Essays in banking and corporate finance

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Résumé français

Cette thèse est structurée autour de deux thèmes différents: l'activisme politique des entreprises et l'intégration financière à travers les régions par le biais de l'entrée des banques. Le premier chapitre examine les motifs de l'activisme politique des entreprises en mettant l'accent sur l'accès au financement. Les deux derniers articles étudient l'intégration bancaire avec l'ambition de comprendre ses effets sur l'économie réelle en utilisant les déréglementations bancaires aux États-Unis entre 1978 et 1997.

L'effet d'offre de crédit sur des liens politiques: Les preuves provenant des États-Unis

Peu de débats attirent l'attention égale des universitaires, des décideurs politiques et du public autant que l'activisme politique des entreprises. Les contributions de campagne par des sociétés ont été au centre de cette discussion ces dernières années, suite aux décisions judiciaires qui ont assoupli les contraintes sur les dépenses des sociétés pour des élections (Citizens United v. FEC, 2010; McCutcheon v. FEC, 2014). Le débat porte sur la question de savoir si les contributions de campagne influent sur le comportement des politiciens en faveur des sociétés ou sont simplement une expression d'opinion politique protégée par la Constitution américaine.

Ce chapitre intitulé « *L'effet d'offre de crédit sur des liens politiques: Les preuves provenant des États-Unis* » examine le lien entre l'offre de crédit et l'investissement politique des entreprises non financières. Je fais l'hypothèse que les entreprises augmentent leur activisme politique quand ils font face à un choc de crédit bancaire indésirable qui est exogène. Un choc négatif de crédit bancaire peut générer une contrainte de financement immédiate ou une attente de telle contrainte dans un proche avenir. Les liens politiques peuvent aider à atténuer les contraintes de financement, principalement en augmentant l'accès aux contrats et subventions du gouvernement qui génèrent un flux de trésorerie fiable. En outre, les contrats gouvernementaux peuvent réduire le risque de crédit et attirer le financement par des banques privées. Christopher (2011) et Goldman, Rocholl et So (2013) montrent que les entreprises politiquement connectées sont plus susceptibles de recevoir des contrats gouvernementaux. Goldman (2015) suggère que le gouvernement fédéral est un client remarquablement stable et les contrats gouvernementaux agissaient comme une couverture au cours de la crise financière de 2008. Houston et al. (2014) montrent que les liens politiques réduisent les écarts de prêts et que le lien est plus fort pour les entreprises des secteurs qui dépendent davantage des contrats gouvernementaux.

Mon approche empirique repose sur la relation de crédit qui est établie entre une banque et un emprunteur au fil du temps. Comme la banque recueille des informations sur l'emprunteur, il devient coûteux pour l'entreprise emprunteuse de changer de prêteur. Ainsi, un emprunteur d'un prêteur en difficulté aurait une plus grande difficulté à obtenir un financement bancaire par rapport à un emprunteur d'un prêteur en bonne santé financière.

Ma stratégie d'identification exploite également le fait que la crise financière 2007-2008 est née sur le marché immobilier et que la première période de la crise financière fournit un choc bancaire orthogonal au secteur des entreprises. J'utilise ce choc pour isoler l'effet de l'offre de crédit de celui de la demande de crédit. Outre son caractère exogène, la crise financière offre un cadre favorable pour mettre à l'épreuve mon hypothèse pour deux autres raisons: (i) la crise a commencé à l'approche des élections législatives de 2008, lorsque l'importance des contributions électorales était la plus haute ; (ii) le comportement de prêt des institutions financières a été affecté de manière différentielle par la crise en raison des différents niveaux d'exposition à l'effondrement du marché des prêts hypothécaires à risque. J'exploite une telle variation entre les banques pour identifier empiriquement l'effet de santé des prêteurs attitrés sur les contributions de campagne par leurs emprunteurs.

Pour illustrer la stratégie d'identification, je compare la santé financière de deux banques, le dossier d'emprunt de leurs clients et l'activisme politique pendant la période de crise. Par exemple, U.S. Bancorp a eu une exposition relativement restreinte aux titres adossés à des créances hypothécaires, et ses résultats financiers ont demeurés relativement stables en période de turbulences, de sorte que cette banque a pu augmenter ses prêts au quatrième trimestre de 2008. D'autre part, Lehman Brothers a été fortement impliqué dans la titrisation et a émis une grande quantité de titres adossés à des hypothèques. Comme la confiance des investisseurs s'est érodée en 2008, Lehman a déposé son dossier de faillite selon le chapitre 11 du Code de faillite le 15 septembre 2008. Au cours de la période de neuf mois comprise entre le troisième trimestre de 2007 et le premier trimestre de 2008, U.S. Bancorp et Lehman Brothers ont réduit leurs prêts de 21% et 45% respectivement par rapport à la même période de l'année précédente. En comparant deux entreprises relativement similaires opérant dans le même secteur, Atmos Energy Corp., qui avait une relation de prêt d'avant la crise avec U.S. Bancorp, a obtenu un prêt au cours des trimestres suivants de 2008, alors que Covanta Energy Corp., qui avait une relation de prêt d'avant la crise avec Lehman Brothers, n'a pas eu de prêt pendant la même période. Pendant ce temps, Atmos Energy a augmenté ses contributions de campagne de 59 950 dollars américains dans le cycle électoral 2005-2007 à 83 900 dollars dans le cycle électoral 2007-2009 (une augmentation de 40%). Covanta Energy a contribué

11 450 et 54 750 dollars aux campagnes politiques dans les cycles électoraux 2005-2007 et 2007-2009, respectivement (une augmentation de 378%).

