that banks manage their league table positions using other equally efficient methods that we failed to identify.

[Insert Table 3.9 here.]

We test this in Table 3.9, in which we run panel regressions explaining the change in rank at the end of a given quarter compared to one year before. The main variable of interest in these tests is the year-on-year change in $Pct\_fo\_co_q$, the percentage of fairness opinions done in a co-mandate context (those that we identified as likely to be motivated by league table concerns) in quarter $q$. We control for the change in the bank's market share (measured by its total deal value) over the same period. In the last column, we also include bank fixed effects to control for time-invariant bank characteristics. In all three columns, an increase in a bank's percentage of FOs is statistically associated with a future increase in the bank's rank. In terms of economic magnitude of this effect, an increase of one within-bank standard deviation of $\Delta Pct\_fo\_co_q$ leads to an average gain of about 0.3 ranks at the end of the quarter. These results suggest that league table management is effective.

Next, we explore the quality of transactions done to manage league table rankings. It might be the case that FOs initiated by banks in search of league table credits are of lower quality. We examine the quality of fairness opinions done in a co-mandate context. We use three measures of quality of the FO. The first one is the size of the valuation range provided by the bank to assess the fairness of the offer price, measured by the variable *Valuation_range*. We assume that when the valuation range is larger, the uncertainty on the fair price of the transaction is higher and the credibility of the fairness opinion is lower. Our second measure of FO quality is the probability of deal completion.[23] We assume here that a deal failure indicates *ex-post* that the pricing conditions were not "fair", and by extension that the FO was of low quality. Our third measure of the quality of FOs is the combined cumulative abnormal return in a two-day window around the deal announcement date (*Comb_CAR*). We follow Betton, Eckbo and Thorburn (2008) and determine this CAR by weighting the bidder's and

[23] Kisgen et al. (2009) find that the presence of a FO increases the probability of deal completion.

target's abnormal returns by their market capitalizations 42 days before the announcement of the transaction. We use this variable as a proxy for the amount of synergies of the deal and, like Kisgen et al. (2009), we consider that fairness opinions are of poor quality when this variable takes low values. We examine whether FOs are of lower quality when they are more likely to be motivated by league table concerns, i.e., when they have a strong league table effect for the banks that provide them or when they are done by banks with poorer recent league table performance.

[Insert Table 3.10 here.]

In Table 3.10, we use OLS regressions with fixed effects in which the dependent variable is the size of the valuation range in specification (1), a dummy variable equal to 1 if the deal is withdrawn eventually in specification (2), and the combined CAR around the deal announcement in specification (3). The main explanatory variables are the effect of the deal on the relative league table position of the bank (*LT_contribution*) and the recent league table performance of the bank (*deviation*). The recent league table performance of the bank does not affect the quality of the FO, but the effect of the deal on the relative league table position of the bank does: FOs with stronger league table implications for the bank (i.e., a larger *LT_contribution*) are associated with a wider valuation range, a smaller probability of deal completion and smaller combined CARs. This is partially consistent with the hypothesis that fairness opinions done for league table management purposes are of lower quality.

## 3.6. Conclusion

This paper shows that league tables have a significant influence on M&A advisory business practice. The rank of a bank in the league table is a significant predictor of its future deal flow. This creates strong incentives for banks to engage in league table management, by

selling fairness opinions or reducing their fees. Such behaviors depend on banks' incentives. They are more prevalent when participating in a transaction is more likely to imply substantial changes in the league table position of the bank or for banks that have performed poorly in recent league table rankings. The incentives created by league tables have real effects for M&A clients. First, as they affect future rankings, they may affect M&A clients in their selection of financial advisors. Second they can lead bankers to deliver services of lower quality. We provide some evidence of such real consequences by showing that fairness opinions with a strong league table effect are associated with higher uncertainty about the fairness of the price, lower probability of deal completion, and lower deal synergies.

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## 3.8. Appendix

### *3.8.1. M&A league table published by Thomson Financial for 2006*

M&A financial advisor league table for the period 01/01/2006 - 31/12/2006. The ranking includes any financial advisor role in any deal announced by a U.S. M&A client.

**Any US Involvement Announced (AD41)**

| | 1/1/2006 - 12/31/2006 | | | | 1/1/2005 - 12/31/2005 | | |
|---|---|---|---|---|---|---|---|
| Financial Advisor | Rank Value US$m | Rank | Mkt. Share | No. Deals | Rank Value US$m | Rank | % Chg. in Rank Val |
| Goldman Sachs & Co | 634,232.0 | 1 | 33.4 | 263 | 485,422.9 | 1 | 30.7 |
| Citigroup | 505,330.1 | 2 | 26.6 | 210 | 290,354.3 | 4 | 74.0 |
| Morgan Stanley | 482,534.1 | 3 | 25.4 | 217 | 433,197.2 | 2 | 11.4 |
| JP Morgan | 480,471.9 | 4 | 25.3 | 248 | 335,617.4 | 3 | 43.2 |
| Credit Suisse | 393,516.4 | 5 | 20.7 | 207 | 189,470.7 | 8 | 107.7 |
| Lehman Brothers | 387,351.5 | 6 | 20.4 | 162 | 232,769.4 | 6 | 66.4 |
| Merrill Lynch | 365,073.8 | 7 | 19.2 | 179 | 252,363.8 | 5 | 44.7 |
| UBS | 280,066.0 | 8 | 14.7 | 214 | 226,264.2 | 7 | 23.8 |
| Banc of America Securities LLC | 230,822.9 | 9 | 12.2 | 118 | 149,283.2 | 10 | 54.6 |
| Lazard | 205,304.8 | 10 | 10.8 | 96 | 146,742.2 | 11 | 39.9 |
| Evercore Partners | 187,245.7 | 11 | 9.9 | 31 | 59,966.9 | 14 | 212.2 |
| Deutsche Bank AG | 173,182.5 | 12 | 9.1 | 94 | 110,419.1 | 12 | 56.8 |
| Bear Stearns & Co Inc | 147,879.2 | 13 | 7.8 | 54 | 178,781.0 | 9 | -17.3 |
| Houlihan Lokey Howard & Zukin | 117,652.0 | 14 | 6.2 | 147 | 30,638.5 | 19 | 284.0 |
| Wachovia Corp | 109,021.9 | 15 | 5.7 | 59 | 48,773.6 | 15 | 123.5 |
| Blackstone Group LP | 106,101.5 | 16 | 5.6 | 25 | 61,944.2 | 13 | 71.3 |
| Rothschild | 82,898.1 | 17 | 4.4 | 81 | 26,461.8 | 22 | 213.3 |
| BNP Paribas SA | 46,015.2 | 18 | 2.4 | 16 | 6,420.9 | 41 | 616.6 |
| ABN AMRO | 41,764.8 | 19 | 2.2 | 30 | 8,521.3 | 35 | 390.1 |
| Sandler O'Neill Partners | 36,835.2 | 20 | 1.9 | 69 | 6,731.1 | 40 | 447.2 |
| Greenhill & Co, LLC | 29,754.1 | 21 | 1.6 | 22 | 19,630.9 | 24 | 51.6 |
| Peter J. Solomon Co Ltd | 28,909.8 | 22 | 1.5 | 7 | 18,635.0 | 25 | 55.1 |
| Global Leisure Partners LLP | 27,388.7 | 23 | 1.4 | 1 | - | - | - |
| Macquarie Bank | 26,239.3 | 24 | 1.4 | 37 | 8,692.2 | 34 | 201.9 |
| Societe Generale | 25,386.9 | 25 | 1.3 | 8 | 5,287.3 | 46 | 380.1 |
| Subtotal without Financial Advisor | 211,354.1 | - | 11.1 | 10,317 | 165,380.9 | - | 27.8 |
| Subtotal with Financial Advisor | 1,689,116.4 | - | 88.9 | 3,090 | 1,207,015.7 | - | 39.9 |
| Industry Total | 1,900,470.4 | - | 100.0 | 13,407 | 1,372,396.5 | - | 38.5 |

* tie

Source: Thomson Financial

### *3.8.2. League table construction*

This appendix describes the procedure we use to construct league tables in the 1999-2010 period. We use the same criteria as Thomson. Specifically, to calculate the league table credit of a bank in period p, we use the three steps below.

