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THÈSE

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COMMENT L'EXPOSITION AU RISQUE

AFFECTE LA VALEUR ?

LES FUSIONS TRANSFRONTALIERES

ET

LES EFFETS DU PLAN PAULSON

JURY

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TABLE DES MATIÈRES

CHAPITRE 1: INTRODUCTION

La gestion du risque est un point primordial pour la santé d'un marché financier, des entreprises qui y trouvent leurs financements et plus globalement de l'économie du pays. Dès l'apparition de la finance moderne au cours du siècle dernier, les chercheurs ont investi ce domaine. Markowitz (1952) s'est intéressé à la sélection d'actifs dans un portefeuille efficient et a souligné l'importance de la diversification du risque. Les sujets du choix du financement, de la valeur et du risque ont été ensuite analysés par de nombreux auteurs. Les études suivantes ont démontré que le contrôle des pertes (Cho 1988) et la mise en place d'une gestion du risque par les firmes (Cassidy, Constand et Corbett 1990) permettent de créer de la valeur. En outre, le recours à des techniques de couverture (*hedging*) peut être bénéfique. Par exemple les firmes ayant tendance à utiliser les produits dérivés (Dodle 1993; Leland 1998) bénéficient d'une hausse de leur capacité d'endettement (Graham et Rogers 2002). Plus récemment, les études sur la crise financière ont révélé l'importance des choix en matière de gestion des risques et des financements sur la valeur des firmes (Kashyap et Zingales 2010).

Avec la modernisation du marché financier, le développement des produits dérivés et la vague de titrisation, le risque potentiel incorporé dans le système financier est de plus en plus difficile et compliqué à analyser. Bookstaber (1999) décrit que la gestion du risque se base souvent sur des éléments observables, tels que le risque du marché, le risque de faillite et le risque opérationnel. Néanmoins, le risque inobservable généré par une structure financière complexe est plus crucial. Du côté de l'entreprise, les risques accompagnent une compétition intense, un marché financier complexe, et la globalisation. Du côté du secteur financier, le marché augmente les risques en créant des interdépendances de plus en plus nombreuses. Mais l'accumulation des risques détériore à son tour le bon roulement du marché.

La chute du géant Lehman Brothers a fait réfléchir sur le rôle vital du risque, non seulement au niveau de l'entreprise mais aussi au niveau de l'État. Cette crise financière a offert un cadre riche d'investigation des comportements des agents économiques, de leur prise de risque, et surtout de leur stratégie de gestion du risque. C'est à partir de ce cadre-là que nous avons élaboré les idées développées tout au long de cette thèse. La problématique est abordée en trois essais : (1) la gestion opérationnelle du risque au niveau des firmes ; (2) l'effet de la décision des gouvernements de sauver le secteur bancaire ; (3) l'effet de bord (*spillover effect*) du plan de sauvetage sur les firmes.

1. Motivation et synthèse des trois essais

Le premier essai de notre programme de recherche s'intéresse à la gestion du risque de change par les entreprises. Pour les firmes en interactions avec des fournisseurs étrangers, des clients étrangers et/ou simplement des concurrents étrangers sur leur marché domestique, l'exposition au risque de change est inévitable. Les entreprises ont tendance à recourir aux produits dérivés pour couvrir ce type de risque (Stulz 2004; Bartram, Brown et Fehle 2009). Cependant, Guay et Kothari (2003) montrent que l'utilisation de produits dérivés par les entreprises est relativement modeste par rapport à leur taille et leur exposition opérationnelle. Mais les firmes peuvent aussi générer des couvertures efficaces en réalisant des opérations financières comme par exemple les fusions et acquisitions (F&A) (Pantzalis, Simkins et Laux 2001; Kim, Mathur et Nam 2006; Garfinkel et Hankins 2011). Notre étude s'intéresse à la question de savoir dans quelle mesure les entreprises implémentent des opérations de F&A transfrontalières pour réduire leur exposition au risque de change. Nous avons constitué un échantillon de 152 opérations de F&A transfrontalières réalisées par 250 firmes françaises figurant dans l'indice SBF250. Puis, nous avons estimé l'exposition au taux de change pour chaque acquéreur à l'aide du modèle de marché ajusté selon la procédure d'Adler et Dumas (1984). En séparant l'échantillon en exportateurs nets (exposition négative au taux de change, qui bénéficient d'une sous-évaluation de la monnaie locale) et importateurs nets (exposition positive au taux de change, qui bénéficient d'une surévaluation de la monnaie locale), nous avons trouvé que les F&A transfrontalières affectent positivement l'exposition au taux de change pour les exportateurs nets, tandis que l'impact est négatif pour les importateurs nets. Ces résultats indiquent que les opérations de F&A transfrontalières procurent en moyenne une couverture opérationnelle pour les acquéreurs. De surcroit, nous adoptons différentes approches économétriques (en particulier, l'approche *difference-in-differences*, ou DID) en vue de tenir compte des variables inobservables qui différencient les acquéreurs des entreprises qui ne procèdent pas à de telles opérations. Nos résultats sont robustes. La méthode DID permet de mettre en lumière que les firmes traitées (les firmes qui ont réalisé des opérations transfrontalières) sont significativement plus exposées aux risques liés aux taux de change avant les opérations, que les firmes de contrôle (choisies en fonction de deux critères : 1. des firmes qui n'ont pas réalisé d'opérations transfrontalières dans le pays de

la cible pendant deux ans autour de l'annonce d'opération transfrontalière par l'acquéreur ; 2. des firmes aléatoirement tirées dans le même secteur que celui de l'acquéreur). Ce résultat soutient l'hypothèse que la couverture du risque de change est un déterminant important de décision des F&A transfrontalières. Dans un deuxième temps, nous avons analysé l'effet de richesse créé par cette couverture opérationnelle. On peut s'attendre à ce que la couverture opérationnelle affecte positivement la valeur de l'entreprise, puisqu'elle réduit la volatilité des flux futurs de trésorerie. Les rentabilités anormales cumulées (CAR) montrent que la décroissance de l'exposition au risque de change (en valeur absolue) est positivement et significativement liée aux CAR à l'annonce de l'opération de F&A. En moyenne, pour une opération transfrontalière de 100 millions de dollars, la valeur additionnelle créée est approximativement de 1.68 million de dollars pour les actionnaires des acquéreurs.

Suite au premier essai, le deuxième chapitre de notre thèse aborde l'effet de la gestion du risque financier au niveau national. La crise de 2007, initiée par la crise des prêts hypothécaires à risque (*subprimes*), a obligé le gouvernement américain à intervenir. La mise en œuvre d'un plan de sauvetage de l'industrie bancaire s'est révélée être indispensable pour arrêter la panique bancaire des créditeurs à court terme (Gorton et Metrick 2012) et garantir la liquidité et la stabilité du système financier (Diamond et Rajan 2009; Bernstein, Hughson, et Weidenmier 2010).

La crise a également mis en exergue l'importance du risque systémique. Fuji et Kawai (2010) souligne combien une intervention gouvernementale est indispensable pour traiter ce type de problème. Par exemple, durant la crise systémique de 1997 à 1998 au Japon, il a fallu intervenir à travers la nationalisation des banques en difficulté mais encore viables, la réduction des prêts non performants (*non-performing loans*) et la restructuration des dettes pour les firmes les plus en difficulté. En revanche, en cas d'absence d'intervention, le risque systémique peut aboutir à des conséquences très graves sur la croissance économique à long terme (Kupiec et Ramirez 2013). En octobre 2008, le *U.S. Treasury* et la *Federal Deposit Insurance Corporation (FDIC)* ont annoncé ensemble un plan de sauvetage (appelé aussi plan Paulson, du nom de Monsieur Henry Paulson alors secrétaire du *U.S. Treasury*) de 700 milliards de dollars, dont 250 milliards de dollars pour les institutions financières, sous la dénomination de *Capital Purchase Program (CPP)*.

Notre objectif est d'analyser l'effet de ce plan pour remédier à cette crise financière. En outre, nous nous intéressons aux effets de biais sur la concurrence entre les banques. L'intervention gouvernementale peut réduire le coût de refinancement pour les banques sous protection par rapport à celui que supportent leurs concurrents non assistés (Gropp, Hakenes et Schnabel 2011). Cela peut aussi créer un problème d'aléa-moral : les banques comptant sur les aides gouvernementales pourraient être incitées à prendre plus de risques (Berger, Bouwman, Kick et Schaeck 2010; Duchin et Sosyura 2014). Inspiré par ces études, nous avons tenté de vérifier si le plan Paulson a créé des effets de distorsion entre les banques.

Dans un marché efficient, les prix reflètent immédiatement l'information disponible. Si la distorsion existe, on doit observer une réponse à travers la variation des cours boursiers qui affectent directement la richesse des actionnaires. Pour ce faire, nous analysons les CAR pendant plusieurs jours autour de l'annonce. En outre, l'emploi de l'indicateur *distance-to-default* (DD, une mesure du risque de faillite introduite par Merton 1974[1]), nous permet d'analyser la probabilité de faillite des institutions financières. Si le CPP est neutre par rapport à la taille de banque, on espère que : (1) les CAR autour de l'annonce initiale du CPP ne sont pas systématiquement liés à la taille de l'établissement car chaque banque doit avoir la même probabilité d'être soutenue, indépendamment de sa taille ; (2) les CAR ne sont pas systématiquement corrélés à la taille relative de l'injection de fonds (par rapport à la taille du bénéficiaire), car chaque dollar du capital injecté devrait générer une valeur équivalente. Une relation positive et significative peut indiquer des effets plus favorables du CPP pour les grandes banques. Les résultats empiriques montrent que : (1) les CAR sont significativement positifs autour de l'annonce initiale du plan Paulson, mais sont négatifs et significatifs pour les injections suivantes ; (2) la relation significative entre les CAR et la taille, et celle entre la DD et la taille, mettent en évidence l'hétérogénéité des effets du plan de sauvetage entre les banques bénéficiaires. Nous avons étendu ensuite notre analyse aux années suivantes, en estimant les rentabilités anormales selon l'approche des *Calendar Time Abnormal Returns* (CTAR). Les rentabilités anormales trouvées sont négatives et significatives pour les petites banques (inférieures à la médiane de capitalisation boursière) alors que ce n'est pas le cas pour

[1] La méthode de calcul est proposée par Vassalou et Xing (2004) à partir du modèle de Merton (1974). En résumé, la distance au défaut mesure la probabilité de faillite : plus la distance est longue, moins probable est la faillite, et vice versa.

les banques de grande taille. En résumé, ce deuxième chapitre consacré aux conséquences du plan Paulson confirme son effet positif autour de son annonce initiale comme l'avaient déjà montré les travaux de recherche précédents. Néanmoins, notre apport a été de mettre en évidence les effets de distorsion en matière de compétition entre les banques participantes. Nos résultats ont été confirmés par des tests de robustesse. Les grands joueurs sont les gagnants.

Si on se réfère au rapport trimestriel du plan de sauvetage au Congrès émis par le SIGTARP[2], deux objectifs ont été attribués au CPP : le premier concerne le renforcement du système financier ; le deuxième est d'encourager les prêts bancaires et supporter un redémarrage de l'activité économique. Ce deuxième objectif concerne l'impact du plan Paulson sur les firmes à travers l'étude des comportements des banques en matière d'octroi de prêts. Dans ce cadre, on peut distinguer les bénéficiaires directs des fonds injectés qui sont les banques participantes au plan de sauvetage, et les emprunteurs auprès de ces mêmes établissements qui sont les utilisateurs finaux des prêts bancaires. En général, les entreprises emprunteuses sont des composants importants de l'économie nationale. Les prêts bancaires sont des supports vitaux pour leurs activités opérationnelles et leurs projets d'investissements.

Pour cette raison, notre troisième essai analyse les effets de bord du CPP. Deux études récentes ont montré un effet positif d'incitation sur l'offre des prêts bancaires (Li 2013), et sur les rentabilités journalières des firmes (Norden, Roosenboom et Wang 2013). Cependant ces études étaient concentrées sur la période de 2008 à 2009, la période d'après crise n'a pas été analysée. De ce fait, nous avons élargi notre recherche non seulement aux années de la crise financière mais aussi aux années suivantes. Comme les prêts bancaires participent de manière importante à la structure financière des firmes, leurs caractéristiques peuvent affecter sensiblement le risque de faillite. Dans ce troisième essai, nous avons étudié les effets de bord du CPP sur les pratiques d'octroi de prêts et plus particulièrement sur les caractéristiques des crédits accordés.

Suite à Li (2013), nous avons commencé à analyser les effets de bord sur la taille des prêts bancaires. La maturité de la dette est aussi un élément crucial pour l'entreprise surtout lorsque la situation du marché se détériore, comme ce fût le cas durant la crise financière

[2] *Office of the Special Inspector General for the Troubled Asset Relief Program*

de 2007. En particulier, les dettes à court terme sont plus exposées aux risques de refinancement (Daianu et Lungu 2008; Duchin, Ozbas et Sensoy 2010) et de surendettement[3] (*debt overhang*) dans certains cas (Almeida, Campello, Laranjeira et Weisbenner 2011; Diamond et He 2014). Le risque de refinancement est en plus corrélé au risque de crédit pendant la période d'absence de liquidité (He et Xiong 2012). La maturité de la dette dépend aussi du niveau de risque pour les firmes, par exemple, les entreprises les moins risquées et celles très risquées choisissent plutôt les dettes à court-terme (Diamond 1991). Nous nous intéressons à la maturité pour détecter si les effets de bord y jouent un rôle.

Nos analyses se sont basées sur un échantillon de prêts syndiqués, car ces derniers nous permettent de bien mesurer le lien entre les firmes et les banques. Ensuite, nous avons recouru à des mesures relatives de la maturité et de la taille des prêts syndiqués en les corrigeant de la médiane du secteur estimée avant la crise. La procédure d'identification des banques leaders pour chaque prêt syndiqué nous a permis de mesurer le niveau de l'impact du CPP. Plus précisément, nous avons calculé un ratio d'intervention pour chaque prêt syndiqué par la somme de la taille relative d'intervention gouvernementale pour chaque banque leader dans cette syndication. Après avoir neutralisé l'effet de différentes variables au niveau de l'emprunteur et du prêt, nos résultats ont fait apparaître une corrélation positive et significative entre le ratio d'intervention et la variation relative de maturité pendant les années après la crise. Pourtant cette relation positive et significative n'est pas vérifiée pour la variation relative de la taille des prêts syndiqués. Ces résultats se sont révélés robustes aux approches alternatives de calcul du ratio d'intervention et de contrôle de la performance de la syndication. En résumé, le troisième essai analyse les effets de bord de l'intervention gouvernementale et dévoile un impact positif et significatif du plan Paulson sur la maturité des prêts bancaires syndiqués après la crise.

[3] Le problème de surendettement peut négativement impacter la valeur de l'entreprise. Si l'entreprise a trop de dettes, elle peut abandonner des projets à valeur actuelle nette positive dans certains cas, même si ces projets peuvent augmenter la valeur d'entreprise (Myers 1977).

2. Revue de la littérature

2.1 Gestion du risque au niveau des firmes

2.1.1 Théorie classique

Dans la première étape de cette revue de la littérature, il est indispensable de présenter les théories classiques concernant le risque. Le célèbre lauréat du prix Nobel de 1990, Markowitz, mesure le risque par la volatilité de la rentabilité du cours boursier. Dans son article de 1952, il décrit l'effet de la diversification et conclut qu'il faut choisir un portefeuille avec un maximum de rentabilité espérée en supposant un certain niveau de variance ou celui avec une espérance de rentabilité donnée pour une variance minimale. Il met aussi en évidence que l'effet de diversification est plus grand pour un portefeuille transversal car la covariance entre les actifs financiers est plus petite quand ils ne sont pas concentrés dans un même secteur. De ce fait, une frontière efficiente peut être construite avec toutes les combinaisons possibles d'actifs (les portefeuilles) qui offrent les meilleures rentabilités, pour différents niveaux de risque et il n'est pas possible de trouver d'autres portefeuilles qui génèrent des rentabilités supérieures pour un même niveau de risque.

Une autre branche de la littérature est aussi importante à citer. Il s'agit d'un courant qui aborde la gestion du risque à partir d'un point de vue psychologique, i.e., ce qui est convenu de désigner comme la finance comportementale. Dans un texte fondateur, Kahneman et Tversky (1979) ont introduit la théorie du prospect. Cette théorie tient compte des biais de comportement et fournit une explication aux différences observées entre les décisions prises par les investisseurs et celles prédites par la théorie de l'espérance de l'utilité. Ils ont incorporé ainsi dans leur modèle les phénomènes de surestimation de la certitude, d'aversion du risque en cas de gain et de poursuite du risque en cas de perte. Dans sa revue de littérature, Barberis (2013) conclut que le manque d'applications directes a freiné le développement de cette théorie pendant trois décennies. Pourtant, de plus en plus de modèles d'aujourd'hui prennent en compte des éléments comportementaux et la théorie du prospect trouve sa place dans la finance, l'assurance et d'autres domaines concernés.

Si la gestion du risque est efficace, on peut espérer qu'une volatilité plus petite de la trésorerie se traduit par une valeur plus élevée de l'entreprise. Cho (1988) propose une fonction exponentielle du contrôle de perte et l'applique dans le modèle d'évaluation d'entreprise de Gordon. L'auteur montre que l'efficience du contrôle de perte est vitale pour déterminer le niveau optimal de la gestion des risques. Comme cette dernière a un impact sur le coût du capital, elle influence donc la valeur de la firme. Sous certaines conditions, cette influence est positive. Un test empirique par Cassidy, Constand et Corbett (1990) observe la réaction du marché lorsqu'une entreprise développe son service de gestion du risque. Leur échantillon de 80 actions indique des CAR positifs et significatifs pendant la période précédant la date réelle d'annonce officielle. Ceci implique une évaluation positive par les investisseurs concernant cet aspect du management.

2.1.2 Les techniques de couverture

(1) La couverture par produits dérivés

Une première solution vis-à-vis du risque concerne l'utilisation de produits dérivés, tels que les contrats à terme, les options et les swaps, aussi bien pour la gestion des risques de taux d'intérêt, de taux de change que de prix des matières premières, comme par exemple le prix du pétrole.

Les produits dérivés peuvent être utilisés pour la couverture du risque, mais aussi pour la spéculation. Un souci principal est donc d'identifier le vrai but d'utilisation des produits dérivés. Selon l'enquête[4] de Dodle (1993), sur les 244 firmes classées dans la liste Fortune 500, la plupart de ces firmes ont principalement utilisé les produits dérivés pour couvrir leurs risques. Mais, seulement 20% d'entre elles avaient pour objectif de couvrir totalement leur risque. Cependant, ce type d'enquête ne peut pas être complètement objectif, car les firmes sont plutôt réticentes à avouer leur motivation spéculative.

Mian (1996) analyse les déterminants de la décision de couverture, en prenant en compte toute une série de coûts : le coût de sous-investissement, le coût de faillite, les taxes, le coût lié au financement externe provenant des imperfections du marché financier. Les résultats sont mitigés pour la plupart de ces déterminants, mais l'auteur trouve un

[4] Nance, Smith et Smithson (1993) et Wharton et Chase Manhattan (1995) ont aussi étudié la fréquence d'utilisation des produits dérivés.

résultat robuste à savoir que les grandes firmes ont plus tendance à utiliser les techniques de couverture. Plus précisément, Leland (1998) argumente que ces techniques permettent à l'entreprise d'avoir plus de dettes et que le bénéfice de la couverture est plus élevé si les coûts d'agence sont faibles. Graham et Smith (1999) ont réalisé une simulation et ont montré que le bénéfice de la couverture était lié à la présence d'une fonction convexe d'imposition. Mais la convexité ne constitue pas une motivation majeure de couverture. Similairement, Graham et Rogers (2002) ont démontré que les firmes couvrent leur risque afin d'augmenter leur capacité d'endettement et de bénéficier de plus de charges financières déductibles. Ces avantages représentent d'après eux environ 1,1% de la valeur de l'entreprise. La couverture constitue une réponse à la présence d'un coût de faillite et elle est liée à la taille de l'entreprise. Bartram, Brown et Conrad (2011) ont élargi leur échantillon à 47 pays et ont montré que la couverture par produits dérivés permet de réduire le risque total et le risque systémique.

Toutefois, l'effet de la couverture par des produits dérivés est discutable. Guay et Kothari (2003) ont constitué un échantillon aléatoire de 234 grandes firmes non financières. Ils ont mesuré la sensibilité de leurs trésoreries et celle de la valeur marchande du portefeuille de produits dérivés, en envisageant des changements simultanés et extrêmes de 3 écart-types pour les taux d'intérêt, le taux de change et les prix des matières premières. Leurs résultats ont révélé que ces sensibilités sont négligeables par rapport à la taille de l'entreprise, aux flux de trésorerie d'exploitation et à ceux des investissements.

(2) La couverture opérationnelle

Parallèlement, la couverture opérationnelle est une autre stratégie pour la gestion du risque. La couverture opérationnelle joue un rôle complémentaire (plutôt que de substitution) aux techniques de couverture par les produits financiers, et elle permet à l'entreprise de réduire le risque de change et de créer de la valeur (Lim et Wang 2001; Pentzalis, Simkins et Laux 2001; Carter, Pantzalis et Simkins 2001; Kim, Mathur et Nam 2006). Ainsi, Garfinkel et Hankins (2011) ont suggéré que l'incertitude pesant sur les flux de trésorerie incite les firmes à s'engager dans des fusions verticales. S'ensuit une vague de fusions.

a. Les études sur les Fusions & Acquisitions en général

Le marché des Fusions & Acquisitions est toujours très important, de l'ordre de milliers de milliards de dollars, et il continue de grandir. Selon Thomson Financial, la taille du marché des F&A au niveau mondial s'est élevée à 1 576 milliards (27 463 opérations), 2 436 milliards (39 833 opérations) et 2 361 milliards (37 853 opérations) de dollars en 2011, 2012 et 2013 [5] respectivement. D'un point de vue géographique, 51% des opérations ont été réalisées en 2013 aux Amériques, 25% en Europe et 24% dans le reste du monde.

De ce fait, les sujets de recherche sur les F&A sont très vastes. Betton, Eckbo et Thorburn (2008) ont mis en exergue les sujets les plus importants abordés dans ce domaine : les vagues de fusions ; les méthodes de paiement, en liquide ou en titres ou en combinant les deux ; le recours au *toehold* (où l'acquéreur a déjà une position initiale dans le capital de la cible avant l'offre publique d'achat) ; la prime d'offre ; la défense par la cible ; l'achat des cibles en situation de faillite ; la création de valeur ; les caractéristiques des dirigeants ; les impacts de la concurrence et les effets des législations antitrust.

Afin d'apprécier la création de valeur pour les actionnaires occasionnée par ces opérations, les chercheurs calculent le plus souvent les CAR autour des annonces. Ils les considèrent comme représentant la valeur créée aussi bien pour les acquéreurs que pour les cibles. En sommant les CAR des deux (acquéreurs et cibles), ils obtiennent des mesures de synergie. Les résultats de la plupart des études ont montré que les acquéreurs ne gagnent sensiblement rien, alors que les cibles réalisent des rentabilités anormales positives et significatives. L'ensemble des bénéfices pour les deux parties est positif (Jensen et Ruback 1983; Andrade, Mitchell et Stafford 2001; Moeller, Schlingemann et Stulz 2004; Betton, Eckbo et Thorburn 2008; Eckbo 2013). En plus, on a trouvé que les rentabilités anormales sont corrélées avec la taille de l'acquéreur (Moeller, Schlingemann et Stulz 2004), le type de la cible (Chang 1998; Fuller, Netter et Stegemoller 2002; Capron et Shen 2007), le degré d'hostilité de la proposition (Morck, Shleifer et Vishny 1990; Schwert 2000), la nature horizontale ou verticale de l'acquisition (Morck, Shleifer et Vishny 1990; Maquieira, Megginson et Nail 1998; Schoar 2002; Fan et Goyal 2006), et les clauses d'opposition au rachat (Masulis, Wang et Xie 2007). Il y a d'autres facteurs qui

[5] Les chiffres sont exprimés en valeur nette (après déduction de la dette des cibles).

influencent aussi cette richesse créée, comme les caractéristiques des dirigeants des acquéreurs tels que leur orgueil excessif (*CEO hubris*, Roll 1986) et leur expérience (Custodio et Metzger 2013; Aktas, de Bodt et Roll 2013).

b. Fusions & Acquisition transfrontalières

Parmi les moyens de couverture opérationnelle, les F&A transfrontalières sont de plus en plus utilisées par les firmes de nos jours, en partie pour faire face à l'incertitude entourant l'évolution des taux de change. Dans leur revue de la littérature sur les F&A transfrontalières, Shimizu, Hitt, Vaidyanath et Pisano (2004) ont mis en évidence trois axes de recherche privilégiés : (1) les F&A transfrontalières facilitent l'accès à un marché étranger ; (2) les F&A transfrontalières permettent aux acquéreurs de choisir de meilleures cibles ; (3) les F&A transfrontalières créent de la valeur. Parmi les motivations qui poussent à réaliser des F&A transfrontalières, on trouve la concurrence (Bjorvatn 2004; Neary 2005), le taux de change (Georgopoulos 2008), les barrières commerciales et les taxes (Hijzen, Gorg et Manchin 2008; Huizinga et Voget 2009), et d'autres raisons comme la géographie, la qualité de la règlementation comptable, la valorisation du marché par le ratio de cours/valeur comptable (Erel, Liao et Weisbach 2012).

En rapport avec la création de valeur par la couverture opérationnelle, Francis, Hasan et Sun (2008) ont montré que les rentabilités anormales pour les acquéreurs sont plus élevées quand ils achètent des cibles sur un marché segmenté. Bris et Cabolis (2008) ont conclu que la prime payée est positivement liée au niveau de protection des actionnaires et à la qualité des informations comptables fournies. De même, Danbolt et Maciver (2012) ont mis en évidence que la qualité du système de gouvernance affecte positivement la valeur créée par les cibles. De plus, avec un échantillon limité à la Grande-Bretagne, les auteurs ont présenté des résultats plus positifs pour les F&A transfrontalières que ceux associés aux F&A locales. Du côté des aspects négatifs, Aybar et Ficici (2009) ont dévoilé que pour les acquéreurs venant des pays émergents, les F&A transfrontalières n'ont pas créé de valeur, voire en ont détruit pour la moitié des transactions de leur échantillon. Une raison possible de cette baisse de valeur a été suggérée par Brannen et Peterson (2009). Ces auteurs argumentent que l'insuffisance d'effort en matière de coordination entre les différentes cultures de travail a des conséquences sérieuses sur la suite des opérations de fusions transfrontalières.

2.1.3 La gestion de dette et la maturité

Le recours à la dette dans le financement d'une entreprise entraine toute une série de problèmes et de coûts : risque de faillite, coût d'agence et autres problèmes potentiels. Une des questions de recherche les plus anciennes concernant la gestion de la dette est la mise en évidence d'une structure financière optimale afin de maximiser la valeur d'entreprise. Modigliani et Miller (MM, 1958) ont élaboré leur analyse dans un environnement de marché parfait sans impôt sur les sociétés, et ont illustré le fait que la structure financière d'une firme n'affecte pas sa valeur et que le coût moyen du capital reste le même, quelque soit le niveau de dette choisi par la firme. Myers (1977) a décrit un environnement où la valeur de la firme dépend du capital initial et de la valeur des options réelles d'investissement sur des projets futurs. Son modèle implique un effet négatif de la dette sur la motivation à sélectionner de bons projets, et suggère une baisse de la valeur marchande pour l'entreprise. Concernant la gestion de l'endettement lui-même, cet auteur défend l'idée selon laquelle il est préférable de recourir aux dettes à court terme car elles confèrent plus de flexibilité à la firme. Cependant, elles sont aussi sources de coûts de renégociation et exigent de maintenir de bonnes relations bancaires à long terme. Barnea, Haugen et Senbet (1980) ont analysé ce point en détail suivant trois dimensions : information asymétrique, accroissement du risque par les actionnaires au détriment des créanciers (Black et Scholes 1973), et projets sous-optimaux (Myers 1977). Ces auteurs ont proposé que la réduction de la maturité et l'incorporation d'une clause de rachat (*call provision*) peuvent amortir l'effet de ces problèmes (voir aussi Leland et Toft 1996[6]). Le choix de la maturité dépend aussi de la longueur de vie des actifs détenus par l'entreprise et de leur usage. Par exemple, une firme endettée préfère les dettes à court-terme pour le paiement de salaire ou l'achat de matières premières. Il y a un aspect important concernant la maturité de la dette : les dettes à court-terme sont plus favorables aux créanciers car ils disposent de plus de pouvoir de contrôle et ont la possibilité de forcer la liquidation plus rapidement. En revanche, les contrats à long-terme offrent une protection plus forte aux emprunteurs (Hart et Moore 1994). Les modèles théoriques suggèrent que les dettes à long-terme peuvent créer un problème de sous-investissement et que les dettes à court-terme peuvent donner lieu à un problème

[6] Leland et Toft (1996) ont montré que les dettes à long-terme procurent un meilleur avantage fiscal. Pourtant elles génèrent un coût d'agence plus élevé, mais ce problème peut être atténué en recourant à des dettes à plus court terme.

de surinvestissement. L'optimal serait donc être un système plus sophistiqué combinant les deux (Hart et Moore 1995).

Dans un marché financier où prévaut une asymétrie d'information, la maturité de la dette peut jouer un rôle de signalisation (*signaling*). Les entreprises, connaissant leur qualité, optent pour des maturités différentes afin d'améliorer leur valorisation. Il y a deux études connues sur ce sujet. D'après Flannery (1986), les bonnes (mauvaises) firmes préfèrent les dettes à court-terme (long-terme) dans un environnement asymétrique, car leurs dettes à long-terme sont sous-évaluées (surévaluées). Cet équilibre de séparation peut être atteint lorsque le coût d'émission est très élevé et que les firmes de qualité inférieure n'arrivent pas à le supporter. Si ce n'est pas le cas, un équilibre de mélange (*pooling equilibrium*) peut exister et toutes les firmes choisissent l'endettement à court-terme, même si leur besoin est à long-terme. Diamond (1991) a incorporé en plus le risque de liquidité dans son modèle car les dettes à court-terme font courir ce risque lorsque le refinancement se révèle impossible. Ses résultats principaux indiquent une relation non monotone entre la maturité et la notation des firmes : les firmes très mal notées n'ont que le choix du court-terme ; les firmes de bonne qualité profitent des nouvelles informations positives et préfèrent aussi les dettes à court-terme ; les autres firmes moyennes optent plutôt pour des dettes à long-terme parce qu'elles donnent plus de poids au risque de liquidité qu'aux effets positifs apportés par les nouvelles informations.

Des études empiriques ont approfondi les connaissances des effets de la maturité en exploitant des données réelles et en testant les déterminants potentiels suggérés par la théorie. Guedes et Opler (1996) ont trouvé que les firmes les plus risquées évitent de choisir une maturité longue. C'est un résultat cohérent avec l'équilibre de séparation. D'autres chercheurs ont plutôt trouvé une relation non monotone : Scherr et Hulburt (2001) ont trouvé que les petites firmes les moins risquées et celles les plus risquées ont une maturité plus courte que les firmes qui ont un risque intermédiaire (voir aussi Stohs et Mauer 1996). Berger, Espinosa-Vega, Frame et Miller (2005) ont élaboré un test explicite selon différents niveaux d'asymétrie d'information. Lorsque l'asymétrie est moindre, la maturité des prêts de faible risque connait une hausse significative (en cohérence avec la théorie selon laquelle les firmes de faible risque augmentent la maturité lorsque l'asymétrie d'information est faible), mais la baisse de maturité n'est pas évidente pour les prêts de risque élevé (contrastant avec la théorie selon laquelle les

firmes très risquées utilisent les dettes à court-terme). D'autres éléments impactent aussi le choix de la maturité : la politique de détention d'espèces (*cash holding policy*) au sein des firmes (relation positive, voir Brick et Liao 2013 ou relation négative, voir Custodio, Ferreira et Laureano 2013, et Harford, Clasa et Maxwell 2014) et la capacité d'accès au marché obligataire (Faulkender et Petersen 2006).

2.2 Gestion du risque au niveau de l'État

2.2.1 La crise financière de 2008

Cette crise financière a suivi la crise des prêts hypothécaires risqués, car cette dernière a été le point de départ d'un effet de contagion (Longstaff 2010). Cet effet de contagion ne s'est pas limité au marché hypothécaire, mais a contaminé tout le marché financier. Aliber et Kindleberger (2005) passent en revue les crises financières dans l'histoire mondiale, y compris la grande dépression aux États-Unis durant les années 30, la crise des banques au Japon autour des années 90, les faillites des banques dans les pays nordiques durant la même période, et les crises dans les pays asiatiques à la fin de l'année 90. Un point commun à toutes ces crises est qu'une bulle s'était formée et avait très vite gonflé sur le marché hypothécaire pendant les années précédant la crise. Les auteurs de cet ouvrage analysent aussi l'effet de contagion d'un pays à l'autre : la corrélation entre les marchés des actions des différents pays était forte lorsque la variation des prix des actions était élevée. La panique s'est transmise dans le monde via les mouvements d'argent[7] (entrée et sortie), la coopération entre les institutions financières et monétaires, voire via des raisons psychologiques. Un papier plus récent par Castrén, Dées et Zaher (2008) analyse l'effet de contagion des crises vers les firmes emprunteuses. Les auteurs modélisent le lien entre les facteurs macro-financiers et la probabilité de faillite des firmes. Ils montrent que la probabilité de faillite (mesurée par les fréquences espérées de défaut, EDF) est liée principalement aux chocs du PIB (Produit Intérieur Brut), du taux de change, du prix du pétrole et du cours boursier. La titrisation devient aussi un point très discuté par les praticiens et les chercheurs. Buiter (2007) s'attache aux problèmes consécutifs à la

[7] Voir aussi l'analyse de Kaminsky, Reinhart et Végh (2005) sur le cycle du flux de capital et la politique monétaire des pays émergents et de l'organisation de coopération et de développement économiques (OCDE).

titrisation : cette dernière permet aux investisseurs de prendre plus de risques au lieu de servir de moyen de couverture ; le modèle dit « octroyer et céder » (*originate-and-distribute*) est une source de perte d'informations en comparaison avec ce que produit le modèle dit « octroyer et garder » (*originate-and-hold*).