Plus précisément, je commence par construire un jeu de données avec les contributions de campagne et les données comptables des entreprises non financières pour les périodes 2005-2008. Parmi ces entreprises-ci, j'identifie celles qui établissent une relation de prêt d'avant crise avec au moins une banque entre le deuxième trimestre 2002 et le deuxième trimestre 2007 sur le marché de prêts syndiqués. Ensuite, je construis une mesure de prêts relationnels pour chaque paire banque–entreprise, calculée comme le rapport du nombre de prêts accordés à l'emprunteur par le syndicat dans lequel la banque donnée occupe une place prépondérante au nombre total de prêts syndiqués de l'emprunteur pendant la période de cinq ans. Suite à Chodorow-Reich (2014), je définis l'offre de crédit au niveau d'entreprise comme la moyenne pondérée des crédits globaux par des prêteurs attitrés de l'entreprise (à d'autres emprunteurs) en utilisant les mesures de prêts relationnels comme pondérations.

J'adopte un cadre des doubles différences et explique les changements dans les contributions de campagne des entreprises avant et pendant la crise financière par les changements de l'offre de crédit au niveau de l'entreprise. Cette spécification correspond à une estimation de la méthode des doubles différences avec une variable de traitement hétérogène (changement dans l'offre de prêt des prêteurs attitrés). Le coefficient d'intérêt permet d'estimer si les entreprises qui connaissent des baisses plus importantes d'offre de prêts de la part de leurs prêteurs attitrés (groupe traité) ont une augmentation plus forte de leurs contributions de campagne pendant la période de crise (période de traitement). Le moment du choc exogène de l'offre de prêts est central pour le choix de la période de traitement. Je me concentre sur la première période de la crise et mesure l'offre de prêts au cours de la période de traitement allant de Q3 2007 à Q1 2008. Je mesure les contributions de campagne entre Q2 2008 et Q4 2008, c'est-à-dire jusqu'à la fin du cycle électoral. Je calcule les variables de contrôle en même temps avec le choc de prêts bancaires, c'est-à-dire de Q3 2007 à Q1 2008. Pour la période de prétraitement, je prends en compte la saisonnalité du marché de prêts syndiqués ainsi que le cycle biennal des contributions de campagne. La méthode la plus simple permettant de tenir compte de ces modèles est de comparer des trimestres identiques dans les périodes de prétraitement et de traitement, ce qui me donne une période de prétraitement de Q3 2005 à Q4 2006.

J'ai trouvé qu'une diminution de 10% de l'offre de prêts entraîne une augmentation de 9% des contributions de campagne en 2008. Les contributions sont penchées vers des candidats puissants tels que des politiciens titulaires et des membres des comités du Congrès

pertinents à l'industrie de l'entreprise. Les contributions se penchent également vers les candidats qui pourraient être plus disposés à aider l'entreprise comme les politiciens de son état de résidence. Conformément à mon hypothèse de contrainte de financement, les entreprises qui ont un prêt dû au deuxième semestre 2008 augmentent leurs contributions de campagne de 13% supplémentaires. Au contraire, les entreprises qui émettent des obligations de sociétés pendant la période d'avant la crise diminuent leurs contributions de campagne (relativement) de 5%.

Ensuite, je présente d'autres preuves empiriques étayant ma conjecture. Une hausse d'écart-type dans les contributions de campagne est associée à une probabilité de plus de 4% d'obtenir un contrat et à une augmentation de 6,8 millions de dollars du montant contractuel pour l'entreprise médiane au cours du prochain cycle électoral (valeur médiane: 10 millions de dollars). Plus loin, une hausse d'écart-type dans le niveau des contributions antérieures est associée à une diminution des marges sur les prêts de 6 points de base et à une augmentation du montant de prêt pour l'entreprise médiane de 93 millions de dollars (valeur médiane: 112 points de base, 775 millions de dollars). Le statut d'entrepreneur du gouvernement est associé à une réduction des marges sur les prêts de 7 points de base.