1. For each deal, construct an indicator variable equal to 1 if the bank is part of the deal and its role in the deal is eligible for league table purposes. This variable is equal to 1 if the following conditions are met and 0 otherwise:
   - the deal announcement date is in period p,
   - the date at which the financial advisor is added to the SDC database is in period p,
   - the deal status is either completed or withdrawn,
   - if the deal status is withdrawn, the withdrawal date is after the end of period p,
   - the target or the acquirer or any of their parent companies is in the U.S.

2. Calculate league table credit, equal to the last historical deal value available at the time of the construction of the league table, plus the net debt of the target company if 100% of the economic interest of the target is acquired from an initial holding of less than 50%.

3. Accumulate the value credited at the level of bank's parent. For that purpose, we manually identify the parent of each financial advisor at the time of the publication of the league table.

### *3.8.3. Comparison of published and estimated league tables*

This appendix presents a comparison between ranks in historical league tables published in the press by Thomson Financial and those we construct as described in Appendix 2. The matching score on *Rank* is the percentage of banks with the same rank in the two league tables. The matching score on *Rank value* is the average of the ratio of estimated to published total accumulated deal value. *Rank deviation* is the difference in absolute terms between the estimated rank and the published rank. Total mean is the average rank deviation across all 25 banks in the ranking. Non-matched mean is the average rank deviation across banks with estimated ranks different from their published ranks.

| **Year** | **Quarter** | **Matching score** | | **Rank deviation** | |
|---|---|---|---|---|---|
| | | Rank | Rank value | Total mean | Non-matched mean |
| 2000 | Q4 | 60.0% | 92.5% | 0.76 | 1.90 |
| 2001 | Q1 | 60.0% | 93.9% | 0.76 | 1.90 |
| | Q2 | 56.0% | 94.6% | 0.72 | 1.64 |
| | Q3 | 64.0% | 94.9% | 0.56 | 1.56 |
| | Q4 | 72.0% | 94.8% | 0.48 | 1.71 |
| 2002 | Q1 | 60.0% | 95.2% | 0.52 | 1.30 |
| | Q2 | 84.0% | 92.9% | 0.16 | 1.00 |
| | Q3 | 68.0% | 94.4% | 0.48 | 1.50 |
| | Q4 | 76.0% | 96.3% | 0.40 | 1.67 |
| 2003 | Q1 | 52.0% | 83.5% | 0.88 | 1.83 |
| | Q2 | 72.0% | 94.1% | 0.32 | 1.14 |
| | Q3 | 92.0% | 98.7% | 0.08 | 1.00 |
| | Q4 | 76.0% | 97.8% | 0.24 | 1.00 |
| 2004 | Q1 | 84.0% | 96.3% | 0.20 | 1.25 |
| | Q2 | 92.0% | 97.4% | 0.08 | 1.00 |
| | Q3 | 92.0% | 97.6% | 0.08 | 1.00 |
| | Q4 | 92.0% | 99.1% | 0.08 | 1.00 |
| 2005 | Q1 | 76.0% | 92.1% | 0.56 | 2.33 |
| | Q2 | 92.0% | 96.4% | 0.08 | 1.00 |
| | Q3 | 88.0% | 98.0% | 0.16 | 1.33 |
| | Q4 | 72.0% | 96.8% | 0.28 | 1.00 |
| 2006 | Q1 | 56.0% | 93.4% | 0.64 | 1.45 |
| | Q2 | 76.0% | 97.1% | 0.32 | 1.33 |
| | Q3 | 64.0% | 95.9% | 0.44 | 1.22 |
| | Q4 | 84.0% | 97.9% | 0.16 | 1.00 |
| 2007 | Q1 | 84.0% | 98.2% | 0.16 | 1.00 |
| | Q2 | 84.0% | 97.7% | 0.16 | 1.00 |
| | Q3 | 76.0% | 98.0% | 0.32 | 1.33 |
| | Q4 | 68.0% | 97.0% | 0.32 | 1.00 |
| 2008 | Q1 | 68.0% | 91.9% | 0.52 | 1.63 |
| | Q2 | 72.0% | 96.5% | 0.32 | 1.14 |
| | Q3 | 76.0% | 98.4% | 0.32 | 1.33 |
| | Q4 | 92.0% | 97.9% | 0.08 | 1.00 |
| 2009 | Q1 | 84.0% | 92.3% | 0.36 | 2.25 |
| | Q2 | 72.0% | 91.9% | 0.44 | 1.57 |
| | Q3 | 76.0% | 94.8% | 0.24 | 1.00 |
| | Q4 | 72.0% | 94.2% | 0.40 | 1.43 |
| 2010 | Q1 | 84.0% | 97.9% | 0.16 | 1.00 |
| | Q2 | 80.0% | 95.3% | 0.40 | 2.00 |
| | Q3 | 100.0% | 98.7% | 0.00 | 0.00 |
| | Q4 | 64.0% | 96.0% | 0.56 | 1.56 |
| **Mean** | | **75.9%** | **95.6%** | **0.35** | **1.32** |
| **Median** | | **76.0%** | **96.3%** | **0.32** | **1.30** |

### *3.8.4. Variables definition*

*Variables used in mandate-level tests*

***Challenge***: Indicator variable equal to 1 if the deal is reported as a challenged deal by Thomson SDC
***Combined_CAR***: Average of the target's CAR (-1,+1) and the acquirer's CAR (-1,+1), weighted by their market capitalizations 42 days before the deal announcement. CAR is the stock return in excess of the S&P 500 index
***Comb_runup***: Average of the target's runup in (-42,-2) and the acquirer's runup in (-42,2), weighted by their market capitalizations 42 days before the deal announcement. CAR is the stock return in excess of the S&P 500 index
***Cross_border***: Indicator variable equal to 1 if the acquirer and the target have different nation codes
***Deal_size***: Total deal value in US$m
***Deal_value***: Log of the total deal value
***Defense***: Indicator variable equal to 1 if any defense technique was used in the transaction
***Deviation***: Number of ranks gained / lost by the bank since the end of the previous year, calculated at the end of the week prior to the deal announcement
***Fee***: Total fees charged by the bank expressed in basis points of the total deal value
***Fo***: Indicator variable equal to 1 if the bank provides a fairness opinion
***Fo_co***: Indicator variable equal to 1 if the mandate includes a FO and if the bank is not the only financial advisor of the company
***Friendly***: Indicator variable equal to 1 if deal is not reported as "Hostile" or "Non Solicited" by Thomson SDC
***LT_contribution***: Average impact of the deal on the gap in league table credit between the bank and its 24 competitors in the league table at the end of the week before the deal announcement date. For each competitor, this impact is calculated as the league table credit of the deal divided by the absolute value of the difference between the current total league table credit of the bank and that of the competitor
***LT_rank***: Rank of the bank in the league table at the end of the week prior to the week of the deal announcement or at the end of the previous year depending on the specification. Banks not ranked in the league table (top 25 banks) are ranked 26. This variable is multiplied by -1 so that a higher rank indicates a better position in the ranking
***LY_mkt_*share**: Market share of the bank in the previous year based on deal value and defined as the total value of deals advised by the bank divided by the total value of deals announced
***Payment_mix _cash***: Indicator variable equal to 1 if at least 50% of the transaction is paid in cash
***Payment_mix _other***: Indicator variable equal to 1 if at least 50% of the transaction is neither paid in stock or in cash
***Payment_mix _stock***: Indicator variable equal to 1 if at least 50% of the transaction is paid in stock
***Payment_mix_unknown***: Indicator variable equal to 1 if at least 50% of the transaction type of payment is unknown
***Prev_deals_ acquiror***: Number of M&A transactions done by the acquiring firm in which the bank was a financial advisor in the past 5 years
***Prev_deals_ target***: Number of M&A transactions done by the target firm in which the bank was a financial advisor in the past 5 years
***Prev_deals***: Number of M&A transactions done by the firm in which the bank was a financial advisor in the past 5 years