L'impact de la crise sur l'ensemble de l'économie est très sérieux. Campello, Graham et Harvey (2010) collectent 1 000 questionnaires remplis par les directeurs financiers dans le monde, et ils trouvent que les firmes ont annulé ou différé leurs opportunités d'investissements en raison de la difficulté d'emprunter à l'extérieur. En distinguant l'effet de la demande et de l'offre, Chava et Purnanandam (2011) soulignent la conséquence d'un choc sur l'offre de crédit : les firmes dépendantes du financement bancaire souffrent en comparaison plus que les firmes qui ont accès au marché obligataire. Le lien entre les emprunteurs et les banques joue un rôle essentiel qui affecte aussi l'évaluation marchande de l'entreprise dans la période de crise (Carvalho, Ferreira et Matos 2014). L'accès au marché obligataire n'est pas une garantie parfaite qui permet de compenser le choc négatif (Lemmon et Roberts 2010). De plus, la stratégie de financement comme le profil de maturité peuvent aussi influencer le niveau d'impact de la crise pour les firmes (Duchin, Ozbas et Sensoy 2010 ; Almeida, Campello, Laranjeira et Weisbenner 2011).

Néanmoins, le choc peut être du côté de la demande. Pendant la crise, l'existence de la sélection adverse peut inciter les firmes de bonne qualité à quitter le marché à cause des taux d'intérêt moyens demandés par les banques. Ceci implique une baisse de la demande de crédit des firmes de bonne qualité (Mishkin 1991).

Il est possible que les firmes dépendantes des banques, confrontées à une crise de l'offre de crédit, trouvent difficilement des substituts au financement bancaire (Slovin, Sushka et Polonchek 1993). Kahle et Stulz (2013) discutent cette problématique pour la crise de 2008. On peut s'attendre à ce que les firmes dépendantes de banque, en cas de choc de l'offre, diminuent fortement leurs investissements, réduisent le niveau de leur trésorerie, ainsi que les montants d'émission de dette ou d'augmentation du capital. Pourtant, leurs études empiriques recourant à la technique DID ne mettent pas en évidence d'effets significatifs de ce genre. En outre, les résultats montrent que les firmes non-endettées ont diminué leurs investissements de manière plus marquée que les firmes dépendantes des

banques. Cette évidence n'est pas en faveur de l'argument du choc du côté de l'offre. En revanche, les auteurs suggèrent un choc de la demande en raison de la panique qui a suivi la chute du géant *Lehman*. La diminution de la consommation a provoqué une baisse de valeur des opportunités d'investissement pour les firmes. De plus, la hausse de l'incertitude a un effet négatif sur l'investissement car elle peut conduire de nombreuses firmes à différer l'engagement de leurs projets. Ceci aboutit à un besoin plus faible de capital. D'un point de vue théorique, Bloom (2009) montre à travers une simulation qu'un grand choc d'incertitude provoque une baisse considérable des taux d'embauche et d'investissements pendant les 4 mois suivant ce choc. En effet, l'incertitude augmente la valeur de l'option d'attente, et les firmes diffèrent leurs projets (voir aussi Kim et Kung 2014).

2.2.2 L'intervention gouvernementale

La panique du public à la suite de la crise peut engendrer des conséquences très sérieuses comme les ruées bancaires. Par exemple, à la fin septembre 2008, *Washington Mutual*, la 6ème grande institution financière aux Etats-Unis, fut fermée suite à des ruées bancaires massives, d'un montant de 16.7 milliard de dollars retirés par les clients pendant dix jours avant la fermeture[8]. D'après les implications du modèle d'équilibre développé par Diamond et Dybvig (1983), les clients courent retirer leur argent. Ils forcent alors les banques à liquider et solder leurs actifs. Même les banques en bonne santé peuvent en être négativement affectées. Le niveau optimal de partage de risque est rompu et le bien-être social est endommagé. Leur modèle avance des solutions comme par exemple la garantie de dépôt, ou la banque centrale agissant en dernier recours. La nécessité d'une intervention gouvernementale est illustrée par plusieurs études. Elles montrent que le plan de sauvetage peut servir à garantir la liquidité et la stabilité du système financier (Chari, Christiano et Kehoe 2008; Diamond et Rajan 2009; Bernstein, Hughson et Weidenmier 2010). Montoriol-Garriga et Wang (2011) soutiennent aussi qu'une réponse politique est nécessaire pour maintenir les prêts bancaires et protéger les petites firmes qui sont potentiellement plus touchées par la crise.

Un bel exemple de référence est le plan de sauvetage au Japon durant la crise pendant les années 90. Des études la comparent avec celle plus récente aux États-Unis. Kayana et Woo

[8] Voir le rapport "*OTS 08-046A FACT SHEET*" publié le Septembre 25, 2008 par *Office of Thrift Supervision*.

(2000) listent trois motivations pour l'analyse de la crise Japonaise : (1) les causes typiques comme la forte croissance des actifs, la libéralisation ou la dérégulation à un rythme inapproprié, la faible gouvernance et la vulnérabilité du système ; (2) le fait que cette crise japonaise ait touché un système financier considéré comme robuste et sophistiqué ; (3) le coût énorme de cette crise. Fuji et Kawai (2010) montrent que la réaction tardive du gouvernement japonais, la sous-estimation et la préparation insuffisante vis-à-vis de la sévérité de cette crise ont contribué au ralentissement du redressement économique. Nanto (2008) évalue le plan de sauvetage japonais de 495 milliards de dollars. Il en met en évidence les points négatifs, comme, une fois encore, la sous-estimation de l'influence de la crise, le retard des politiques monétaires et fiscales et le manque de solutions aux problèmes du secteur bancaire (l'absence de traitement des prêts non performants). D'après Bernanke et Gertler (1995), le durcissement de la politique monétaire est aussi un élément important.

(1) Les effets du plan Paulson

Le 3 octobre 2008, un plan initial de sauvetage au nom de « *Troubled Asset Relief Program* » (TARP) a été autorisé et signé, sous l'acte de « *Emergency Economic Stabilization of 2008* » (EESA), avec un montant de 700 milliards de dollars. Le secrétaire du Trésor, Henri Paulson, a ensuite annoncé un plan révisé qui visait à injecter 250 milliards de dollars directement vers les institutions financières.

L'étude de Veronesi et Zingales (2010), l'une des premières, montre que ce plan de sauvetage a créé une valeur de 130 milliards de dollars grâce à un investissement compris entre 21 et 44 milliards de dollars (la valeur nette créée a donc été entre 86 et 109 milliards de dollars). Les auteurs regardent non seulement le marché des actions mais aussi celui des CDS (C*redit Default Swap*) pour estimer la valeur totale des banques. Ils montrent que cette valeur créée a principalement été due à la réduction de la probabilité de défaut (mesurée *ex-ante* par l'indice de panique bancaire). Selon ces auteurs, l'intervention du gouvernement a causé en moyenne un effet négatif (interférence du plan de sauvetage sur la vie opérationnelle des banques) d'un coût de 2,5% de la valeur totale. Mais le coût de faillite s'élevant à 22%, cela prouve la nécessité du plan de sauvetage et son bénéfice évident. Bayazitova et Shivdasani (2012) analysent le plan Paulson sous un autre angle - la volonté de participation des banques, indispensable pour

le succès du plan de sauvetage (Hoshi et Kashyap 2010). Accepter d'y participer peut, pour un établissement bancaire, être interprété comme un signal négatif sur sa situation. Une injection de capitaux sous la forme d'actions prioritaires peut aussi décourager les actionnaires de participer au redressement. Par ailleurs, l'intervention du gouvernement dans l'activité des établissements bancaires peut détériorer leur efficience opérationnelle (Harris, Huerta et Ngo 2013). Ces résultats empiriques montrent que les banques les plus fortes ont été les moins motivées à participer à ce plan. D'un autre côté, le gouvernement n'a approuvé que les bons candidats mais a rejeté les mauvais (selon certains critères comme la qualité des actifs, ou la taille de la banque). Farruggio, Michalak et Uhde (2013) étendent l'analyse sur la création de valeur des injections jusqu'à juin 2010. Les auteurs calculent les CAR moyens pour les banques et trouvent des résultats positifs et significatifs autour de l'annonce initiale, des résultats négatifs et significatifs autour des injections pour les institutions financières, et des résultats positifs et significatifs autour des remboursements de capitaux.

D'un point de vue international, King (2009) étudie la réaction des marchés dans six pays (États-Unis, Grande Bretagne, France, Allemagne, Pays-Bas et Suisse) à la fin 2008. Son échantillon comprend au total 52 grandes institutions financières. L'auteur analyse le marché des CDS (pour les créanciers) et le marché des actions (pour les actionnaires). Les spreads des CDS ont diminué (réduction du risque de défaut) grâce aux plans de sauvetage, mais les marchés des actions ont réagi défavorablement dans la période suivant les annonces (malgré des réactions positives autour des annonces initiales). En résumé pour les six pays, l'auteur conclut que les créanciers ont bénéficié du plan de sauvetage au détriment des actionnaires.

(2) Les effets de bord du plan Paulson

L'impact de l'intervention gouvernementale ne s'arrête pas au secteur financier, car le rôle primordial des banques est celui de l'intermédiation qui consiste à octroyer des crédits aux entreprises qui ont besoin de fonds pour leurs projets d'investissement. En effet, une des objectifs mentionnés par le CPP est de promouvoir les prêts bancaires pour supporter l'économie en général. Les effets de bord du plan Paulson sur les pratiques d'octroi de prêts deviennent donc une problématique à bien élucider.

Gokcen (2010) propose un effet de bord positif du plan Paulson surtout sur les firmes ayant une notation inférieure à « *investment grade* »[9]. Li (2013) exploite les informations contenues dans les bilans des banques. Il trouve un effet stimulant aux offres de prêts bancaires. Ses résultats révèlent que les banques participantes au plan Paulson ont utilisé un tiers des capitaux injectés pour offrir de nouveaux prêts et le reste des fonds pour consolider leurs propres bilans. Brei, Gambacorta et Peter (2013) élargissent l'échantillon à 14 pays influents. Ils dévoilent que l'effet stimulant du plan de sauvetage n'apparaît que pour les banques dont la capitalisation est supérieure à un certain niveau. Les banques moins capitalisées ont eu plutôt tendance à redresser dans un premier temps leurs bilans. Norden, Roosenboom et Wang (2013) testent l'impact indirect du plan Paulson sur les entreprises en élaborant un ratio d'intervention qui prend en compte le lien entre les banques et les firmes. Les auteurs trouvent un effet positif du plan de sauvetage sur les firmes au niveau du marché des actions. Les firmes avec des ratios plus élevés ont montré des rentabilités de leurs actions plus fortes. Cet effet positif est plus important pour les petites firmes, les entreprises les plus risquées, et celles qui sont les plus dépendantes des banques.

Concernant le plan de sauvetage au Japon, les effets de bord sont aussi analysés en détail par Giannetti et Simonov (2013). Ces auteurs soulignent l'importance de la taille des fonds injectés par rapport à la situation de la banque. Les banques qui avaient reçu des fonds suffisants ont accordé leurs prêts aux firmes de bonne qualité. Ces dernières en ont bénéficié pour leurs projets d'investissement et ont accru leur valeur d'entreprise. Ceci a surtout été vrai pour les entreprises qui entretenaient un lien serré avec leurs banques. Cependant, les injections de fonds insuffisantes ont donné lieu à des effets de bord négatifs.

2.2.3 La relation firme-banque

(1) Les prêts bancaires

La relation entre les firmes et les banques se forme principalement à travers les prêts bancaires. De leur côté, les banques ont un accès supérieur aux informations privées de leurs clients - les firmes emprunteuses (Fama 1985). Sharpe (1990) étudie cette relation

[9] Selon la notation par S&P, BBB correspond à « *investment grade* ».

et propose qu'en raison de l'évolution au fil du temps d'une information asymétrique, la qualité des firmes est progressivement connue des banques. Ces dernières peuvent alors offrir dans tous les cas les meilleurs prêts à leurs clients. En outre, il est difficile pour les firmes de signaler leur qualité aux autres banques malgré un marché bancaire compétitif. Ces autres banques ne pouvant pas connaître la qualité des clients de leurs concurrents, ne peuvent pas attirer seulement les bons clients sans risquer de toucher les mauvais. Du côté des firmes, le modèle de Diamond (1991) montre que les nouveaux emprunteurs (souvent les petites firmes) vont établir leur réputation sous la surveillance de leurs banques. Enfin, si elles disposent d'une réputation assez élevée, elles peuvent procéder à leurs propres émissions de dette directement sur le marché obligataire. En incorporant les deux aspects - offre et demande, Holmstrom et Tirole (1997) modélisent la relation firme-banque et indiquent que les firmes de faible valeur nette (*low net worth*) tendent à recourir à leurs banques au lieu d'aller sur le marché obligataire. Les banques peuvent aussi demander moins de garantie avec une supervision plus intensive (voir aussi Berger et Udell 1995; Blackwell et Winters 1997). En résumé, ces études montrent tous les bénéfices de la relation bancaire. Pourtant, la source de ces bénéfices (et celle de coûts éventuels) reste à découvrir plus précisément (Boot 2000).

La crise financière de 2008 offre un champ expérimental de recherche empirique pour l'étude de cette relation. Selon Ivashina et Scharfstein (2010), les prêts bancaires ont connu une baisse importante durant la période de crise. Les auteurs constatent sur le marché des prêts bancaires syndiqués une diminution de 47% pour le quatrième trimestre de 2008 par rapport au trimestre précédent (79% si on le compare au deuxième trimestre de 2007, sommet du montant des prêts accordés). Bien que le montant agrégé des prêts indiqués au bilan des banques américaines soit en hausse de 100 millions de dollars entre septembre et mi-octobre 2008, cette augmentation ne résulte pas de nouveaux prêts bancaires, mais plutôt de l'accroissement de prélèvements de fonds (*drawdown*). Chodorow-Reich (2014) révèle aussi que cette dépendance aux banques affecte les firmes de façon différente. Les entreprises qui entretenaient avant la crise des relations avec les banques les moins saines, ont eu plus de difficultés que les autres à trouver leur financement. De plus, cette détérioration dans les relations bancaires explique entre 33% et 50% de la baisse des embauches au sein des petites

firmes (alors que l'impact sur les grandes firmes n'a pas été significatif, voir aussi Greenston, Mas et Nguyen 2012).

(2) Le marché des prêts syndiqués

La taille du marché des prêts syndiqués au niveau mondial s'est élevée pendant ces trois dernières années à environ 11 538 milliards de dollars (56% aux Amériques, 23% en Europe et 21% dans le reste du monde) pour 27 071 opérations selon Thomson Financial entre le 1 janvier 2011 et le 8 mai 2014. L'importance du marché des prêts syndiqués est mise en évidence dans de nombreuses recherches. Les banques leaders et les participantes à la syndication s'assemblent pour analyser l'emprunteur, surtout lorsque les informations sur ce dernier sont limitées. Ceci implique que le prêt par syndication permet de réduire l'asymétrie d'information (Sufi 2007).

Du point de vue de la recherche empirique, les prêts syndiqués présentent au moins deux avantages : (1) les informations provenant des bilans comptables des banques sont détaillées et mentionnent les prêteurs et les emprunteurs. L'identification de chaque partie devient possible et permet de mesurer l'intensité des liens entre les firmes et leurs banques ; (2) les prêteurs ne sont pas limités aux seules banques, mais figurent aussi les autres types d'institutions comme les fonds de couverture (*hedge funds*), les fonds commun de placement (*mutual funds*), et l'ensemble des autres institutions financières qui constituent ainsi un système bancaire parallèle (*shadow banking*, Ivashina et Scharfstein 2010).

2.3 Revue de la littérature sur les méthodologies

Avant d'aborder en détail chaque essai, nous proposons dans cette dernière partie de revue de la littérature, une brève présentation des deux méthodologies principalement employées dans cette thèse : les études d'évènements et l'approche *Difference-in-Differences* (DID). Cette revue synthétique se concentre essentiellement sur les motivations et les avantages escomptés.

2.3.1 Les études d'évènements

Depuis longtemps les études d'évènements composent une partie importante de la recherche empirique en finance. Eckbo (2007) y consacre le premier chapitre de son

ouvrage « *Handbook of Corporate Finance* ». Il y montre la popularité de cette approche : durant la période de 1974 à 2000, 565 articles publiés dans les 5 journaux leaders[10] ont utilisé les études d'évènements. MacKinlay (1997) revoit l'histoire des études d'évènements. Il indique que cette méthodologie est applicable non seulement dans le monde de la finance et de la comptabilité pour toutes sortes d'évènements comme les annonces des Fusions & Acquisitions (F&A) ou celles des résultats par les entreprises, mais aussi dans le milieu du droit, comme par exemple pour l'annonce d'un changement législatif. De plus, même si la plupart des calculs sont réalisés à partir de la rentabilité des actions, les études d'évènements peuvent aussi être appliquées aux autres produits financiers. D'après cet auteur, on peut trouver la première étude d'évènements dans le papier de Dolley (1933) intitulé « *Characteristics and Procedures of Common Stock Split-Ups* » qui analyse l'effet du fractionnement d'actions sur les cours boursiers.

Fama, Fisher, Jensen et Roll (1969) recourent à cette méthodologie et fournissent sa première application moderne. Dans leur papier, les auteurs analysent la réaction du marché dans les mois qui entourent les annonces de fractionnement d'actions. Ils mesurent les rentabilités anormales (les résidus estimés par les régressions) par rapport aux rentabilités du marché. Les résultats mettent en évidence des rentabilités anormales positives. Cependant, au cours des mois qui suivent les annonces, les firmes qui diminuent leurs dividendes sous performent en moyenne les firmes qui les augmentent. Les auteurs avancent l'hypothèse que dans la plupart des cas, le marché anticipe la hausse de dividende lors de la division des actions. Le marché répond donc défavorablement au cas où la hausse du dividende ne suit pas. Le fractionnement d'action à lui seul ne crée pas de valeur, sauf s'il est porteur d'informations complémentaires. Ce résultat implique que le marché est efficient, vu que le cours boursier réagit rapidement aux nouvelles informations.

En résumé, les études d'évènements calculent les rentabilités anormales autour d'annonces particulières. Cela peut être un même évènement affectant l'ensemble de la population ou des évènements multiples de même type occurrents à différentes dates et propres à chaque firme. La rentabilité anormale est égale à la différence entre une rentabilité normale et une rentabilité espérée estimée selon un certain modèle, par

[10] *Journal of Business ; Journal of Finance ; Journal of Financial Economics ; Journal of Financial and Quantitative Analysis ; Review of Financial Studies.*

exemple, le *Capital Asset Pricing Model* (CAPM, Sharpe 1964; Lintner 1965) ou l' *Arbitrage Pricing Theory* (APT, Ross 1976), voire encore un modèle simplifié comme celui qui prend zéro comme constante et 1 comme pente du modèle de marché (Brown et Warner 1980, 1985).

Les études d'évènements peuvent aussi être utilisées pour analyser l'influence sur une longue période. Une méthodologie souvent utilisée par les chercheurs est l'approche dite Calendar Time Abnormal Returns (CTAR) proposée par Jaffe (1974) et Mandelker (1974). On calcule alors la constante, aussi appelée Jensen's alpha, d'un modèle de régression à facteurs multiples (le modèle de Fama-French à 3 facteurs est souvent utilisé, Fama et French 1993; et dans certains cas un facteur de *momentum* est ajouté, Carhart 1997).

Dans cette thèse, l'application des études d'évènement trouve sa place pour notre analyse autour des annonces des F&A transfrontalières et pour répondre à notre questionnement sur l'effet du plan Paulson lors de son annonce initiale et lors des injections auprès de chaque banque participante.

2.3.2 La méthode *Difference-in-Differences* (DID)

Roberts et Whited (2011) présentent cette approche en détail. Elle repose sur une double comparaison : la comparaison de l'état de firmes ayant subi un évènement avant et après sa réalisation, et la comparaison de l'état d'autres firmes qui n'ont pas connu cet évènement, sur la même période. La méthode DID exploite ces deux comparaisons en en faisant apparaître les différences. Les chercheurs définissent un groupe traité (les firmes qui ont connu l'évènement) et un groupe de contrôle (les firmes qui ne l'ont pas connu). Ils analysent la différence dans le temps pour les deux groupes et calculent finalement la différence entre ces deux différences. Le résultat peut aussi être obtenu à l'aide d'une régression incorporant l'interaction entre une variable nominale de traitement et une variable nominale identifiant la période postérieure à l'évènement.

Cette méthode dépend de l'hypothèse qu'en l'absence de cet évènement, les firmes du groupe traité et celles du groupe de contrôle se seraient comportées de façon similaire. Roberts et Whited (2011) soulignent ce problème d'endogénéité et proposent des tests de robustesse, y compris un test de falsification qui essaie de neutraliser la différence en simulant une date différente de l'évènement. Par exemple, Almeida, Campello, Laranjeira

et Weisbenner (2011) indiquent qu'une simulation fondée sur le choix falsifié d'une année de crise comme 2006 ou 2005, ne génère pas de résultats significatifs pour le coefficient de DID. Mais la vraie année de crise, soit 2007, produit une DID significative entre les deux groupes. Cela renforce leur argument que c'est bien la crise de 2007 qui apporte cette différence au groupe traité. Il y a par ailleurs d'autres types de test avec plusieurs groupes traités ou groupes de contrôle. Ce type de test est plus robuste que la comparaison avec un seul groupe traité ou un seul groupe de contrôle.

En somme, la méthode DID est certainement une très belle approche pour capter l'effet créé par un évènement car elle permet de neutraliser les variables inobservables en rajoutant des effets fixes au niveau des firmes. Nous utilisons principalement cette méthode dans notre premier essai de recherche sur la couverture opérationnelle du risque de change.

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CHAPITRE 2: THE VALUE EFFECT OF OPERATIONAL HEDGING: EVIDENCE FROM FOREIGN TAKEOVERS[11]

[11] This paper is published in « Finance » 34 (3). Co-authors : Nihat Aktas and Jean-Gabriel Cousin

ABSTRACT

The paper examines cross-border takeovers using the lens of currency risk management. With a sample of 152 large, cross-border deals undertaken by listed French firms, the findings reveal that acquirers tend to be firms with greater exposure to the target currency prior to the takeover announcement. The value of the acquiring firm becomes less sensitive to the target currency after the transaction. Acquirer abnormal returns also are positively associated with a decrease in exposure to the target currency; this gain is economically substantial. For example, for an acquirer worth €100 million in equity, a one-unit decrease in currency exposure leads to a gain of €1.68 million.

JEL classification: G34

Keywords: Cross-border takeovers; Currency risk exposure; Abnormal returns

1. Introduction

With the globalization of business in recent decades, almost all domestic firms have been exposed to some currency risk due to fluctuations of exchange rates, whether because they sell outputs abroad (i.e., exporters) or buy inputs from abroad (i.e., importers) or even just compete with foreign firms in their domestic market. In response, firms often use derivatives to hedge currency risk (Stulz, 2004; Bartram, Brown, and Fehle, 2009) but the risk exposures that can be hedged by the use of derivatives tend to be relatively modest compared with firm size (Guay and Kothari, 2003). Hedging with derivatives is not always possible, especially against long-term exposures, and even if it is possible, it can be expensive due to rollover concerns (Froot, Scharfstein, and Stein, 1993; Garfinkel and Hankins, 2011). Beyond the use of derivatives, risk management programs thus tend to include other hedges, such as financial and operating activities (e.g., Pantzalis, Simkins, and Laux, 2001; Kim, Mathur, and Nam, 2006; Garfinkel and Hankins, 2011).

This article focuses on operational hedging, which aims to mitigate corporate risks by increasing operational flexibility and geographical diversification. Operational hedging implies the flexibility to transfer production from one geographical location to another after a change in demand or in response to exchange rate uncertainties. Compared with financial tactics, operational hedging requires more capital expenditures, which makes it better suited for managing long-run exposure (e.g., Chowdhry and Howe, 1999; Pantzalis, Simkins, and Laux, 2001).

A simple way to implement operational hedging involves cross-border mergers and acquisitions (M&A), because it is possible to increase currency-denominated sales or costs by buying a foreign company. If currency risk management is one of the drivers of the decision to implement a cross-border takeover, the value of the acquirer should be less sensitive to the fluctuations of a target currency after its cross-border deal. We test this conjecture with a sample of large companies involved in cross-border M&A transactions. Specifically, our empirical analyses are based on a sample of 152 cross-border M&A deals undertaken by French firms included in the SBF 250 index[12] during 1999–2010. We focus on these SBF 250 firms, the largest listed French firms, because of

[12] The SBF 250 index includes 250 leading French companies listed on Euronext Paris.

their ongoing international activities. Following Adler and Dumas (1984), we use a modified market model to measure the acquirer's currency exposure, such that we regress the acquirer's stock return on the return of the corresponding exchange rate (i.e., exchange rate relative to the target firm's currency), while also controlling for the overall direction of the stock market (see also Pantzalis, Simkins, and Laux 2001; Bartram, Burns, and Helwege, 2010).

We define the exchange rate as the price of the domestic currency in a foreign currency (i.e., the amount of foreign currency necessary to buy one euro). In our sample, 15.1% of the firms experienced significant exposure to the target firm's currency prior to the takeover announcement. Using the same approach as Pantzalis, Simkins, and Laux (2001), we split the sample into net exporters and net importers, according to the sign of the firms' exposure coefficient prior to the takeover announcement. That is, net exporters are negatively exposed to the target currency prior to the deal, whereas net importers enjoy positive exposures.

The univariate analysis in turn indicates that the effect of a cross-border deal on currency risk exposure is significantly positive for net exporters (i.e., exposure decreases in absolute value following the takeover announcement) and significantly negative for the net importer subsample. Consistent with findings from previous studies in the United States (e.g., Akhigbe, Martin, and Newman, 2003; Bartram, Burns, and Helweg, 2010), our results suggest that cross-border deals offer natural hedge, on average, for acquirers.

To control for omitted trends in the evolution of the exposure coefficient and unobserved differences between firms that implement cross-border deals and those that do not, we also rely on a difference-in-differences approach to assess the robustness of our results (see Roberts and Whited, 2011). The treatment is the announcement of a cross-border deal. As a control group, we use firms from the SBF 250 index that did not participate in a cross-border transaction in the target country during a two-year period centered on the announcement date of the considered cross-border deal. We can confirm the results in this difference-in-differences setting. Moreover, the difference-in-differences analysis reveals that, relative to the control group, the treated firms in our sample have significantly higher exposure coefficients in absolute value prior to the deal announcement. This result provides additional support for our initial conjecture, namely,

that operational hedging of currency risk is an important determinant of cross-border M&A decisions.

Our empirical approach further enables us to determine whether operational hedging creates value for shareholders. Some evidence in the literature indicates that financial hedging creates value; for example, Allayannis and Weston (2001) and Carter, Rogers, and Simkins (2006) document that it leads to a 5–10% increase in firm value.[13] However, to the best of our knowledge, the value effects of operational hedging strategies have not been sufficiently explored, though Kim, Mathur, and Nam (2006) investigate a sample of 424 U.S. firms and find that operational hedging increases firm value (as proxied by Tobin's q ratio) by 4.8–17.9%.

In M&A research, a more common approach to measure the value creation associated with a corporate decision relies on abnormal returns around the announcement, as derived from an event study analysis (see Betton, Eckbo, and Thorburn, 2008). Our results confirm that operational hedging creates value for shareholders in the period surrounding M&A announcements. Specifically, acquirer abnormal returns relate positively to a decrease in (absolute) exposure to the target currency. The gain is economically substantial: a decrease in the acquirer's exposure to the target currency by one unit leads to an abnormal gain of 1.68% for acquirer shareholders. Thus an acquirer worth €100 million in equity would gain €1.68 million. However, the positive effect of operational hedging on corporate value in our findings is driven mainly by the cross-border deals undertaken by net exporters. This result is consistent with Chowdhry and Howe's (1999) argument that it is optimal for a multinational corporation to engage in operational hedging when both exchange rate uncertainty and demand uncertainty are high. Net exporters are severely affected by both types of uncertainty, compared with net importers, which are more influenced by exchange rate uncertainty only.[14]

Our research thus complements previous literature that has focused mostly on U.S. multinational firms. Pantzalis, Simkins, and Laux (2001) recognize that greater

[13] Campello et al. (2011) identify two channels through which financial hedging might affect corporate value: hedgers pay low interest spreads and are less likely to have investment restrictions in their loan contracts.

[14] This claim holds if we assume that domestic demand is perceived as less risky than foreign demand.

international network breadth can attenuate exposure to currency risk, whereas a concentration of networks in a few countries increases this exposure. Choi and Jiang (2009) also report that multinational corporations are less exposed to exchange rate risk than are non-multinational firms, so operational hedging could reduce exposure to currency risk. Moreover, Kim, Mathur, and Nam (2006) document that financial and operational hedging activities tend to be complements that both enhance firm value. In contrast to these cross-sectional comparisons of multinational and non-multinational firms, we adopt both cross-sectional and time-series comparisons to analyze decisions to undertake a cross-border deal, using the lens of currency risk management. Our results indicate that the internationalization process, achieved through the implementation of cross-border deals, represents a response to some extent to high currency exposure.

Bartram, Burns, and Helwege (2010) also analyze the evolution of exposure coefficients to the target currency for a sample of U.S. cross-border acquirers. We extend their contributions by focusing on a sample of French acquirers; we also propose a difference-in-differences framework that can better reveal whether a cross-border transaction is a response to high currency exposure. Moreover, we complement their analysis by assessing the value effects associated with operational hedging activities.

Our research also contributes to M&A literature in a broader sense. Substantial corporate finance literature investigates the determinants of an acquirer's abnormal returns around M&A decision announcements and identifies as important the payment means, the target's status, the relative size of the deal, and uncertainty about the target's valuation (see Betton, Eckbo, and Thorburn, 2008). We add a novel determinant to this list in the context of cross-border transactions, namely, a willingness to hedge currency risk operationally. In a related effort, Garfinkel and Hankins (2011) show that merger waves are driven by risk management considerations. In particular, an increase in cash flow uncertainty encourages U.S. firms to integrate vertically, which in turn suggests that vertical M&As represent operational hedging mechanisms that reduce the cost of increased uncertainty. We focus on cross-border takeovers and assess the effect of such transactions on currency risk exposure. Unlike Garfinkel and Hankins (2011), we also assess the value effect of operational hedging mechanisms and show that operational hedging ultimately is value creating for shareholders.

We organize the remainder of this article as follows: in Section 2, we describe the sample of M&As studied and explain our measure of currency exposure, as well as how we estimate the associated value effect. Section 3 is devoted to our empirical analysis, before we conclude in Section 4.

2. Data and methods

2.1. Sample construction

We start by identifying the firms in the SBF 250 index for the period from January 1999 to December 2010. The starting point corresponds to the introduction of the euro, which helps us avoid cross-border deals within the Eurozone. We then track cross-border M&A activities by these firms using the Thomson Securities Data Company (SDC) Mergers and Acquisitions database. We impose three sample selection criteria on our M&A sample: first, the acquirer must be a listed French company that completed a cross-border M&A deal with a deal size greater than 1 million EUR. This transaction must have been announced between 1999 and 2010, and the percentage held by the acquirer must be less than 50% before the deal and 100% after the deal. With these filters, we obtain an initial sample of 446 deals.

Second, the acquirer must appear in the SBF 250 index with available stock prices in the Thomson Reuters Datastream database during 1999–2010, and the target must fall outside the Eurozone. This filter reduces the sample size to 250 deals.

Third, to ensure significant transactions and following Masulis, Wang, and Xie (2007), we impose the requirement that the size of the deal, relative to the size of the acquirer, must be greater than 1%. This final filter reduces the sample to 152 deals undertaken by 74 different companies. Although this final sample might seem small, it is consistent with the sizes used in previous studies of currency risk. For example, Akhigbe, Martin, and Newman (2003) investigate 156 foreign acquisitions by U.S. firms; Bartram, Burns, and Helwege (2010) consider 102 cross-border deals undertaken by U.S. acquirers; and Pantzalis, Simkins, and Laux (2001) include 220 U.S. multinational firms.

Panel A of Table 1 provides the distribution of our sample by announcement year. It exhibits a first peak in the number of transactions between 1999 and 2000, consistent with the well-documented “friendly” M&A wave of the end of the 1990s (Betton, Eckbo, and Thorburn, 2008), and then a second peak in 2006. The differences between the yearly average and median deal sizes highlight the presence of some large transactions in the sample. The average deal size tends to be higher in peak years, consistent with evidence reported by Netter, Stegemoller, and Wintoki (2011). Panel B of Table 1 provides the

sample distribution by target currency (or target nationality). Of the 152 deals, 53.29% feature U.S. target firms, and 13.82% are U.K. firms.