Mes conclusions concordent avec l'idée que les entreprises allègent leurs contraintes de financement par leurs liens politiques. Mes résultats appuient l'idée que les contributions de campagne sont un investissement dans le capital politique plutôt qu'une simple forme de bien de consommation. Je contribue principalement à la littérature sur les mécanismes par lesquels les liens politiques affectent la valeur de l'entreprise. Des recherches antérieures montrent que les entreprises liées politiquement sont plus susceptibles d'être renflouées et de recevoir des fonds d'aide gouvernementale en cas de difficultés (Faccio, Masulis et McConnell 2006; Adelino et Dinc, 2014). Cependant, les entreprises échantillonnées dans ces études peuvent être en difficulté financière et/ou économique, ce qui rend difficile l'identification du rôle de chacune sur l'activisme politique des entreprises. Même si ces entreprises se révèlent financièrement affligées, il n'est pas clair si l'activisme politique des entreprises est entraîné par la difficulté financière en soi plutôt que les facteurs d'entreprise qui la poussent dans cette difficulté financière en premier lieu. Mon article aborde ce problème en identifiant un choc exogène de l'offre de crédit par lequel les entreprises font face à des contraintes de financement.

Intégration financière et croissance: L'exposition antérieure des banques à l'industrie importe

Au cours des quatre dernières décennies, les pays sont devenus beaucoup plus intégrés financièrement, dans de nombreux cas grâce à l'entrée de banques étrangères. Des éléments de preuve indiquent que les effets de l'intégration financière vont au-delà de la simple mise à disposition de fonds supplémentaires. En fait, un certain nombre d'articles indiquent une réaffectation du capital entre les industries suite à l'intégration financière (Fisman et Love, 2004; Acharya, Imbs et Sturgess, 2011). Cependant, nous savons peu sur les micro-mécanismes derrière la preuve macro-économique de la convergence économique observée qui suit l'intégration financière. Le chapitre deux intitulé « *Intégration financière et croissance: L'exposition antérieure des banques à l'industrie importe* » découvre un canal particulier dans ce processus de réaffectation, à savoir la collecte d'informations spécifiques à l'industrie par les institutions financières sur leur marché domestique et le transfert de ces informations lors de la fourniture de capitaux aux entreprises situées sur les nouveaux marchés qu'elles pénètrent.

Plus précisément, nous testons un canal qui fonctionne à travers l'exposition des banques commerciales à des industries plus répandues dans leur état de résidence. Notre conjecture est que l'intégration financière avec les banques hors-état qui sont mieux informés sur une industrie devrait conduire à une croissance plus rapide dans ce secteur. Nous présumons que les banques d'un état donné qui sont plus spécialisées dans une industrie donneraient naturellement plus d'importance à ce secteur en moyenne, car elles auraient plus d'informations sur le fonctionnement et les perspectives de ce secteur par rapport aux établissements qui opèrent dans des états qui sont moins spécialisés. Les informations recueillies et traitées par les banques opérant dans les industries plus éminentes (plus spécialisées) de leur état de résidence se traduiraient par la capacité de ces banques à dépister et surveiller les prêts dans ce secteur (par exemple, par l'entremise de spécialisation des agents prêteurs ou de l'utilisation de systèmes de notation de crédits propriétaires). Lorsque ces mêmes banques entrent pour la première fois dans un autre état (généralement par acquisition d'une banque locale dans leur état « de séjour » après la déréglementation de l'entrée de banques), leur exposition à l'industrie dans leur état de résidence donnerait à ces institutions de crédit un avantage naturel dans le dépistage de prêts. Cet avantage informationnel se produirait, par exemple, par le partage (avec la banque acquise) des agents prêteurs qui connaissent bien une industrie particulière ou des modèles de notation de crédit exclusifs. Ainsi, nous conjecturons que l'intégration financière peut affecter la croissance de

cette industrie différemment compte tenu de l'exposition précédente des institutions financières entrant dans le marché à la même industrie.

Nous testons cette hypothèse à l'aide des données sectorielles sur les paires d'états et d'une série d'expériences quasi-naturelles: échelonnages des déréglementations bancaires au niveau des paires d'états pendant les années 1980 et 1990. Nous procédons comme suit. Premièrement, nous définissons la spécialisation d'une industrie manufacturière dans un état donné comme le rapport de sa part de production manufacturière dans ce secteur (c'est-à-dire la valeur ajoutée) à sa part de la production manufacturière globale des États-Unis. Nous vérifions si les industries dans les états qui sont classés comme moins spécialisés dans ces secteurs croissent plus rapidement que les mêmes industries dans les états qui sont classés comme plus spécialisés. Nous n'observons aucune différence entre les mesures de croissance pour les industries situées dans les états qui sont moins spécialisés vis-à-vis de ceux plus spécialisés. Ces observations sont importantes, car nous voudrions écarter la possibilité que nos résultats soient conduits par des mesures de croissance différentielle entre les états moins spécialisés et ceux plus spécialisés. Ensuite, nous effectuons des ensembles de régressions en utilisant des variables de test et des estimateurs différents. Nous commençons par vérifier s'il y a un changement significatif dans la croissance relative sectorielle [taux de croissance différentiel entre les états dans une industrie donnée, où le premier (deuxième) état est celui moins (plus) spécialisé de la paire à la date de déréglementation interétatique effective] après la déréglementation bancaire interétatique. Notre variable de test est une variable indicatrice qui est égale à 1 à partir de l'année *quisuit* (et pour toutes les années qui suivent) l'ouverture effective par la paire d'états de leurs marchés à leurs banques respectives, et 0 sinon. Dans cette régression, la variable dépendante est au niveau de la paire d'états, de l'industrie et du temps, alors que la variable de test varie au niveau de la paire d'états. Une façon d'améliorer l'identification consiste à tenir compte de l'écart de spécialisation entre les deux états dans une industrie donnée. Une possibilité consiste à effectuer la régression après avoir classé toutes les observations relatives aux paires d'états industrie par industrie selon la différence de spécialisation dans ce secteur (à partir de la déréglementation bancaire interétatique) et à effectuer des régressions distinctes. Plus la différence de spécialisation entre les états d'une paire d'états en déréglementation est grande, plus l'effet que nous supposons devrait être élevé. Nous le faisons après avoir classé toutes les observations dans une paire d'états dans une industrie donnée en quartiles de différence de spécialisation. Nous exécutons également une version modifiée de l'équation en intégrant la variable de déréglementation avec la différence de spécialisation.