***Prev_M&A***: Number of M&A transactions done by the firm in the past 5 years
***Same_industry***: Indicator variable equal to 1 if the acquirer and the target are in the same two-digit SIC code
***Sell_side***: Indicator variable equal to 1 if the mandate is a sell-side mandate
***Side_added_order***: Order of notification of the advisory role of the bank to Thomson Financial for league table purposes compared to the other banks also mandated on the same side of the deal.
***Tender***: Indicator variable equal to 1 if the deal is reported as a tender offer by Thomson SDC
***Time_to_ notif***: Number of days between the announcement date of the deal and the date of notification of the advisory role to Thomson Financial for league table purposes
***Toehold***: Percentage of the target's stock held by the acquirer prior to the deal announcement
***Valuation_Range***: Size of the valuation range (max value – min value) disclosed in the fairness opinion, where max value (min value) is the high value (low value) obtained with the DCF methodology
***Win***: Indicator variable equal to 1 if the bank obtains the mandate
***Withdrawn***: Indicator variable equal to 1 if the deal was withdrawn

*Variables used in bank-level tests*

***Δ Above25$_q$***: Indicator variable equal to 1 if the bank entered the league table, -1 if the bank exited the league table, and 0 if it remained either inside or outside the league table in the year ending at the end of quarter *q*
***Δ Full_rank$_q$***: Annual number of ranks gained / lost in the full ranking of M&A advisors at the end of quarter *q* of year *y* ($Full\_Rank_{q,y}$ - $Full\_Rank_{q,y-1}$)
***Δ LT_rank$_q$***: Annual number of ranks gained / lost in the league table at the end of quarter *q* of year *y* ($LT\_rank_{q,y}$ - $LT\_rank_{q,y-1}$)
***Δ Mandates$_q$***: Change in the number of M&A mandates, measured as the annual growth of the total number of mandates of the bank in quarter *q* of year *y* ($Mandates_{q,y}$ / $Mandates_{q,y-1}$ - 1)
***Δ Mkt share$_q$***: Change in market share, measured as the annual industry-adjusted growth of the total value of deals advised by the bank in quarter *q* of year *y* [($Deal\_value_{i,q,y}$ / $Deal\_value_{i,q,y-1}$) - ($Total\_deal\ \_value_{q,y}$ / $Total\_deal\ \_value_{q,y-1}$)]
***Δ Pct_fo_co$_q$***: Annual ***c***hange in the percentage of co-fairness opinions in quarter *q* of year *y* ($Pct\_fo\_co_{q,y}$ - $Pct\_fo\_co_{q,y-1}$)
***Above25$_q$***: Indicator variable equal to 1 if the bank is among the 25 banks in the league table at the end quarter *q*
***Deal_value$_q$***: Total value of deals announced and advised by the bank during quarter *q*
***Deviation$_{q-1}$***: Number of ranks gained / lost by the bank between the end of the previous year and the end of the previous quarter (*q*-1). Tests that use this variable exclude first-quarter observations
***Exit***: Indicator variable equal to 1 if the bank gains ranks in the year ending at the end of the previous quarter after a competitor exits the league table due to a bank merger
***Exit-$_1$:*** Indicator variable equal to 1 if *Exit* is equal to 1 in the year ending one year before the end of the current quarter
***Exit$_{+1}$:*** Indicator variable equal to 1 if *Exit* is equal to 1 in the year starting at the end of the current quarter
***Full_rank$_q$***: Rank of the bank in the full ranking of M&A advisors at the end of quarter *q*. This variable is multiplied by -1 so that a higher rank indicates a better position in the ranking

***LT_rank*$_q$**: Rank of the bank in the league table at the end of quarter *q*. Banks not ranked in the league table (top 25 banks) are ranked 26. This variable is multiplied by -1 so that a higher rank indicates a better position in the ranking

***Mandates*$_q$**: Total number of deals announced and advised by the bank during quarter *q*

***Nb_fo_co*$_q$**: Total number of fairness opinions done by the bank in a co-mandate context during quarter *q*

***Pct_fo_co*$_q$**: Total number of fairness opinions done by the bank in a co-mandate context as a percentage of total mandates during quarter *q*

***Total_deal_value*$_q$**: Total value of deals announced during quarter *q*

### *3.1.1. List of bank mergers*

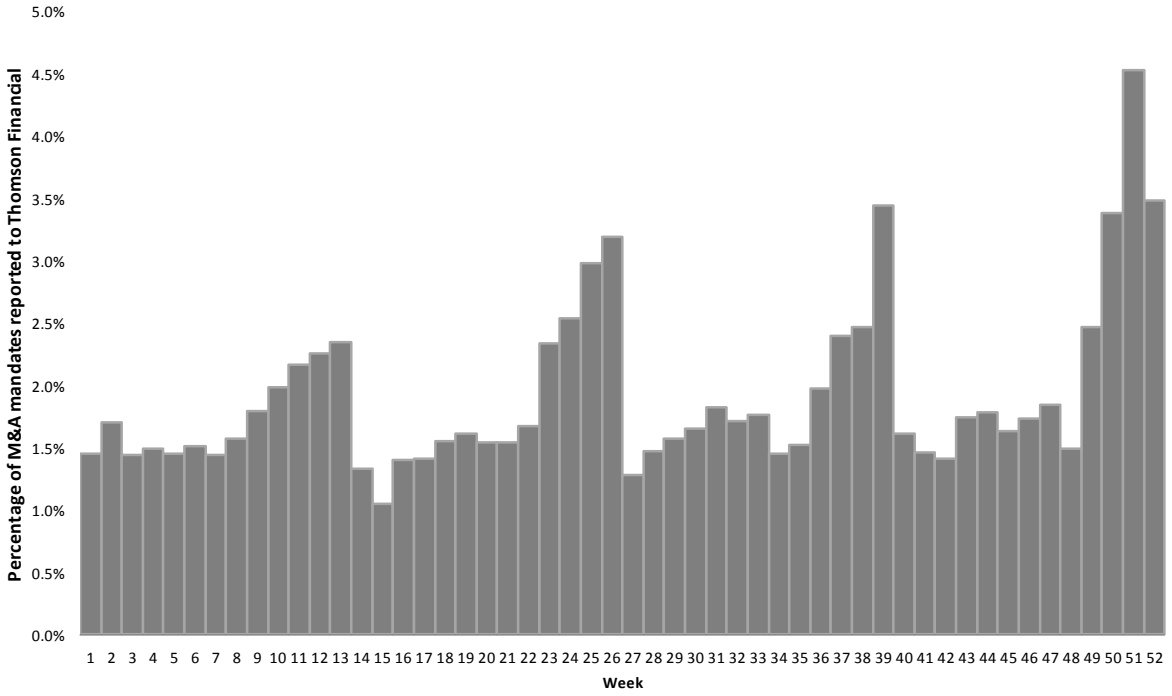
This appendix presents the list of mergers between investment banks that occurred over our sample period and led to a change in the ranking of M&A advisors in the U.S. league tables. *Announcement date* is the date at which the merger is announced. *Effective date* is the date at which the merger is effective. *Last LT report date* is the date of the last league table in which the target bank was ranked before its exit as a result of a merger. *Last LT rank* is the last rank reported in the league table for the target (acquiring) bank before the merger is effective. *Last Q4 LT* rank is the last full year rank reported in the league table for the target (acquiring) bank before the merger is effective.