[Insert Table 1 About Here]

2.2. Measuring currency exposures

Adler and Dumas (1984) suggest estimating currency exposures using a time-series regression that relates the firm's stock returns to corresponding exchange rate returns, while controlling for the return of the market portfolio (e.g., Bartram, Burns, and Helwege, 2010; Choi and Jiang, 2009; Pantzalis, Simkins, and Laux, 2001). For each acquirer in the sample, we estimate the following regression equation:

$$R_{i,t} = \alpha_i + \beta_{1,i} R_{M,t} + \beta_{2,i} R_{FX,t} + \varepsilon_{i,t}, \quad (1)$$

where $R_{i,t}$ is the return for acquirer *I*; $R_{M,t}$ is the return of the French stock market index; $R_{FX,t}$ is the return on the foreign exchange rate (*FX*), $\varepsilon_{i,t}$ is the regression residual, and t denotes the time subscript. For our empirical setting, the returns are computed weekly, and the exchange rate *FX* corresponds to the price of the domestic currency (i.e., currency of the acquirer's country) in foreign currency (i.e., currency of the target firm's country).[15] For each firm *I*, the estimated coefficient β_2 is a measure of its exposure, relative to the corresponding target currency.

A firm's exposure to currency risk results from unexpected exchange rate variations, which affect its cost structure (e.g., raw materials, labor costs) and output prices. The firm is not exposed to currency risk (β_2=0) if input costs and output prices can be determined locally, without any effect of foreign competition, or if the firm already has hedged, financially or operationally, against both expected and unexpected changes in exchange rates.

A negative (positive) value for $R_{FX,t}$ indicates a depreciation (appreciation) of the domestic currency between time t – 1 and time t, relative to the foreign currency. For example, if the exchange rate between EUR (domestic currency) and USD (foreign currency) moves from EUR/USD = 1.446 at time t – 1 to 1.430 at time t, it translates into

[15] For our study, the exchange rate is the amount of foreign currency necessary to buy one euro).

a negative $R_{FX,t}$ of –1.11% (= 1.430/1.446 – 1) for the period and indicates a depreciation of the euro relative to the U.S. dollar. This scenario would benefit French exporters. A negative (positive) change in FX then suggests that domestic exporters find it easier (more difficult) to sell their goods in the United States, in that USD-denominated sales have more (less) value on a euro basis, whereas domestic importers will find USD-denominated foreign goods more (less) expensive. After accounting for foreign operations (sales and input purchases), industry competition, and hedging activities, we thus can assert that a negative (positive) value for β_2 indicates that the firm is a net exporter (net importer).

To estimate Equation 2, we rely on weekly stock prices and exchange rates downloaded from the Thomson Reuters Datastream database. For exposure with respect to the target currency, we use the one-year window prior to the takeover announcement. The estimation window starts at week –52 and ends at week –1, relative to the deal announcement week.

Panel A of Table 2 reports summary statistics regarding the exposure of French acquirers to the currency of the target firm's country, prior to the takeover announcement. For the full sample, the average acquirer in the sample is negatively exposed to the target currency, with a coefficient value of –0.11 and a corresponding *p*-value of 0.03. The range of exposures is quite substantial, with minimum and maximum exposure coefficients of –1.69 and 1.72, respectively. Regarding the statistical significance of the exposure coefficients, we find that the stock returns of 7.9% (15.1%) of the sample acquirers are significantly exposed to the target currency at the 5% (10%) level, across the full sample. The cross-sectional distribution in column 1 of Panel A (Table 2) suggests that some firms are positively exposed, while others are negatively affected by exchange rate movements. The last two columns in Panel A contain summary statistics related to the exposure coefficients, separately for firms with positive and negative exposures. Of 152 firms, 57 experience positive (37.5%) and 95 have negative (62.5%) exposures. Half the net importers exhibit an exposure in the range of [0.43; 1.72], whereas half of the net exporters fall in the range of [–1.69; –0.38].

[Insert Table 2 About Here]

2.3. Abnormal stock performance

To assess the value creation effects of operational hedging strategies, we rely on short-term abnormal returns. Since Fama et al. (1969) published their study, the accepted method for isolating the impact of a particular event on market valuations is an event study. Therefore, we begin by constructing a model of normal returns, that is, the individual firm returns that would have occurred without the event. We use a classical market model to estimate these normal returns:

$$R_{i,t} = \alpha_i + \beta_i R_{M,t} + \varepsilon_{i,t}, \tag{2}$$

where $R_{i,t}$ is the observed return for firm I on day t; $R_{M,t}$ is the return of a concurrent local country stock market index on day t; α_i and β_i are the estimated ordinary least squares regression intercept and slope, respectively; and $\varepsilon_{i,t}$ is the regression residual.[16] We estimate the market model parameters for the period from event day –250 to event day –10, where event day 0 is the announcement date.

The abnormal return (AR) for day t corresponds to the difference between the observed return on day t and the return estimated using the market model:

$$AR_{i,t} = R_{i,t} - (\alpha_i + \beta_i R_{M,t}). \tag{3}$$

To compute the cumulative abnormal return (CAR), we determine for each firm the AR across the three days around the announcement day (from day –1 to day +1, where day 0 is the announcement day).

In Panel B of Table 2, we report the summary statistics for the acquirer CARs. Of the 152 deals, we are able to compute the acquirer announcement CAR only for 105 firms.[17] The average acquirer CAR is positive and statistically significant with a value of 1.18% (p-value = 0.02), which implies that the cross-border acquisitions in our sample are value-creating decisions for acquirer shareholders, on average. This result is largely consistent

[16] Brown and Warner (1985) confirm the robustness of the short-term event study method to the choice of the return-generating process (see also Kothari and Warner, 2007).

[17] For the value effect analysis, firms with more than 100 missing observations or 50 reported stock prices in the estimation window are excluded from the sample. We further exclude firms with at least one missing observation or one reported stock price in the 3-day event window.

with recent literature that shows that acquirers' CARs around the announcements of cross-border acquisitions are slightly positive. For example, Faccio, McConnell, and Stolin (2006) report an average acquirer CAR of 1.13% in an international sample of cross-border deals; Aktas, Cousin, and de Bodt (2011) document an average acquirer CAR of 2%.[18] Over the period we consider, the cross-border acquisitions of companies negatively exposed to the target currency are more value creating (average acquirer CAR = 1.89%) than those positively exposed (average acquirer CAR = –0.12%).

However, the results in Panel B of Table 2 do not allow us to ascertain whether the value-creation effect observed for net exporters results from an operational hedging motive. We return to this issue in Section 3, within the framework of our multivariate regression approach.

2.4. Empirical methods

2.4.1. Difference-in-differences estimator

To assess the effect of a cross-border acquisition on an acquirer's exposure to the target's currency, we adopt a difference-in-differences (DD) approach (Roberts and Whited, 2011) that combines two single-difference estimators: cross-sectional and time-series differences. The cross-sectional difference controls for omitted trends by comparing treated groups with a control group over the same period. The time-series comparison instead controls for unobserved differences between two groups by looking at the same firms before and after a particular treatment. In our framework, the treatment is the announcement of a cross-border deal in a given target country. Therefore, we obtain the DD estimator with the following regression model:

$$\beta_{2,i,TP} = c_i + c_y + \alpha_1 TP_i + \alpha_2 TG_i + \alpha_3 (TG_i \times TP_i) + \varepsilon_i, \qquad (4)$$

where β_2 is the outcome variable (i.e., exposure coefficient to the target currency, both treated and control firms), I is the firm index, c_i and c_y are firm- and year-fixed effects,[19]

[18] Recent large-sample U.S. studies document an average acquirer CAR of around 1% too (e.g., Betton, Eckbo, and Thorburn, 2008; Netter, Stegemoller, and Wintoki, 2011).

[19] Year-fixed effects are used to control for general business and economic conditions, and they are coded with respect to the deal announcement year.

and *TP* and *TG* are dummy variables that identify the post-treatment period (with respect to the pre-treatment period) and the treated group (with respect to the control group), respectively. The post-treatment period starts at week +1, following the observation of the treatment at week 0 (i.e., the announcement of a cross-border deal), and it ends at week +52 relative to the deal announcement week. The pre-treatment period corresponds to the weeks [–52, –1] prior to the announcement week. The subscript *TP* on the left-hand side denotes the time period over which we compute the outcome variable, β_2. We consider two outcomes per firm in the sample: one computed over the pre-treatment period (*TP* = 0), and another measured over the post-treatment period (*TP* = 1). Firms that have undertaken a cross-border acquisition are "treated" firms.

We use two alternative approaches to identify the control group (i.e., firms that are not treated). For a given cross-border deal, the first approach considers all SBF 250 firms that have not announced a deal in the target country during a two-year period centered on the announcement date of the considered cross-border deal.[20] In the second approach, for a given cross-border deal, the control group includes a single firm from the acquirer industry and randomly drawn from the initial list of SBF 250 firms. To identify industries, we use the 2-digit industry classification available in Thomson Reuters Datastream.

The net effect of the cross-border deal on the target's currency exposure can be measured by α_3, which corresponds to the difference-in-differences estimator.

Figure 1 graphically represents the DD estimator for acquirers that are net importers (i.e., firms positively exposed to the target currency prior to the takeover announcement).[21]

[20] We do not allow overlapping observation. For a given year and target currency, a firm appears only once in the control group.

[21] For net exporters, the graphical representation is the same, but the exposure coefficient should be in absolute values on the y-axis.

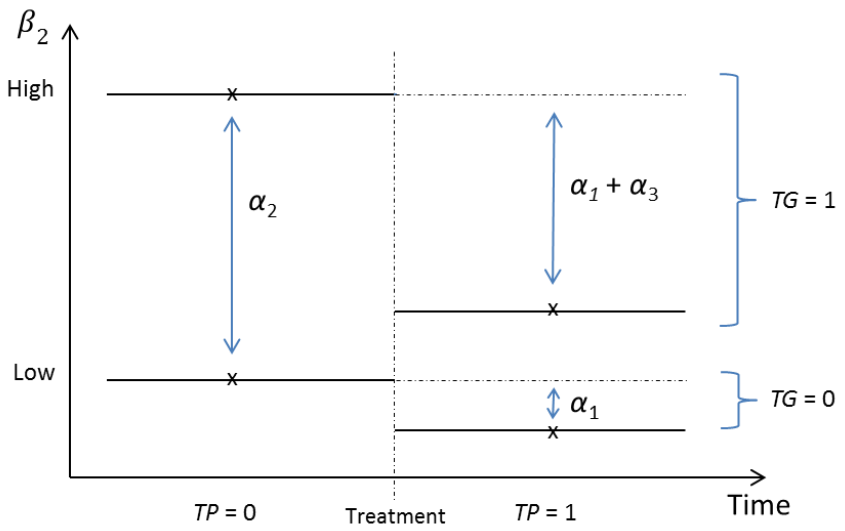

Figure 1. Graphical representation of the difference-in-differences estimator for acquirers that are net importers (i.e., firms positively exposed to the target currency prior to the takeover announcement). The treatment is the announcement of a cross-border takeover. *TG* is a dummy variable that identifies the treated group (1 = treated firms, 0 = control group). *TP* is a dummy variable that identifies the post-treatment period (1 = post-treatment period, 0 = pre-treatment period). The x- and y-axes represent the time period and the exposure coefficient to the target currency (β_2), respectively. The coefficients α_1, α_2 and α_3 are from Equation (4). The difference-in-differences estimator is given by α_3.

If operational hedging is a motive for cross-border takeovers, our sample acquirers (high β_2) should be significantly more exposed to the target currency in the pre-treatment period than the control group is (low β_2). This intuition is captured by the coefficient α_2 in Figure 1. The treatment then should affect only the treated group, though the exposure coefficient of the sample firms (both treated and control) could change over time for other, unobserved reasons (e.g., a macroeconomic shock). The coefficient α_1 accounts for any change that is not due to the treatment. With Figure 1, we depict a situation in which the change is negative, but it also could be null or positive. To isolate the part of the change that is due to the treatment, the DD method compares the exposure coefficient of the treated group with its level prior to the treatment, as well as the change observed for the control group. The net effect of the treatment is therefore given by the coefficient α_3.

2.4.2. p-Values

The *p*-values in the multivariate regression that analyzes the determinant of the acquirer CAR are derived from a percentile *t* bootstrap procedure (see Horowitz, 2002). Specifically, we bootstrap the student statistics of each coefficient using the following procedure: From the original data matrix, we draw, with replacements, 1,000 bootstrap samples with the same number of observations as in the original sample. For each bootstrap sample, we estimate the regression coefficients and compute corresponding adjusted standard errors. This step provides bootstrap *t*-statistics. Finally, we tabulate the empirical distribution of the bootstrap *t*-statistics for each coefficient and use it to test the significance of the regression coefficients. We use case-by-case resampling, which is robust to heteroskedasticity.

3. Results

3.1. Impacts of cross-border deals on currency exposure

In Table 3 we present the univariate analysis to compare the average exposure coefficient of acquirers to the corresponding target currency following a transaction against the average exposure coefficient during the pre-treatment period. Panel A of Table 3 reports on all cases, while Panel B focuses on the subsample of firms significantly exposed to the target currency prior to the deal (at the 10% level). In Panel A, exposure to the target currency decreases on average for net importers, from 0.498 before the treatment to 0.262 after the treatment, but it increases on average for net exporters, from –0.473 before to 0.064 after the treatment. The impact of cross-border deals on currency exposure thus is statistically significant for both net importers and net exporters. Panel B of Table 3 shows that the hedging effect is even more pronounced for the subsample of firms significantly exposed to the target currency prior to the deal. In Panel B, the average exposure coefficient goes from 0.848 before the deal to –0.052 after the deal for net importers, and from –0.851 before the deal to 0.006 after the deal for net exporters. It is also worthwhile to mention that both for net importers and net exporters, the average exposure coefficient after the deal is not statistically different from 0 (unreported result). This result suggests that the cross-border deals in our sample offer, on average, an almost perfect hedging mechanism against target currency movements.

[Insert Table 3 About Here]

We confirm these univariate results in Table 4 using a DD framework. In Panel A of Table 4, we consider all sample firms, whether their exposure to target currency is significant or not before the deal. The coefficient of the interaction variable $TP \times TG$ (i.e., the difference-in-differences estimator), is significantly negative with a value of –0.295 for net importers (p-value = 0.07) but is significantly positive for net exporters with a value of 0.523 (p-value = 0.00). Thus, for net importers, the treatment (i.e., announcement of a cross-border transaction in the target country) negatively affects their exposure coefficient relative to both the pre-treatment period and the control group. The impact of the treatment is positive for net exporters though. Panel B of Table 4 considers the subsample of firms with significant exposure before the deal, and shows that the hedging effect is even stronger for these firms. Moreover, Panel C of Table 4 indicates that our results seem not to be sensitive to the way the control group is identified.

[Insert Table 4 About Here]

The results in Tables 3 and 4 are consistent with the idea that the decision to undertake a cross-border transaction might reflect an operational hedging motive, because the exposure coefficient to the target currency decreases in absolute value following the announcement of the deal. In addition, our DD analysis indicates that prior to the deal announcement the treated firms in our sample have significantly higher exposure coefficients relative to the control group. In Table 4, the coefficient of the variable identifying the treated group (TG) is always significantly positive for acquirers that are net importers (the coefficient value ranges between 0.451 (in Panel A) and 0.927 (in Panel C)) and always significantly negative for those that are net exporters (the coefficient value ranges between –0.806 (in Panel B) and –0.446 (in Panel A)). Therefore, the firms involved in cross-border transactions are the ones that are highly exposed (in absolute value) to the target currency prior to the deal, in additional support of our initial conjecture that the operational hedging of currency risk plays an important role in the decision to undertake cross-border M&A deals.

3.2 Value effects

This sub-section tests whether operational hedging strategies, through the implementation of cross-border deals, are value creating for shareholders (the value creation hypothesis). In Table 5, we present multivariate regression analyses of acquirers' announcement abnormal returns. The sample in each column includes both net importers and net exporters. Each specification also includes several control variables: *deal size*, *relative size*, *stock*, *related*, *U.S. target*, and *private target*. Their definitions appear in the notes to Table 5.

[Insert Table 5 About Here]

In column (1), the variables of interest are $|\beta_2|$ *pre-treatment* (i.e., the absolute value of the exposure coefficient estimated over the pre-treatment period) and $|\beta_2|$ *post-treatment* (i.e., the absolute value of the exposure coefficient estimated over the post-treatment period). Our previous results in Tables 3 and 4 indicate that the acquirer's exposure to the target currency decreases on average (in absolute value) following cross-border deals, suggesting that these deals offer a natural hedge, on average, for acquirers. Consequently, we expect, under the value creation hypothesis, that the acquirer announcement abnormal returns (CAR) to be greater for firms that are highly exposed (in absolute value) prior to the deal announcement and lower for firms that are highly exposed (in absolute value) after the deal. The results in column (1) of Table 5 are consistent with our intuition. The coefficient of $|\beta_2|$ *pre-treatment* is positive and statistically significant, suggesting that the higher the potential gain from hedging, the higher is the announcement CAR. The coefficient of $|\beta_2|$ *post-treatment* is negative and statistically significant. This suggests that the lower the currency exposure after the cross-border deal, the higher is the announcement CAR. These results are consistent with our value creation hypothesis.

To better assess the value effect associated with operational hedging, we use the variable *Delta* $|\beta_2|$ in column (2) of Table 5. The variable *Delta* $|\beta_2|$ corresponds to the acquirer's post-treatment exposure coefficient to the target currency in absolute value minus the acquirer's pre-treatment exposure coefficient to the target currency in absolute value. Hedging currency risk consists in making the return of the firm (or its cash flows) insensitive to target currency movements. A positive (negative) *Delta* $|\beta_2|$ indicates that

the exposure of the acquirer has increased (decreased) in absolute value after the cross border deal. Therefore, the more negative is *Delta* $|\beta_2|$, the higher is the hedging effect. The coefficient of *Delta* $|\beta_2|$ is negative and statistically significant, so acquirer abnormal returns are positively associated with a decrease in exposure to the target currency. The gain also appears economically substantial. For example, a decrease in the acquirer exposure to the target currency by one unit leads to an abnormal gain of 1.68% for the acquirer's shareholders. For an acquirer worth €100 million in equity, this finding indicates a gain of €1.68 million.

In column (3) of Table 5, we assess whether the value effect of operational hedging is the same for importers and exporters. The dummy variables identifying net exporters and net importers are interacted with a dummy variable identifying whether the exposure in absolute value has decreased following the cross border deal or not. If *Delta* $|\beta_2|$ is negative the variable "*Dummy decrease"* takes the value of 1, and 0 otherwise. In this new specification, among the variables of interest, only "*Exporters × Dummy decrease"* is statically significant, with a *p*-value of 0.05. The associated coefficient is positive, so the announcement CARs of next exporters is higher when their exposure to target currency movements decrease in absolute value. The evidence in column 3 of Table 5 thus indicates that the positive effect of operational hedging on corporate value is driven mainly by cross-border deals of net exporters. This result is consistent to some extent with the theoretical argument provided by Chowdhry and Howe (1999). According to these authors, it is optimal for a multinational corporation to engage in operational hedging when both exchange rate uncertainty and demand uncertainty are high. Net exporters are severely affected by both uncertainties, whereas net importers are mainly affected by exchange rate uncertainty.

With regard to the control variables, three retain their sign and (almost) their significance across the three specifications in Table 5. Consistent with prior literature, relative size is associated with higher acquirer abnormal returns (Moeller, Schlingemann, and Stulz, 2004), and an acquisition fully paid for in stock generates lower abnormal returns for the acquirer's shareholders (e.g., Mitchell, Pulvino, and Stafford, 2004; Travlos, 1987). Finally, private target acquisitions in our sample affect acquirer abnormal returns negatively, in contrast with the results reported by Fuller, Netter, and Stegemoller (2002) for U.S. domestic deals. The acquisition of private foreign targets might seem more risky to

acquirer shareholders, compared with listed targets for which more information is available.

4. Conclusion

As a complement to financial hedges, firms use operational hedging mechanisms too (Kim, Mathur, and Nan, 2006). This article focuses on cross-border takeovers and analyzes takeover decisions through a lens of currency risk management. The sample of large, listed, French firms reveals that acquirers are firms with higher exposure to target currencies, and their market values become less sensitive to fluctuations of the target currency after the takeover. These results suggest that operational hedging of currency risk exposure might drive the decision to undertake cross-border deals.

With our classical event study results, we provide evidence that operational hedging is value creating for shareholders. Our findings specifically reveal that acquirer gains are positively associated with changes in currency risk exposure. The value effect also is stronger for firms that are net exporters.

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Table 1. Sample distribution

Panel A reports the sample distribution by announcement year. Panel B provides the sample distribution by the currency of the target company. *N* and % denote, respectively, the number of acquisitions and the percentage of the sample in each year or in each target currency. The mean and the median deal sizes are in million EUR. The deal size is defined by SDC as the total value of the consideration paid by the acquirer, excluding fees and expenses.

Panel A. Sample distribution by announcement year

Year	*N*	Proportion (%)	Deal size	
			Mean	Median
1999	22	14.47%	1,266.63	393.76
2000	19	12.50%	6,251.35	1,064.65
2001	13	8.55%	2,032.60	328.14
2002	7	4.61%	650.81	105.02
2003	7	4.61%	127.71	51.50
2004	14	9.21%	186.01	181.34
2005	13	8.55%	620.49	131.62
2006	18	11.84%	1,883.72	346.11
2007	14	9.21%	1,081.43	364.40
2008	14	9.21%	587.64	143.70
2009	11	7.24%	557.13	276.09
Total	152	100.00%	1,661.76	252.78

Panel B. Sample distribution by target currency

Target currency	*N*	Proportion (%)	Mean deal size
Austrian Dollar	2	1.32%	270.04
Brazilian Real	4	2.63%	354.88
British Pound	21	13.82%	3,414.84
Canadian Dollar	6	3.95%	8,336.57
Danish Krone	2	1.32%	607.35
Hungarian Forint	2	1.32%	355.00
Indian Rupee	5	3.29%	90.18
Korean Won	4	2.63%	233.07
Norwegian Krone	3	1.97%	183.61
Polish Zloty	2	1.32%	232.24
Swedish Krona	4	2.63%	1,696.41
Swiss Franc	6	3.95%	1,555.29
U.S. Dollar	81	53.29%	1,185.21
Other	10	6.58%	1,245.46
Total	152	100.00%	1,661.76

Table 2. Summary statistics

Panel A provides summary statistics for the currency risk exposure of French acquirers to the target currency. Panel B reports the acquirer's abnormal returns around the announcement. Currency risk exposure is measured using the estimate of β_2 from Equation 2. The exposure coefficient is estimated using firm-level ordinary least squares regression with weekly data over a one-year period prior to the deal announcement. The acquirer announcement abnormal returns are three-day, market-adjusted abnormal returns estimated around the deal announcement day. In Panel B, the *p*-value is computed using Boehmer, Musumeci, and Poulsen's (1991) standardized approach, which is robust to event-induced variance phenomena affecting announcement returns. "n.a." stands for not applicable.

Panel A. Currency risk prior to the deal announcement

Currency risk exposure (β_2)	**Full sample**	**Net importers ($\beta_2 > 0$)**	**Net exporters ($\beta_2 < 0$)**
Average	–0.11	0.50	–0.47
(*p*-value)	(0.03)	n.a.	n.a.
Minimum	–1.69	0.01	–1.69
First quartile	–0.44	0.18	–0.69
Median	–0.12	0.43	–0.38
Third quartile	0.26	0.71	–0.14
Maximum	1.72	1.72	–0.01
Number of firms (% of total sample firms)	152 (100%)	57 (37.5%)	95 (62.5%)
Firms (%) significantly exposed at 1% level	3 (2%)	2 (3.5%)	1 (1.1%)
Firms (%) significantly exposed at 5% level	12 (7.9%)	5 (8.8%)	7 (7.4%)
Firms (%) significantly exposed at 10% level	23 (15.1%)	7 (12.3%)	16 (16.8%)

Panel B. Short-term abnormal return

Three-day abnormal returns	**Full sample**	**Net importers ($\beta_2 > 0$)**	**Net exporters ($\beta_2 < 0$)**
Average	1.18%	–0.12%	1.89%
(*p*-value)	(0.02)	(0.93)	(0.01)
Minimum	–9.73%	–8.45%	–9.73%
First quartile	–1.69%	–2.10%	–1.33%
Median	0.42%	–0.35%	0.69%
Third quartile	3.41%	3.01%	4.19%
Maximum	21.87%	10.45%	21.87%
Number of observations	105	37	68

Table 3. Cross-border deals and currency risk: Univariate analysis

This table presents our univariate analysis. The acquirer's currency risk exposure is measured using the estimate of β_2 from Equation 2. The exposure coefficient is estimated over a one-year period before and a one-year period after the takeover announcement, using firm-level ordinary least squares regression with weekly data. Following Pantzalis, Simkins, and Laux (2001), we split the sample into net exporters and net importers, according to the sign of the firms' exposure coefficient before to the takeover announcement (i.e., net exporters are negatively exposed to the target currency prior to the deal, whereas net importers enjoy positive exposures). Panel A reports on all cases, and Panel B focuses on the subsample of firms significantly exposed to the target currency prior to the deal (at the 10% level). We report the *p*-value from a test of mean differences between the exposure coefficient estimated before and after the takeover announcement.

Panel A. All cases

	Net importers ($\beta_2 > 0$)			**Net exporters ($\beta_2 < 0$)**		
	Before	After	*p*-value	Before	After	*p*-value
Currency risk exposure (β_2)	0.498	0.262	*0.05*	–0.473	0.064	*0.00*
Number of observations	57			95		

Panel B. Significant exposure coefficients

	Net importers ($\beta_2 > 0$, and significant at 10%)			**Net exporters ($\beta_2 < 0$, and significant at 10%)**		
	Before	After	*p*-value	Before	After	*p*-value
Currency risk exposure (β_2)	0.848	–0.052	*0.00*	–0.851	0.006	*0.00*
Number of observations	7			16		

Table 4. Cross-border deals and currency risk management: Difference-in-differences estimator

This table presents the estimation results of Equation 4, obtained using the ordinary least square estimator with asymptotic standard error. The dependent variable is β_2, the exposure coefficient to the target currency. *TP* is a dummy variable that identifies the treatment period (*TP* = 1 if β_2 is estimated over the treatment period [+1, +52], and *TP* = 0 if β_2 is estimated over the pre-treatment period [–52, –1]). *TG* is a dummy variable that identifies the treated group (*TG* = 1 if the firm undertakes a cross-border transaction, *TG* = 0 otherwise). All panels include year fixed effects. Panel A reports on all cases, and Panel B focuses on the subsample of firms significantly exposed to the target currency prior to the deal (at the 10% level). In Panels A and B, for each deal in the sample the control group includes all SBF 250 firms that have not undertaken a cross-border deal in the target country during a two-year period centered on the announcement date of the considered cross-border deal. In Panel C, the control group includes a random firm from the acquirer industry (see Section 2.4).

Panel A. All cases, control firms are all SBF 250 firms

	Net importers ($\beta_2 > 0$)		**Net exporters ($\beta_2 < 0$)**	
	Coef.	*p*-value	Coef.	*p*-value
TP	–0.021	*0.08*	0.027	*0.00*
TG	0.451	*0.00*	–0.446	*0.00*
TP × *TG*	–0.295	*0.07*	0.523	*0.00*
Firm fixed effect	yes		yes	
F-statistic	5.64	*0.00*	6.88	*0.00*
Adjusted R^2	5.20%		3.80%	
Observations	21,770		38,417	

Panel. B. Significant cases, control firms are all SBF 250 firms

	Net importers ($\beta_2 > 0$, and significant at 10%)		**Net exporters ($\beta_2 < 0$, and significant at 10%)**	
	Coef.	*p*-value	Coef.	*p*-value
TP	0.020	*0.36*	0.029	*0.03*
TG	0.816	*0.00*	–0.806	*0.00*
TP × *TG*	–0.920	*0.00*	0.828	*0.00*
Firm fixed effect	yes		yes	
F-statistic	1.23	*0.01*	8.32	*0.00*
Adjusted R^2	2.00%		1.20%	
Observations	2,820		6,887	

Panel C. Significant cases, control firm is a random firm from the acquirer industry

	Net importers (β_2 > 0, and significant at 10%)		**Net exporters (β_2 < 0, and significant at 10%)**	
	Coef.	*p*-value	Coef.	*p*-value
TP	0.121	*0.60*	0.161	*0.37*
TG	0.927	*0.00*	–0.716	*0.00*
TP × *TG*	–0.961	*0.01*	0.688	*0.01*
Firm fixed effect	no		no	
F-statistic	7.15	*0.00*	16.71	*0.00*
Adjusted R^2	40.60%		32.00%	
Number of observations	28		64	

Table 5. Operational hedging and value effects

The dependent variables are the three-day, market-adjusted abnormal returns estimated around the announcement date of the M&A deal. The currency risk exposure is measured using the estimate of β_2 from Equation 2. The variables $|\beta_2|$ *pre-treatment* and $|\beta_2|$ *post-treatment* are the absolute value of the exposure coefficient estimated over the pre-treatment and post-treatment period, respectively. *Delta* $|\beta_2|$ corresponds to $|\beta_2|$ *post-treatment* minus $|\beta_2|$ *pre-treatment. Importers* and *Exporters* are two dummy variables that identify net importers and net exporters, respectively. *Dummy decrease* is a variable that identifies whether the exposure coefficient in absolute value decreases or not after the deal announcement. In other words, it is equal to 1 when *Delta* $|\beta_2|$ is negative, and 0 otherwise. *Deal size* is the natural log of the deal value, defined by SDC as the total value of the consideration paid by the acquirer, excluding fees and expenses. *Relative size* is the ratio of the deal value to the acquirer's market value. *Stock* is a dummy variable that takes a value of 1 for purely stock-financed deals, and 0 otherwise. *Related* is a dummy variable that takes a value of 1 when the acquirer and the target are from the same industry (two-digit standard industrial classification code), and 0 otherwise. *U.S. target* and *Private target* are dummy variables identifying U.S. targets and private targets, respectively. Standard errors are obtained using the procedure described in subsection 2.4.2. R^2 and *F*-statistic denote the *R*-square and Fisher statistic for the regression, respectively. *N* denotes the number of observations.

Variable	**(1)**		**(2)**		**(3)**	
	Coef.	*p*-value	Coef.	*p*-value	Coef.	*p*-value
Variable of interest						
$\|\beta_2\|$ pre-treatment	0.0161	*0.05*				
$\|\beta_2\|$ post-treatment	–0.0179	*0.09*				
Delta $\|\beta_2\|$			–0.0168	0.05		
Importers × Dummy decrease					–0.0125	*0.16*
Exporters × Dummy decrease					0.0196	*0.05*
Control variable						
Deal size	0.0006	*0.35*	0.0004	0.33	–0.0002	*0.52*
Relative size	0.0391	*0.11*	0.0390	0.10	0.0397	*0.09*
Stock	–0.0330	*0.08*	–0.0326	0.09	–0.0309	*0.10*
Related	0.0022	*0.40*	0.0023	0.39	0.0044	*0.32*
U.S. target	0.0141	*0.11*	0.0143	0.08	0.0105	*0.15*
Private target	–0.0247	*0.03*	–0.0249	0.02	–0.0234	*0.02*
F-statistic	1.64	*0.12*	1.88	0.08	1.94	*0.06*
Adjusted R^2	6.50%		6.50%		7.60%	
N	105		105		105	

CHAPITRE 3: THE PAULSON PLAN: WHO ARE THE WINNERS?[22]

[22] This paper is published in « Bankers, Markets and Investors » N. 130 May-June 2014. Co-authors : Eric de Bodt and Frédéric Lobez

ABSTRACT

The joint plan by the U.S. Treasury and the Federal Deposit Insurance Corporation (called the Paulson plan), announced on October 13, 2008, represented the largest financial transfer from taxpayers to financial institutions in U.S. history. Existing research has analyzed whether this massive state intervention improved the recipients' financial health, and thus in this study, we focus on its competitive distortion effects. Our investigation reveals that the Paulson plan was anything but neutral with respect to competition among industry participants. Both short- and long-term results confirm that the winners were the largest banks.

JEL classification: G2

Keywords: TARP; the Paulson plan; value effect; event study; distance to default

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1. Introduction

On October 13, 2008, the U.S. Treasury and Federal Deposit Insurance Corporation (FDIC) together announced a massive financial bailout initiative. Under the designation Troubled Asset Relief Program (TARP), the U.S. Treasury Secretary Henry Paulson and the Federal Reserve Chairman Ben Bernanke introduced an expansive plan (hereinafter referred to as the Paulson plan) that included equity infusions, government bank unsecured debt issue guarantees, and FDIC deposit insurance extensions.

The Paulson plan generated significant controversy and debate in the political arena, both in the United States and abroad (especially after the initiation of comparable initiatives in European countries). The motivations and consequences of this massive transfer of funds from U.S. taxpayers to the financial industry have been widely criticized. In the words of Brendan Murray, Treasury chief under President George Bush, "The consequences of it are unbelievably bad in terms of public intrusion into the private sector"[23]. In this paper, as a first step toward addressing the question of potential competition distortions generated by the Paulson plan, we investigate whether there were clear TARP winners (and losers).