L'utilisation d'une variable indicatrice pour contrôler l'entrée bancaire ne peut pas prendre en compte l'intégration bancaire effective qui a lieu. Pour remédier à ce problème, dans le deuxième ensemble de régressions, nous remplaçons la variable de déréglementation par l'intégration bancaire réelle dans une paire d'états au fil du temps. Elle est définie comme la somme des actifs bancaires interétatiques détenus par les banques des deux états au sein de la paire, divisé par la somme des actifs bancaires des deux états. Toutefois, intégration bancaire peut être endogène aux différentiels de croissance des secteurs manufacturiers. Pour faire face à ce problème potentiel, nous utilisons l'estimation des variables instrumentales (IV). En tant qu'instrument, nous utilisons le nombre moyen d'années depuis que la paire d'états ouvre effectivement leurs marchés à leurs banques respectives.

Outre l'endogénéité, nous faisons face à d'autres défis empiriques supplémentaires et connexes. Une préoccupation potentielle est le retour à la moyenne dans notre variable dépendante. Des industries relativement plus petites dans un état sont susceptibles de croître beaucoup plus rapidement que celles plus grandes. Notre deuxième préoccupation est la persistance potentielle dans la différence de croissance d'une industrie donnée dans une paire d'états (la valeur de la variable est étroitement liée à sa valeur précédente). Par conséquent, dans certaines de nos régressions, nous utilisons les décalages de nos variables dépendantes pour contrôler le retour à la moyenne et la persistance pour nous assurer de la robustesse de nos résultats. Cependant, les retards de la variable dépendante introduisent un biais de panel dynamique, en créant une corrélation entre le régresseur et le terme d'erreur (Nickell, 1981). Ce biais serait prononcé pour notre panel de « petit T, large N » ($T = 17$ et N a le maximum de 21 342 observations dans chaque année pour 19 industries manufacturières dans $(48\times47)/2 = 1\,128$ paires d'états). De plus, notre régression inclut un grand nombre d'effets fixes (effets fixes d'année, d'année-état et d'industrie-année autres que l'effet fixe d'industrie-paire d'états qui est l'identificateur du panel) qui auraient pour effet d'exacerber le biais. Pour alléger ce problème, nous utilisons l'estimateur Arellano-Bond (Arellano et Bover, 1995; Blundell et Bond, 1998) qui s'appuie sur la méthode généralisée des moments (GMM) et fournit une solution d'estimation efficace des panels dynamiques. Cet estimateur corrige l'endogénéité de la variable dépendante retardée (qui est introduite pour contrôler sa persistance ou son retour à la moyenne) et fournit des estimations de paramètres cohérentes même en présence de variables endogènes de droite (dans notre cas, l'intégration bancaire variable). Il permet également de tenir compte des termes d'erreur d'effets fixes, d'hétéroscédasticité et d'autorégression.

Les résultats confirment notre hypothèse. Nous observons une croissance plus forte dans les industries manufacturières moins spécialisées dans un état donné lorsque le système bancaire de l'état devient intégré avec celui d'un autre état qui est plus spécialisé dans le même secteur. En outre, plus grande est la différence de spécialisation dans un secteur entre les états d'une paire, plus grand est l'impact d'intégration bancaire sur la croissance de ce secteur dans l'état qui est moins spécialisé. Les résultats sont prononcés pour les industries à forte dépendance financière externe. De plus, ces résultats sont robustes aux changements de l'échantillon, de la période d'estimation et de la méthode d'estimation (MCO avec IV contre Arellano-Bond avec IV), ainsi que du type d'effets fixes inclus dans la régression.