| Exit timing | | | | Target investment bank | | | Acquiror | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | Announcement date | Effective date | Last LT report date | Name | Last LT rank | Last Q4 LT rank | Name | Last LT rank | Last Q4 LT rank |
| 2001 | 9/18/2000 | 1/5/2001 | 12/31/2000 | Wasserstein | 7 | 7 | Dresdner | 13 | 13 |
| 2001 | 1/30/2001 | 4/30/2001 | 12/31/2000 | ING Baring US Operations | 16 | 16 | ABN-AMRO | 26 | 26 |
| 2003 | 12/19/2003 | 12/23/2003 | 9/30/2003 | Broadview | 26 | 26 | Jefferies | 26 | 26 |
| 2006 | 8/22/2006 | 8/22/2006 | 6/30/2006 | Rohatyn | 11 | 22 | Lehman | 2 | 5 |
| 2007 | 10/8/2007 | 10/9/2007 | 12/31/2007 | ABN Amro | 19 | 19 | Royal Bank of Scotland | 26 | 26 |
| 2008 | 11/4/2007 | 1/14/2008 | 12/31/2007 | CIBC World | 24 | 26 | New Oppenheimer & Co | 26 | 26 |
| 2008 | 3/16/2008 | 5/30/2008 | 12/31/2007 | Bear Stern | 13 | 13 | JP Morgan | 4 | 4 |
| 2008 | 9/16/2008 | 9/22/2008 | 9/30/2008 | Lehman Brothers | 7 | 5 | Barclays | 26 | 26 |
| 2008 | 10/3/2008 | 12/31/2008 | 12/31/2008 | Wachovia | 14 | 14 | Wells Fargo | 26 | 26 |
| 2009 | 9/14/2008 | 1/1/2009 | 12/31/2008 | Merril Lynch | 6 | 6 | Bank of America | 11 | 11 |
| 2009 | 9/30/2009 | 10/2/2009 | 12/31/2008 | Fox-Pitt | 19 | 19 | Macquarie Bank | 26 | 26 |

## Figure 3.1

### *Percentage of M&A advisory mandates reported to Thomson Financial per week*

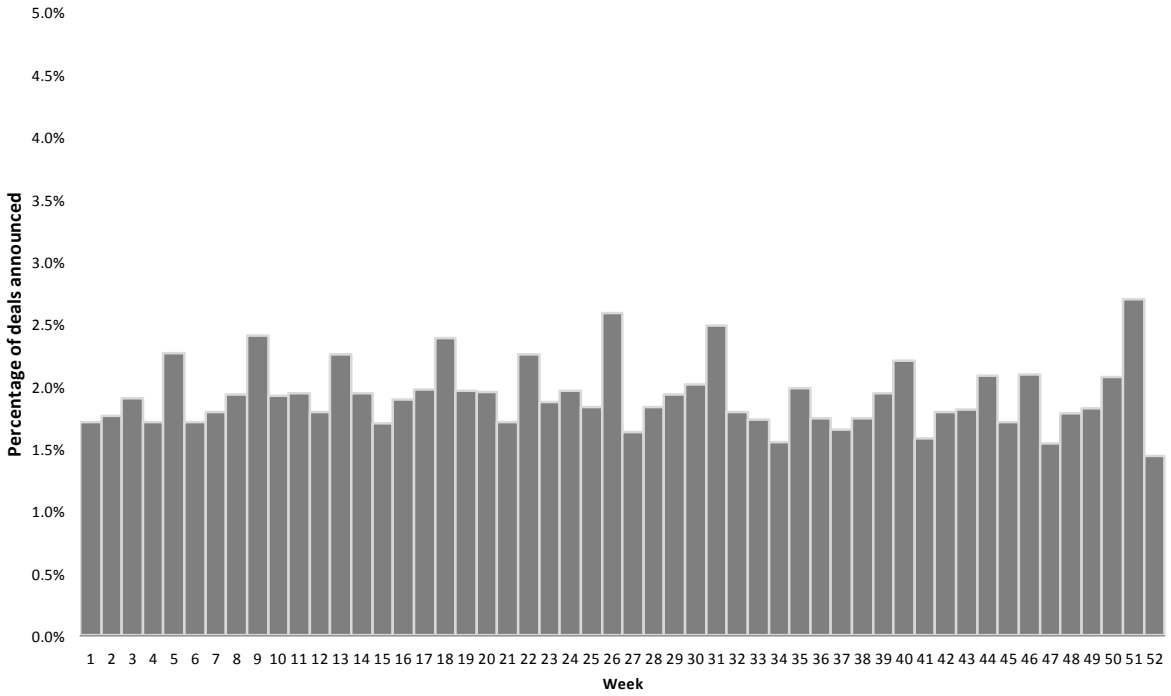
This figure presents the number of M&A mandates reported by banks to Thomson Financial each week as a percentage of the total number of mandates. The sample includes 55,760 deal–bank observations (mandates), corresponding to any M&A financial advisor involvement in the U.S. in Thomson SDC over the 1999-2010 period.