Current academic research offers several contributions that examine the Paulson plan's consequences. Veronesi and Zingales (2010) assess the costs and benefits of the initial $125 billion preferred equity infusion granted to the nine largest U.S. commercial banks. They estimate that the infusion increased the value of the banks' financial claims by $131 billion, at a cost to taxpayers of $25–$47 billion. The net benefit therefore appears to range between $84 and $107 billion. The authors' analysis also indicates that this net benefit resulted mainly from the reduction in the probability of bankruptcy for the recipients. Veronesi and Zingales (2010) also indicate significant cross-sectional variation in the Paulson plan's effects across their sample of nine initial recipients, such that the "big winners were the three former investment banks and Citigroup, while the loser was J.P. Morgan." Such heterogeneity could create significant distortions in competition among financial institutions.

[23] Brendan Murray, "Paulson Bank Rescue Proposal Is Crazy, O'Neill Says," October 2008, http://www.bloomberg.com/apps/news?pid=newsarchive&sid=atJMmClVjevU

Bayazitova and Shivdasani (2011) expand this analysis by including additional TARP capital injections in the months following the initial equity infusion. These injections are associated with large positive excess stock returns for the recipient institutions around their announcements, particularly for larger and earlier recipients. The results also suggest that TARP funding was directed toward financial institutions most likely to pose systemic risks and those exposed to funding uncertainties, despite their good-quality loan portfolios. Therefore, the authors conclude that the Paulson plan lowered financial distress costs more than it kept out-of-the-water unviable banks. Ng et al. (2011) confirm that recipients were on average more profitable or held a loan portfolio of better quality but faced liquidity pressures. Other factors also affected the probability to obtain TARP funding however. Duchin and Sosyura (2012) highlight the role of political connections, in particular between recipients and members of the U.S. Congress. Li (2011) uncovers a similar role of political ties but also sheds light on the positive impact of TARP funding on credit supply. Pay packages for CEOs also seemed important. Cadman et al. (2012) find that financial institutions complying with TARP funding admissibility criteria required effective funding injections less frequently when their CEOs benefited from a more favorable pay package. Finally, King (2009) reviews market reactions to rescue packages for 52 banks in six countries (the United States, the United Kingdom, France, Germany, the Netherlands, and Switzerland). King disentangles the impacts on shareholders (using stock market prices) and creditors (using available credit default swap rates) and concludes that rescue packages helped creditors at the expense of shareholders.

To gain a better understanding of the Paulson plan's wealth effects and to identify which banks among the recipients were the winners, we first examine investors' short-term reactions around the October 13, 2008, announcement date[24] and 250 subsequent capital purchase transactions from October 2008 to December 2009. Our sample includes all Paulson plan recipients for which the necessary information appears in the Center for Research in Security Prices (CRSP) and COMPUSTAT databases. We focus on consequences for shareholders because changes in competitive positions directly affect firm value[25]. We begin by adopting a classic short-term event study approach and

[24] Section 2.1. outlines the detailed succession of events that led to the October 13, 2008, announcement. We refer to October 13, 2008, as the Paulson plan announcement date.

[25] Competitive effects should also affect creditors through their impact on the liquidation value of assets, but only in the case of bankruptcy.

compute cumulative abnormal returns ($CARs$) around the initial Paulson plan announcement date and subsequent capital purchase transactions. The $CARs$ reveal, however, only net information effects of the analyzed events in the eyes of investors. Thus, we supplement the short-term $CARs$ analysis in three directions:

1. We examine recipients' $CARs$ around the Paulson plan initial announcement date and subsequent capital purchase transactions as a function of recipient size, relative size of transaction (the ratio of capital purchase to recipient market value), and the chronological order of intervention (controlling for many other factors, including recipients' risk, financial structure, source of funding, and so forth).
2. We assess variations of the Merton- (1974) based distance to default (DD) that Vassalou and Xing (2004) introduced around the Paulson plan's initial announcement date and subsequent capital purchase transactions. Using DD enables us to reveal the Paulson plan's impact on bankruptcy probability for a large sample of financial institutions while existing research that relies on credit default swap rates is limited to small samples of large financial institutions.
3. We develop a long term performance analysis based on calendar time abnormal returns ($CTAR$). The $CTAR$ approach controls for many mis-specification biases related to long-term abnormal performance measurements (Lyon et al., 1999) and provides an elegant framework to assess the performance of recipients relative to their industry pairs.

Our empirical analyses generated five main results. First, recipients' $CARs$ surrounding the Paulson plan's initial announcement are positive and statistically significant, consistent with the findings of Veronesi and Zingales (2010) and Bayazitova and Shivdasani (2011). Second, recipients' $CARs$ in response to subsequent capital purchase transactions are negative and statistically significant. This result contrasts with that of Bayazitova and Shivdasani, who report a small, positive, marginally significant average CAR, though they also report a negative median CAR.

Third, our regression analyses uncover a positive and significant relationship between recipients' $CARs$ around the initial announcement date and their size (after controlling

for many factors correlated with size, such as risk, financial structure, and sources of funding). The economic effect is sizable: in ranking banks by market value, the average wealth effect difference between the third and the first size quartile is approximately $403 million. We also find a positive and significant relationship between recipients' $CARs$ around subsequent capital purchase transactions and the capital purchase transaction relative size (again after controlling for many factors correlated with size). The economic effect is again significant, with an average wealth effect difference between the third and the first relative size quartile of approximately $78 million. Finally, we uncover a negative and significant relationship between recipients' $CARs$ around subsequent capital purchase transactions and the chronological order of the transactions. Taken together, these results significantly expand the evidence that Veronesi and Zingales (2010) report on the heterogeneity of the Paulson plan's effects on recipient shareholders' wealth.

Fourth, we also observe that the average DD increased around the initial Paulson plan announcement date. Our results clearly show that this DD increase is driven mainly by large recipients: the DD variation difference between the third and the first size quartile is 9%. We uncover this size effect because our sample is not restricted to the largest recipients, in contrast with previous studies. Moreover, we show that the higher the capital purchase relative size (capital purchase amount divided by the recipient size), the higher is the increase in DD. These results highlight the limits of generalizations from samples of financial institutions restricted to the largest players. They further confirm the heterogeneity of the Paulson plan's impact on recipients.

Fifth, our long-term performance analysis reveals that (1) the Paulson plan recipients underperformed their industry pairs significantly in the 36-month period following the capital infusion and (2) small beneficiaries were at the origin of this underperformance. The long-term evidence is consistent with the short-term event study results and emphasizes the Paulson plan competition distortion effects.

We make three main contributions to existing research. First, we confirm a significant size effect in the Paulson plan's initial announcement on recipient financial institutions' value and probability of default. Second, we uncover a positive relationship between the Paulson plan's subsequent capital injections and recipients' $CARs$ and DD and capital

injection relative size (capital purchase amount divided by recipient size). This relative size effect reveals that the direct shareholders' wealth effects (driven by a decrease in the probability of bankruptcy) dominated the private information effect (capital injections revealed to investors the extent of financial difficulties). Third, to the best of our knowledge, we are the first to provide a long-term analysis of the Paulson plan effects. Our results reveal that the Paulson plan was anything but neutral in its effect on competition among financial institutions and even seems to have increased pressure on small players in the industry.

In section 2, we present the Paulson plan, followed by a review of research contributions to the field and a summary of the Paulson plan's potential information effects. In section 3, we describe our data sources and sample composition and detail our short-term and long-term event study approaches and DD computation. In section 4, we present our results and various robustness checks. We end with a brief conclusion.

2. Market reactions to the Paulson plan

2.1. The Paulson plan

On September 19, 2008, Henry Paulson and Ben Bernanke announced a first version of the TARP, designed to help the U.S. financial industry overcome the severe financial crisis in the wake of Lehman Brothers' bankruptcy. Congress rejected the plan on September 29, 2008; a revised version of the $700 billion TARP plan received Congressional approval on October 3, 2008, under the Emergency Economic Stabilization Act. However, this announcement failed to convince investors, and stock markets struggled through a particularly difficult week in which they lost 18% from October 6 to October 10, 2008. This situation increased pressure from financial markets and led the U.S. Treasury, U.S. Federal Reserve, and FDIC to announce a revised plan on October 13, 2008[26]. The revised plan contained three main components:

[26] The details of a $125 billion equity injection into Citigroup, Wells Fargo, JPMorgan, Bank of America, Goldman Sachs, Morgan Stanley, State Street, Bank of New York Mellon, and Merrill Lynch were announced on October 14, 2008, simultaneous with the $250 billion CPP.

We focus on the first component, the CPP, which had the most direct potential effect on shareholders of recipient financial institutions. The FDIC guarantee extensions instead were directed toward creditors and depositors.

Veronesi and Zingales (2010) provide a clear summary of the main motivations behind the CPP. First, the CPP was designed to offer an alternative to new equity issues on the stock market. In October 2008, the financial turmoil was such that no new equity issues (especially from overly indebted financial institutions) were possible. The freeze was rooted in the classic debt overhang problem (Myers, 1977): new equity issues by overly indebted firms would generate a wealth transfer in favor of creditors. Because potential new shareholders anticipated this wealth transfer, they refused to participate. During the second half of 2008, the combination of the new equity issue freeze and (so-called) toxic assets existence that led to heavy losses by financial institutions caused financial institutions to adopt very restrictive credit policies. The conditions for the financial crisis to spread throughout the real economy were in place. Ivashina and Scharfstein (2009) report that credit offers to large firms dropped by 50% during the last quarter of 2008, compared with 2007, and Udell (2009) reports that the net percentage of banks that reported tighter standards for small businesses in July 2008 was 65.3%. Unlocking access to new equity therefore was a central issue.

A second goal of the CPP was to stop the liquidity crisis and avoid the risk of bank runs. In October 2008, two kinds of runs appeared possible: a depositors' run, in the spirit of Diamond and Dybvig (1983), and a short-term lenders' run. Questions about the stability of the global financial system were regularly raised in the press. A collective loss of trust could lead to massive withdrawals. The depositors' run never materialized, but the short-term lenders' run did. Liquidity on the interbank market was drained, and short-term lenders refused to renew their funding (see Gorton and Metrick, 2009). The absence of clear, reliable information on toxic asset losses initiated collective distrust among financial institutions. Pressure on liquidity also was reinforced by the willingness of financial institutions to strengthen their liquidity buffer to cope with even greater potential liquidity shocks (e.g., depositors' runs). The fear index (i.e., aggregate implicit volatility derived from stock options, which measures investors' perceptions of uncertainty) reached a peak on October 10, 2008. The U.S. Treasury, Federal Reserve, and

FDIC clearly wanted to restore trust in the system and among participants by serving as a last-resort lender.

2.2. The market reaction to the Paulson plan

The importance of the Paulson plan's financial stake (and similar rescue packages in other countries) prompted researchers to investigate the wealth effects of these government interventions. The most commonly used approach has been short-term event studies, which help identify the wealth effects of information events, under the assumption that financial markets are efficient (see Kothari and Warner, 2007). Researchers have used event studies to analyze the Paulson plan's effects on both creditors, using the abnormal reactions of credit default swap rates, and shareholders, using stock return–based classic $CARs$ analyses. Taking into account its goals (see Section 2.1), the Paulson plan should have had positive effects on creditors, in that the CPP and FDIC guarantee extensions should have decreased the probability of default and therefore decreased credit default swap spreads. The potential impacts on shareholders' wealth are less clear however. The Paulson plan's interventions may have conveyed contradictory information about the recipients' financial situation. On the one hand, bailout interventions reduce the bankruptcy risk to which shareholders are exposed and therefore should be a value-creating event (solvability effect); on the other hand, bailout interventions may restrict the dividend policy or the incentive system used by recipients' institutions and/or dilute current shareholders' equity position. Moreover, such adverse effects could be reinforced by the impact of divulging private information: bailout packages can signal to investors the seriousness of the financial difficulties recipients face (private information effect). The net effect of the Paulson plan on shareholders' wealth therefore is an empirical issue.

A fundamental choice when implementing the event study method is the identification of the event itself. One possible choice is to focus on October 13, 2008 (the announcement date of the rescue package plan), in consideration of the systemic dimension of the 2008 financial crisis and the willingness of public authorities to send a clear and credible signal to all market participants to restore trust in the financial system. However, this choice ignores idiosyncratic effects of bailout interventions. Alternatively, the succession of

specific bailout interventions can be analyzed to identify the rescue package consequences for individual recipients.

Veronesi and Zingales (2010) focus solely on the Paulson plan announcement date and investigate the nine largest U.S. banks[27]. Their results are consistent with evidence reported by King (2009): creditors are the main beneficiaries, and evidence about shareholders indicates smaller gains (the dollar $CARs$ on common equity are negative but more than compensated by the dollar $CARs$ on preferred stocks). Veronesi and Zingales stress that these average results hide great cross-sectional variation in the results. For example, JPMorgan shareholders lost $34 billion in the days around the Paulson plan announcement on common stocks, but Morgan Stanley, Citigroup, and Goldman Sachs shareholders earned $11, $8, and $8 billion, respectively.

Bayazitova and Shivdasani (2011) expand this analysis to equity injections subsequent to the initial announcement, but they restrict their analysis to recipients' shareholders. They group the capital purchase transactions into three sub-samples (or rounds). The first includes banks recapitalized immediately after the initial Paulson plan announcement. The second sub-sample includes 50 recipient banks recapitalized between October 15, 2008, and November 14, 2008. The third sub-sample gathers 173 banks that received assistance between November 15, 2008, and April 24, 2009. Shareholders in the first round benefited from the Paulson plan. However, from round to round, the capital purchase transactions are associated with declining $CARs$, though they remain positive. Elyasiani et al. (2011), focusing on a sample of U.S. banks, also report that investors reacted positively to the Paulson plan's capital injections.

King (2009) examines rescue packages announced across six countries (the United States, the United Kingdom, France, Germany, the Netherlands, and Switzerland) between October 2008 and January 2009. Using a sample of 52 large banks, King analyzes reactions, as summarized in stock prices and credit default swap spreads, around six announcement dates. On average, creditors benefited from the rescue packages, but shareholders were mostly penalized (though to a lesser extent in the United States). The negative net impact of rescue packages on shareholders' wealth suggests that restrictions

[27] The sample includes 10 banks, if Wachovia is recognized as an independent entity at that time.

on dividends and incentives, dilution, and private information effects dominated value creation because of decreased bankruptcy probability (solvability effect).

2.3. The Paulson plan's wealth effect determinants

Bayazitova and Shivdasani (2011) show that the Paulson plan recipients' $CARs$ declined over time, such that they were most positive in the first round, in the immediate wake of the Paulson plan announcement. The first-round recipients were the largest, most troubled banks, subject to the largest equity injections. This existing empirical evidence prompts us to focus on two potential determinants of the Paulson plan's effects on recipient shareholders' wealth: recipient size and capital purchase transaction relative size. Under the assumption that the Paulson plan is neutral with respect to size among financial industry participants, we expect the following:

- The shareholders' wealth effect ($CARs$) from the Paulson plan's initial announcement should not be systematically related to the potential recipient's size. Each player in the financial industry should have the same probability of being bailed out, regardless of size. A positive and significant relationship between recipients' $CARs$ and their respective size would be a clear indication that a size effect was at play.
- The shareholders' wealth effect ($CARs$) of subsequent capital purchase transactions should not be systematically related to the relative size of the capital purchase transaction (i.e., the ratio of capital purchase transaction size to recipients' market value). Each dollar of equity injection should generate (on average and after controlling for recipient size) the same wealth effect. A significant, positive relationship between the recipients' $CARs$ and the capital purchase transaction's relative size would indicate that recipients of large equity injections received preferential treatment.

Thus, we test the neutrality of the Paulson plan with the two following regression equations:

$$CAR_{i,t_{ini}} = \alpha_0 + \alpha_1 \times Size_i + Controls_i + \varepsilon_i, \qquad (1)$$

where i is a recipient financial institution, $Size$ is the natural logarithm of the market value of the recipient 21 days before the announcement date, and t_{ini} is the Paulson plan's initial announcement date, and

$$CAR_i = \beta_0 + \beta_1 \times CPP\ Relative\ Size_i + \beta_2\ \times Chron\ Order_i + Controls_i + \varepsilon_i, (2)$$

where i is a capital purchase transaction, CPP stands for capital purchase program, $CPP\ relative\ size$ is the capital purchase amount divided by recipient market value 21 days before the announcement date, and $Chron\ Order_i$ is the chronological order of transaction i. Under the null hypothesis of competitive neutrality, α_1 and β_1 should not be significantly different from 0. We explicitly identify $Order_i$ as a control variable because Bayazitova and Shivdasani's (2011) results show that the $CARs$ impact of capital purchase transactions declined over time and that large financial institutions received help first. In the absence of this control variable, a positive β_1 could just capture this correlation between size and chronological order of intervention.

A parallel analysis applies to the probability of bankruptcy. If the Paulson plan is neutral with respect to the competitive game inside the financial industry, its consequences on the probability of bankruptcy should not be systematically related to recipient size or capital purchase transaction relative size. We replace the dependent variables in Equations (1) and (2) with a measure of the probability of bankruptcy—namely the Merton- (1874) based DD introduced in Vassalou and Xing (2004)—as the dependent variable.

Other control variables in our regressions come from existing contributions. More specifically, we use the ratio of total deposits to total assets (Deposit%) to disentangle the return on assets (ROA), computed as the ratio of earnings before interests and taxes to total assets, between retail and investment banks to control for profitability; the Tier 1 capital ratio (Tier 1) established for banks by the 1988 Basel Accord[28] (COMPUSTAT item: CAPR1) to capture the bank solvency; and the ratio of total liability to total assets (Leverage) as an additional measure of solvency. We also add the specification of CEO compensation (CEO comp), which is a dummy variable equal to 1 if the compensation is

[28] Basel Committee on Banking Supervision for reporting risk-adjusted capital adequacy.

greater than $500,000 and 0 if otherwise[29]. Note that in CAR regression, we also add the average 2007 DD ratio to control for risk before the financial crisis, computed as the average monthly value of DD during 2007.

2.4. Data sources and sample composition

We obtained the list of CPP recipients from Propublica[30], using the update from August 20, 2010. We cross-checked the list against the Quarterly Report to Congress provided by the special inspector general for TARP on July 21, 2010. The list includes the 706 financial institutions that received support from the CPP program between October 2008 and December 2009.

Panel A in Table 1 contains the CPP descriptive statistics, including the monthly number of financial institution recipients and the corresponding mean, median, minimum, maximum, and total funding. The initial CPP transactions were directed toward a few large financial institutions (8 in October 2008, total funding of $125 billion[31]), and then the monthly number of recipients increased up to December 2008 to reach a peak of 161 financial institutions, though the average CPP transaction size decreased sharply during this time span (from $15.6 billion in October to $0.153 billion in December 2008). The CPP interventions remained intensive in January and February 2009 (146 and 107 transactions, respectively) but thereafter decreased quickly, as Figure 1 shows. Panel A in Table 1 also highlights the considerable heterogeneity in CPP transaction sizes. In October 2008, the ratio of the maximum to the minimum transaction size was already 12.5, despite the initial focus on large financial institutions. The ratio reached 5,052 in December 2008, with a maximum of $7.579 billion and a corresponding minimum of $0.0015 billion. This size heterogeneity is also apparent in the comparison of the mean and median CPP transaction funding by month.

Table 1, Panel B, displays the distribution of CPP recipients by recipient type (public, private, community development banks, and insurance company). Only two insurance companies were among the recipients (Hartford Financial Services and Lincoln National

[29] The $500,000 threshold was announced by the U.S. Treasury as an interim rule that limited executive compensation for TARP recipients.
[30] See http://www.propublica.org
[31] Veronesi and Zingales (2010) identify nine recipients (excluding Wachovia). We exclude Merrill Lynch because of its purchase by Bank of America, which was announced on September 14, 2008.

Corporation). The CPP interventions were mostly directed toward public and private banks, with a dominant proportion of private banks (56.5% of the total recipients).

We collect data on stock returns, prices, and number of outstanding shares from the CRSP database. Other accounting data (e.g., assets, liabilities, debt, deposits) come from COMPUSTAT. CEO compensation information derives from the Execucomp database. Data needed for the long-term abnormal performance study are stock returns and market values from the CRSP database and the Kenneth French website[32]. We analyze the Paulson plan's initial announcement effects using the sub-sample of listed recipients (250 financial institutions) because we use $CARs$ to measure shareholders' wealth effect. For the same reason, we include subsequent interventions only in favor of listed recipients. Finally, we limit the sample to CPP transactions of more than $1 million to ensure a focus on economically significant events. The final sample thus contains 250 CPP transactions, after the initial Paulson plan announcement.

3. Methods

3.1 Short-term event study

To isolate the wealth effect of the Paulson plan announcement and subsequent CPP transactions on shareholders, we use the classic event study approach introduced by Fama et al. (1969) and compute abnormal returns using the market model (Sharpe, 1964) as a return-generating process:

$$r_{i,t} = \alpha_i + \beta_i r_{m,t} + \varepsilon_{i,t} \tag{3}$$

$$AR_{i,t} = r_{i,t} - \left(\hat{\alpha}_i + \widehat{\beta_i}\, r_{m,t}\right), \tag{4}$$

where $r_{i,t}$ denotes the return to financial institution i at date t, $r_{m,t}$ denotes the return of the S&P 500 index at date t, α_i and β_i are the parameters of the market model, $\hat{\alpha}_i$ and $\widehat{\beta_i}$ are the corresponding estimated values, $\varepsilon_{i,t}$ denotes the residuals of the market model,

[32] See http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.htm

and $AR_{i,t}$ refers to the abnormal returns of financial institution i at date t. We estimate the market model parameters α_i and β_i on a 200-day estimation window, from day –235 to day –36, relative to the event date t_0. For each event, we compute the $CARs$ for 3-day, 7-day, and 11-day event windows centered on t_0:

$$CAR_i = \sum_{t_0-x}^{t_0+x} AR_{i,t} \ (x = 1,3,5). \tag{5}$$

These are respectively denoted as CAR (–1,1), CAR (–3,3), and CAR (–5,5). The average cumulative abnormal returns ($ACARs$) for a sample of announcements represent the arithmetic average of CAR_i:

$$ACAR = \frac{\sum_{i=1}^{n} CAR_i}{n}, \tag{6}$$

where n is the number of observations in the sample. To test the statistical significance of $ACAR$, we compute its cross-sectional standard deviation:

$$SE(ACAR) = \sqrt{\frac{\sum_{i=1}^{n}(CAR_i - ACAR)^2}{n\,(n-1)}}, \tag{7}$$

where $ACAR$ is defined as in Equation (6).

3.2 Distance to default

We analyze the Paulson plan's impact on the recipients' probability of bankruptcy using Vassalou and Xing's (2004) DD, which for financial institution i at time t is as follows:

$$DD_{i,t} = \frac{\ln\left({}^{V_{A,i,t}}/_{X_{i,t}}\right) + \left(\left(\mu_{A,i} - \left({}^{1}/_{2}\sigma_{A,i}^2\right)\right) \times T\right)}{\sigma_{A,i} \times \sqrt{T}}, \tag{8}$$

where $V_{A,i,t}$ is financial institution i's asset value at time t, $X_{i,t}$ is the corresponding debt value, $\mu_{A,i}$ is the expected rate of return of financial institution i's assets, $\sigma_{A,i}^2$ is the

corresponding variance, and T is the time horizon. The model assumes that $V_{A,i,t}$ follows a Brownian motion.

However, $V_{A,i,t}$ is not directly observable. We follow the estimation procedure that Vassalou and Xing (2004) advocate and use Black and Scholes's (1973) formula to infer $V_{A,i,t}$ from the financial institution's market value of its equity $V_{E,i,t}$. For the risk-free rate, we use the one-year Treasury bill rate published by the Federal Reserve [33]. The parameters $\mu_{A,i}$ and $\sigma_{A,i}^2$ are the arithmetic average and the variance of daily estimated $V_{A,i,t}$ returns, respectively. We estimate these parameters on an estimation window of 12 months before the event period. We use these estimates iteratively to compute $V_{A,i,t}$ until convergence. We set the initial values of $\mu_{A,i}$ and $\sigma_{A,i}^2$ to corresponding values for equity, taking into account firm leverage (see Vassalou and Xing, 2004, p. 835).

Finally, $X_{i,t}$ is financial institution i's debt value at time t. We estimate the debt value by summing the book value of debts and deposits of each financial institution. Taking deposits into account is particularly important because we analyze banks. Similar to Vassalou and Xing (2004), we divide long-term debts by 2, because these debts must not be rolled over on short horizons and therefore are less likely to lead to default than short-term debts.

We compute DD for each day from day –5 to day +5 relative to the event day. We calculate three variants of DD variations, denoted as $\Delta DD1$, DD_{t+1} minus DD_{t-1} ; $\Delta DD3$, $\sum_{t+1}^{t+3} DD_t/3 - \sum_{t-1}^{t-3} DD_t/3$; and $\Delta DD5$, $\sum_{t+1}^{t+5} DD_t/5 - \sum_{t-1}^{t-5} DD_t/5$.

3.3 Long-term abnormal performance

We employ the CTAR approach to evaluate the capital injection effects in the long run. This method was first introduced by Jaffe (1974) and Mandelker (1974) and recommended in Fama (1998).

For the bank participants in our sample, we construct equally weighted and value weighted portfolios for each month from October 2008 to December 2011, a total of 39 months. We rebalance the portfolios each month by removing recipients for which the

[33] See http://www.federalreserve.gov/econresdata/releases/statisticsdata.htm

capital injection date is older than 36 months and including new recipients of the month. We obtain the $CTARs$ by running the following regression:

$$R_{p,t} - R_{f,t} = \alpha_p + \beta_p(R_{m,t} - R_{f,t}) + \gamma_p SMB_t + \delta_p HML_t + \theta_p MOM_t + \varepsilon_{p.t} \quad (9)$$

We control for the three Fama and French (1992) factors ($R_{m,t} - R_{f,t}$, the excess market return; SMB_t, the size premium; and HML_t, the market-to-book premium) and add MOM_t, the momentum. The intercept α_p from the regression model measures the abnormal return of the portfolio. Under the null hypothesis of no abnormal performance, α_p equals 0. To test whether α_p is significantly different from zero, we use two approaches.

The first approach is bootstrap, introduced by Mitchell and Stafford (2000). For each month from October 2008 to December 2011, we simulate a control portfolio of the same size as the portfolio of CPP recipients. The control portfolio contains banks that are not CPP participants but belong to the same SIC two-digit industries[34]. The banks included in the control portfolio are randomly selected, and we repeat this procedure 1,000 times to get 1,000 portfolios. For each portfolio, we estimate Equation (9). This procedure generates 1,000 α_p values. We obtain the t-statistics by dividing the difference between the intercept α_p from recipients' portfolio and the mean of 1,000 α_p from simulated control portfolios by the standard deviation of the simulated α_p,:

$$t^* = \frac{\alpha_0 - \overline{\alpha_s}}{Sd_s}, \quad (10)$$

where t^* represents the t-statistics, α_0 is the abnormal return of CPP recipients' portfolio, $\overline{\alpha_s}$ is the average abnormal return for 1,000 control portfolios, and Sd_s is the corresponding standard deviation.

The second approach, industry pairs matching, is to regress the difference between the return of recipients' portfolio and the control portfolio on the four factors included in Equation (9). To do so, for each month, we form our control portfolio by including all banks belonging to the SIC two-digit industries 60, 61, 62, and 67, at the exclusion of the

[34] We use SIC codes 60, 61, 62, and 67, 2525 financial institutions in total, because the participants in our samples are classified in these industries.

Paulson plan beneficiaries. The α_p test of significance is then directly obtained from the regression results.

4. Results

We first analyze shareholders' wealth effects of the Paulson's plan initial announcement and of the subsequent CPP transactions. We then focus on their impacts on recipients' probability of bankruptcy and on the long-term abnormal performance of the Paulson plan beneficiaries.

4.1. Shareholders' wealth effects of the Paulson plan announcement

In Table 2, Panel A, we report the $ACAR$ for 249 financial institutions (one missing observation with respect to the initial sample size) for three event window lengths, centered on October 13, 2008. This sample includes the equity infusions for the eight initially rescued institutions. The $ACARs$ for the three event windows are positive and significant and range from 4.7% to 8.3%. The Paulson plan created value for the shareholders of potential recipients. Panel A also reports the percentage of recipients for which $CARs$ were positive and the number of recipients for which $CARs$ were positive (negative) and significant at the 10% statistical level. The results are consistent with an average value creation effect but reveal the large cross-sectional heterogeneity of the Paulson plan announcement on shareholders' wealth effects. For example, among the 249 recipients, with the day –1 to day +1 event window, 94 recipients experienced a positive and significant CAR while 20 displayed a negative and significant reaction.

Table 2 also reports the results when we split our sample by size into two sub-groups. We use the median market value as the threshold, estimated 21 days before the announcement date. We find that large banks show significantly higher abnormal returns than their smaller counterparts.

The primary goal of the Paulson plan announcement was to address the largest financial crisis since the 1929 market crash and the Great Depression. The systemic nature of the 2008 financial crisis implied that the largest financial institutions, likely to be systemic players, should be the ones to which the plan was by priority directed. In Panel B of Table 2, we explore the relationship between financial institution recipients' $CARs$ and their

Size (natural logarithm of market value, estimated 21 days before the announcement date). The coefficient of the *Size* variable is positive and highly significant for 7-day and 11-day estimation windows. Shareholders of the largest financial institutions thus were indeed winners, consistent with the presence of a size effect highlighted in Panel A. This size effect is robust to the inclusion of control variables featuring beneficiaries' financial structure (Tier 1, Leverage), source of funding (Deposit%), risk (average DD), and CEO incentives (CEO comp). Size seems to play a role *per se*. These results provide a first indication of distortions for the competition between small and large players in the industry.

4.2. Shareholders' wealth effects of capital purchase program transactions

In Table 3, we examine the wealth effect of CPP transactions subsequent to the initial Paulson plan announcement. Panel A contains the results of the event study, and Panel B displays the multivariate analysis of the $CARs$ determinants.

The 250 CPP transactions of $ACARs$ are negative, ranging from –1.3% to –2.3% depending on the event window, and significant in the 3-day and 11-day cases. The $CARs$ are negative for approximately 60% of the observations. These results are striking for two reasons. First, the 250 CPP transactions include the initial eight transactions for Citigroup, Wells Fargo, JPMorgan, Bank of America, Goldman Sachs, Morgan Stanley, State Street, and Bank of New York Mellon. Veronesi and Zingales (2010) show that the Paulson plan was value creating for shareholders of these banks (they also include Merrill Lynch). Second, these results contradict Bayazitova and Shivdasani's (2012) finding of small, positive, marginally significant $CARs$ with a comparable sample of CPP transactions (see Table 4, Panel B, Announcement Effects column, for All Rounds sample)[35]. Our results are similar to Kim (2010), who finds significant, negative 3-day $CARs$ for January 16, February 4, February 13, and June 10 of 2009. The potential restrictions on the dividend policy and incentives, dilution, and/or negative private information effects related to CPP transactions thus seem to dominate the direct effect on the probability of bankruptcy.

[35] Bayazitova and Shivdasani (2012) use a two-day event window, from day –1 to the event day. This difference in event window length may explain this divergence of results.

The analysis of $CARs$ determinants in Table 3, Panel B, reveals the role of $CPP\ relative\ size$ (i.e., ratio of CPP transaction size to recipients' market value 21 days before the announcement date) and chronological order of the CPP transaction (obtained by sorting CPP transactions in the sample by announcement date). Column 1 in Table 3, Panel B, reports the results for the whole sample using 11-day $CARs$ as the dependent variable. Columns 2 to 4 report the results for large banks, and columns 5 to 7 present those for small banks, respectively, for the 3-day, 7-day, and 11-day event windows. We discriminate between large and small banks using the median market value of banks 21 days before each announcement date. Overall, the $CPP\ relative\ size$ coefficient is positive for the whole sample under the 11-day event window setting (but not on shorter windows). The results of the large and small bank sub-samples show that this positive effect of relative size is much greater for large banks. Some evidence reveals that the larger the CPP-related equity infusion relative to the recipient's size, the more it was profitable, in particular large banks. The chronological order coefficient is always negative for large banks and highly significant for the 7-day event window. This result provides some support for the decline in $CARs$ for large banks over time, as Bayazitova and Shivdasani (2011) report. For small banks, the chronological order is never significant. Because small banks were helped later on, this result is again consistent with a change in CPP transaction targets over time. The CPP transactions were directed earlier toward large banks with weaker capital ratios, whereas later rounds focused on smaller and better-capitalized institutions. We also note that the divergent results we obtain for large and small banks offer added support for the existence of competition distortion effects of the Paulson plan among recipients.

4.3. Capital purchase program effect on the distance to default

Next, we use the DD as a proxy for the probability of bankruptcy (a higher DD means a lower probability of bankruptcy, as perceived by investors). We examine the variation of the DD around the announcement date and subsequent CPP transactions. This complements the $CARs$ analyses by providing information specific to default risk. Working with the variation in DD is important to neutralize DD-level heterogeneity across the sample and to isolate CPP intervention effects.