Nos estimations de coefficients présentent des grandeurs raisonnables. Nous constatons que pour les états dont les industries sont moins spécialisées, l'augmentation d'intégration bancaire avec les banques des états plus spécialisés de zéro à 1,2% (la moyenne de l'échantillon d'estimation) entraîne une augmentation de 1,05% de la croissance différentielle de valeur ajoutée au-delà d'un indice de référence comparable de la même industrie dans les états plus spécialisés. Nous obtenons des résultats similaires pour l'excédent brut d'exploitation (0,93%), la rémunération totale (0,44%), le nombre total d'employés (0,17%), la productivité (0,50%) et les salaires (0,21%). Ces résultats sont plus forts lorsque nous divisons l'échantillon en quartiles en fonction de la différence des spécialisations sectorielles des paires d'états. Une augmentation monotone dans les estimations de coefficients qui nous intéressent est observée alors que le différentiel de spécialisation sectorielle d'une paire d'états s'élargit. Par exemple, les estimations $\beta_1$pour la croissance sectorielle sont de: 0,1299 (non statistiquement significative) pour le premier quartile ; 0,5315 (et statistiquement significative au niveau de 1%) pour le deuxième quartile ; 1,1835 (et statistiquement significative au niveau de 1%) pour le troisième quartile ; et 1,6907 (et statistiquement significative au niveau de 1%) pour le quatrième quartile. Une augmentation monotone similaire des estimations β_1 est observable pour toutes les variables de sortie.

Les implications de notre travail vont au-delà de la curiosité académique. Nos résultats suggèrent que les origines des institutions acquérant (ou fusionnant avec) des banques d'une autre région économique peuvent avoir une influence importante sur la structure industrielle de cette dernière: les banques, étant donné leur exposition antérieure à l'industrie, peuvent jouer un rôle non négligeable en façonnant la structure sectorielle des économies qu'elles pénètrent.

Intégration bancaire interétatique, fusions, acquisitions et cessions d'entreprises aux États-Unis

Le chapitre trois intitulé « *Intégration bancaire interétatique, fusions, acquisitions et cessions d'entreprises aux États-Unis* » examine si l'intégration financière entre les régions peut avoir un effet sur le marché de prise de contrôle d'entreprises. Pour construire notre hypothèse, nous nous appuyons sur deux volets différents de littérature. Le premier domaine de recherche auquel nous faisons appel se concentre sur le rôle des banques commerciales dans la facilitation des fusions et acquisitions (M&As) intra-industrielles entre leurs clients emprunteurs, tandis que le deuxième (plus important) est dédié aux effets réels de l'intégration financière interétatique aux États-Unis. Ivashina et al. (2009) montrent la probabilité qu'une entreprise sera une cible augmente avec l'intensité de prêts bancaires. En outre, les entreprises qui ont des relations de crédit avec des banques qui ont plus de clients dans la même industrie sont plus susceptibles d'être soumis à une tentative de prise de contrôle. Ivashina et al. (2009) constatent également que les entreprises qui passent à une nouvelle banque attitrée sont également plus susceptibles d'acquérir des entreprises qui empruntent à cette nouvelle institution. Ces résultats suggèrent que les banques non seulement recueillent des informations pour surveiller leurs emprunteurs, mais aussi transmettent cette information à leurs autres emprunteurs pour les aider à trouver des cibles potentielles parmi le réseau de la clientèle de la banque.

Suite à l'élimination de barrières interétatiques à l'entrée de banques, les banques commerciales entrent pour la première fois dans de nouveaux états par acquisition de banques locales. Une implication naturelle des résultats d'Ivashina et al. (2009) est que la hausse d'entrées de banques observée après les déréglementations bancaires interétatiques devrait conduire à une plus forte activité sur le marché de prise de contrôle d'entreprises au sein des paires d'états financièrement intégrés. Nous supposons que ces entrées de banques élargiront l'ensemble de correspondances potentielles pour M&As et cessions du secteur non financier (ventes d'actifs) pour leurs entreprises clientes situées sur les marchés de résidence et d'entrée nouvelle des banques. Par conséquent, après avoir tenu compte de la possibilité que l'entrée de banques après la déréglementation pourrait être endogène, nous devrions observer plus d'activités de M&A et cession entre les paires d'états avec une intégration bancaire plus élevée par rapport à celles avec peu ou pas d'intégration.

Plus précisément, nous utilisons les données pour les paires d'états au fil des années et vérifions si le nombre total et la valeur des transactions de M&A ou de cession augmentent à mesure que l'intégration bancaire entre les régions économiques s'accroît après la