## Figure 3.2

### *Percentage of deals announced per week*

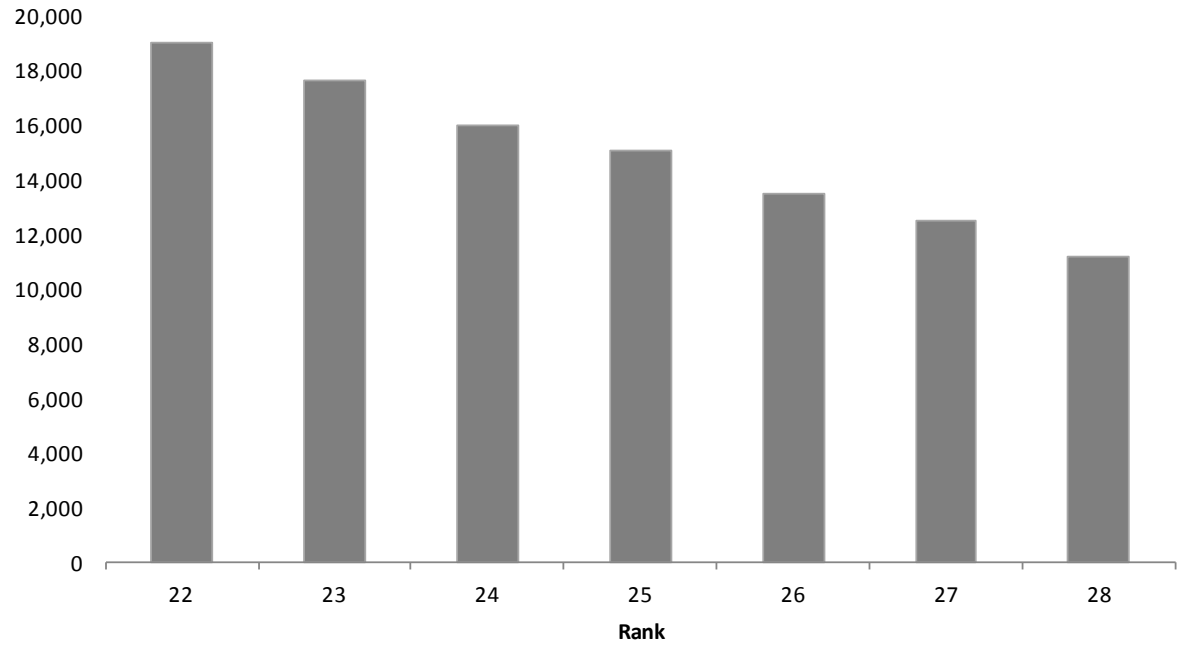
This figure presents the number of deals announced each week as a percentage of the total number of deals. The sample includes 55,760 deal–bank observations (mandates), corresponding to any M&A financial advisor involvement in the U.S. in Thomson SDC over the 1999-2010 period.



## Figure 3.3

### *Average League table credit by rank around rank 25*

This figure presents, for each rank between rank 22 and rank 28, the average accumulated deal value (in US$m) credited in the league table at the end the calendar year over the 1999-2010 period.



## Table 3.1

### *Summary statistics*

This table presents summary statistics of our sample. The sample includes 39,690 deal–bank observations (mandates), corresponding to any M&A financial advisor involvement in the U.S. in Thomson SDC over the 1999-2010 period, for 80 banks that announce at least 2 deals per year on average and are ranked in the league table at least one time in the sample period. Panel A includes observations at the deal-bank level. Panel B includes quarterly observations at the bank level. All continuous variables are winsorized at the 1% level in each tail. All variables are defined in Appendix.

| Main employed variables | N | Mean | p25 | Median | p75 | Standard deviation | | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | Overall | Between | Within |
| **Panel A - Mandate level observations** | | | | | | | | |
| Combined_CAR | 5,948 | 1.62% | 0.03% | 0.82% | 2.92% | 3.86% | | |
| Cross_border | 39,690 | 30.57% | 0 | 0 | 1 | 46.07% | | |
| Deal_size | 30,316 | 1,338 | 85 | 282 | 937 | 3,435 | | |
| Deal_value (Log) | 30,316 | 5.65 | 4.44 | 5.64 | 6.84 | 1.81 | | |
| Deviation | 35,469 | -0.07 | -2.00 | 0.00 | 1.00 | 5.07 | | |
| Fee | 3,052 | 87.29 | 27.06 | 63.93 | 108.86 | 98.49 | | |
| Fo | 39,690 | 12.38% | 0 | 0 | 0 | 32.93% | | |
| Fo_co | 39,690 | 4.17% | 0 | 0 | 0 | 19.99% | | |
| Friendly | 39,690 | 97.49% | 1 | 1 | 1 | 15.64% | | |
| LT_contribution | 22,907 | -4.46 | -5.90 | -4.62 | -3.17 | 2.10 | | |
| LT_rank | 39,690 | -13.19 | -26.00 | -11.00 | -4.00 | 9.56 | | |
| LY_mkt_share | 35,469 | 14.14% | 1.36% | 10.98% | 23.77% | 13.92% | | |
| Payment_mix_cash | 39,690 | 38.80% | 0 | 0 | 1 | 48.73% | | |
| Payment_mix_other | 39,690 | 4.48% | 0 | 0 | 0 | 20.69% | | |
| Payment_mix_stock | 39,690 | 11.91% | 0 | 0 | 0 | 32.39% | | |
| Payment_mix_unknown | 39,690 | 20.59% | 0.00% | 0.00% | 0.00% | 40.44% | | |
| Prev_deals_acquiror | 39,412 | 5.55 | 0.00 | 0.00 | 2.00 | 16.58 | | |
| Prev_deals_target | 39,412 | 1.07 | 0.00 | 0.00 | 1.00 | 3.13 | | |
| Same_industry | 39,690 | 47.44% | 0 | 0 | 1 | 49.94% | | |
| Sell_side | 39,690 | 60.50% | 0 | 1 | 1 | 48.89% | | |
| Tender | 39,690 | 6.81% | 0 | 0 | 0 | 25.18% | | |
| Toehold | 39,690 | 2.64% | 0.00% | 0.00% | 0.00% | 12.10% | | |
| Valuation_range | 1,248 | 40.41% | 22.17% | 32.66% | 48.27% | 29.09% | | |
| Withdrawn | 39,690 | 5.66% | 0 | 0 | 0 | 23.12% | | |
| **Panel B - Bank level quarterly observations** | | | | | | | | |
| $\Delta$ Full_rank$_{q-1}$ | 2,702 | 3.93 | -11.00 | 1.00 | 15.00 | 52.61 | 23.87 | 51.00 |
| $\Delta$ LT_rank$_{q-1}$ | 2,702 | 0.21 | 0.00 | 0.00 | 0.00 | 4.31 | 0.88 | 4.30 |
| $\Delta$ Mandates$_q$ | 2,418 | 24.2% | -40.0% | 0.0% | 50.0% | 117.3% | 42.6% | 113.4% |
| $\Delta$ Mkt share$_q$ | 2,343 | 313.7% | -61.1% | -9.9% | 144.6% | 1196.6% | 307.8% | 1162.8% |
| Deviation$_{q-1}$ | 2,060 | 0.12 | 0.00 | 0.00 | 0.00 | 4.00 | 1.33 | 3.90 |
| Full_rank$_q$ | 2,969 | -56.66 | -75.00 | -36.00 | -17.00 | 58.44 | 37.76 | 44.70 |
| LT_rank$_q$ | 2,969 | -21.04 | -26.00 | -26.00 | -17.00 | 7.78 | 6.91 | 3.40 |
| Deal_value$_q$ | 2,969 | 14,796 | 160 | 1,230 | 9,575 | 33,372 | 25,095 | 21,169 |
| Mandates$_q$ | 2,969 | 12.83 | 2.00 | 6.00 | 16.00 | 16.45 | 15.29 | 6.70 |
| Nb_fo_co$_q$ | 2,969 | 0.53 | 0.00 | 0.00 | 1.00 | 1.14 | 0.73 | 0.80 |
| Pct_fo_co$_q$ | 2,969 | 3.94% | 0.00% | 0.00% | 2.44% | 11.47% | 3.75% | 10.90% |