Table 4, Panel A, displays the results of four regressions using *DD* variation around the initial Paulson plan announcement as the dependent variable. The sample size falls to 200 financial institutions because of limitations inherent to *DD* computations. We use three types of *DD* variation: ΔDD5 indicates the change in average *DD* ratio between (–1,–5) and (1,5) relative to the announcement date (October 13, 2008), and ΔDD3 and ΔDD1 represent similar definitions using a shorter window average. In Column 1, we regress ΔDD5 on a constant. The estimated constant therefore captures the average *DD* variation around the Paulson plan announcement date. The coefficient is positive and highly significant. This initial evidence confirms that for investors, on average, the Paulson plan announcement increased their *DD* (reduced the probability of bankruptcy). Columns 2 to 4 report results for ΔDD1, ΔDD3, and ΔDD5, while controlling for size (natural logarithm of market value, estimated 21 days before the announcement date), financial structure (Tier 1, Leverage), source of funding (Deposit%), profitability (ROA), and CEO incentives (CEO comp). Coefficients of size for ΔDD3 and ΔDD5 are positive and significant: larger financial institution recipients experience a greater positive impact of the announcement on their *DD*. This result is consistent with the positive effect of *Size* on *CAR*, as we report in Table 2, Panel B, and offers additional evidence of the presence of a size effect. This result also confirms that creditors of large financial institutions benefited from the Paulson plan more than creditors of smaller financial institutions. In Figure 2, we depict recipients' *Size* (natural logarithm of market value, estimated 21 days before the announcement date) and the corresponding variation of *DD* around the Paulson plan's initial announcement (using ΔDD5). For small recipients, the Paulson plan announcement decreased *DD* (increased bankruptcy probability, according to investors). For these small banks, the Paulson plan's private information revelation effect dominated the solvability improvement effect for investors. Another, and potentially complementary, interpretation is that the Paulson plan increased competitive pressure on small participants in the financial industry. Our results also reveal that using a sample selected on financial institution size (whether directly or indirectly, due to data availability constraints) is not innocuous; it is a form of endogenous sample selection. The results obtained from samples comprising large recipients cannot be generalized.

Panel B of Table 4 provides univariate analyses of *DD* variation for CPP transactions. Columns 1 to 3 report the results for ΔDD1, ΔDD3, and ΔDD5, respectively. We find that

the average DD variations are negative but only significant for ΔDD5. The results offer some support for a negative market response to subsequent capital injections. The negative private information effect seems to dominate.

Finally, in Table 4, Panel C, we report the results for the multivariate analyses of DD variations around subsequent CPP transactions. Column 1 reports the results for the whole sample using ΔDD5 as the dependent variable. Columns 2 to 4 report the results for large banks and columns 5 to 7 for small banks, respectively, for ΔDD1, ΔDD3, and ΔDD5. We regress the DD variation on $CPP\ relative\ size$ (CCP transaction size divided by recipients' market value 21 days before the announcement date), chronological order (by announcement date), and our set of control variables. We discriminate between large and small banks using the median market value of 21 days before the announcement date. For large banks, we find a positive and significant effect of relative size on DD variation for ΔDD5. For large players, some evidence shows that the more important the equity infusion with respect to the recipient's size, the larger is the increase in its DD. For small banks, ΔDD1 shows a significantly positive effect of $CPP\ relative\ size$ on DD variation, but the coefficient of size is itself negative and significant (which is not the case for large banks). In summary, the evidence shows that (1) on average, large financial institutions benefited from CPP transactions (the solvability improvement due to equity injection dominated the negative private information revelation effect) and (2) the consequences of CPP transactions on DD were heterogeneous with respect to size.

4.4. Long-term abnormal performance

Table 5 displays the results obtained using the CTAR approach. In columns 1 and 2, we use bootstrap to test for statistical significance. The results in columns 3 and 4 rely on the industry pairs matching procedure. Columns 1 and 3 represent the equally weighted portfolio. Columns 2 and 4 report the corresponding results of the value weighted portfolio. Finally, α_p is the measure of long-term abnormal performance.

The coefficient α_p is negative and significant for equally weighted portfolio regressions, while the effect appears to be weaker for value weighted portfolios (or even positive). This apparently divergent result delivers an important insight about the long-term abnormal performance of Paulson plan recipients. Using the value weighting scheme, large financial institutions drive the results, while using the equally weighted scheme,

small industry players dominate because of their numbers. These results suggest that the long-term abnormal performance of small industry players has been significantly negative, while this has not been the case for large recipients.

To provide support for this interpretation, we first display in Figure 3 the evolution of the recipients' portfolio returns over time. Panel A presents the equally weighted scheme and Panel B the value weighted scheme. The difference in the recipients' behavior between the two panels is striking and explains the results reported in Table 5. We also report in Table 5, columns 5 and 6, the results using industry pairs matching and equally weighted portfolio for large and small recipients, respectively. Small recipients are those with market value below the sample median market value (and vice versa). Small recipients have a significantly negative α_p of –0.016, while α_p is not statistically different from zero for large recipients. Small recipients displayed long-term significant abnormal performance, but no such evidence appears for large recipients.

4.5. Robustness checks: Event clustering and event-induced variance

Our event studies might be affected by abnormal return heteroskedasticity, event-induced variance, and clustering of events over time, especially with respect to the initial announcement. We therefore investigate the robustness of our results by adopting the standardized abnormal return test (Patell, 1976), with a cross-sectional estimate of $CARs$ standard error (Boehmer et al., 1991), and Jaffe's (1974) and Mandelker's (1974) portfolio approach.

More precisely, abnormal returns are standardized by their corresponding standard deviation, to control for heteroskedasticity:

$$SAR_{i,t} = \frac{AR_{i,t}}{SE(AR_i)}, \tag{11}$$

where $SE(AR_i)$ is estimated as $SE(AR_i) = \sqrt{\frac{\sum_{t=-235}^{-36}(AR_{i,t}-\overline{AR_i})^2}{n-1}}$ and $\overline{AR_i}$ is the average of $AR_{i,t}$ during the estimation window: $\overline{AR_i} = \frac{\sum_{t=-235}^{-36} AR_{i,t}}{n}$. Standardized abnormal returns then can be cumulated for the event window for CAR computations of 3-day, 7-day, and 11-day, to form the cumulative standardized abnormal returns $CSARs_i$:

$$CSARs_i = \sum_{t_0-x}^{t_0+x} SAR_{i,t} \ (x = 1,3,5) \quad . \tag{12}$$

The average cumulative standardized abnormal return, $ACSAR$, is the arithmetic average of the $CSARs_i$ for the selected sample of events:

$$ACSAR = \frac{\sum_{i=1}^{n} CSAR_i}{n} \quad . \tag{13}$$

To compute the standard deviation of $ACSAR$, we use the cross-sectional estimate of the standard deviation of $CSARs_i$:

$$SE(ACSAR) = \sqrt{\frac{\sum_{i=1}^{n}(CSARs_i - ACSAR)^2}{n\,(n-1)}}, \tag{14}$$

where $ACSAR$ is as defined in Equation (13).

Jaffe (1974) and Mandelker (1974) propose an alternative approach to control for event-day clustering by forming a unique portfolio of all firms subject to the event. The portfolio can be either equally or value weighted, after which the classic event study method is applied to the returns of the portfolio. This approach controls for cross-sectional correlation among observations due to event-day clustering because one unique observation (the portfolio) remains. Its statistical power is weak however because the sample now comprises one unique observation.

Panels A and B of Table 6 summarize our results, obtained using Boehmer et al.'s (1991), Jaffe's (1974), and Mandelker's (1974) methods. Panels A and B focus on the Paulson plan announcement date, and Panel C reports the results for subsequent CPP transactions. Our results are confirmed. The $ACAR$ around the Paulson plan announcement date is positive and significant for each event window. The $CARs$ around subsequent CPP transaction announcements are negative and significant. Heteroskedasticity and event-date clustering of observations thus do not affect our results.

5. Conclusion

The 2008 financial crisis represented a dramatic event for modern financial systems. The risk of a global collapse of the banking industry triggered a massive intervention by public authorities. The U.S. Treasury and FDIC jointly implemented the so-called TARP, mobilizing hundreds of billions of U.S. dollars to bail out the financial system. This direct

intervention by public authorities, which we refer to as the Paulson plan, raises many questions: Was taxpayers' money usefully invested? Did the Paulson plan help troubled financial institutions? Was the choice of recipients adequate? Did this intervention affect competition among financial industry participants?

The academic community has produced rapid evidence to address several of these questions, but existing studies do not address the potential competitive distortion effects of the Paulson plan. To investigate this issue, we assess investors' reactions to the initial Paulson plan announcement and 250 subsequent capital purchase transactions, in terms of wealth effects ($CARs$), probability of default (DD), and the long-term abnormal performance of Paulson plan recipients. Our results show (1) a positive relationship between recipients' $CARs$ around the initial Paulson plan announcement and their size, (2) a positive relationship between recipients' $CARs$ and capital purchase transactions relative to size (the capital purchase amount divided by the recipient market value) for subsequent capital injections, (3) a positive relationship between the recipients' DD variation and their size around the initial announcement date, and, (4) for subsequent capital injections, a positive relationship between DD variation and capital purchase transaction relative size. We also find that (5) small recipients displayed a negative long-term abnormal performance, which is not the case for large beneficiaries.

Our results provide evidence that the Paulson plan was anything but neutral with respect to financial industry participants. Large players were the winners.

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Table 1. CPP Descriptive Statistics

Table 1 provides information about capital purchases under the Paulson plan (U.S. Treasury–FDIC joint plan announced on October 13, 2008). Panel A reports, by month, the number of banks involved and the mean, median, minimum, maximum, and total funding. Panel B displays the repartition by recipient type.

Panel A: Funding

		Amounts (million USD)				
Period	**Number of recipients**	**Mean**	**Median**	**Minimum**	**Maximum**	**Total**
10/2008	8	15,625.0	17,500.0	2,000.0	25,000.0	125,000.0
11/2008	44	859.7	182.3	11.4	6,599.0	37,827.2
12/2008	161	153.8	27.0	1.5	7,579.2	24,768.5
1/2009	146	53.7	11.7	1.0	3388.9	7,845.3
2/2009	107	13.5	6.9	0.3	116.0	1,449.4
3/2009	65	31.0	7.5	0.4	1,224.6	2,013.5
4/2009	33	7.6	4.2	1.3	60.0	250.8
5/2009	48	8.5	5.8	1.1	25.0	408.3
6/2009	35	108.0	6.9	1.6	3,400.0	3,778.5
7/2009	13	87.7	11.0	2.5	950.0	1,140.4
8/2009	10	13.0	7.4	0.0	50.2	129.9
9/2009	13	9.8	7.0	1.5	52.0	126.8
10/2009	5	10.3	6.3	4.0	22.3	51.4
11/2009	5	5.6	6.0	0.0	10.8	27.9
12/2009	13	6.3	4.5	1.3	22.0	82.3
Total	706	290.2	11.0	0.0	25,000.0	204,900.0

Panel B: Recipients

Type	Number	Percentage
Public bank	285	40.4%
Private bank	399	56.5%
Community development bank	18	2.5%
Insurance company	2	0.3%

Table 2. Paulson Plan Announcement Date

Panel A: Event Study and Statistical Results

Panel A contains the *ACARs* around October 13, 2008, the Paulson plan announcement date. The sample consists of 249 financial institution recipients. Abnormal returns are computed using the market model as the return-generating process. The estimation window spans from day –235 to day –36, relative to the announcement date. CARs are obtained on three event windows: (–1,1), (–3,3), and (–5,5), relative to the announcement date. Columns correspond to the three event windows. Student statistics (t-stat) are calculated using abnormal return standard errors, as described in Section 2, and appear in brackets. We report the number of recipients for which *CARs* are positive in percentage (Positive%) and the number of significantly positive and significantly negative CARs at the 10% level (Sig Positive and Sig Negative). We separate the sample into large and small banks by the median market value of banks 21 days before the announcement date, and we report difference of means tests. *** Significant at 1% level, ** significant at 5% level, *significant at 10% level.

		(1) CAR(-1,1)	(2) CAR(-3,3)	(3) CAR(-5,5)
All banks	Mean	0.060***	0.083***	0.047***
		[7.59]	[9.78]	[5.45]
	Observation	249	249	249
	Positive%	57.03%	78.31%	63.45%
	Sig Positive	94	75	35
	Sig Negative	20	12	13
Large banks	Mean	0.077***	0.142***	0.103***
		[7.14]	[12.13]	[10.02]
	Observation	125	125	125
Small banks	Mean	0.044*	0.024**	-0.010
		[3.78]	[2.43]	[-0.87]
	Observation	124	124	124
Difference large-small	Mean	0.033**	0.118***	0.113***
		[2.09]	[7.73]	[7.25]

Panel B: Multivariate Analysis

Panel B displays the results of a regression of financial institution recipients' *CARs* on their size and other control variables. *CARs* are obtained as described in Panel A. Columns correspond to the three event windows. Size is the natural logarithm of the market value 21 days before the announcement date. Control variables are described in section 2.3. Coefficients are estimated using ordinary least squares. Student statistics (t-stat) are obtained using White heteroskedastic robust standard errors. F is the Fisher statistic; R^2 is the R-square coefficient. T-statistics appear in brackets under each coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) **CAR(-1,1)**	**(2)** **CAR(-3,3)**	**(3)** **CAR(-5,5)**
Constant	0.260	-0.440	-0.394
	[0.61]	[-1.01]	[-0.76]
Size	0.005	0.034***	0.036***
	[0.72]	[4.59]	[4.04]
Control Variables			
Tier 1	0.002	0.002	0.001
	[0.49]	[0.53]	[0.19]
Deposit%	-0.129	-0.173	-0.001
	[-1.19]	[-1.49]	[-0.01]
ROA	-0.490	0.212	0.451
	[-0.57]	[0.24]	[-0.42]
Leverage	-0.216	0.016	-0.231
	[-0.49]	[0.04]	[-0.49]
CEO comp	0.009	-0.009	-0.018
	[0.41]	[-0.39]	[-0.69]
Average DD	-0.008*	-0.012**	-0.018***
	[-1.65]	[-2.35]	[-2.96]
Observation	224	224	224
R^2	0.028	0.200	0.143
F	0.882	7.722***	5.135***
P(F)	0.522	0.000	0.000

Table 3. CPP

Panel A: Event Study and Statistical Results

Panel A presents the *ACARs* around capital purchase transactions for listed financial institution recipients. Abnormal returns are computed using the market model as the return-generating process. The estimation window spans from day –235 to day –36, relative to the transaction date. CARs are obtained on three event windows: (–1,1), (–3,3), and (–5,5), relative to the announcement date. Columns correspond to the three event windows. Student statistics (t-stat) are calculated using abnormal return standard errors, as described in Section 2, and appear in brackets. We report the number of recipients for which *CARs* were negative and statistically significant (10% level) and positive and statistically significant (10% level). *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) CAR(-1,1)	(2) CAR(-3,3)	(3) CAR(-5,5)
Mean	-0.013**	-0.013	-0.023***
	[-1.97]	[-1.45]	[-2.59]
Observation	250	250	250
Positive%	38.00%	42.40%	40.80%
Sig positive	18	13	10
Sig negative	36	19	17

Panel B: Multivariate Analysis

Panel B displays the results from a regression of financial institution recipients' *CARs* on capital purchase size and chronological order of bailout. The CPP relative size is the capital purchase amount divided by recipient market value 21 days before the announcement date. Chron order is an integer variable ranking capital purchase transactions by chronological order (highest value equals most recent capital purchase transaction). Dependent variables are *CARs* obtained on three event windows: (–1,1), (–3,3), and (–5,5). Column 1 reports the results for the whole sample using the 11-day event window as the dependent variable, columns 2 to 4 pertain to regressions of large banks, and columns 5 to 7 report those for small banks, classified by the median market value of banks 21 days before the announcement. Control variables are described in section 2.3. Coefficients are estimated using ordinary least squares. Student statistics (t-stat) are obtained using White heteroskedastic robust standard errors and appear in brackets under each coefficient. F is the Fisher statistic, and R^2 is the R-square coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	All Banks		**Large Banks**		**Small Banks**		
	(1) CAR(-5,5)	**(2) CAR(-1,1)**	**(3) CAR(-3,3)**	**(4) CAR(-5,5)**	**(5) CAR(-1,1)**	**(6) CAR(-3,3)**	**(7) CAR(-5,5)**
Constant	0.842*	0.532*	0.452	0.096	0.143	-0.143	1.601*
	[1.65]	[2.04]	[1.00]	[0.16]	[0.18]	[-0.18]	[1.89]
CPP relative size	0.101***	0.032	0.084	0.134***	0.026	0.017	0.081**
	[3.23]	[0.90]	[1.56]	[2.96]	[0.61]	[0.40]	[2.41]
Chron order	-0.000	-0.000	-0.001***	-0.000	0.000	0.000*	0.000
	[-0.27]	[-1.34]	[-3.42]	[-1.50]	[0.71]	[1.66]	[0.58]
Control variables							
Ln(Assets)	-0.015*	-0.010*	-0.021**	-0.014	-0.014	0.012	-0.024
	[-1.71]	[-1.74]	[-2.04]	[-1.33]	[-0.69]	[0.46]	[-1.01]
Tier 1	0.006	0.001	-0.001	0.004	0.010	0.010	0.006
	[1.16]	[0.20]	[-0.28]	[0.92]	[1.32]	[1.29]	[0.68]
Deposit%	-0.093	0.122	-0.148	-0.044	-0.025	0.034	-0.036
	[-0.80]	[1.24]	[-0.95]	[-0.29]	[-0.12]	[0.17]	[-0.21]
ROA	1.161	1.037	-0.491	2.429	-0.704	-1.356	-1.121
	[0.88]	[1.50]	[-0.38]	[1.36]	[-0.34]	[-0.56]	[-0.59]
Leverage	-0.616	-0.489*	0.106	0.072	-0.048	-0.304	-1.261
	[-1.27]	[-1.79]	[0.23]	[0.12]	[-0.06]	[-0.36]	[-1.58]
CEO comp	0.022	0.018	0.029	0.019	0.028	-0.156*	-0.066
	[0.81]	[1.56]	[1.29]	[0.73]	[0.23]	[-1.70]	[-0.55]
Average DD	-0.003	0.000	0.005	0.012*	0.004	-0.000	-0.012
	[-0.55]	[0.12]	[0.92]	[1.71]	[0.53]	[-0.04]	[-1.41]
Observation	220	113	113	113	107	107	107
R^2	0.120	0.153	0.183	0.172	0.034	0.074	0.103
F	3.183***	2.062**	2.555**	2.379**	0.382	0.863	1.803*
P(F)	0.001	0.040	0.011	0.017	0.941	0.561	0.077

Table 4. Distance to Default

The *DD* comes from Merton's (1974) model. We follow Vassalou and Xing's (2004) approach to estimate it (see Section 2.2).

Panel A: Distance to Default around the Paulson Plan Announcement Date, Multivariate Analysis

Panel A displays the results from *DD* variation regressions around October 13, 2008, the announcement date of the Paulson plan. The dependent variable is the variation of the average daily DD from different window lengths before and after the announcement date of October 13, 2008 (e.g., ΔDD5 is the change from average *DD* ratio for window (–5,–1) to the average *DD* ratio for window (1,5) relative to October 13, 2008). In Column 1, the DD variation is regressed on a constant. Columns 2 to 4 report multivariate analyses for the three window lengths. Size is the natural logarithm of the market value 21 days before the announcement date. Control variables are described in section 2.3. Coefficients are estimated using ordinary least squares. Student statistics (t-stat) are obtained using White heteroskedastic robust standard errors. F is the Fisher statistic, and $R^2$is the R-square coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) **ΔDD5**	**(2)** **ΔDD1**	**(3)** **ΔDD3**	**(4)** **ΔDD5**
Constant	0.071***	0.226	0.066	0.026
	[8.51]	[0.48]	[0.16]	[0.06]
Size		0.012	0.027***	0.025***
		[1.33]	[3.73]	[3.49]
Control variables				
Tier 1		0.001	0.001	0.005
		[0.25]	[0.16]	[1.24]
Deposit%		-0.060	-0.123	-0.116
		[-0.47]	[-1.05]	[-1.00]
ROA		-0.345	0.472	1.118
		[-0.31]	[0.60]	[1.40]
Leverage		-0.391	-0.416	-0.437
		[-0.79]	[-1.04]	[-1.06]
CEO Comp		0.060**	0.034	0.027
		[2.25]	[1.42]	[1.14]
Average DD		0.001	-0.009*	-0.010*
		[0.12]	[-1.90]	[-1.82]
Observation	211	200	200	200
R^2	0.000	0.129	0.251	0.209
F	0.000	4.077***	9.208***	7.258***
P(F)	1.000	0.002	0.000	0.000

Panel B: Distance to Default around Capital Purchase Transactions

Panel B reports the univariate results from *DD* variations around subsequent capital injections. *DD* variation is the change of the average daily DD from different sets of window lengths before and after the capital injection date (e.g., ΔDD5 is the change from average *DD* ratio for window (–5,–1) to the average DD ratio for window(1,5) relative to each capital injection date). Student statistics (t-stat) appear in brackets under each coefficient.

	ΔDD1	**ΔDD3**	**ΔDD5**
Mean	0.001	-0.014	-0.028***
	[0.16]	[-1.51]	[-2.78]
Observation	222	222	222
Positive%	49.55%	42.34%	42.79%

Panel C: Distance to Default around Capital Purchase Interventions, Multivariate Analysis

Panel C provides the results of regressions of financial institution recipients' DD variation on capital purchase size and chronological order of bailout. The dependent variable is the variation of the average daily *DD* from different sets of window lengths before and after the capital injection date (e.g., ΔDD5 is the change from average DD ratio for window (–5,–1) to the average DD ratio for window (1,5) relative to each capital injection date). The CPP relative size is the capital purchase amount divided by recipient market value. Chron order of bailout is an integer variable ranking capital purchase transactions by increasing chronological order (highest value equals most recent capital purchase transaction). Column 1 reports results for the whole sample using ΔDD5 as the dependent variable, columns 2 to 4 pertain to regressions of large banks, and columns 5 to 7 report those for small banks, classified by the median market value of banks 21 days before the announcement. Control variables are described in section 2.3. Coefficients are estimated using ordinary least squares. Student statistics (t-stat) are obtained using White heteroskedastic robust standard errors and appear in brackets under each coefficient. F is the Fisher statistic, and R^2 is the R-square coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	All Banks		**Large Banks**		**Small Banks**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
	ΔDD5	**ΔDD1**	**ΔDD3**	**ΔDD5**	**ΔDD1**	**ΔDD3**	**ΔDD5**
Constant	0.599	0.935	0.659	0.418	0.477	0.776	1.034
	[1.10]	[1.35]	[0.99]	[0.49]	[0.79]	[1.10]	[1.38]
CPP relative size	0.044**	0.029	0.064	0.107*	0.057**	0.025	0.029
	[2.43]	[0.69]	[1.36]	[1.85]	[2.46]	[0.90]	[1.07]
Chron order	-0.000	-0.001***	-0.001***	-0.000**	-0.000	0.000	0.000
	[-1.41]	[-3.83]	[-3.83]	[-2.41]	[-0.18]	[0.42]	[0.00]
Control Variables							
Ln(Assets)	-0.010	-0.011	-0.001	-0.014	-0.048**	-0.033	-0.035*
	[-0.88]	[-0.77]	[-0.70]	[-0.82]	[-2.52]	[-1.58]	[-1.69]
Tier 1	0.005	0.002	0.004	0.006	0.010	0.008	0.005
	[1.28]	[0.29]	[0.87]	[1.07]	[1.43]	[1.06]	[0.78]
Deposit%	0.038	0.060	0.072	0.037	0.149	0.099	0.111
	[0.26]	[0.35]	[0.45]	[0.18]	[0.77]	[0.49]	[0.54]
ROA	0.950	1.184	1.854	2.255	-0.206	-0.721	-0.951
	[0.84]	[1.10]	[1.47]	[1.36]	[-0.15]	[-0.41]	[-0.51]
Leverage	-0.547	-0.798	-0.593	-0.305	0.299	-0.313	-0.509
	[-1.10]	[-1.24]	[-1.00]	[-0.41]	[0.57]	[-0.49]	[-0.74]
CEO comp	0.012	-0.002	0.003	-0.002	-0.025	0.008	0.005
	[0.46]	[-0.10]	[0.15]	[-0.09]	[-0.45]	[0.137]	[0.09]
Average DD	0.000	-0.001	0.005	0.012	-0.003	-0.001	-0.005
	[0.02]	[-0.08]	[0.61]	[1.23]	[-0.26]	[-0.14]	[-0.44]
Observation	209	113	113	113	96	96	96
R^2	0.054	0.207	0.257	0.172	0.100	0.060	0.053
F	1.255	2.992 **	3.954**	2.380**	1.063	0.608	0.539
P(F)	0.264	0.003	0.000	0.017	0.398	0.787	0.842

Table 5. Long-Term Abnormal Performance

Table 5 reports the results for $CTAR$ bootstrap and industry pairs matching tests introduced in Section 3.3. $CTAR$ are obtained by regressing equally weighted (EW) and value weighted (VW) portfolio returns in excess of 1-month T-bill rate of CPP recipients on the four-factor model (see Equation 9 in Section 3.3). Columns 1 and 2 report bootstrap test results. Columns 3 and 4 report the corresponding industry pairs matching results. Column 5 and 6 show results for large and small recipients (we use the median market value as the cutoff point), under the method of industry pairs matching by equally weighted scheme. $\alpha_p$is the intercept, the measure of long-term abnormal returns. $R_{m,t} - R_{f,t}$ is the market return in excess of 1-month T-bill rate, SMB is the size premium, HML is the market-to-book premium, and MOM is the momentum premium. Student statistics (t-stat) appear in brackets under each coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	Bootstrap		**Industry Pairs Matching**		**Large Recipients**	**Small Recipients**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	EW	**VW**	**EW**	**VW**	**EW**	**EW**
α_p	-0.003***	0.001*	-0.012*	-0.005	-0.002	-0.016*
t	[-0.38]	[0.88]	[-1.72]	[-0.67]	[-0.31]	[-1.75]
t*	[-10.41]	[1.75]				
$R_{m,t} - R_{f,t}$	0.318*	0.646***	-0.079	-0.011	-0.029	-0.248
	[1.91]	[3.09]	[-0.55]	[-0.07]	[-0.17]	[-1.31]
SMB	0.555	-0.158	0.291	-0.278	0.379	0.037
	[1.65]	[-0.37]	[1.00]	[-0.87]	[1.11]	[0.10]
HML	1.268**	1.435***	1.042***	1.165***	1.38***	0.682**
	[4.26]	[3.85]	[4.07]	[4.15]	[4.59]	[2.01]
MOM	0.01	-0.182	0.155	-0.046	0.215*	0.123
	[0.08]	[-1.20]	[1.49]	[-0.40]	[1.75]	[0.89]
Observation	39	39	39	39	39	39
R^2	0.689	0.710	0.413	0.453	0.526	0.122
F	18.843***	20.781***	5.992***	7.035***	9.419***	1.177
P(F)	0.000	0.000	0.001	0.000	0.000	0.338

Table 6. Robustness Checks

Panel A. Paulson Plan Announcement Date– Cumulative Standardized Abnormal Returns

Panel A reproduces the results of Table 2, Panel A, using ASCARs to control for *CARs* heteroskedasticity (obtained by dividing abnormal returns by their corresponding standard deviation) and portfolio cumulative (standardized) abnormal returns. CARs are obtained on three event windows: (–1,1), (–3,3), and (–5,5), relative to the announcement date. Columns correspond to the three event windows. Student statistics (t-stat) appear in brackets under each coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) CSAR(-1,1)	**(2) CSAR(-3,3)**	**(3) CSAR(-5,5)**
Mean	1.64***	2.19***	1.15***
	[7.87]	[9.97]	[4.87]
Observation	249	249	249
Positive%	67.87%	74.30%	63.86%

Panel B. Paulson Plan Announcement date– Jaffe (1974) and Mandelker (1974) Portfolio

Panel B uses Jaffe's (1974) and Mandelker's (1974) portfolio approach to control for event-day clustering by forming a unique portfolio of all firms subject to the event. We report results for equally weighted (EW) and value weighted (VW) portfolios on three event windows: (–1,1), (–3,3), and (–5,5), relative to the announcement date. Columns correspond to the three event windows. Student statistics (t-stat) appear in brackets under each coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) CAR(-1,1)	**(2) CAR(-3,3)**	**(3) CAR(-5,5)**
Portfolio CARs EW	6.01%***	8.29%***	4.67%***
	[16.96]	[10.02]	[3.59]
Portfolio CARs VW	7.50%***	15.02%***	7.43%***
	[11.21]	[9.63]	[3.03]

Panel C. CPP Event Study

Panel C reproduces the results of Table 3, Panel A, using ASCARs to control for *CARs* heteroskedasticity (obtained by dividing abnormal returns by their corresponding standard deviation). CARs are obtained on three event windows: (–1,1), (–3,3), and (–5,5), relative to the announcement date. Columns correspond to the three event windows. Student statistics (t-stat) appear in brackets under each coefficient. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

	(1) CSAR(-1,1)	**(2) CSAR(-3,3)**	**(3) CSAR(-5,5)**
Mean	-0.328**	-0.397**	-0.606***
	[-2.20]	[-2.07]	[-3.01]
Observation	250	250	250
Positive%	38.00%	42.40%	40.80%

Figure 1. CPP over Time

Figure 1 depicts the number of financial institutions bailed out and the total amount of funding provided under the CPP by month from October 2008 to December 2009. The left vertical axis provides the number of banks; the right vertical axis provides the aggregate funding amount in billions of U.S. dollars.

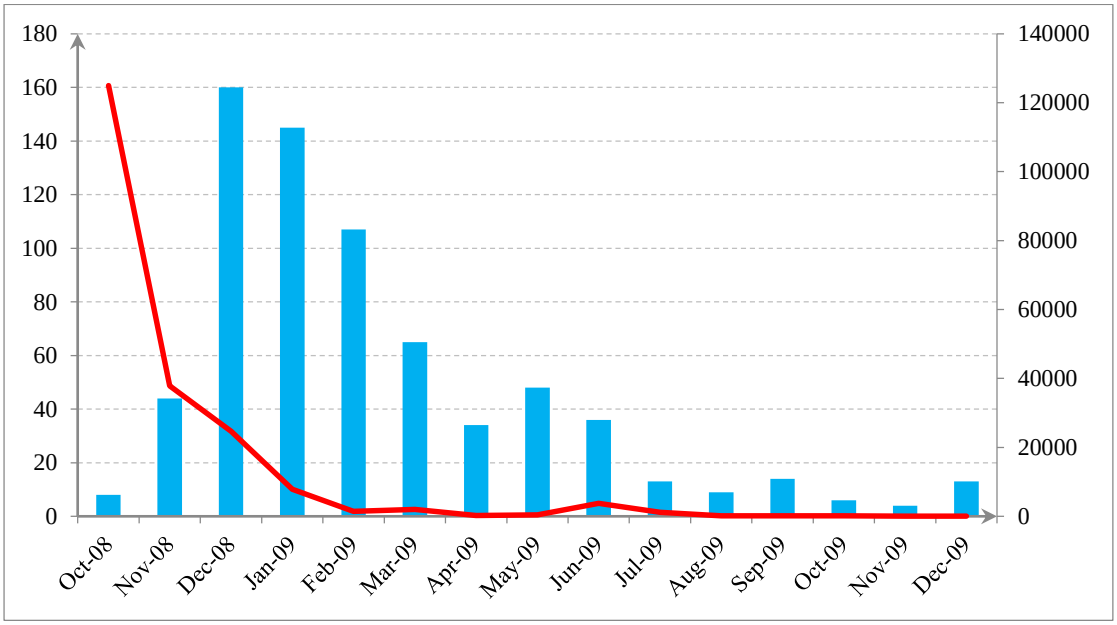


Figure 2. Variation of the DD with Respect to Recipients' Size

Figure 2 displays the recipients' size (natural logarithm of market value 21 days before the announcement date) and the corresponding variation of the *DD* (ΔDD5) around the Paulson plan's initial announcement. The red line is a linear relationship simulation between DD variation and the natural logarithm of market value (Ln(MV)) of recipients.