déréglementation d'entrée bancaire. Nous définissons l'intégration bancaire comme la somme des actifs bancaires interétatiques détenus par les banques des deux états au sein de la paire, divisé par la somme des actifs bancaires des deux états. Notre stratégie d'identification se concentre sur la variation (à l'intérieur) des paires d'états au fil du temps après avoir tenu compte des effets fixes d'année et les effets fixes variables dans le temps pour chacun des états d'une paire séparément. L'utilisation de la variation au niveau de la paire d'états-année nous permet de tenir compte des caractéristiques de paires d'état fixes dans le temps (par exemple, distance). Les effets fixes d'année absorbent les facteurs variables dans le temps potentiels au niveau des États-Unis. Les effets fixes d'état-année capturent des facteurs observables (comme le taux de croissance annuelle de chaque état dans une paire donnée) ainsi que non observables (telles que les lois anti-OPA, les modifications de législation ou les impôts sur les sociétés qui sont difficiles à suivre) qui varient dans le temps et entre les états. Un autre avantage majeur des effets fixes d'année-état est qu'ils tiennent compte de l'intégration bancaire des états de la paire d'états avec les 46 états restants. L'augmentation de l'intégration bancaire totale pourrait suppléer à l'augmentation du financement bancaire, et nos résultats pourraient être influencés par l'augmentation de disponibilité des fonds pour les M&As plutôt que par le partage des informations par les filiales de banques entre les régions et la facilitation des correspondances potentielles acquéreur-cible. Les effets fixes d'état-année atténuent cette préoccupation en tenant compte de l'effet de la déréglementation sur la disponibilité de financements bancaires dans une année donnée dans chaque état d'une paire d'états.

Les résultats des régressions MCO nous permettent de constater un impact positif de l'intégration bancaire sur le nombre total et la valeur des M&As et cessions. Toutefois, de telles constatations sont sujettes à la critique valable que l'intégration bancaire puisse ne pas avoir eu lieu de manière aléatoire. Les banques d'un état cherchant des opportunités de prêt plus élevées peuvent entrer dans des états à plus forte croissance, qui sont susceptible d'être les régions avec des activités de M&As naturellement plus élevées. Pour faire face à ces préoccupations d'endogénéité possible, nous comptons sur l'estimation des variables instrumentales (IV) dans notre ensemble principal de résultats. Nous utilisons la nature décalée des déréglementations d'entrée bancaire dans des paires d'états qui ont eu lieu aux États-Unis pour suivre une approche d'estimation IV. Nous utilisons une combinaison de (i) nombre moyen d'années depuis la déréglementation d'entrée bancaire effective entre les états d'une paire (l'un vers l'autre et vice-versa), ainsi que son carré ou sa racine carrée, et (ii) une variable indicatrice qui prend la valeur de 0 avant la date de déréglementation effective

d'entrée bancaire et celle de 1 pour les années après la déréglementation (le choix de ces instruments est similaire à ceux de Morgan, Rime et Strahan, 2004 ou Michalski et Ors, 2012). Il est fort peu probable que nos instruments soient corrélés aux activités de fusions et acquisitions d'entreprises dans un état donné, mais ils sont fortement corrélés à l'intégration bancaire: premièrement, il ne peut y avoir d'augmentation d'intégration bancaire sans une déréglementation d'entrée bancaire. Deuxièmement, plus le temps passe après une déréglementation, plus le potentiel d'intégration est élevé. Les estimations IV suggèrent des effets plus marqués de l'intégration bancaire interétatique sur les M&As et cessions d'entreprises.

Nous constatons qu'une hausse d'écart-type dans l'intégration bancaire entre une paire d'états (qui est égale à 0,0108 et correspond approximativement à une augmentation de 1% de l'intégration bancaire) conduit à 0,38 transactions supplémentaires par an (44% de la valeur moyenne) et à une augmentation de la valeur totale des opérations de M&As de 44,15 millions de dollars. Pour les cessions, une hausse d'écart-type dans l'intégration bancaire entre la paire d'états moyenne conduit à 0,12 transactions transfrontalières supplémentaires (23% de la valeur moyenne). Cependant, nous n'observons aucune augmentation statistiquement significative de la valeur totale des cessions entre les paires d'états (même si les estimations des coefficients sont positives).

Si l'intégration bancaire facilite le processus d'appariement des cibles et acquéreurs potentiels par le partage d'information, elle devrait le faire davantage si les cibles et les acquéreurs sont situés à une certaine distance car la collecte d'informations est plus coûteuse. Nous testons cette conjecture pour le sous-échantillon de paires d'états dont les états sont séparés les uns des autres au-dessus de la distance entre la paire d'états médiane. Une augmentation d'écart-type dans l'intégration bancaire pour ce sous-échantillon (0,0029) conduit à 1,92 transactions M&A supplémentaires entre paires d'état en moyenne. Cet effet est environ cinq fois plus fort que l'effet mesuré pour l'échantillon principal.

Ensuite, nous testons notre hypothèse pour un sous-échantillon d'opérations dans lesquelles les entreprises acquéreuse et cible sont cotées en bourse. Nous nous attendons à ce que l'incidence de l'intégration bancaire soit moindre étant donné que la collecte d'informations sur les entreprises publiques est relativement facile par rapport aux entreprises privées. Deuxièmement, les entreprises publiques sont probablement moins dépendantes du financement bancaire. Le coefficient d'intérêt est d'environ un huitième et un vingtième des coefficients dans l'échantillon complet. Nous observons également un effet plus faible sur la valeur totale des transactions.