## Table 3.2

### *The effect of the league table rank on the number of M&A mandates*

This table presents panel regressions examining the effect of a change in rank in the league table on the growth of the number of mandates. The analysis is at the quarter-bank level. The dependent variable is $\Delta\ Mandates_{q}$, the year-on-year growth of the number of M&A mandates observed for bank *i* at quarter *q* of year *y* ($Mandates_{i,q,y}$ / $Mandates_{i,q,y-1}$ − 1). $\Delta\ LT\_rank_{q-1}$ is the number of ranks gained / lost *inside* the league table by bank *i* at the end of quarter *q-1* of year *y* on a year-on-year basis ($LT\_rank_{i,q-1,y}$ - $LT\_rank_{i,q-1,y-1}$). $LT\_Rank_{q-1}$ is the rank of the bank in the league table at the end of quarter *q-1*, multiplied by -1. $\Delta\ Mkt\ share_{q-1}$ is the change in market share, measured as the annual industry-adjusted growth of the total value of deals advised by the bank in quarter *q-1* of year *y* [($Deal\_value_{i,q-1,y}$ / $Deal\_value_{i,q-1,y-1}$)- ($Total\_deal\_value_{q-1,y}$ / $Total\_deal\_value_{q-1,y-1}$)]. All other variables are described in Appendix. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Dependent variable | Δ $Mandates_{q}$ | Δ $Mandates_{q}$ | Δ $Mandates_{q}$ | Δ $Mandates_{q}$ | Δ $Mandates_{q}$ |
| Δ $LT\_rank_{q-1}$ | 1.701** | 2.207*** | 2.118** | 1.825** | 1.482* |
| | (2.34) | (3.02) | (2.57) | (2.48) | (1.69) |
| $LT\_rank_{q-1}$ | | -0.950*** | -0.911*** | -0.880*** | -0.685 |
| | | (-3.96) | (-3.77) | (-3.83) | (-0.68) |
| Δ Mkt $share_{q-1}$ | | | 0.001 | -0.002 | -0.003 |
| | | | (0.51) | (-1.01) | (-1.21) |
| Δ $Mandates_{q-1}$ | | | | 0.111** | 0.052 |
| | | | | (2.59) | (1.39) |
| Year-quarter fixed effects | Yes | Yes | Yes | Yes | Yes |
| Bank fixed effects | No | No | No | No | Yes |
| Adj. $R^2$ | 4.7% | 5.0% | 6.7% | 7.6% | 10.0% |
| N | 2,354 | 2,354 | 2,126 | 2,126 | 2,126 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 3.3

### *The effect of rank 25 on the number of M&A mandates*

This table presents local linear regressions examining the effects of rank variations around rank 25 on the number of mandates. The sample is restricted to banks with a rank between 21 and 30 at the end of the previous quarter in the full ranking of M&A advisors ($Full\_rank_{q-1}$ variable). The dependent variable is $\Delta\ Mandates_q$, the year-on-year growth of the number of M&A mandates observed for bank $i$ at quarter $q$ of year $y$ ($Mandates_{i,q,y}$ / $Mandates_{i,q,y-1}$ -1). $\Delta\ Above25_{q-1}$ is a variable equal to 1 if the bank entered the league table, -1 if it exited the league table, and 0 if it remained either inside or outside the league table in the year ending at the end of the previous quarter. In panel A, we present the results of our baseline estimation. In panel B, we present the results of falsification tests that replicate the baseline analysis using different ranking thresholds $d$ (from 21 to 29) and different restrictions (from 3 to 6 ranks) around the threshold $d$. We report the regression coefficient estimated on the main variable of interest $\Delta Above d_{q-1}$ only. t-statistics are in parentheses. Standard errors are clustered at the bank level.

*Panel A : Baseline estimation*

| Dependent variable | $\Delta$ Mandates$_q$ |
|---|---|
| $\Delta$ Above25$_{q-1}$ | 19.070** |
| | (2.04) |
| $\Delta$ Full_rank$_{q-1}$ | 0.087 |
| | (0.76) |
| $\Delta$ (Full_rank x Above25)$_{q-1}$ | -2.669 |
| | (-1.55) |
| Year-quarter fixed effects | Yes |
| Observation restrictions | 20 < Full_rank$_{q-1}$ ≤ 30 |
| Adj. $R^2$ | 14.5% |
| N | 345 |

*Panel B :Falsification tests*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Observation restrictions | $d$ -6<Full_rank$_{q-1}$≤$d$ +6 | $d$ -5<Full_rank$_{q-1}$≤$d$ +5 | $d$ -4<Full_rank$_{q-1}$≤$d$ +4 | $d$ -3<Full_rank$_{q-1}$≤$d$ +3 |
| Discontinuity test at Full_rank$_{q-1}$ = | | | | |
| 21 | 12.8 | 2.8 | 2.3 | 5.4 |
| | (0.9) | (0.2) | (0.16) | (0.37) |
| 22 | -0.6 | -1 | -3.1 | -18.2 |
| | (-0.04) | (-0.06) | (-0.17) | (-0.9) |
| 23 | 15.2* | 11.5 | 3.1 | -7.3 |
| | (1.75) | (1.1) | (0.25) | (-0.57) |
| 24 | 8.3 | 4 | 6.1 | -9 |
| | (0.96) | (0.41) | (0.51) | (-0.52) |
| 25 | 21.4** | 19.1** | 18.2* | 28.2** |
| | (2.45) | (2.04) | (1.74) | (2.23) |
| 26 | 17.7 | 15.8 | 11.2 | 8.9 |
| | (1.61) | (1.42) | (0.94) | (0.64) |
| 27 | 4.3 | 11.6 | 10.1 | 1 |
| | (0.42) | (1.08) | (0.91) | (0.08) |
| 28 | -8.9 | -11.5 | -6.7 | -13.5 |
| | (-0.73) | (-0.99) | (-0.58) | (-1.47) |
| 29 | 1.9 | -1.7 | -6.2 | 11 |
| | (0.17) | (-0.13) | (-0.51) | (0.87) |

## Table 3.4

### *The effect of exogenous rank changes on the number of M&A mandates*

This table presents a difference-in-differences analysis examining the effects of the exit of a competitor from the league table following a bank merger on the number of M&A mandates. The analysis is at the quarter-bank level. The sample excludes banks ranked outside the league table at the beginning of the quarter and at the beginning of the same quarter of the previous year. The dependent variable is $Mandates_q$, the total number of M&A mandates of the bank during the quarter. *Exit* is an indicator variable equal to 1 if the bank gained ranks in the year ending at the end of the previous quarter after a competitor exited the league table due to a bank merger. $Exit_{-1}$ is an indicator variable equal to 1 if *Exit* is equal to 1 in the year ending one year before the end of the current quarter. $Exit_{+1}$ is an indicator variable equal to 1 if *Exit* is equal to 1 in the year starting at the end of the current quarter. $LT\_rank_{q-1}$ is the rank of the bank *inside* the league table at the end of the previous quarter (ignoring the number of ranks gained following the exit of the competitor), multiplied by -1. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable | $Mandates_q$ | $Mandates_q$ | $Mandates_q$ |
| $Exit_{-1}$ | | | 0.334 |
| | | | (0.28) |
| $Exit_0$ | 3.942*** | 4.174*** | 4.177*** |
| | (3.34) | (3.42) | (3.42) |
| $Exit_{+1}$ | | | 0.368 |
| | | | (0.33) |
| $LT\_rank_{q-1}$ | | 0.219** | 0.219** |
| | | (2.21) | (2.18) |
| Year-quarter fixed effects | Yes | Yes | Yes |
| Bank fixed effects | Yes | Yes | Yes |
| Adj. $R^2$ | 85.4% | 85.5% | 85.5% |
| N | 835 | 835 | 835 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 5