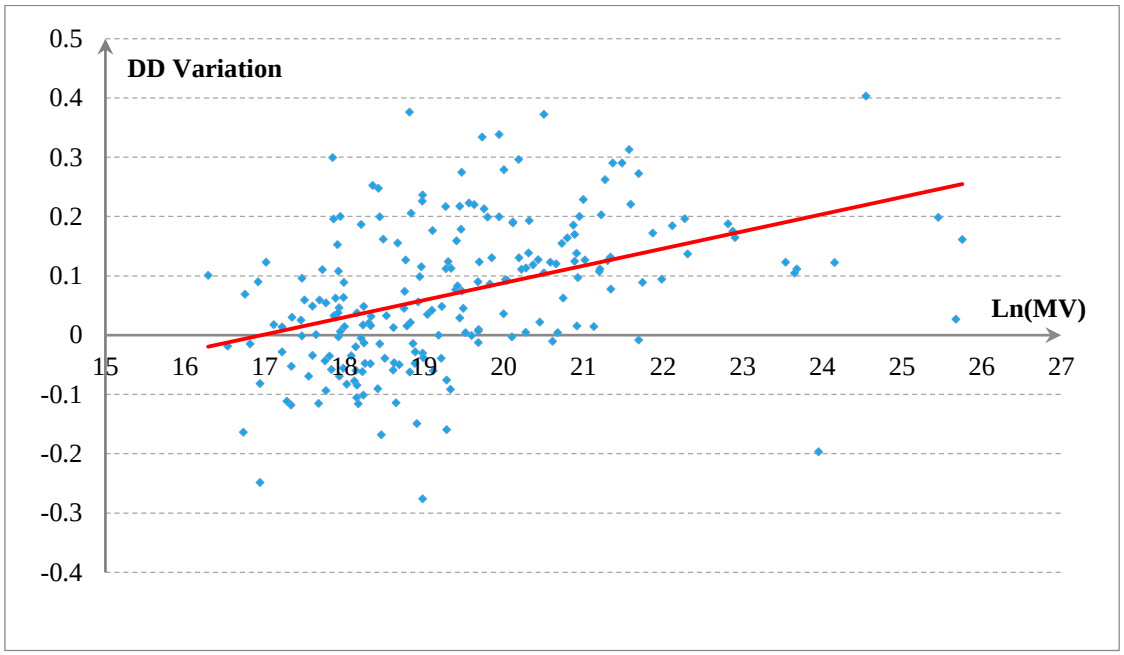
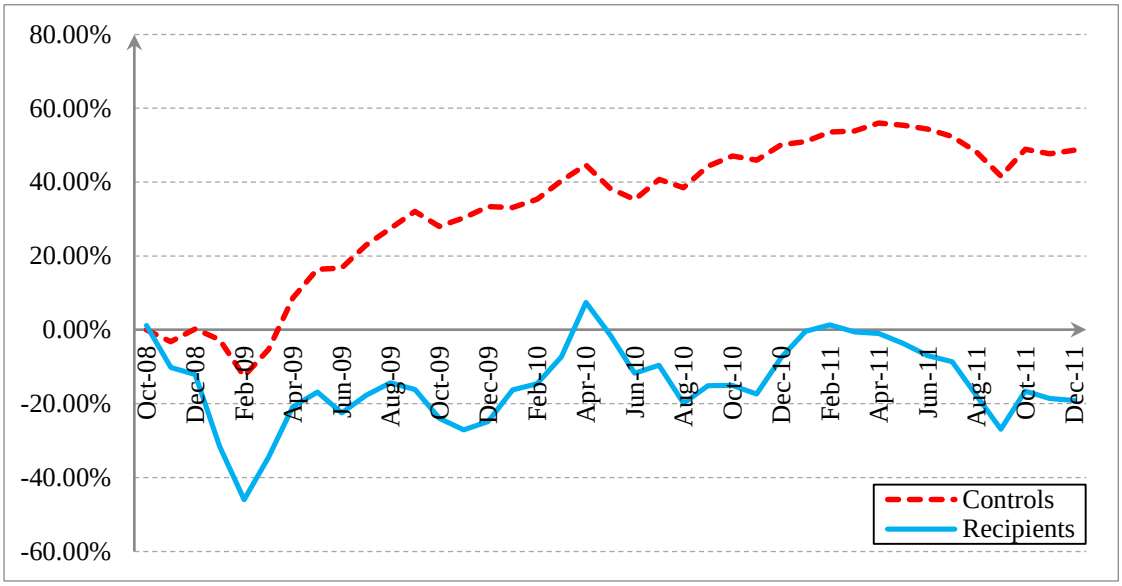


Figure 3. Long-Term Abnormal Performance

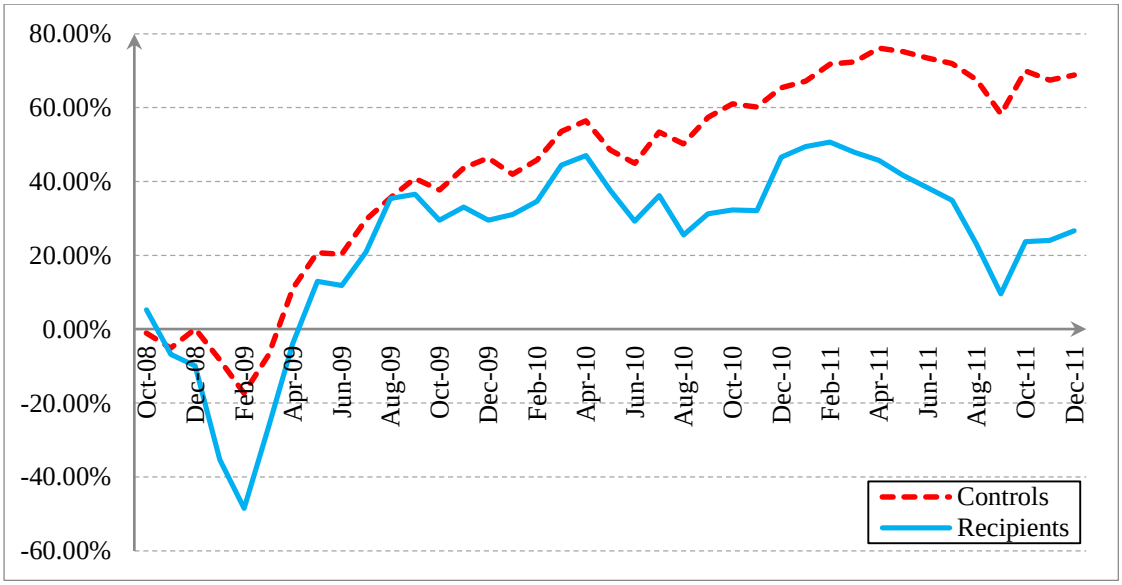
Panel A: Equally Weighted (EW) Portfolio Returns Comparison

Panel A displays two curves of EW portfolio returns. The solid line depicts the monthly returns of the CPP recipients' portfolio, and the dotted line portrays the monthly returns of the control portfolio. The period is from October 2008 to December 2011, a total of 39 months.



Panel B: Value Weighted (VW) Portfolio Returns Comparison

Panel B displays two curves of VW portfolio returns. The solid line depicts the monthly returns of the CPP recipients' portfolio, and the dotted line portrays the monthly returns of the control portfolio. The period is from October 2008 to December 2011, a total of 39 months.



CHAPITRE 4: PAULSON PLAN'S SPILLOVER EFFECT ON SYNDICATED LOANS*

* I specially thank my director Eric de Bodt for his insightful suggestions on this chapter. I also thank Michel Levasseur, Nihat Aktas, Jean-Gabriel Cousin, Helen Bolaert, and Gael Imad'eddine for their precious comments.

Abstract

This paper tests the impact of Paulson plan's capital injections on syndicated loans - the potential spillover effect of the plan to corporate borrowers. I estimate the government intervention ratio for each loan deal and investigate the effect on loan relative maturity change and relative size change. The results suggest a strong positive spillover effect during the post-crisis period from 2010 to 2012 on loan maturities. Specifically, one standard deviation increase in intervention ratio will increase the loan relative maturity change by 4.7% for the post-crisis period. However, the effect on loan relative size change is not significant. The results are robust to different calculation methods of intervention ratio and other control variables.

Keywords: Paulson plan, bailout, financial crisis, spillover, syndicated loans

JEL classification: G21, G28, G32

1. Introduction

During the recent financial crisis, banks tended to slow down their lending activity (Chava and Purnanandam 2011; Ivashina and Scharfstein 2010). As a result of the lack of sufficient financing source, corporate borrowers suffered valuation drops and the capacity of access to public debt markets did not offset these losses (Carvalho, Ferreira, and Matos 2014). The financial crisis brought a seriously negative impact to both financial institutions and the corporate sector. In October 2008, the Troubled Asset Relief Program (TARP, or the Paulson plan) was announced to restore the financial industry liquidity and solvency. Veronesi and Zingales (2010) soon report a positive effect of the initial intervention to the largest U.S. banks. Most research mainly focuses on the direct impact to the financial industry, yet the spillover effect of this plan to corporate borrowers is less investigated.

In order to discover the impact of the government intervention on the corporate sector, it is necessary to establish the firm-bank link measure. Recent studies pay attention to the syndicated loan market, because syndicated loans offer detailed information on loan terms, and more importantly the names of lenders which make the quantitative estimation of firm-bank relationship possible (Sufi 2007; Ivashina and Scharfstein 2010; Norden, Roosenboom, and Wang 2013). In this paper, I pay attention not only to the spillover effect of the bailout plan on the loan package size, but also on the loan maturity, because the latter under financial crisis is less analyzed by the past research but still an important aspect for firms financing policy (Almeida, Campello, Laranjeira, and Weisbenner 2011).

The existing research finds a positive impact of the government intervention on bank loan supply (Li 2013; Chu, Zhang, and Zhao 2014), bank risk-taking incentives (Duchin and Sosyura 2014; Black and Hazelwood 2012) and on corporate borrowers' daily stock returns (Norden, Roosenboom, and Wang 2013). However, little attention is paid to the spillover effect for the post-crisis period. Because the syndicated loan itself needs time to complete, it is possible that the injected funds are not used immediately by banks into the loans to corporate borrowers. By extending the sample period to the post-crisis years, I measure the loan maturity and size by the relative maturity change and relative size change comparing to the pre-crisis (2004Q1 to 2007Q2) industry average levels for both

the crisis-period (2008-2009) and the post-crisis period (2010-2012), and calculate the intervention ratios which quantify the level of influence of the government intervention on borrowing firms through bank lenders. The empirical tests show a strong positive and significant relationship between the intervention ratio and the relative maturity change for the post-crisis period, but the impact of the government intervention is not significant on the relative loan size change. This implies that a positive and lagged spillover effect is detected for the loan maturity, but not for the loan size. Alternative measures of intervention and other robustness checks confirm these results.

This paper mainly contributes to the topic of the Paulson plan's spillover effect on the corporate sector. To the best of my knowledge, this is the first analysis of the bailout plan's effect on syndicated loans including the post-crisis period by highlighting the impact on loan maturity. Second, it complements the literature of the government intervention into the banking sector by investigating its potential impact on the corporate sector. Third, it also relates to the broad literature of the financial crisis, which affects seriously the players in the real economy.

The rest of this paper is structured as follows: section 2 introduces briefly the background of the Paulson plan; section 3 reviews the related literature; hypotheses are described in section 4 and section 5 presents the methodologies; in section 6, I portray the data collection and sample filtering procedure; the empirical results are illustrated in section 7; section 8 shows robustness checks and the last section concludes.

2. The Paulson plan's background[36]

The initial authorization of the Troubled Asset Relief Program (TARP) was signed on Oct 3, under the Emergency Economic Stabilization Act of 2008 (EESA). EESA agreed on a $700 billion for the purpose of recovery of U.S. financial system's liquidity and solvency. On October 14, 2008, the secretary Henry Paulson announced a revised version of the program implementation that the Treasury directly injected to financial institutions of $250 billion under the Capital Purchase Program (CPP). This is one of the major program through the Financial Institution Support Programs, the capital injections were realized through the purchase of preferred stock or subordinated debentures in qualifying

[36] The main information of TARP and CPP is from SIGTARP, Quarterly Report to Congress, January 29, 2014

financial institutions (QFI) and it was motivated to strengthen the capital base of healthy and viable institutions and to encourage their lending activities.

The CPP's main target is to increase the capital base of financial system and to enable the lending activities to the U.S. businesses and to support the economy. Here are two dimensions: (1) ameliorate the situation of healthy but temporarily illiquid banks; (2) promote bank lending. This paper focuses on the second dimension to test whether the CPP has a positive effect through syndicated loans to the corporate borrowers.

The participation to CPP was open for potential candidate banks, and it was the U.S. Treasury who made final decision by checking the admissibility of each candidate. Once the candidate was accepted, the Treasury would purchase the preferred stock of the candidate bank and in return receive a payment of 5% dividend during the first five years and up to 9% afterwards. By the end of December 29, 2009, the Treasury had injected $204.9 billion into 707 institutions through CPP. On December 31, 2013, 621 banks had paid back the capital injection funds and had no longer outstanding principal investments in CPP but there were still $6.8 billion owned to the taxpayers until this day. The Treasury however continued to manage the investment portfolio of CPP for certain institutions and had held 22 sets of auctions to sell preferred stocks at a discounted price which resulted a loss to the Treasury.

3. Related literature

3.1 The spillover effect

The first strand of literature that relates closely to the paper is focuses on the spillover effect of bailout plans. From the macro-economic viewpoint, Bernanke and Getler (1995) point out that the central bank's behavior could affect firms from two dimensions: the *balance sheet channel* and the *bank lending channel*, the latter is realized through the financial institutions from the loan supply side. Empirically, Gokcen (2010) highlights this bank lending channel and shows that stock markets reacted more positively to those firms who had relationships with the largest nine U.S banks before the crisis. Song and Uzmanoglu (2011) reveal the discrepancy of lending behavior between unhealthy and healthy banks, such that unhealthy banks tended to offer more liquidity to high quality

borrowers but healthy banks did not. Their study offers some evidence of the heterogeneous effect of bank loans to the corporate sector.

More recently, Li (2013) demonstrates that the TARP plan beneficiaries used one third of the received capital for their new loan offering and the rest for the repairing of their own situation. His sample is based on the balance sheet of all the banks in the U.S. banking industry, however, the balance sheet information does not reveal borrower's information and a clear firm-bank relationship measure is not feasible. One possible solution is the employment of syndicated loans, but syndicated loan market usually has large corporate borrowers and limited number of syndicate bank players. Norden, Roosenboom and Wang (2013) investigate this market and they establish a daily measurement of the government intervention score according to the pre-crisis period firm-bank relationship and updated after each CPP bank's capital injection date. Their results conclude a positive relationship between the intervention score and corporate borrowers' daily stock returns. They mention that the positive spillover effect is more detected for small, risky and bank-dependent firms. Other studies also pay attention to the spillover effect on bank risk-taking incentives. Duchin and Sosyura (2014) do not find evidence that CPP approved banks increase their credit lending relative to CPP denied banks, and they show that CPP approved banks convert to activities with more risk. Black and Hazelwood (2012) complement the bank risk-taking incentives from CPP capital injections by differentiating banks by their size, such that large (small) recipient banks took more (less) risk while originating loans relative to non-CPP banks.

3.2 The government intervention and financial crisis

The financial crisis causes panics of the public, such as bank runs, which affect further the financial institutions and accelerate the bankruptcy of troubled banks, even healthy banks would be negatively affected (Diamond and Dybvig 1983). Consequently, a bailout plan is indispensable to guarantee the market liquidity and stableness (Chari, Christiano and Kehoe 2008; Diamond and Rajan 2009; Bernstein, Hughson and Weidenmier 2010). An early stage analysis by Veronesi and Zingales (2010) focuses on the wealth effect on the initial bailout target banks. The authors conclude that on average, the net gain ranges from $86 to $109 billion for the 9 largest U.S. banks, and they point out the necessity of the government intervention which reduced the default risk of those banks.

Internationally, King (2009) investigates the bailout plans in six countries and finds that in general, the default risk was mitigated but the stock market reactions were negative following the initial announcements of the plans. Another analysis offers a review of the plan's effect covering a longer period, Farruggio, Michalak and Uhde (2013) confirm the positive stock market reactions around the initial announcement of the TARP and around the capital repayment days, but negative reactions to the capital injection days.

The impact of financial crisis to the corporate borrowers draws researchers' attention in a broader way. Campello, Graham and Harvey (2010) collect responses from a survey to more than 1000 CFOs from U.S., Europe and Asia and they demonstrate that during the crisis, constrained firms canceled or postponed their investment opportunities as a result of the difficulty of borrowing from outside. Chava and Purnanandam (2011) provides evidence by using 1998 Russian crisis as an exogenous shock to U.S banking system in order to separate the effect from the demand and the supply. They illustrate that the shock to the supply side of credit affected more negatively the borrowers who relied primarily on banking relationships than those who had access to the public-debt market. Carvalho, Ferreira and Matos (2014) expand the sample to 34 countries and confirm the adverse effect of financial crisis of 2008 to the corporate borrowers such that firms with strongest links to banks suffered the most in terms of valuation losses and investment cuts. However, their worldwide sample proposes that the access to the public-debt market did not offset these negative effects (see also Lemmon and Roberts 2010). The 2008 financial crisis also affects seriously the corporate borrowers' investment activities, Duchin, Ozbas and Sensoy (2010) mention that the credit supply shock of 2008 drew down the corporate investment level, this negative influence is mostly detected for financially constrained firms with low cash reserve and dependent on short term debt.

One concern of the crisis analysis is to identify the shock by the distinction between the credit supply from financial institutions and the credit demand from the corporate borrowers. Lemons problem exists between borrowers and lenders during the crisis (Mishkin 1991), whereas the high quality firms quit the market as a result of the average interest rate set by banks, and this implies a demand decrease for high quality borrowers; adversely, banks could deny loans to high risk firms which further draw down the supply of loans. The earlier discussion about the financial crisis of 2008 tends to consider it as an external shock to the supply side of credit. (Shleifer and Vishny 2010; Almeida,

Campello, Laranjeira and Weisbenner 2011; Ivashina and Scharfstein 2010; Ciccarelli, Maddaloli, and Peydro 2010). However, if the credit supply shock came into play as a first-order role, then it is expected that bank-dependent firms would suffer more from a drop in capital expenditure, cash holdings, and net debt issuance, and they might seek for alternative financing approaches such as the equity offerings. More recently, Kahle and Stulz (2013) do not find evident support for the dominant role of the supply shock, based on the results from difference-in-differences analyses of different subsets of firms. Alternatively, they propose that a demand shock could better explain the insignificant differences across different groups of firms. Another evidence offered by Chari, Christiano, and Kehoe (2008) is also in line with this viewpoint, they show that in the aggregate data, nonfinancial firms could support their capital expenditures by their retained earnings without asking for outside financing resources. Further, among the issued corporate debt, 80% were realized outside the banking system, such as the nonbank financial institutions.

3.3 The loan maturity

Classical theory indicates the irrelevance of debt maturity structure in a frictionless market (Modigliani and Miller 1958). However, too much debt may lead to the debt-overhang problem of the negative impact of debt pressure on the firm's choice of future investment projects, which in turn harms the firm's value. One possible solution is the use of short-term debt which offers more flexibility at the cost of renegotiation and of long term relationship maintenance with banks (Myers 1977; Barnea, Haugen and Senbet 1980), and shorter term debt can lead to lower agency cost (Leland and Toft 1996). However, long-term debt gives better protection to the borrowers and the optimal structure could be a combination of different maturities (Hart and Moore 1994; Hart and Moore 1995).

The maturity preference also relates to firm's quality and risk. Flannery (1986) points out that under information asymmetry, good firms prefer short-term debt and bad firms prefer long-term debt, this happens while the issuing cost is high enough that bad firms cannot afford. Otherwise, a pooling equilibrium will appear that all firms choose short-term debt. Diamond (1991) incorporates the liquidity risk in the model and discovers a nonlinear relationship between firm's risk and maturity choice. Firms with the highest

risk profile have no choice but short maturity debt; low-risk firms profit from forthcoming good news and take short-term debt as well; other intermediate firms choose longer maturities because their liquidity risk is more important. Empirically, the results are not conclusive. Guedes and Opler (1996) confirm that risky firms use shot-term debt; the non-monotonic relationship is indicated such that both low-risk and high-risk firms use shorter maturities (Scherr and Hulburt 2001; Stohs and Mauer 1996); Berger, Espinosa-Vega, Frame and Miller (2005) do not find evidence for the separating equilibrium, and they show that risky firms do not increase obviously their debt maturities while the information asymmetry is reduced.

A recent analysis by Almeida, Campello, Laranjeira, and Weisbenner (2011) looks at the 2008 financial crisis and the debt maturity profile. The authors find that the firms whose long-term debt matured just before the crisis decreased their investment significantly more than their counterparts. Even firms with smaller debt ratio could be affected more seriously by the financial crisis if they have a worse debt maturity structure.

4. Hypotheses development

The hypotheses in this paper are mainly based on empirical research outcomes, because there are very few theoretical predictions which combine the government intervention and bank loan terms. One crucial aim of the capital injection is to prompt bank lending which was heavily disturbed during the crisis years. If the aim of the bailout plan were attained, one would expect a positive stimulus effect of the government intervention. Li (2013) discloses that banks use one third of the injected funds to promote their lending activities after controlling the supply side from the demand side of the loan supply growth. Black and Hazelwood (2012) also show that CPP banks offer larger loans relative to their counterparts without capital injections. If the number of clients of one bank is relatively steady[37], one could expect that the growth of loans of CPP banks would benefit (in terms of number of loans and loan size) each corporate borrower on average. On the syndicated loan market, according to Sufi (2007), one firm has on average 1.12 previous syndicated

[37] Sharpe (1990) demonstrates that under asymmetric information, borrower's quality is best captured by its bank and the latter offers the best loans to their clients. It is difficult to switch to other banks even in a competitive banking industry, because other banks cannot easily attract good clients from their competitors without touching bad ones. Thus it is expected that the number of clients of a bank is relatively steady during a certain time period.

loan contracts from 1992 to 2003. Consequently, it is expected that during the 5 years from 2008 to 2012, one firm on average would have only one syndicated loan contract. It is thus logical to expect a stimulus effect on the loan size rather than on the frequency of syndicated loan originations.

Further, loans are not as fast as the stock market reactions. The process of syndicated loans takes time to complete: the borrower first contacts one or several lead banks and sign a preliminary agreement which specifies covenants of the loan, loan fees and collateral as well as the loan amount and the range for the interest rate. Then the lead banks search for potential participant banks. When participant lenders agree to join the syndicate, the agreement is finally signed by all parties (Sufi 2007). Because this procedure is not instant, it is then logical to consider that the government intervention spillover effect to the corporate sector regarding syndicated loans is lagged by time. As a result, I check the post-crisis period in order to find out the existence of the lagged effect on corporate borrowers' syndicated loan size.

H1: The government intervention on banks has a lagged and positive impact on the syndicated loan package size.

The liquidity risk is a major problem during the financial crisis, one aim and function of the government intervention is to guarantee the liquidity of money flows (Chari, Christiano and Kehoe 2008; Diamond and Rajan 2009). From the supply side, bailout banks face to less liquidity problems thanks to capital injections, and they could offer longer maturity loans. According to the results from Black and Hazelwood (2012), after the capital injections, CPP banks offered loans with longer maturities comparing to non-CPP banks[38]. Hence, it is expected that the capital injections have a positive impact on loan maturities. Similarly to the first hypothesis, a lagged effect is expected to exist on the loan maturity.

[38] This paper differs from their analysis from two dimensions. First, they use primarily the Survey of Terms of Business Lending (STBL), which aims to offer information on bank side. My sample is based on syndicated loans relying on the corporate borrower's side and measures the government intervention ratio. Second, their main point is about the TARP's effect on bank risk taking profile across large and small banks, while I emphasize the spillover effect on bank loan size and maturity.

H2: The government intervention on banks has a lagged and positive impact on the syndicated loan maturity.

5. Methodology

5.1 Maturity and size measurement

Loan terms may be dependent on several aspects: the situation of the lending banks; the borrowers' financial characteristics; the industry, because the loan terms and covenants could vary across industries; the timing of the contract, if the loan is announced during the crisis period or not. Similar to related studies which use pre-crisis data as a reference to establish their measurement, I calculate the pre-crisis average maturity at industry level, then I compare it to the crisis and post-crisis period and compute a relative maturity change, as indicated by equation (1):

$$\Delta \mathrm{M}_i = \frac{M_i - \overline{M_{i,j,pre}}}{\overline{M_{i,j,pre}}} \quad (1)$$

M_i is the maturity of loan i, and $\overline{M_{i,j,pre}}$ represents the average (value weighted by loan size) industry maturity of pre-crisis period of loan i's borrower's industry j. ΔM_i is the main variable of interest of the analysis - the relative variation comparing to pre-crisis years. This ΔM_i is calculated for both crisis and post-crisis years, as previously mentioned that the spillover effect on loans may not be contemporaneous, and the post-crisis analysis allows to detect the lagged effect. A positive (negative) ΔM_i means an increase (decrease) of loan maturity relative to pre-crisis industry average.

Since the range of loan size is extremely dispersed, the direct comparison with pre-crisis industry median loan size will create outliers especially for small loans. In order to calculate a relative variation, I convert the size value by its natural logarithm and get the relative change on the natural logarithm of loan size:

$$\Delta S_i = Ln(S_i) - Ln(\overline{S_{j,pre}}) \quad (2)$$

5.2 Government intervention ratio

The measurement of the government intervention at loan level is the following: for each deal, I count the number of lead banks satisfying the criteria that (1) the lead bank is on the list of

CPP recipients (2) the date of capital injection into this lead bank is before the loan announcement date. In order to neutralize the size effect of banks, the injection size is scaled by the pre-crisis book value of banks' equity, because the capital injections are realized mostly by the purchase of preferred stocks or the warranty[39]. The following equation shows the procedure of calculation:

$$\mathrm{IR}_{i,t} = \frac{1}{N}\sum_{j=1}^{N}\left(\frac{\sum Injection\ Size_{i,j,t}}{Total\ Equity_{i,j,2006}} \times CPP_{i,j,t}\right); \qquad (3)$$

$\mathrm{IR}_{i,t}$ represents the intervention ratio for loan i at time t. In fact, the bailout funds are not always offered once for the entirety but may be realized through two or three times, accordingly the intervention ratio varies across time. During the period from 2009 to 2012, most banks repaid partially or fully the bailout funds, however, I do not consider the repayment behavior to reduce the intervention ratio in the main analysis, because the influence of government intervention on bank lending may suddenly end up with the repayment of the funds but continues to exist[40]. N is the number of lead banks for loan i; $\sum Injection\ Size_{i,j,t}$ is the cumulative capital injection size for loan i's lead bank j at time t; the denominator is the lead bank j's book value of equity, I fix the value to the year 2006 which is before the crisis in order to minimize biases created by the crisis; $CPP_{i,j,t}$ is a dummy variable which takes the value of 1 if loan i's lead bank j has received capital injections after time t. The whole equation sums up the relative importance of the capital injections to CPP participant's equity for each syndicated loan, and is then weighted by the number of lead banks because there is no information on each lead bank's contribution, it is assumed that each leader plays an equally important role in the syndicate: the more the lead banks, the less the influence of each bank. Take one real example of syndicated loans for instance, 1-800-Flowers.com Inc is a floral and gift retail and distribution company, it signed a syndicated loan contract on April 16, 2010 for four-year maturity of $135 million. Five lead banks were present: JP Morgan, Bank of America, Wells Fargo, Capital One, and Royal bank of Scotland. However, the Royal bank of Scotland is from U.K. and did not participate in the U.S. bailout plan. The other four banks are American banks and all of them are among the list of government financial aids. For

[39] The alternative choice of scaling by banks' total assets does not change our results.
[40] This setting is relaxed in section 8, and the results still hold.

Bank of America, it received in total $25 billion until April 16, 2010, it had an equity value in year 2006 of about $109 billion, and hence the relative ratio of the government intervention into Bank of America is about 0.23. Similarly, the relative ratios are 0.64, 0.26, and 0.62 for Capital One, JP Morgan, and Wells Fargo respectively. Finally, the four relative ratios are added up to 1.75, and this value is then averaged by the number of lead banks, which is 5 in this case. The intervention ratio on this syndicated loan for 1-800-Flowers.com Inc is 0.35.

5.3 Regression model

At loan level, loan terms should be considered. Following Godlewski (2014), I take into account the number of tranches, term loan and revolving loan dummies, if the loan is secured by a guarantor, and if the loan has covenants. Ivashina and Sun (2011) show that institutions which participate into the loan amendments outperform their counterparts, I add further this element into control.

More importantly, I take into account borrower's financial characteristics, such as the relative loan size, firm's size, leverage, ROA, Tobin's q, cash holdings. Following Chava and Purnanandam (2011), I use the S&P ratings as the proxy for the bank dependence dummy, firms are considered as bank dependent if they have lower-than-investment grade (BBB) ratings or not rated. The final regression model for the analysis on loan maturity and size is disclosed in the following equation:

$$\Delta Y_i = IR_{i,t} + Postcrisis_i + (IR_{i,t} \times Postcrisis_i) + controls_i + industry_i + time_t \qquad (4)$$

ΔY_i is the dependent variable, it represents either the relative maturity change ΔM_i or the relative size change ΔS_i for syndicated loan i. $IR_{i,t}$ represents the intervention ratio for loan I contracted at time t. $Postcrisis_i$ is a dummy variable which takes value of one if the loan is signed during the period from 2010 to 2012. $(IR_{i,t} \times Postcrisis_i)$ is the interaction between the intervention ratio and the post-crisis dummy variables, it is the variable of interest which captures the difference-in-differences effect of the government intervention for the post-crisis years. Control variables are portrayed previously with

firm characteristics[41] and loan level information. Borrower's industry and loan contract time fixed effect are also included.

To better capture the spillover effect of government intervention for each year, the equation (3) is further detailed in the following two parts. The first regression takes the interactions between the intervention ratio and the crisis years of 2008 and 2009; the second regression takes the interactions between the intervention ratio and the post-crisis years of 2010, 2011 and 2012. These two regression models allows to detect more clearly the contribution to the spillover effect for each single year.

$$\Delta Y_i = IR_{i,t} + Postcrisis_i + (IR_{i,t} \times Y2008_i) + (IR_{i,t} \times Y2009_i) + Controls_i + industry_i + time_t \quad (5)$$

$$\Delta Y_i = IR_{i,t} + Postcrisis_i + (IR_{i,t} \times Y2010_i) + (IR_{i,t} \times Y2011_i) + (IR_{i,t} \times Y2012_i) + Controls_i + industry_i + time_t \quad (6)$$

6. Data and sample

The data collection starts from the bank syndicated loans which come from the Thomson One banker database. I take all loans between January 2004 and June 2007 (the pre-crisis period, see Norden, Roosenboom, and Wang 2013; Carvalho, Ferreira and Matos 2014; Ivashina and Scharfstein 2010), the loans during 2008 and 2009 crisis years and post-crisis period loans from 2010 to 2012 that match the following criteria: (1) loans for U.S. borrowers (at parent level) of non-financial firms (excluding SIC6000-6999), non-utility companies (excluding SIC4910-4940) and from non-public sectors (excluding SIC 9000-9999); (2) total loan package size no less than 1 million U.S. dollars; (3) deals with complete information of the lead banks (excluding deals with missing or undisclosed bank names); (4) borrowers (at parent level) are publicly listed companies with available financial information. Because syndicated loans usually have more than one tranche, I aggregate the tranches into a single observation at the deal level. Sufi (2007) explains the reason of this aggregation, he shows that the actual syndicated loan is contracted with the same lenders at the deal level regardless of the number of tranches, though different tranches of one deal may have different maturities and sizes, all lenders are selected collectively on those tranches but not independently; he argues further that if observations are based on the tranche level, this would create biased smaller standard deviations, and the results will be more significant. In order to minimize computation

[41] Firm accounting variables are lagged by one quarter in regressions.

biases, I calculate the deal level maturity with value weighted maturities by the size of each tranche. Finally, the sample falls into 8426 observations, among which 2987 syndicated loans were announced between the year 2008 and 2012 with all available information for regressions[42].

The choice of syndicated loan data is not arbitrary, since it offers details of the loan itself such as the loan maturity, the size of the offer, the number of tranches etc. Further, it incorporates lead bank names for the borrower which permit to make the quantitative measurement of the intervention ratio. Finally, syndicated loans are sufficiently representative: this market grows to almost 1 trillion dollars per year which occupy approximately 15% of the aggregate debt outstanding for U.S. firms; about 90% of the largest 500 corporate borrowers got involved in syndicated loans during 1994 and 2002 (Sufi 2007); the borrowers account for more than 50% in terms of market capitalization in the United States (Norden, Roosenboom, and Wang 2013).

Figure 1 plots the average loan maturity and size variation from 2004 to 2012 for 8426 loans. For the whole sample period, it reveals that the average loan size and maturity move in similar ways. The shadow area highlights the crisis period, it is evident to see a V-shape trend for both maturity and size. The maturity and size of the post-crisis period from 2010 to 2012 recovers almost to the pre-crisis period level. More specifically, the maturity drops from 4.8 years in 2007 to 3.6 years in 2008, and even worse in 2009 to the trough of about 3 years. Regarding the loan size, along with the research on recent crisis (Ivashina and Scharfstein 2010), the plot shows a significant drop of the average loan size of about 45% loss from $951 million in 2007 to $527 million in 2009.

The initial syndicated loan data leave 538 lead banks internationally, I checked the name of each lender to identify its nationality (290 U.S. banks) and status (public or private, subsidiary or parent) from online information sources, usually from the banks' websites or sometimes the SEC filings data of loans if there is ambiguous information on bank names. Lenders are reclassified to the parent bank level, because the CPP recipients list is based on parent level. The Treasury Department's website offers the list of the 707

[42] Loan covenants dummy is not included in regressions, because from 2008 to 2012, only 3 of the 2987 syndicated loans in my sample contain covenants, which may generate biased results. Nevertheless, covenants are more prevalent in pre-crisis (2004Q1-2007Q2) sample. Related research argues that covenants can strengthen the monitoring and harmonize syndicate participant banks. (see Dass, Nanda, and Wang 2012)

beneficiaries of CPP with detailed injection and repayment amount and the date of each capital transaction, by matching the name of the banks, I get a final list of 392 unique lenders with 167 U.S. banks and 45 of them are on the bailout recipients list. U.S. banks are matched with quarterly call reports issued by the Federal Financial Institutions Examination Council (FFIEC) and COMPUSTAT database for their accounting information. For non-U.S. banks, the accounting information comes from Bureau van Dijk Bankscope database.