Nous demandons également s'il existe un effet différentiel de l'intégration bancaire dans les industries non financières. Si la hausse de M&As et cessions est due à la taille du réseau client des banques, l'effet observé devrait être prononcé pour les industries qui présentent une dépendance financière extérieure élevée (comme défini dans Rajan et Zingales, 1998). Deuxièmement, nous présumons que les banques d'un état spécialisées dans une industrie (par rapport à la moyenne des États-Unis) tendent à prêter davantage à ce secteur, ce qui se traduit par un réseau industrie-client plus important. Ainsi, à la suite de l'entrée dans un nouveau marché dans un autre état, un appariement potentiel acquéreur-cible est plus probable pour les entreprises de ces industries. Notre recherche montre que les résultats sont prononcés pour les industries à forte dépendance financière externe qui sont développées (par rapport à la moyenne américaine) dans un état au moins.

La contribution principale de cet article est de tester les implications d'Ivashina et al. (2009) et relier leurs conclusions à la littérature plus large sur l'intégration financière et l'activité économique réelle à travers les régions. Nos résultats sont cohérents avec les flux d'information entre (i) les banques et leurs clients emprunteurs et (ii) les filiales des sociétés à portefeuille multi-bancaires, ce qui facilite le processus d'appariement des entreprises acquéreuses et cibles. Nous fournissons des preuves pour un micro-canal particulier par lequel l'intégration économique a lieu suite à l'intégration financière: le rôle des banques multirégionales comme catalyseurs dans le marché de prise de contrôle d'entreprises à travers les régions.

Introduction

This dissertation is structured around two different topics: corporate political activism and financial integration across regions through bank-entry. The first chapter examines the motives of corporate political activism with an emphasis on access to finance. The last two papers study banking integration with the ambition of understanding its effects on the real economy by using the bank-entry deregulations in the US between 1978 and 1997.

The Effect of Credit Supply on Political Connections: Evidence from the US

Few debates get attention from academics, policy makers, and public equally as corporate political activism. Campaign contributions by corporations have been the center of this discussion in recent years following the court rulings that relaxed constraints on corporate spending on elections (Citizens United v. FEC, 2010; and McCutcheon v. FEC, 2014). The debate revolves around the question of whether campaign contributions affect the politicians' behavior in favor of the corporations or are simply an expression of political opinion as protected by the US Constitution.

This chapter entitled *The Effect of Credit Supply on Political Connections: Evidence from the US* investigates the link between credit supply and non-financial firms' political investment. I hypothesize that firms increase their political activism when they face an adverse bank lending shock that is exogenous. A negative bank lending shock can generate an immediate financing constraint or an expectation of one in the near future. Political connections can help alleviate the financing constraints mainly through increasing access to government contracts and subsidies that generate a reliable cash flow. Further, government contracts may decrease credit risk and attract privately-owned bank financing.

My empirical approach relies on the lending relationship that is established between a bank and a borrower over time. As the bank collect information about the borrower, switching lenders become costly for the borrower-firm. Thus, a borrower of a distressed lender would have greater difficulty in obtaining bank financing compared to a borrower of a financially healthy lender.

My identification strategy also exploits the fact that the 2007-2008 financial crisis originated from the real-estate market and the early period of the financial crisis provides a banking shock that is orthogonal to the corporate sector. I use this shock to isolate the effect of credit supply from credit demand. Apart from its exogenous character, the financial crisis

provides a favorable setting to test my hypothesis for two more reasons: (i) The crisis began in the run-up to the 2008 congressional elections when campaign contributions matter most. (ii) the lending behavior of the financial institutions was differentially affected by the crisis due to the different levels of exposures to the meltdown of the subprime mortgage market. I exploit such variation across banks to empirically identify the effect of relationship lenders' health on their borrowers' campaign contributions.

More precisely, I start by constructing a dataset with the campaign contributions and accounting data of non-financial firms for the 2005-2008 periods. Among these firms, I identify the ones that establish a pre-crisis lending relationship with at least one bank between the second quarter of 2002 and the second quarter of 2007 in the syndicated loan market. Then, I construct a relationship lending measure for each bank-firm pair computed as the ratio of the number of loans granted to the borrower by the syndicate in which the given bank takes a lead role to the total number of syndicated loans of the borrower during the five-year period. Following Chodorow-Reich (2014), I define firm-level credit supply as the weighted average of the overall lending of the firm's relationship lenders (to other borrowers) using the relationship lending measures as weights. I adopt a difference-in-differences framework, and I explain the changes in the firms' campaign contributions before and during the financial crisis with the changes in firm-level credit supply. This specification corresponds to a difference-in-difference estimation with a heterogeneous treatment variable (change in loan supply of relationship lenders). The coefficient of interest estimates whether firms that experience larger drops in loan supply from their relationship lenders (treated group) have a stronger increase in their campaign contributions during the crisis period (treatment period).