### *Rank, client experience, and new mandate origination likelihood*

This table presents OLS regressions examining the effects of the rank of a bank on its probability of obtaining an M&A advisory mandate. The analysis is at the deal-client (mandate) level. The dependent variable is *win*, an indicator variable equal to 1 if the bank obtains the mandate, and 0 otherwise. *LT_rank* is the rank of the bank at the end of the previous year, multiplied by -1. *LY_mkt_share* is the market share of the bank in the previous year based on deal value. *Prev_deals* is the number of deals of the same client advised by the bank in the past 5 years. *Prev_M&A* is the number of M&A deals done by the client in the past 5 years. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable | Win | Win | Win |
| LT_rank | 0.00043*** | 0.00065*** | 0.00055*** |
| | (2.77) | (3.97) | (3.53) |
| LT_rank x Prev_deals | | -0.00117*** | -0.00068*** |
| | | (-7.45) | (-3.36) |
| LT_rank x Prev_M&A | | -0.00003*** | -0.00002** |
| | | (-3.19) | (-2.33) |
| Prev_deals | 0.01617*** | 0.01132*** | 0.03515*** |
| | (5.86) | (3.99) | (8.15) |
| LY_mkt_share | 0.02746* | 0.03643** | 0.02728 |
| | (1.70) | (2.09) | (1.63) |
| LY_mkt_share x Prev_deals | | | 0.00916 |
| | | | (0.97) |
| LY_mkt_share x Prev_M&A | | | -0.00037 |
| | | | (-0.33) |
| Prev_deals x Prev_M&A | | | -0.00022*** |
| | | | (-7.94) |
| Bank fixed effects | Yes | Yes | Yes |
| Deal-client fixed effects | Yes | Yes | Yes |
| Adj. $R^2$ | 3.0% | 3.5% | 4.7% |
| N | 1,084,690 | 1,084,690 | 1,084,690 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 6

### *Determinants of fairness opinions*

This table presents OLS regressions examining the effect of league table incentives on the probability to be the bank providing a fairness opinion when there are multiple banks on the same side of a transaction. The analysis is at the mandate level. The sample excludes transactions done in January, and is restricted to co-mandate observations. The dependent variable is *Fo*, an indicator variable equal to 1 if the mandate is a FO. *LT_contribution* is the average impact of the deal on the gap in league table credit between the bank and its 24 competitors in the league table at the end of the week before the deal announcement date. For each competitor, this impact is calculated as the league table credit of the deal divided by the absolute value of the difference between the current total league table credit of the bank and that of the competitor. *Deviation* is the number of ranks gained / lost by the bank in the league table since the beginning of the year, calculated at the end of the week before the deal announcement date. *LT_rank* is the rank of the bank in the league table at the end of the week before the deal announcement date, multiplied by -1. All other variables are described in Appendix 4. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) |
|---|---|---|
| Dependent variable | Fo | Fo |
| LT_contribution | 0.043*** | 0.041*** |
| | (3.43) | (3.21) |
| Deviation | -0.003** | -0.003** |
| | (-2.10) | (-2.00) |
| LT_rank | 0.007*** | 0.007*** |
| | (2.76) | (2.80) |
| Prev_deals_target | | 0.003 |
| | | (1.33) |
| Prev_deals_acquiror | | -0.001* |
| | | (-1.71) |
| Prev_deals_target x Sell_side | | -0.003 |
| | | (-0.88) |
| Prev_deals_acquiror x Sell_side | | 0 |
| | | (0.18) |
| LY_mkt_share | | -0.034 |
| | | (-0.47) |
| Bank fixed effects | Yes | Yes |
| Deal-client fixed effects | Yes | Yes |
| Adj. $R^2$ | 53.6% | 53.7% |
| N | 6,797 | 6,767 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 7

### *The effect of a loss/gain of ranks on the number of fairness opinions provided by the bank*

This table presents panel regressions examining the effect of a change in the league table ranking on the number of fairness opinions provided by the bank in a co-mandate context. The analysis is at the quarter-bank level. The sample excludes transactions done in the first quarter of each year. In specifications (1) and (3), the dependent variable is the quarterly number of FOs done by the bank in a co-mandate context as a percentage of its total number of mandates. In specifications (2) and (4), the dependent variable is the quarterly number of FOs done by the bank in a co-mandate context. In specifications (1) and (2), we consider the effect of a loss/gain of ranks *inside* the league table only. In specifications (3) and (4), we consider the effect of a loss/gain of ranks in the *full* ranking of M&A advisors. $Deviation_{q-1}$ is the number of ranks gained / lost between the end of the previous year and the end of the previous quarter. $LT\_rank_{q-1}$ is the rank of the bank in the league table at the end of the previous quarter, multiplied by -1. $Full\_rank_{q-1}$ is the rank of the bank in the full ranking of M&A advisors at the end of the previous quarter, multiplied by -1. $Above25_{q-1}$ is a dummy variable equal to 1 if the bank was ranked inside the league table at the end of the previous quarter. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | *Published ranking* | | *Full ranking* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dependent variable | $Pct\_fo\_co_q$ | $Nb\_fo\_co_q$ | $Pct\_fo\_co_q$ | $Nb\_fo\_co_q$ |
| $Deviation_{q-1}$ | -0.188** | -0.033*** | 0.005 | -0.001 |
| | (-2.15) | (-3.85) | (0.72) | (-1.04) |
| $Above25_{q-1}$ | | | 0.518 | 0.045 |
| | | | (0.68) | (0.84) |
| $Deviation_{q-1}$ x $Above25_{q-1}$ | | | -0.050*** | -0.002*** |
| | | | (-2.87) | (-2.70) |
| $LT\_rank_{q-1}$ | 0.033 | 0.032*** | | |
| | (0.47) | (3.26) | | |
| $Full\_rank_{q-1}$ | | | 0.010* | 0.002*** |
| | | | (1.71) | (3.06) |
| Year-quarter fixed effects | Yes | Yes | Yes | Yes |
| Bank fixed effects | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 10.1% | 49.9% | 10.8% | 49.8% |
| N | 2,060 | 2,060 | 2,060 | 2,060 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 8

### *Determinants of fees*

This table presents OLS regressions examining the effect of league table incentives on the amount of fees when there are multiple banks on the same side of a transaction. The analysis is at the mandate level. The sample excludes transactions done in January, and is restricted to co-mandate observations. The dependent variable is the total fee of the bank divided by the total deal value (expressed in basis points). *LT_contribution* is the average impact of the deal on the gap in league table credit between the bank and its 24 competitors in the league table at the end of the week before the deal announcement date. For each competitor, this impact is calculated as the league table credit of the deal divided by the absolute value of the difference between the current total league table credit of the bank and that of the competitor. *Deviation* is the number of ranks gained / lost by the bank in the league table since the beginning of the year, calculated at the end of the week before the deal announcement date. *LT_rank* is the rank of the bank in the league table at the end of the week before the deal announcement date, multiplied by -1. All other variables are described in Appendix 4. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) |
|---|---|---|
| Dependent variable | Fee | Fee |
| LT_contribution | -9.269** | -9.812** |
| | (-2.24) | (-2.60) |
| Deviation | 1.930** | 3.026** |
| | (2.08) | (2.22) |
| LT_rank | -0.964 | -2.061* |
| | (-0.83) | (-1.76) |
| Fo | -31.909* | -32.803* |
| | (-1.86) | (-1.95) |
| Prev_deals_target | | -1.173 |
| | | (-1.27) |
| Prev_deals_acquiror | | -0.78 |
| | | (-0.58) |
| Prev_deals_target x Sell_side | | 2.191** |
| | | (2.04) |
| Prev_deals_acquiror x Sell_side | | 0.678 |
| | | (0.53) |
| LY_mkt_share | | 95.985* |
| | | (1.84) |
| Side_added_order | | 5.944 |
| | | (1.13) |
| Time_to_notif | | -0.203* |
| | | (-1.92) |
| Bank fixed effects | Yes | Yes |
| Deal-client fixed effects | Yes | Yes |
| Adj. $R^2$ | 76.0% | 75.3% |
| N | 648 | 647 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 9