Mergers and acquisitions during the period of the analysis indicated in other related studies should be treated as well. My aggregation procedure to parent bank level assumes that the bank is independent if it is not acquired by another entity before the loan announcement, otherwise, it is considered as part of its acquirer bank from then on.

For corporate borrowers, I glean the stock market information from CRSP and the accounting information from COMPUSTAT database. Table 1 offers the descriptions of variables with coefficients of mean, standard deviation and a range of percentiles from 5% to 95%. The average maturity from 2008 to 2012 is about 3% lower than pre-crisis period, together with Figure 1, the low maturity is driven mainly by the crisis years. However, on average, the loan size is almost at the same level comparing to the pre-crisis years, but the median value shows a decrease in size. Intervention ratio, ranges from 0 at 5% to 0.7 at 95% with an average about 0.25. At firm level, on average, about half firms are bank dependent, with total assets about $13 billion and a relative loan size of 24%. The debt structure shows that firms have on average 60% total liabilities to total assets, with a profit of 3.5% and cash holdings up to around 10%. At loan level, 21% are term loans and revolving loans occupy 77%; about 32% loans are with amendments and 3% of the loans have guarantor's protection; institutional investors engage in about 5% loan contracts; loans on average have 1.29 tranches; on average, there are about 3.4 lead banks and 7 lead banks at 95% percentile. The last four ratios are based on lead banks average ratios which are controlled in the robustness checks, the observation drops to 2914 due to data availability. It shows that at the syndicate level, the lead bank average size is about $600 billion, with 0.8% non-performing loans to total loans, and a capital ratio of 11% which is relatively safe comparing to Basel Accords and a performance of 1.6%.

7. Empirical test results

Table 2 demonstrates the statistical details of difference-in-differences analyses for relative maturity change (panel A, ΔMaturity) and relative size change (panel B, ΔSize) by subgroups of low and high (by median) intervention ratio (IR), through crisis and post-crisis period. In column 1 and 2 of panel A, for both low and high IR group during the crisis period, the relative maturity change is negative, indicating that relative to pre-crisis industry median level, the average maturity of loans decreases; for the post-crisis period, however, the relative maturity change is positive, this indicates an increase of average maturity of loans comparing with the pre-crisis industry median value. The tests of difference are reported at the bottom line, and results show significant increases of relative maturity change for both groups. This corresponds to the figure 1, which portrays a V-shape variation of the maturity. In column 3, the results suggest that high IR group loans in the crisis period has on average a significantly lower relative maturity change than low IR group, nevertheless it shows a significantly higher coefficient than the low IR group during the post-crisis period. The difference-in-differences result in the bottom part of column 3 is significant with a value of 0.17, which implies that high IR group loans have on average 17% higher relative loan maturity change than the low IR group from the crisis to the post-crisis period.

In panel B of table 2, the analysis is based on the relative size change. From column 1 to 2, results are all significant, that the relative size change is negative and significantly different from zero during the crisis period, this inverses for the post-crisis period with significant and positive coefficients. The difference for each group from the between post-crisis period and crisis period is positive and significant, these results are consistent with figure 1. However, in column 3, none of the results are significant and they are relatively much closer to zero. There is no evidence that the high IR group loans are statistically different from low IR group loans in terms of relative size change, during crisis or post-crisis period, and the difference-in-differences test does not reveal any significant result.

In sum, the difference-in-differences analysis discloses evidence of the lagged spillover effect on the loan maturity but not on the loan size. However, the separation by the median of intervention ratio offers only a partial view. Regressions results are more convincing after controlling for other effects.

7.1 Maturity of syndicated loans

The regression model on maturity is offered by equation (4) and panel A of table 3 shows the result details of this regression. Column 1, 2, and 3 report regression results with the interaction of intervention ratio and post-crisis dummy for different sets of control variables of firm and loan levels. It is obvious to see significant and positive coefficients across the three columns for the interaction. For instance, in column 3, the coefficient of the (IR × Post Crisis) interaction is 0.15, one standard deviation (0.31) increase in IR will prolong the relative maturity change by 4.7% for the post-crisis period. In the last two columns, the period dummy is further detailed for each specific year. Column 4 reports the interaction of the intervention ratio with the year 2008 and 2009, the coefficients are negative and marginally significant, suggesting that higher intervention ratio during the crisis years lead to shorter maturity loans. In column 5, results from the first three columns are separated in the three years of the post-crisis period. However, the interactions suggest that the positive and lagged effect of the government intervention is mainly driven by the year 2011 and 2012, and it shows that there is an increasing impact of the intervention ratio through time, as the coefficients of the interactions vary from 0.11 to 0.21 and their t-statistics increase as well.

Looking at the control variables, bank dependent firms have significantly shorter maturity term, this offers some evidence that high risk firms tend to have short term loans (Flannery 1986; Diamond 1991; Mitchell 1993; Ortiz-Molina and Penas 2007). Relative size also reveals a highly significant coefficient, which implies that a relative larger loan package also has a longer maturity on average. Better performing firms gain higher maturity loans. Term loans, revolving loans, loans with more tranches and more lead banks have longer maturities on average, this suggests the positive effect of lender's supervision role. Moreover, the existence of institutional investors extends also the maturity, this is in line with the positive impact of the institutional investors (Ivashina and Sun 2011). In sum, the results reported in panel A of table 3 confirm the difference-in-differences test results in table 2 and support the first hypothesis that the government intervention has a positive and lagged impact on the loan maturity.

7.2 Loan size

Panel B of table 3 reports the regression results of the impact of government intervention on loan relative size change. Similar to panel A, column 1 to 3 illustrate the results of the interaction of intervention ratio with post-crisis dummy under various sets of control variables. In column 1, post crisis dummy indicate a strong positive results which indicate that the relative size change during the post-crisis period is higher than during the crisis period, though the interaction coefficient is not significant. In column 2, after controlling for firm characteristics, none of the coefficients is evident. In column 3, loan level control variables are added, however, the interaction between the intervention ratio and the post-crisis dummy still shows an insignificant coefficient, and the post-crisis variable even inverses the positive sign in column 1. In column 4, there is no evidence that the government intervention has a significant impact on loan size during the year 2008 or 2009. Again and similarly, in column 5, throughout the three years during the post-crisis period, none of the coefficients of the interactions are significant.

At the firm level, bank independent and large firms obtain larger loans. Similar to table 3, the loan level information indicates that loans with amendments, institutional investors, more tranches and more lead banks tend to have larger loan package amount, but terms loans are smaller in size.

Overall, for the loan size, the results do not support the hypothesis of a positive impact of government intervention, neither in crisis period nor ib post-crisis period. This finding is somehow not in line with the positive stimulus effect reported by Li (2013), two reasons may explain this inconsistence: (1) he uses the balance sheet of banks while I analyze the syndicated loans; (2) syndicated loan lead banks may also be non-U.S. banks, but his sample includes only the U.S. banks; (3) I measure the impact of government intervention on loan size using a relative size change measure comparing to pre-crisis industry level while he calculates the loan supply growth. Nevertheless, my results on loan size support the findings by Duchin and Sosyura (2014) who find no significant difference between CPP approved banks and CPP declined banks.

8. Robustness checks

In this section, I check for the robustness by using alternative measurement of intervention ratios, Heckman two-stage analysis, and other types of control variables.

8.1 Intervention ratios weighted by total assets

The original calculation method of intervention ratios takes the bank's equity to scale the capital injection funds, but banks may differ one through another by their size. I rerun the regressions on loan maturity and size with this intervention ratio scaled by banks' total assets[43] of the year 2006 in table 4. In panel A, column 1 to 3 confirm the positive and significant spillover effect of government intervention through CPP banks on the loan maturity, albeit the 10% significant level. In column 4, different to panel A of table 3, only the interaction of the intervention ratio with the year 2008 is marginally significant. In column 5, results hold for the year 2011 and 2012 that the positive effect of government intervention mainly comes from these two years during the post-crisis period. The coefficients are relatively ten times larger, because bank's equity on average represents 10% of the total assets in the sample.

In panel B of table 4, the dependent variable is switched to the relative size change. Again, similar to panel B of table 3, there is no evidence that government intervention creates positive impact on loan size. None of the interactions of the intervention ratio with time dummies shows significant result.

8.2 Intervention ratios considering the repayment

In the main analyses, the capital repayment by CPP participants are not taken into account. However, this condition can be relaxed in this section. While the firm repays the injected capital, the intervention ratio is then immediately reduced by the relative amount repaid to the government, as explained by the following equation:

$$\mathrm{IR}_{i,t} = \frac{1}{N}\sum_{j=1}^{N}\left(\frac{\sum Injection\ Size_{i,j,t} - \sum Repayment\ Size_{i,j,t}}{Total\ Equity_{i,j,2006}} \times CPP_{i,j,t}\right); \quad (8)$$

[43] Norden, Roosenboom and Wang (2013) also use the total assets on the denominator.

$IR_{i,t}$ represents the intervention ratio by incorporating the repayment of loan i at time t. Based on equation (3), equation (8) adds $\sum Repayment\ Size_{i,j,t}$ on the numerator of the fraction inside the parentheses, it sums up the total amount of repayment by loan i's lead bank j until time t, and it is subtracted from the cumulative capital injections for bank j at time t.

Results are displayed in table 5. Panel A relates to the loan maturity change. In column 1, the interaction between the intervention ratio and the post-crisis dummy shows significant coefficient. However, the significance is reduced and become insignificant in column 2 and 3 after controlling for firm characteristics and loan terms. In column 4, the significant results of the interaction with crisis years' dummies in panel A of table 3 disappear. However in column 5, by separating the post-crisis dummy into each year dummy, the interactions with year 2011 and 2012 confirm the previous finding of the positive and lagged effect of the government intervention.

In panel B of table 5, regressions with relative size change across all the columns do not show significant results for the interactions between the intervention ratio and the time dummies. The only exception is located in column 5, where the interaction with the year 2012 reveals a negative and significant result, which indicates a negative government intervention impact on loan size. However, the incorporation of capital repayment is somehow less appropriate in the analysis of syndicated loans in two dimensions: (1) the government intervention impact in banks may not disappear immediately for syndicated loans which needs time to complete; (2) large amount of repayment of funds happens in the year 2009, and the intervention ratio after 2010 then becomes too small and approach to zero in 2012[44], and the instant reduction of the intervention ratio may lead to inconsistent results.

8.3 Sample selection bias

Syndicated borrowers are often large firms, the possibility of signing syndicated loan contract also varies according to borrowers' financial situations, industries and time period, hence potential problems of sample selection bias may come into play. In order

[44] For instance, the average intervention ratios considering capital repayment across the year 2008, 2009, 2010, 2011, and 2012 are 0.03, 0.20, 0.05, 0.02, and 0.01 respectively.

to mitigate this latent bias, I employ the Heckman two-stage approach. The first step predicts the probability of being syndicated borrowers, based on firm's total assets, leverage, ROA, Tobin's Q, cash holdings, and industry and time dummies. The second step replicates previous regressions by further incorporating the Heckman lambda from the first stage.

The first-stage is estimated on a firm-quarter panel from 2008 to 2012. Untabulated results disclose that large firms with less cash holdings are more probable to obtain syndicated loans. The second stage refocuses on the sample of 2987 loans and the results are presented in table 6. From column 1 to 5, the coefficients of interactions are almost the same as the results reported in column 3 of panel A in table 3. However the Heckman lambda is not significant here, implying that the sample selection bias does not materialize in the analysis of loan maturity. The results in Panel A confirm the positive and lagged effect of the government intervention on loan maturity. Panel B of table 6 reproduces the results for relative size change by Heckman analysis. Similarly, there is no evidence of the government intervention's positive impact on the loan size albeit the lambda takes significant and negative coefficients.

8.4 Fixed-time firm accounting control variables

In the baseline regressions, the borrowers' accounting control variables are quarterly based and not fixed through time. In this section, I fix the time window of borrowers' accounting control variables to pre-crisis quarters such as 2006Q4, 2007Q1, or 2007Q2. Untabulated results confirm the baseline regression results.

8.5 Syndicate size and performance

It is possible that the variation in syndicate loan maturity is mainly due to the syndicate size and performance, because firms could contact larger and better performing banks to have better loan contracts. Although the number of lead banks in the syndicate is already controlled in the previous regressions, the performance is nevertheless not considered. One practical issue raised here is the data availability, because a non U.S. private bank could possibly be inside the syndicate while there is no information disclosed for this bank. Thus the inclusion of the lender's size and performance is biased to some extent due to the lack of data. Norden, Roosenboom, and Wang (2013) complement also their

analyses with the bank level information by calculating a pre-crisis firm-bank linkage ratio, and they use this ratio as the weight for the construction of their bank level variables for each loan, such as ROA, capital ratio and total assets. These firm-bank relationship weighted ratios will however underestimate the syndicate size, because if the borrower does not have any relation with this lead bank before, the ratio then does not take into account this bank's size into the syndicate; the performance, similarly will be under or overestimated due to the omission of the banks without pre-crisis relationship. As a result, I take into account all banks with available information and calculate the average for each syndicate by the size of banks and the performance which represent to the maximum extent of the reality. Following Berger, Espinosa-Vega, Frame, and Miller (2005), I also take into account the non-performing loans as a proxy of bank's health status. Finally, there are four syndicate average bank variables, the total assets, the non-performing loans (scaled by total loans), the bank capital ratio (tier 1 and tier 2 capital scaled by risk-weighted assets), and the bank ROA.

Table 7 reports the results by considering the syndicate characteristics. In panel A, the interaction (IR × Post Crisis) displays a significant value of 0.14, similar to column 3 of panel A in table 3. While in column 2, the interactions with crisis years' dummies do not show significant results (almost significant at 10% level) after controlling for syndicate characteristics. The coefficients of the interactions with post-crisis years' dummies confirm the previous findings that the significance of the post-crisis dummy comes mainly from the year 2011 and 2012. In terms of the syndicate variables, it reveals that lead banks with higher capital ratio and ROA offer longer maturity loans.

In panel B, regressions are repeated with relative size change. As expected, I do not find evidence of the positive impact of the bailout plan on loan size. The inclusion of bank variables demonstrate that larger banks with less non-performing loans tend to offer larger loan package in size. In sum, controlling for the syndicate characteristics of lead bank size and performance confirms the previous findings of the positive effect of the government intervention on loan maturity, and no evidence of the positive effect on the loan size.

9. Conclusion

In this paper, I investigate the spillover effect of government interventions on the syndicated loans. By analyzing 8426 loans in total from 2004 to 2012 (2987 loans from 2008 to 2012) and checking the list of Capital Purchase Program (CPP) recipients, I test the government intervention effect on syndicated loans size and maturity and find: (1) there is a positive and significant impact of the government intervention for the post-crisis period on syndicated loan maturities; (2) the relationship between the government intervention and the syndicated loan size is not significant.

By extending the research to the post-crisis period, this study contributes to the literature on TARP bailout spillover effect through bank loans. It highlights the relationship between government intervention and the loan maturities, which is a crucial profile for firm's investment decision (Diamond and He 2014). Though long term debt may create underinvestment problem (Myers 1977), the positive effect of the bailout plan on loan maturities during the post-crisis period has a positive side to corporate borrowers. Because it offers more flexibility for firms to realize the optimal maturity profile by combining loans with different maturities (Hart and Moore 1995).

The use of syndicated loans has its limitation such that syndicated loan borrowers are in general large corporations, and the spillover effect on smaller and private firms is still unclear. There are also unanswered questions such that if corporate borrowers benefit from the spillover effect in the long run in terms of their performance, which could be a future research opportunity.

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Appendix: variable descriptions

Loan's information	
ΔMaturity	Relative maturity change, presented in equation (1). This ratio is measured for each loan, by the variation of the loan maturity to the pre-crisis (2004 to 2007) Fama-French 48 industry level average (by loan size) loan maturity, winsorized at 1% level.
ΔSize	Relative size change, presented in equation (2). This ratio is measured for each loan, by the variation of the loan size to the pre-crisis (2004 to 2007) Fama-French 48 industry level median loan size, winsorized at 1% level.
#Tranches	The number of loan tranches. It is measured by the natural logarithm of the number of tranches plus one in regressions.
#Lead banks	The number of lead banks for a loan. It is measured by the natural logarithm of the number of lead banks plus one in regressions.
Term loan	A dummy variable which equals to 1 if the loan is a term loan, and 0 otherwise.
Revolving loan	A dummy variable which equals to 1 if the loan is a term loan, and 0 otherwise.
Guarantor	A dummy variable which equals to 1 if the loan is a guaranteed by a guarantor, and 0 otherwise.
Amendments	A dummy variable which equals to 1 if amendments exist, and 0 otherwise.
Institution	A dummy variable which equals to 1 if institution investors exist, and 0 otherwise.
Intervention Ratios	
IR	The government intervention ratio on syndicated loans, see equation (3)
Time dummies	
Post Crisis	A dummy variable which equals to 1 if the loan is announced during the post-crisis years from 2010 to 2012
Year dummies	Year dummies for each year from 2008 to 2012, with value 1 if the announcement year is the dummy year.
Firm Characteristics	
Bank dependence	A dummy variable which equals to 1 if the firm has a credit rating above investment grade (BBB by S&P), it equals to 0 if the firm is rated below the investment grade or not rated.
Relative size	The weight of loan size scaled by the firm's total assets. Quarterly data lagged by one quarter in regressions.
Total assets	Firm's total assets. It is measured by the natural logarithm of total assets in regressions.
Leverage	The ratio of total liabilities over total assets, winsorized at 1% level.
ROA	The ratio of EBITDA over total assets, winsorized at 1% level.
Tobin's Q	The ratio of market value of firm's assets over book value of assets, winsorized at 1% level.
Cash holdings	The ratio of cash and short term investments over total assets, winsorized at 1% level.
Syndicate Characteristics	
Bank total assets	The natural logarithm of the average lead banks' total assets inside the syndicate
Bank non-performing loans	The average ratio of lead banks' non-performing loans to total loans inside the syndicate
Bank capital ratio	The average ratio of lead banks' tier 1 and tier 2 capital over risk-weighted assets
Bank ROA	The average ratio of lead banks' ROA

Table 1 Descriptive statistics of variables

This table introduces the descriptive statistics of variables of the sample of 2987 syndicated loans. The statistics include the mean, the standard deviation (sd), the different level of percentiles (from 5% to 95%) and the number of observations (obs). ΔMaturity is the relative maturity change comparing with pre-crisis industry median maturity; ΔSize is the relative loan size change comparing with pre-crisis industry median loan size; Total Assets are in billions of dollars; details of other variables are presented in appendix. Bank level variables are only treated in robustness checks, and due to data availability, the observation drops to 2914.

Variables	mean	sd	p5	p25	p50	p75	p95	obs
ΔMaturity	-0.034	0.319	-0.581	-0.258	-0.004	0.178	0.480	2987
ΔSize	0.000	1.255	-3.157	-2.127	-0.854	0.062	0.859	2987
IR (Intervention ratio)	0.249	0.314	0.000	0.068	0.222	0.295	0.716	2987
Bank dependence	0.487	0.500	0.000	0.000	0.000	1.000	1.000	2987
Relative size	0.238	0.229	0.015	0.077	0.171	0.328	0.689	2987
Total Assets ($ billion)	13.32	57.35	0.186	0.703	1.870	5.718	43.62	2987
Leverage	0.588	0.223	0.244	0.442	0.572	0.711	0.999	2987
ROA	0.035	0.026	-0.005	0.022	0.034	0.047	0.076	2987
Tobin's Q	1.610	0.795	0.812	1.101	1.387	1.878	3.110	2987
Cash holdings	0.096	0.107	0.002	0.020	0.061	0.128	0.322	2987
Term loan	0.209	0.407	0.000	0.000	0.000	0.000	1.000	2987
Revolving loan	0.772	0.420	0.000	1.000	1.000	1.000	1.000	2987
Amendments	0.319	0.466	0.000	0.000	0.000	1.000	1.000	2987
Guarantor	0.029	0.168	0.000	0.000	0.000	0.000	0.000	2987
Institution	0.048	0.213	0.000	0.000	0.000	0.000	0.000	2987
#Tranches	1.290	0.599	1.000	1.000	1.000	1.000	2.000	2987
#Lead banks	3.443	2.028	1.000	2.000	3.000	5.000	7.000	2987
Bank total assets	597.0	345.0	5.650	399.0	583.0	797.0	1200.	2914
Bank non-performing	0.008	0.004	0.003	0.006	0.009	0.010	0.019	2914
Bank capital ratio	0.111	0.024	0.078	0.111	0.114	0.118	0.125	2914
Bank ROA	0.016	0.017	0.009	0.011	0.013	0.015	0.027	2914

Table 2 - Statistical results: difference-in-differences analyses

This table reports the difference-in-differences analyses results of relative maturity change (ΔMaturity, Panel A), relative size change (ΔSize, Panel B), by subgroups of high (column 1) and low (column 2) intervention ratio groups (by the median value of IR) and the time periods (crisis period from 2008 to 2009 and post crisis period from 2010 to 2012). Column 3 reports the difference between high IR group and low IR group. The bottom part of each panel presents the difference of the post crisis period and the crisis period, and the difference-in-differences results are displayed in the last row of column 3. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel A: ΔMaturity		**(1)**	**(2)**	**(3)**
Crisis		Low IR Group	High IR Group	Difference High-Low
	Mean	-0.101***	-0.247***	-0.146***
		(-6.95)	(-18.73)	(-7.20)
	Observation	549	404	
Post Crisis		Low IR Group	High IR Group	Difference High-Low
	Mean	0.013	0.039***	0.026*
		(1.42)	(4.05)	(1.94)
	Observation	1018	1016	
		ΔLow IR Group	ΔHigh IR Group	Difference-in-Differences
Post Crisis - Crisis	Mean	0.114***	0.286***	0.172***
		(6.63)	(17.52)	(7.08)

Panel B: ΔSize		**(1)**	**(2)**	**(3)**
Crisis period		Low IR Group	High IR Group	Difference High - Low
	Mean	-0.435***	-0.504***	-0.069
		(-7.77)	(-8.03)	(-0.82)
	Observation	549	404	
Post Crisis period		Low IR Group	High IR Group	Difference High - Low
	Mean	0.245***	0.189***	-0.055
		(6.40)	(5.32)	(-1.06)
	Observation	1018	1016	
		ΔLow IR Group	ΔHigh IR Group	Difference-in-Differences
Post Crisis - Crisis	Mean	0.679***	0.693***	0.014
		(10.03)	(9.61)	(0.14)

Table 3 - Intervention impact on loan relative maturity change and loan size change
Panel A: relative maturity change
This panel reports the regression results of the intervention ratio's (IR) impact on the loan maturity. The dependent variable is the relative maturity change (ΔMaruity) to the pre-crisis industry median maturity. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel A: ΔMaturity					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-0.144**	-0.131*	-0.123**	0.028	-0.124**
	(-2.10)	(-1.88)	(-1.98)	(1.46)	(-1.98)
Post Crisis	0.058*	0.049	0.016		
	(1.85)	(1.57)	(0.53)		
IR × Post Crisis	0.180**	0.160**	0.152**		
	(2.51)	(2.22)	(2.34)		
IR × Year 2008				-0.243*	
				(-1.69)	
IR × Year 2009				-0.139*	
				(-1.92)	
IR × Year 2010					0.111
					(1.52)
IR × Year 2011					0.146**
					(2.19)
IR × Year 2012					0.206***
					(2.80)
Firm characteristics					
Bank dependence		-0.047***	-0.034***	-0.034***	-0.034***
		(-3.90)	(-2.86)	(-2.86)	(-2.87)
Relative size		0.198***	0.101***	0.101***	0.100***
		(6.35)	(3.18)	(3.16)	(3.16)
Total Assets		0.001	-0.010*	-0.010*	-0.010*
		(0.14)	(-1.80)	(-1.81)	(-1.85)
Leverage		0.024	-0.004	-0.005	-0.005
		(0.80)	(-0.16)	(-0.18)	(-0.18)
ROA		0.448*	0.485**	0.478**	0.498**
		(1.90)	(2.11)	(2.08)	(2.17)
Tobin's Q		-0.014*	-0.010	-0.010	-0.010
		(-1.76)	(-1.29)	(-1.28)	(-1.34)
Cash holdings		0.001	0.025	0.024	0.025
		(0.01)	(0.44)	(0.42)	(0.45)
Loan terms					
Term loan			0.364***	0.364***	0.367***
			(5.68)	(5.67)	(5.75)
Revolving loan			0.305***	0.305***	0.306***
			(4.84)	(4.84)	(4.89)
Amendments			-0.017	-0.017	-0.018*
			(-1.62)	(-1.60)	(-1.66)
Guarantor			-0.002	-0.002	-0.001
			(-0.08)	(-0.09)	(-0.05)
Institution			0.198***	0.198***	0.197***
			(6.88)	(6.88)	(6.86)
#Tranches			0.050*	0.050*	0.049*
			(1.73)	(1.73)	(1.69)
#Lead banks			0.079***	0.079***	0.079***
			(5.83)	(5.83)	(5.81)
Intercept	-0.285*	-0.320*	-0.546***	-0.545***	-0.544***
	(-1.91)	(-1.68)	(-2.87)	(-2.86)	(-2.86)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.335	0.355	0.416	0.416	0.417
Observation	2987	2987	2987	2987	2987

Panel B: relative size change

This table reports the regression results of the intervention ratio's (IR) impact on the loan size. The dependent variable is the relative size change (ΔSize) to the pre-crisis industry median size. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel B: ΔSize					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-0.103	0.119	0.139	0.097**	0.139
	(-0.29)	(0.64)	(0.76)	(2.02)	(0.76)
Post Crisis	0.333**	-0.091	-0.169**		
	(2.33)	(-1.07)	(-2.12)		
IR × Post Crisis	0.296	-0.053	-0.043		
	(0.82)	(-0.28)	(-0.23)		
IR × Year 2008				-0.120	
				(-0.34)	
IR × Year 2009				0.066	
				(0.31)	
IR × Year 2010					-0.075
					(-0.37)
IR × Year 2011					-0.024
					(-0.13)
IR × Year 2012					-0.055
					(-0.26)
Firm characteristics					
Bank dependence		-0.153***	-0.119***	-0.119***	-0.119***
		(-4.41)	(-3.83)	(-3.83)	(-3.84)
Relative size		3.125***	2.688***	2.687***	2.687***
		(27.59)	(22.00)	(21.97)	(21.98)
Total Assets		0.695***	0.596***	0.596***	0.596***
		(18.05)	(14.31)	(14.30)	(14.28)
Leverage		-0.137	-0.082	-0.083	-0.082
		(-1.28)	(-0.85)	(-0.86)	(-0.85)
ROA		0.897	0.570	0.559	0.569
		(1.25)	(0.85)	(0.83)	(0.85)
Tobin's Q		0.033	0.029	0.029	0.029
		(1.61)	(1.60)	(1.60)	(1.61)
Cash holdings		-0.352**	-0.115	-0.117	-0.116
		(-2.14)	(-0.83)	(-0.83)	(-0.83)
Loan terms					
Term loan			-0.249**	-0.249**	-0.248**
			(-2.02)	(-2.02)	(-2.01)
Revolving loan			-0.025	-0.026	-0.024
			(-0.22)	(-0.22)	(-0.21)
Amendments			0.078***	0.078***	0.078***
			(2.62)	(2.62)	(2.62)
Guarantor			0.059	0.059	0.059
			(1.15)	(1.15)	(1.14)
Institution			0.205***	0.205***	0.205***
			(2.83)	(2.83)	(2.82)
#Tranches			0.364***	0.364***	0.365***
			(5.37)	(5.37)	(5.39)
#Lead banks			0.610***	0.610***	0.610***
			(10.02)	(10.02)	(10.05)
Intercept	-0.748	-16.265***	-15.192***	-15.190***	-15.193***
	(-1.32)	(-18.07)	(-17.02)	(-17.00)	(-16.99)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.122	0.698	0.742	0.741	0.741
Observation	2987	2987	2987	2987	2987

Table 4 - Robustness check: intervention ratio weighted by bank's total assets
Panel A: relative maturity change
This panel reports the regression results of the intervention ratio's (IR, scaled by bank's total assets) impact on the loan maturity. The dependent variable is the relative maturity change (ΔMaruity) to the pre-crisis industry median maturity. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel A: ΔMaturity					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-1.132	-1.027	-0.981	0.159	-0.982
	(-1.61)	(-1.44)	(-1.59)	(1.33)	(-1.59)
Post Crisis	0.062**	0.052*	0.019		
	(1.99)	(1.68)	(0.65)		
IR × Post Crisis	1.357*	1.204*	1.140*		
	(1.90)	(1.67)	(1.82)		
IR × Year 2008				-2.514*	
				(-1.78)	
IR × Year 2009				-1.008	
				(-1.50)	
IR × Year 2010					0.932
					(1.41)
IR × Year 2011					1.086*
					(1.72)
IR × Year 2012					1.527**
					(2.26)
Firm characteristics					
Bank dependence		-0.048***	-0.034***	-0.034***	-0.034***
		(-3.91)	(-2.88)	(-2.88)	(-2.88)
Relative size		0.198***	0.101***	0.101***	0.101***
		(6.36)	(3.19)	(3.17)	(3.17)
Total Assets		0.001	-0.010*	-0.010*	-0.010*
		(0.16)	(-1.77)	(-1.78)	(-1.81)
Leverage		0.024	-0.005	-0.006	-0.005
		(0.79)	(-0.18)	(-0.20)	(-0.19)
ROA		0.449*	0.486**	0.477**	0.499**
		(1.90)	(2.11)	(2.07)	(2.17)
Tobin's Q		-0.014*	-0.010	-0.010	-0.010
		(-1.73)	(-1.26)	(-1.24)	(-1.30)
Cash holdings		0.001	0.025	0.023	0.024
		(0.01)	(0.45)	(0.42)	(0.43)
Loan terms					
Term loan			0.363***	0.363***	0.366***
			(5.67)	(5.67)	(5.74)
Revolving loan			0.304***	0.304***	0.305***
			(4.84)	(4.83)	(4.87)
Amendments			-0.017	-0.017	-0.017
			(-1.61)	(-1.60)	(-1.64)
Guarantor			-0.002	-0.002	-0.001
			(-0.07)	(-0.07)	(-0.04)
Institution			0.198***	0.198***	0.197***
			(6.87)	(6.87)	(6.86)
#Tranches			0.050*	0.050*	0.048*
			(1.73)	(1.73)	(1.67)
#Lead banks			0.079***	0.079***	0.078***
			(5.84)	(5.84)	(5.81)
Intercept	-0.285*	-0.324*	-0.550***	-0.547***	-0.546***
	(-1.91)	(-1.69)	(-2.88)	(-2.87)	(-2.87)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.335	0.355	0.416	0.416	0.416
Observation	2987	2987	2987	2987	2987

Panel B: relative size change

This panel reports the regression results of the intervention ratio's (IR, scaled by bank's total assets) impact on the loan size. The dependent variable is the relative size change (ΔSize) to the pre-crisis industry median size. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel B: ΔSize					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-0.023	0.615	0.698	0.464	0.694
	(-0.01)	(0.33)	(0.38)	(1.48)	(0.37)
Post Crisis	0.358**	-0.081	-0.155*		
	(2.53)	(-0.96)	(-1.96)		
IR × Post Crisis	1.078	-0.300	-0.234		
	(0.34)	(-0.16)	(-0.13)		
IR × Year 2008				-1.475	
				(-0.42)	
IR × Year 2009				0.398	
				(0.19)	
IR × Year 2010					-0.470
					(-0.24)
IR × Year 2011					-0.094
					(-0.05)
IR × Year 2012					-0.413
					(-0.21)
Firm characteristics					
Bank dependence		-0.154***	-0.120***	-0.120***	-0.120***
		(-4.44)	(-3.87)	(-3.87)	(-3.89)
Relative size		3.125***	2.689***	2.688***	2.688***
		(27.56)	(21.97)	(21.93)	(21.95)
Total Assets		0.695***	0.596***	0.596***	0.596***
		(18.03)	(14.31)	(14.29)	(14.26)
Leverage		-0.140	-0.086	-0.087	-0.086
		(-1.31)	(-0.89)	(-0.90)	(-0.89)
ROA		0.897	0.571	0.559	0.566
		(1.25)	(0.85)	(0.83)	(0.85)
Tobin's Q		0.033	0.029	0.029	0.029
		(1.60)	(1.59)	(1.59)	(1.60)
Cash holdings		-0.347**	-0.109	-0.111	-0.109
		(-2.11)	(-0.78)	(-0.79)	(-0.78)
Loan terms					
Term loan			-0.248**	-0.248**	-0.247**
			(-2.01)	(-2.01)	(-2.01)
Revolving loan			-0.024	-0.024	-0.023
			(-0.21)	(-0.21)	(-0.20)
Amendments			0.078***	0.078***	0.078***
			(2.62)	(2.62)	(2.63)
Guarantor			0.061	0.061	0.060
			(1.18)	(1.18)	(1.17)
Institution			0.205***	0.206***	0.205***
			(2.84)	(2.84)	(2.83)
#Tranches			0.365***	0.365***	0.367***
			(5.39)	(5.39)	(5.44)
#Lead banks			0.608***	0.608***	0.608***
			(10.01)	(10.01)	(10.06)
Intercept	-0.753	-16.267***	-15.200***	-15.197***	-15.575***
	(-1.32)	(-18.05)	(-17.01)	(-16.98)	(-18.03)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.122	0.698	0.741	0.741	0.741
Observation	2987	2987	2987	2987	2987