### *The effect of fairness opinions on future rankings*

This table presents panel regressions examining the effects of fairness opinions on the rank of the bank in the league table. It focuses on fairness opinions done in a co-mandate context. The analysis is at the quarter-bank level. The dependent variable is $\Delta$ $LT\_rank_q$, the year-on-year change in the rank of the bank in the league table at the end of quarter $q$ ($LT\_rank_{i,q,y}$ - $LT\_rank_{i,q,y-1}$). $\Delta$ $Pct\_fo\_co_q$ is the year-on-year change in the percentage of FOs done by the bank in a co-mandate context during quarter $q$ ($Pct\_fo\_co_{i,q,y}$ - $Pct\_fo\_co_{i,q,y-1}$). All other variables are described in Appendix 4. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable | $\Delta$ LT_rank$_q$ | $\Delta$ LT_rank$_q$ | $\Delta$ LT_rank$_q$ |
| $\Delta$ Pct_fo_co$_q$ | 2.260*** | 1.690** | 1.732** |
| | (2.85) | (2.23) | (2.25) |
| $\Delta$ Mkt_share$_q$ | | 22.055*** | 21.556*** |
| | | (6.33) | (6.27) |
| Year-quarter fixed effects | Yes | Yes | Yes |
| Bank fixed effects | No | No | Yes |
| Adj. $R^2$ | 1.1% | 11.0% | 10.6% |
| N | 2710 | 2710 | 2710 |

* significant at 10%; ** significant at 5%; ***significant at 1%

## Table 10

### *The effect of league table incentives on the quality of fairness opinions*

This table presents OLS regressions examining the effects of league table incentives on the quality of fairness opinions. In specification (1), the dependent variable is the size of the valuation range disclosed by the fairness opinion provider scaled by the offer price of the deal. In specification (2), the dependent variable is an indicator variable equal to 1 if the deal is withdrawn, and 0 otherwise. In specification (3), the dependent variable is the combined CAR (-1, +1) at the deal announcement date. *LT_contribution* is the average impact of the deal on the gap in league table credit between the bank and its 24 competitors in the league table at the end of the week before the deal announcement date. For each competitor, this impact is calculated as the league table credit of the deal divided by the absolute value of the difference between the current total league table credit of the bank and that of the competitor. *Deviation* is the number of ranks gained / lost by the bank in the league table since the beginning of the year, calculated at the end of the week before the deal announcement date. All other variables are described in Appendix. Standard errors are clustered at the bank level. t-statistics are in parentheses.

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable | Valuation_range | Withdrawn | Combined_CAR |
| LT_contribution | 0.048*** | 0.038* | -0.006*** |
| | (3.10) | (1.84) | (-3.44) |
| Deviation | -0.003 | 0.004 | 0.000 |
| | (-0.71) | (1.01) | (-0.10) |
| LT_rank | 0.006 | 0.002 | 0.000 |
| | (1.04) | (0.27) | (0.05) |
| Deal_value | -0.047 | 0.012 | 0.01 |
| | (-1.00) | (0.48) | (0.75) |
| Deal_value² | -0.002 | -0.004** | 0.000 |
| | (-0.48) | (-2.21) | (-0.22) |
| Sell_side | 0.018 | 0.000 | -0.007 |
| | (0.43) | (0.00) | (-1.58) |
| Friendly | 0.072** | -0.093 | -0.008 |
| | (2.60) | (-1.50) | (-0.92) |
| Tender | -0.089* | -0.067*** | -0.006 |
| | (-1.86) | (-2.76) | (-0.95) |
| Toehold | 0.001 | 0.001 | 0.000 |
| | (0.92) | (1.18) | (-1.46) |
| Payment_mix_stock | -0.002 | -0.032 | -0.020*** |
| | (-0.05) | (-0.98) | (-2.87) |
| Payment_mix_cash | -0.022 | -0.035 | -0.007 |
| | (-0.49) | (-1.47) | (-0.81) |
| Payment_mix_other | 0.225* | 0.044 | -0.047*** |
| | (2.00) | (0.92) | (-4.28) |
| Same_industry | 0.009 | -0.023 | 0.004 |
| | (0.26) | (-1.45) | (0.87) |
| Cross_border | 0.084* | -0.060*** | -0.004 |
| | (2.01) | (-2.69) | (-0.81) |
| Challenge | | 0.325*** | |
| | | (12.50) | |
| Defense | | -0.067** | |
| | | (-2.21) | |
| Comb_runup | | | -0.083* |
| | | | (-1.93) |
| Year fixed effects | Yes | Yes | Yes |
| Bank fixed effects | Yes | Yes | Yes |
| Adj. R² | 26.5% | 10.2% | 14.8% |
| N | 373 | 1,067 | 458 |

* significant at 10%; ** significant at 5%; ***significant at 1%

**Essays in Empirical Corporate Finance**

This dissertation is made of three distinct chapters. The first chapter shows that managers overreact to salient risks. They respond to the occurrence of a hurricane event when their firms are located in the neighborhood of the disaster area. The sudden shock to the perceived liquidity risk leads them to temporarily increase the amount of corporate cash holdings, even though the real liquidity risk remains unchanged. The second chapter examines earnings announcements by US firms, and how far in advance notice of the event is given (the "advance notice period"). Such advance notice period affects how much investors pay attention to earnings news. This variation in investors' attention affects short-run and long-run stock prices, thereby creating incentives for firms to strategically reduce the advance notice period when they plan to disclose bad news. The third chapter studies M&A league tables, which provide rankings of investment banks. The rank of a bank in the league table predicts its future deal flow. This creates strong incentives for banks to manage their ranks in the league table.

Keywords: Salience, Availability heuristic, Risk management, Limited attention, Earnings announcements, League tables, Investment banking, Mergers and acquisitions

**Essais en Finance d'Entreprise Empirique**

Cette thèse est composée de trois chapitres distincts. Le premier chapitre montre que les dirigeants réagissent de façon excessive face aux risques qui frappent l'attention. Après un ouragan, le choc produit par la catastrophe sur le risque de liquidité perçu conduit les entreprises situées dans le voisinage de la zone sinistrée à augmenter temporairement leur détention de liquidités alors que le risque réel n'a pas changé. Le deuxième chapitre montre que les dirigeants influencent de façon stratégique l'attention des investisseurs aux annonces de résultats en les prévenant plus ou moins tardivement de la date de l'événement. Cette stratégie leur permet de lisser dans le temps l'impact de mauvais résultats sur leur cours de bourse. Le troisième chapitre étudie l'effet des *league tables* dans les activités de fusions-acquisitions. Les *league tables* classent les banques d'investissement. Le rang d'une banque dans la *league table* prédit sa capacité à engendre des affaires nouvelles dans le future, ce qui incite les banques à manipuler leur classement.

Mots-Clés: Saillance, Gestion des risques, Attention limitée, Annonces de résultat, League tables, Banques d'investissement, Fusions et acquisitions