Table 5 - Robustness check: intervention ratio with repayments
Panel A: relative maturity change
This panel reports the regression results of the intervention ratio's (IR, considering the capital repayment) impact on the loan maturity. The dependent variable is the relative maturity change (ΔMaturity) to the pre-crisis industry median maturity. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel A: ΔMaturity					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-0.120*	-0.103	-0.092	0.006	-0.092
	(-1.66)	(-1.41)	(-1.41)	(0.34)	(-1.41)
Post Crisis	0.069**	0.057*	0.024		
	(2.24)	(1.87)	(0.82)		
IR × Post Crisis	0.127*	0.111	0.098		
	(1.73)	(1.50)	(1.46)		
IR × Year 2008				-0.220	
				(-1.54)	
IR × Year 2009				-0.079	
				(-1.05)	
IR × Year 2010					0.029
					(0.39)
IR × Year 2011					0.110*
					(1.68)
IR × Year 2012					0.233***
					(3.39)
Firm characteristics					
Bank dependence		-0.048***	-0.035***	-0.035***	-0.036***
		(-3.98)	(-2.94)	(-2.93)	(-3.02)
Relative size		0.200***	0.102***	0.102***	0.102***
		(6.37)	(3.20)	(3.19)	(3.20)
Total Assets		0.001	-0.010*	-0.010*	-0.010*
		(0.15)	(-1.76)	(-1.76)	(-1.81)
Leverage		0.023	-0.005	-0.006	-0.006
		(0.78)	(-0.20)	(-0.22)	(-0.23)
ROA		0.450*	0.487**	0.478**	0.496**
		(1.90)	(2.11)	(2.07)	(2.15)
Tobin's Q		-0.014*	-0.010	-0.010	-0.010
		(-1.74)	(-1.26)	(-1.25)	(-1.28)
Cash holdings		0.004	0.027	0.026	0.027
		(0.07)	(0.49)	(0.47)	(0.49)
Loan terms					
Term loan			0.363***	0.363***	0.364***
			(5.67)	(5.66)	(5.68)
Revolving loan			0.304***	0.304***	0.304***
			(4.83)	(4.83)	(4.83)
Amendments			-0.017	-0.017	-0.017
			(-1.59)	(-1.58)	(-1.59)
Guarantor			-0.002	-0.002	-0.002
			(-0.07)	(-0.07)	(-0.07)
Institution			0.199***	0.199***	0.199***
			(6.89)	(6.89)	(6.86)
#Tranches			0.051*	0.051*	0.050*
			(1.76)	(1.76)	(1.73)
#Lead banks			0.078***	0.078***	0.078***
			(5.74)	(5.74)	(5.76)
Intercept	-0.287*	-0.324*	-0.551***	-0.550***	-0.544***
	(-1.92)	(-1.69)	(-2.89)	(-2.88)	(-2.86)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.334	0.354	0.415	0.415	0.416
Observation	2987	2987	2987	2987	2987

Panel B: relative size change

This panel reports the regression results of the intervention ratio's (IR, considering the capital repayment) impact on the loan size. The dependent variable is the relative size change (ΔSize) to the pre-crisis industry median size. Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel B: ΔSize					
	(1)	(2)	(3)	(4)	(5)
Intervention ratio					
IR	-0.359	-0.029	0.047	-0.122	0.047
	(-0.97)	(-0.15)	(0.24)	(-1.01)	(0.24)
Post Crisis	0.394***	-0.070	-0.139*		
	(2.83)	(-0.85)	(-1.80)		
IR × Post Crisis	0.125	-0.095	-0.169		
	(0.32)	(-0.44)	(-0.75)		
IR × Year 2008				0.094	
				(0.25)	
IR × Year 2009				0.181	
				(0.73)	
IR × Year 2010					-0.278
					(-1.22)
IR × Year 2011					-0.101
					(-0.43)
IR × Year 2012					-0.463*
					(-1.84)
Firm characteristics					
Bank dependence		-0.155***	-0.121***	-0.121***	-0.121***
		(-4.45)	(-3.91)	(-3.91)	(-3.89)
Relative size		3.127***	2.693***	2.693***	2.692***
		(27.63)	(22.04)	(22.03)	(22.03)
Total Assets		0.695***	0.597***	0.597***	0.596***
		(18.03)	(14.31)	(14.30)	(14.28)
Leverage		-0.142	-0.089	-0.089	-0.087
		(-1.34)	(-0.92)	(-0.93)	(-0.90)
ROA		0.890	0.568	0.562	0.544
		(1.23)	(0.85)	(0.83)	(0.81)
Tobin's Q		0.031	0.027	0.027	0.027
		(1.51)	(1.49)	(1.49)	(1.47)
Cash holdings		-0.345**	-0.107	-0.107	-0.109
		(-2.10)	(-0.77)	(-0.77)	(-0.78)
Loan terms					
Term loan			-0.245**	-0.245**	-0.244**
			(-1.99)	(-1.99)	(-1.97)
Revolving loan			-0.022	-0.022	-0.021
			(-0.19)	(-0.19)	(-0.18)
Amendments			0.076**	0.076**	0.076**
			(2.55)	(2.55)	(2.53)
Guarantor			0.064	0.064	0.064
			(1.24)	(1.24)	(1.24)
Institution			0.203***	0.204***	0.202***
			(2.80)	(2.80)	(2.78)
#Tranches			0.369***	0.369***	0.367***
			(5.49)	(5.49)	(5.48)
#Lead banks			0.607***	0.607***	0.608***
			(10.00)	(10.00)	(10.03)
Intercept	-0.766	-16.268***	-15.208***	-15.208***	-15.206***
	(-1.33)	(-18.04)	(-17.01)	(-16.99)	(-16.97)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.122	0.698	0.741	0.741	0.741
Observation	2987	2987	2987	2987	2987

Table 6 - Robustness check: Heckman two-stage analyses

This table reports the second-stage regression results of Heckman two-stage analyses about the intervention ratio's effect on the loan maturity and size. The first stage is estimated with probit regressions of the sample selection on firm's total assets, leverage, ROA, Tobin's Q, cash holdings, and industry and quarter dummies. For Panel A, the dependent variable is the relative maturity change ΔM; for panel B, the dependent variable is the relative change of the loan size (ΔSize). Column 1, 2, and 3 show the different specifications with the interaction between IR and post crisis dummy; column 4 and 5 segment the analyses by the interacting with year dummies from 2008 to 2009 (the crisis period) and 2010 to 2012 (the post crisis period). Control variables include borrower's characteristics and loan terms, detailed explanations can be traced in the appendix. Industry and quarter fixed effects are considered for each regression model. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

Panel A: ΔMaturity					
	(1)	(2)	(3)	(4)	(5)
Heckman lambda	0.047	-0.110	-0.010	-0.003	0.011
	(0.63)	(-0.21)	(-0.02)	(-0.00)	(0.02)
Intervention ratio					
IR	-0.145**	-0.131*	-0.124**	0.028	-0.124**
	(-2.11)	(-1.88)	(-1.98)	(1.45)	(-1.99)
Post Crisis	0.066**	0.032	0.014		
	(1.97)	(0.36)	(0.15)		
IR × Post Crisis	0.180**	0.160**	0.152**		
	(2.52)	(2.22)	(2.34)		
IR × Year 2008				-0.242*	
				(-1.69)	
IR × Year 2009				-0.139*	
				(-1.92)	
IR × Year 2010					0.111
					(1.52)
IR × Year 2011					0.146**
					(2.19)
IR × Year 2012					0.206***
					(2.80)
Firm characteristics					
Bank dependence		-0.047***	-0.034***	-0.034***	-0.034***
		(-3.88)	(-2.85)	(-2.84)	(-2.86)
Relative size		0.198***	0.101***	0.101***	0.100***
		(6.37)	(3.19)	(3.17)	(3.17)
Total Assets		-0.004	-0.010	-0.010	-0.010
		(-0.19)	(-0.42)	(-0.41)	(-0.40)
Leverage		0.022	-0.003	-0.003	-0.003
		(0.71)	(-0.11)	(-0.12)	(-0.10)
ROA		0.469**	0.491**	0.484**	0.500**
		(1.98)	(2.09)	(2.06)	(2.13)
Tobin's Q		-0.013	-0.010	-0.010	-0.011
		(-1.42)	(-1.09)	(-1.09)	(-1.16)
Cash holdings		0.049	0.029	0.025	0.020
		(0.21)	(0.12)	(0.10)	(0.08)
Loan terms					
Term loan			0.364***	0.364***	0.367***
			(5.68)	(5.67)	(5.74)
Revolving loan			0.305***	0.305***	0.306***
			(4.84)	(4.84)	(4.88)
Amendments			-0.017	-0.017	-0.018*
			(-1.62)	(-1.61)	(-1.66)
Guarantor			-0.002	-0.002	-0.002
			(-0.08)	(-0.09)	(-0.05)
Institution			0.198***	0.198***	0.197***
			(6.87)	(6.86)	(6.84)
#Tranches			0.050*	0.050*	0.049*
			(1.71)	(1.71)	(1.68)
#Lead banks			0.079***	0.079***	0.079***
			(5.73)	(5.73)	(5.71)
Intercept	-0.359*	-0.042	-0.519	-0.536	-0.569
	(-1.91)	(-0.03)	(-0.36)	(-0.37)	(-0.41)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.335	0.355	0.416	0.416	0.417
Observation	2987	2987	2987	2987	2987

Panel B: ΔSize					
	(1)	(2)	(3)	(4)	(5)
Heckman lambda	-7.203***	-9.346***	-7.473***	-7.463***	-7.461***
	(-25.06)	(-8.03)	(-7.10)	(-7.08)	(-7.04)
Intervention ratio					
IR	-0.034	0.090	0.110	0.101**	0.110
	(-0.12)	(0.47)	(0.59)	(2.11)	(0.59)
Post Crisis	-0.879***	-1.439***	-1.231***		
	(-7.05)	(-7.02)	(-6.64)		
IR × Post Crisis	0.250	-0.017	-0.010		
	(0.90)	(-0.09)	(-0.05)		
IR × Year 2008				-0.114	
				(-0.32)	
IR × Year 2009				0.027	
				(0.13)	
IR × Year 2010					-0.046
					(-0.23)
IR × Year 2011					0.000
					(0.00)
IR × Year 2012					0.001
					(0.01)
Firm characteristics					
Bank dependence		-0.142***	-0.110***	-0.110***	-0.110***
		(-4.17)	(-3.50)	(-3.50)	(-3.51)
Relative size		3.007***	2.613***	2.613***	2.612***
		(28.21)	(23.11)	(23.08)	(23.10)
Total Assets		0.252***	0.247***	0.247***	0.247***
		(4.00)	(4.46)	(4.48)	(4.44)
Leverage		-0.298***	-0.216**	-0.216**	-0.216**
		(-3.12)	(-2.45)	(-2.46)	(-2.45)
ROA		2.460***	1.894***	1.884***	1.896***
		(3.73)	(3.07)	(3.03)	(3.06)
Tobin's Q		0.119***	0.097***	0.097***	0.097***
		(5.64)	(5.07)	(5.07)	(5.05)
Cash holdings		3.639***	3.063***	3.058***	3.058***
		(7.03)	(6.53)	(6.50)	(6.47)
Loan terms					
Term loan			-0.279**	-0.279**	-0.277**
			(-2.29)	(-2.29)	(-2.28)
Revolving loan			-0.094	-0.094	-0.092
			(-0.81)	(-0.81)	(-0.79)
Amendments			0.086***	0.086***	0.086***
			(3.00)	(3.00)	(2.99)
Guarantor			0.066	0.066	0.066
			(1.26)	(1.26)	(1.26)
Institution			0.189***	0.189***	0.189***
			(2.84)	(2.85)	(2.83)
#Tranches			0.315***	0.315***	0.316***
			(4.82)	(4.82)	(4.83)
#Lead banks			0.555***	0.555***	0.555***
			(10.29)	(10.29)	(10.33)
Intercept	10.636***	7.732**	4.090	4.068	4.060
	(15.44)	(2.54)	(1.55)	(1.54)	(1.52)
Industry dummies	YES	YES	YES	YES	YES
Quarter dummies	YES	YES	YES	YES	YES
Adjusted R^2	0.396	0.695	0.732	0.732	0.732
Observation	2987	2987	2987	2987	2987

Table 7 - Robustness check: control for bank characteristics

This table reports the regression results of the intervention ratio's effect on the loan maturity and size, by considering the syndicate lead banks size and performance into control. For Panel A, the dependent variable is the relative maturity change ΔM; for panel B, the dependent variable is the relative change of the loan size (ΔSize). Control variables include borrower's characteristics and loan's information, details can be found in the appendix. Industry and quarter fixed effects are considered. Standard errors are robust to heteroskedasticity and firm clusters. T-statistics are indicated between parentheses below each coefficient. *, ** and *** represent significance level at 10%, 5% and 1% respectively.

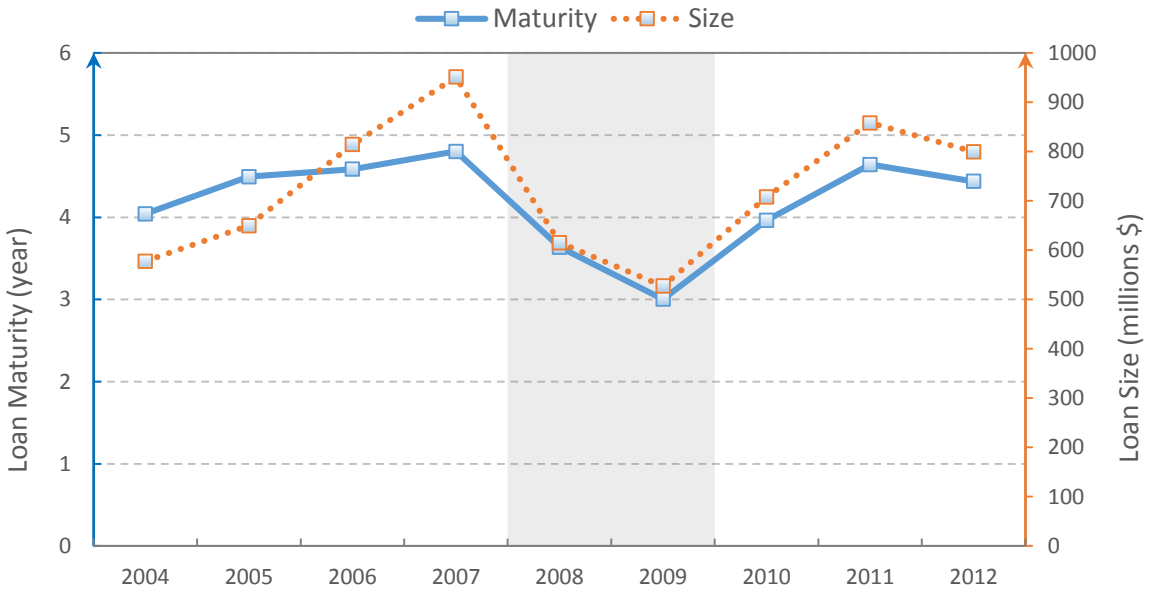
Panel A: ΔMaturity			
	(1)	(2)	(3)
Intervention Ratio			
IR	-0.121*	0.019	-0.123*
	(-1.83)	(0.90)	(-1.85)
Post Crisis	0.008		
	(0.28)		
IR × Post Crisis	0.141**		
	(2.03)		
IR × Year 2008		-0.242	
		(-1.63)	
IR × Year 2009		-0.126	
		(-1.64)	
IR × Year 2010			0.091
			(1.16)
IR × Year 2011			0.134*
			(1.90)
IR × Year 2012			0.207***
			(2.69)
Firm characteristics			
Bank dependence	-0.040***	-0.040***	-0.040***
	(-3.31)	(-3.30)	(-3.33)
Relative size	0.101***	0.101***	0.100***
	(3.19)	(3.17)	(3.17)
Total Assets	-0.011*	-0.011*	-0.011*
	(-1.90)	(-1.90)	(-1.95)
Leverage	-0.012	-0.013	-0.013
	(-0.45)	(-0.46)	(-0.46)
ROA	0.536**	0.528**	0.550**
	(2.23)	(2.19)	(2.28)
Tobin's Q	-0.011	-0.011	-0.012
	(-1.49)	(-1.47)	(-1.54)
Cash holdings	-0.004	-0.006	-0.004
	(-0.07)	(-0.10)	(-0.08)
Loan terms			
Term loan	0.360***	0.360***	0.363***
	(5.59)	(5.59)	(5.69)
Revolving loan	0.303***	0.302***	0.304***
	(4.82)	(4.82)	(4.88)
Amendments	-0.015	-0.015	-0.016
	(-1.46)	(-1.45)	(-1.50)
Guarantor	0.000	0.000	0.001
	(0.01)	(0.00)	(0.05)
Institution	0.203***	0.203***	0.202***
	(6.83)	(6.83)	(6.81)
#Tranches	0.029	0.029	0.028
	(1.04)	(1.04)	(0.98)
#Lead banks	0.080***	0.080***	0.080***
	(5.68)	(5.68)	(5.66)
Syndicate characteristics			
Bank total assets	0.001	0.001	0.001
	(0.27)	(0.27)	(0.29)
Bank non-performing loans	0.448	0.491	0.450
	(0.31)	(0.34)	(0.31)
Bank capital ratio	0.501*	0.498*	0.532*
	(1.81)	(1.79)	(1.94)
Bank ROA	0.923**	0.927**	0.926**
	(2.26)	(2.27)	(2.28)
Intercept	-0.620***	-0.619***	-0.623***
	(-2.77)	(-2.76)	(-2.78)
Industry dummies	YES	YES	YES
Quarter dummies	YES	YES	YES
Adjusted R^2	0.420	0.420	0.421
Observation	2914	2914	2914

Panel B: ΔSize

	(1)	(2)	(3)
Intervention Ratio			
IR	0.133	0.101**	0.132
	(0.71)	(2.15)	(0.70)
Post Crisis	-0.149*		
	(-1.87)		
IR × Post Crisis	-0.032		
	(-0.17)		
IR × Year 2008		-0.005	
		(-0.02)	
IR × Year 2009		0.038	
		(0.17)	
IR × Year 2010			-0.066
			(-0.32)
IR × Year 2011			0.001
			(0.01)
IR × Year 2012			-0.077
			(-0.36)
Firm characteristics			
Bank dependence	-0.111***	-0.111***	-0.111***
	(-3.63)	(-3.63)	(-3.65)
Relative size	2.695***	2.695***	2.695***
	(22.03)	(22.01)	(22.04)
Total Assets	0.594***	0.594***	0.594***
	(14.11)	(14.10)	(14.08)
Leverage	-0.062	-0.062	-0.062
	(-0.62)	(-0.63)	(-0.62)
ROA	0.366	0.363	0.358
	(0.53)	(0.52)	(0.52)
Tobin's Q	0.023	0.023	0.023
	(1.22)	(1.22)	(1.24)
Cash holdings	-0.059	-0.060	-0.059
	(-0.41)	(-0.41)	(-0.41)
Loan terms			
Term loan	-0.307**	-0.307**	-0.307**
	(-2.38)	(-2.38)	(-2.38)
Revolving loan	-0.100	-0.101	-0.099
	(-0.83)	(-0.83)	(-0.82)
Amendments	0.067**	0.067**	0.067**
	(2.26)	(2.26)	(2.26)
Guarantor	0.084	0.083	0.082
	(1.61)	(1.61)	(1.58)
Institution	0.174**	0.174**	0.174**
	(2.36)	(2.36)	(2.36)
#Tranches	0.368***	0.368***	0.371***
	(5.47)	(5.46)	(5.53)
#Lead banks	0.593***	0.593***	0.593***
	(9.80)	(9.80)	(9.81)
Syndicate characteristics			
Bank total assets	0.039***	0.039***	0.040***
	(3.66)	(3.66)	(3.68)
Bank non-performing loans	-10.510***	-10.494***	-10.416**
	(-2.61)	(-2.62)	(-2.57)
Bank capital ratio	-0.695	-0.696	-0.724
	(-1.27)	(-1.27)	(-1.32)
Bank ROA	0.836	0.838	0.841
	(0.67)	(0.67)	(0.68)
Intercept	-15.918***	-15.918***	-15.925***
	(-16.71)	(-16.70)	(-16.67)
Industry dummies	YES	YES	YES
Quarter dummies	YES	YES	YES
Adjusted R^2	0.738	0.738	0.738
Observation	2914	2914	2914

Figure 1 Syndicated loan's maturity and size from 2004 to 2012

This figure plots the trend variation of annual average loan maturity (solid blue line) and size (dot orange line) from 2004 to 2012 for a sample of 8426 syndicated loans (only the first half of year 2007 is considered in the sample). The shadow area indicates the crisis period from 2008 to 2009. The maturity is expressed in number of years and the size in millions of U.S. dollars. The table below offers more detailed data for each year.



Maturity	mean	sd	p5	p25	p50	p75	p95	N
2004	4.04	1.69	1.00	3.00	4.69	5.02	6.54	1183
2005	4.49	1.61	1.00	3.93	5.00	5.06	6.81	1220
2006	4.58	1.59	1.00	4.00	5.00	5.05	6.94	1089
2007	4.80	1.73	1.00	4.02	5.01	5.83	7.07	633
2008	3.64	1.76	0.99	2.33	4.00	5.00	6.00	895
2009	3.00	1.51	1.00	2.00	3.00	3.55	5.39	609
2010	3.96	1.87	1.00	3.00	4.00	5.00	5.97	720
2011	4.64	1.30	1.83	4.21	5.01	5.01	6.38	1148
2012	4.44	1.50	1.03	3.76	5.00	5.01	6.53	929
Total	4.24	1.68	1.00	3.01	5.00	5.01	6.73	8426

Size	mean	sd	p5	p25	p50	p75	p95	N
2004	577.5	1102.2	36.0	125.0	250.0	600.0	2000.0	1183
2005	649.8	1302.8	42.3	127.5	300.0	675.0	2527.5	1220
2006	814.4	1889.5	42.6	130.0	300.0	755.0	3000.0	1089
2007	951.4	2083.0	45.0	158.9	400.0	1000.0	3250.0	633
2008	615.2	1357.1	25.0	90.0	210.0	550.0	2310.0	895
2009	527.3	1407.3	16.0	70.0	175.0	500.0	2000.0	609
2010	707.8	1319.7	40.0	125.0	327.5	800.0	2251.6	720
2011	858.0	1587.8	50.0	200.0	450.0	1000.0	2700.0	1148
2012	799.6	1528.4	50.0	150.0	350.0	850.0	2800.0	929
Total	720.9	1518.5	35.0	125.0	300.0	750.0	2500.0	8426

CHAPITRE 5: CONCLUSION

1. Synthèse et contribution

Cette thèse se concentre sur le thème de la gestion du risque, qui est au cœur des discussions dans la sphère financière, surtout après la dernière crise de 2007-2008. Nous avons bordé ce sujet à travers trois essais empiriques : (1) au niveau des firmes, nous nous sommes intéressés à l'effet d'une décision stratégique (les opérations de Fusions & Acquisitions transfrontalières) sur le risque de change ; (2) au niveau de l'État, nous avons analysé l'effet d'une intervention gouvernementale hors norme au bénéfice du secteur bancaire ; (3) et enfin, nous avons étudié les effets de bord de ce plan de sauvetage.

Le premier essai portait sur un échantillon de 250 entreprises françaises cotées. Le but était d'analyser si les Fusions & Acquisitions transfrontalières ont offert une couverture aux risques de change supporté par les acquéreurs. Les résultats ont indiqué que ces opérations transfrontalières affectent positivement l'exposition au taux de change pour les exportateurs nets. De plus, nous avons évalué l'effet de richesse pour les actionnaires autour des dates d'annonce des opérations. A l'aide des CAR calculés, nous avons mis en évidence des gains positifs et économiquement significatifs pour les acquéreurs qui ont vu leur exposition au taux de change diminuer.

Ce premier essai empirique a apporté plusieurs contributions. Pour les entreprises françaises, la couverture opérationnelle peut être un outil effectif de gestion du risque : les acquéreurs bénéficient d'une diminution de l'exposition au risque de change grâce aux F&A transfrontalières. En outre, la méthode DID nous a permis de confirmer nos résultats d'une façon plus convaincante. De plus, cette baisse de l'exposition au taux de change a bénéficié aux actionnaires des acquéreurs. Enfin, d'un point de vue plus général, nous avons proposé d'enrichir la liste des déterminants retenus habituellement dans la littérature financière pour expliquer les CAR réalisés par les acquéreurs : la couverture opérationnelle (ou la diminution de l'exposition au risque de change) est ce déterminant nouveau et important de création de valeur.

Le deuxième essai a porté notre analyse de la gestion du risque au niveau de l'État. Cette fois, le plan de sauvetage, dit Paulson, aux États-Unis a constitué notre objet de recherche. Nous avons analysé le CPP qui comprenait un apport de 250 milliards de dollars aux banques solvables à terme mais en difficulté de liquidité dans l'immédiat. L'étude des CAR et des ratios de *distance-to-default* (DD) a montré que l'impact du plan Paulson n'a pas

été neutre sur les banques bénéficiaires. Mais il a été différent suivant la taille des établissements concernés. Ainsi, le jour de l'annonce initiale du plan Paulson, la différence de CAR s'est élevée à 403 millions de dollars entre les banques du troisième quartile et celles du premier quartile (classées suivant leur valeur marchande). Enfin, en prolongeant notre étude sur une période de 36 mois, nous avons mis en évidence que les banques participantes sous performent leurs concurrentes, et que les banques de petite taille sont au cœur de cette sous-performance.

Si nous n'avons fait que confirmer l'effet positif du plan Paulson lors de son annonce initiale, la découverte de ses effets hétérogènes entre les banques participantes est notre contribution principale. Au mieux de nos connaissances, nous sommes l'un des premiers à avoir mentionné un effet de biais sur la compétition au sein du secteur bancaire, et à souligner l'importance des pressions subies par les banques de petite taille.

Le troisième essai a abordé la question des effets de bord du plan Paulson sur les pratiques d'octroi de prêts. A partir d'un échantillon de prêts syndiqués, nous avons pu détailler chaque opération en identifiant les caractéristiques de chaque emprunteur et des prêteurs engagés. Pour bien analyser l'effet de l'intervention gouvernementale, nous avons estimé la taille et la maturité des prêts syndiqués au niveau sectoriel, et avons évalué leur importance relative pour chaque prêt. Ensuite, un ratio d'intervention gouvernementale a été calculé pour chaque prêt selon le poids des banques impliquées dans le plan de sauvetage (la taille d'injection relative par rapport à la taille de la banque). Nos résultats ont montré une corrélation positive entre le ratio d'intervention et la variation relative de maturité pour la période postérieure à la crise (de 2010 à 2012). Toutefois, cette association n'est pas positive durant la période de la crise (de 2008 à 2009). Enfin, nous n'avons pas réussi à trouver de relations significatives entre l'intervention gouvernementale et la taille relative des prêts syndiqués. Ces résultats sont robustes suivant plusieurs tests recourant à des mesures différentes du ratio d'intervention et au contrôle de la performance des banques.

2. Limite et opportunité

La réalisation de ces trois essais s'est limitée au champ empirique des entreprises cotées. Le premier essai a porté sur 250 entreprises françaises de l'indice SBF250, qui représentent les plus grandes firmes du marché français. Le deuxième essai a analysé les

banques participantes du plan Paulson. Mais notre échantillon ne contenait que des banques cotées qui ne représentent qu'environ un tiers de tous les participants. Notre dernier essai a analysé des effets de bord et n'a utilisé que les données du marché des prêts syndiqués, sachant que les prêteurs y sont en général les grandes banques internationales, et les emprunteurs de très grandes firmes. De ce fait, il faut être attentif à la généralisation de nos résultats. Certes, la saisie des données concernant les firmes non cotées est difficile, mais pas irréalisable. Au moins pour le secteur bancaire américain, on a la possibilité d'accéder aux bilans de tous les établissements cotés et non cotés à partir de l'année 2000. Il y a également des plateformes et des bases de données offrant des informations sur les entreprises non financières non cotées. Cela peut être une voie de recherche pour le futur. Trois directions pourraient être explorées : (1) quel est l'effet de la couverture opérationnelle pour les entreprises non cotées ? (2) quel a été l'effet du plan Paulson pour les banques non cotées ? (3) comment le plan de sauvetage a-t-il affecté les emprunteurs privés ?

D'autres limites pourraient être dépassées. Concernant le premier essai, notre échantillon ne porte que sur la France, et nos résultats sont muets au niveau de l'Europe. Une extension envisageable est d'élargir cette analyse au continent européen. Cependant, il faudrait porter une grande attention à l'hétérogénéité qui caractérise les pays européens. Les différences culturelles et linguistiques peuvent être sources d'effets incohérents. Le deuxième essai a eu recours à des mesures indirectes de la concurrence. Il serait intéressant de recourir à d'autres ratios de compétition et d'utiliser des échantillons plus larges afin d'arriver à des conclusions encore plus convaincantes. Le troisième essai consacré aux effets de bord du plan Paulson sur les pratiques d'octroi de prêts, constitue un point de départ. Il serait intéressant non seulement d'étendre l'analyse au-delà des prêts syndiqués mais aussi d'arriver à distinguer les effets liés à l'offre et ceux liés à la demande.

La gestion du risque est fondamentale pour les firmes et pour l'État. Elle est aussi une question de recherche qui ouvre un champ d'investigation très vaste aux chercheurs. En conclusion, les trois essais empiriques de notre thèse ont mis en évidence quelques résultats nouveaux, suggèrent des moyens efficaces pour la gestion du risque de change par les firmes, et soulignent des effets de l'intervention publique comme dans le cas du plan Paulson aux États-Unis.

COMMENT L'EXPOSITION AU RISQUE AFFECTE LA VALEUR ?
LES FUSIONS TRANSFRONTALIERES
ET
LES EFFETS DU PLAN PAULSON

Résumé

La gestion du risque est un sujet primordial au niveau d'une entreprise et au niveau de l'Etat. Cette thèse investigue ce thème au travers de trois essais empiriques. Le premier essai s'intéresse à la gestion du risque de change par les entreprises. Les résultats montrent que les Fusions & Acquisitions transfrontalières offrent une couverture opérationnelle contre le risque de change. De plus, cette technique de couverture crée de la valeur pour les actionnaires. La baisse de l'exposition au risque de change (en valeur absolue) est positivement associée avec les rentabilités anormales cumulées (CAR) pour les acquéreurs. Le deuxième et le troisième essai se concentrent sur la gestion du risque au niveau de l'État - le plan de sauvetage (dit « plan Paulson ») pendant la crise financière récente aux États-Unis. Le deuxième essai confirme dans un premier temps l'effet positif du plan sur les CAR et la réduction des probabilités de défaut pour les banques participantes autour de son annonce initiale. Pourtant, l'effet du plan n'est pas neutre au sein des banques : les grands joueurs ont été les gagnants. Le dernier essai analyse les effets de bord du plan Paulson sur les pratiques d'octroi de prêts. Nos résultats révèlent qu'il y a un effet de bord positif et significatif de l'intervention gouvernementale sur la maturité des prêts syndiqués pendant la période postérieure à la crise, de 2010 à 2012. Néanmoins, cet impact positif n'apparaît pas pour la taille des prêts syndiqués. En somme, cette thèse empirique met en lumière d'une part les moyens efficaces d'une gestion du risque de change pour les entreprises, comme par exemple la couverture opérationnelle, et ses implications positives pour les actionnaires des acquéreurs, et d'autre part les résultats attendus d'une intervention de l'Etat, comme celle du plan Paulson, les effets de bord et les effets hétérogènes entre les établissements financiers.

Mots clefs français : gestion du risque, risque de change, crise financière, plan de sauvetage, intervention gouvernementale, effets de bord, prêts syndiqués, maturité de la dette

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HOW THE RISK EXPOSURE AFFECTS THE VALUE?
THE CROSS-BORDER MERGERS
AND
THE PAULSON PLAN'S EFFECTS

Abstract

The risk management is a crucially important topic at firm and country level. This thesis investigates this subject across three empirical essays. The first essay is interested in firm's currency risk management. The results show that the cross-border Mergers & Acquisitions offer an operational hedging to the currency risk. Moreover, this hedging creates value for acquirers' shareholders. The decrease in currency risk exposure (in absolute value) is positively associated with acquirers' cumulative abnormal returns (CAR). The second and the third essay concentrate on the risk management at country level - the bailout plan (the Paulson plan) during the recent financial crisis in United States. The second essay in a first step confirms the positive effect of the plan on bank participants' CAR and on the decrease of default probability around the plan's initial announcement. Nevertheless, the effect of the plan is not neutral among banks and big players were the winners. The last essay analyzes the Paulson plan's spillover effect on the loan supply. Our results reveal that there is a positive and significant spillover effect of the government intervention on syndicated loans' maturities during the post-crisis period from 2010 to 2012. However, this positive impact is not found for the size of syndicated loans. In sum, this empirical thesis from one side sheds light on the efficient ways of firm's currency risk management, such as the operational hedging, and its positive effect for acquirers' shareholders; from another side for the country-level government intervention, it highlights the Paulson plan's spillover effect on syndicated loans and the heterogeneous impact among financial institutions.

Keywords: Risk management, currency risk, financial crisis, bailout plan, government intervention, spillover effect, syndicated loans, debt maturity

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