I find that a 10% decrease in loan supply leads to a 9% increase in campaign contributions in 2008. The contributions are tilted towards powerful candidates such as incumbent politicians and members of the congressional committees relevant to the firm's industry. The contributions also lean towards the candidates that might be more willing to help the firm like home-state politicians. Consistent with my financing constraint hypothesis, firms that have a loan due in the second half of 2008, increase their campaign contributions by an additional 13%. On the contrary, firms that issue corporate bonds during the pre-crisis period (relatively) decrease their campaign contributions by 5%.

Then, I present further empirical evidence supporting my conjecture. One standard deviation increase in the campaign contributions is associated with a 4% greater likelihood in being awarded a contract and a $6.8 million increase in the contract amount for the median firm in the next election cycle (median value: $10 million). Further, one standard deviation

increase in past level of contributions is associated with a 6 basis points decrease in loan spread, and a $93 million increase in loan amount for the median firm (median values: 112 basis points, $775 million). Being a government contractor is further associated with a reduction in loan spread by 7 basis points.

My findings are consistent with the idea that firms alleviate their financing constraints through their political ties. My results lend support to the idea that campaign contributions are an investment in political capital rather than merely a form of consumption good. I mainly contribute to the literature on the mechanisms through which political connections affect firm value. Previous research shows politically connected firms are more likely to be bailed out and to receive government assistance funds in case of distress (Faccio, Masulis, and McConnell 2006; Adelino and Dinc, 2014). However, the sample firms in these studies may be financially and/or economically distressed, making it difficult to identify the role of each on firms' political activism. Even if these firms turn out to be financially distressed; it is not clear whether firms' political activism is driven by financial distress itself rather than the firm-level factors that push the firm into the financial distress in the first place. My paper tackles this problem by identifying an exogenous credit supply shock through which firms face financing constraints.

Financial Integration and Growth: Banks' Previous Industry Exposure Matters

Over the past four decades, countries have become much more integrated financially, in many instances through foreign bank-entry. There is evidence suggesting that the effects of financial integration go beyond the simple provision of additional funds. In fact, a number of papers point to a reallocation of capital across industries following financial integration (Fisman and Love, 2004; Acharya, Imbs, and Sturgess, 2011). However, we know little about the micro-mechanisms behind the macro-level evidence of the observed economic convergence that follows financial integration. The second chapter entitled *Financial Integration and Growth: Banks' Previous Industry Exposure Matters* uncovers a particular channel in this reallocation process: the collection of industry-specific information by financial institutions at their home market and transfer of this information when providing capital to firms located in new markets that they enter.

More specifically, we test for a channel that works through commercial banks' exposure to more prevalent industries in their "home" state. Our conjecture is that financial integration with out-of-state banks that are more knowledgeable about an industry should lead to faster growth in that sector. We presume that banks in a given state that are more-specialized in an

industry would naturally lend more to that sector on average since, they would have more information about the functioning and prospects of that sector, compared to institutions operating in states that are less-specialized. The information collected and processed by the banks in their home state's more prominent (more-specialized) industries would be reflected in their ability to screen and monitor loans in that sector (for ex., through specialization of lending officers or the use of proprietary credit scoring systems). When these same banks enter a new market in another state for the first time (typically through the acquisition of a local bank in their "host" state post bank-entry deregulation), their home state industry exposure would give these lending institutions a natural advantage in screening loans. Thus, we conjecture that financial integration can affect the growth of this industry differently given the market-entrant financial institutions' previous exposure to the same industry.

We test this hypothesis using state-pair-industry-level data and a series of quasi-natural experiments: staggered bank-entry deregulations at the state-pair-level during the 1980s and 1990s. We proceed as follows. First, we define the specialization of a manufacturing industry in a state as the ratio of that sector's share of manufacturing output (i.e., value added) to its share of overall US manufacturing output. We start with testing whether there is a significant change in relative sector-level growth (differential growth rate between states in a given industry where the first (second) state is the less (more)-specialized one within the pair as of the date of effective interstate deregulation) after interstate banking deregulation. Our test variable is an indicator variable that is equal to 1 starting with the year *after* (and including all the subsequent years) the state-pair effectively opens their markets to each other's banks, and 0 otherwise. Then, we improve the identification by taking into account the discrepancy in specialization between the two states in a given industry. First, we run separate regressions after classifying all observations pertaining to state-pairs per industry into quartiles by the difference in specialization in that sector (as of the interstate banking deregulation). The larger the difference in specialization in a deregulating state-pair is, the higher the effect that we hypothesize should be. We also run a modified version of the equation by interacting the deregulation variable with the difference in specialization.

Using an indicator variable to control for bank-entry cannot take into account the actual banking integration that takes place. To remedy this problem, in the next set of regressions we replace deregulation variable with the actual banking integration in a state-pair over time. It is defined as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. However, banking integration can be endogenous to manufacturing sectors' growth differentials. To deal with

this potential problem, we use Instrumental Variables (IV) estimation. As an instrument, we use the average number of years since the state-pair effectively opens their markets to each other's banks.

Besides endogeneity, we face other additional and related empirical challenges. One potential concern is mean-reversion in our dependent variable. Relatively smaller industries in a state are likely to grow much faster than the larger ones. The second concern that we face is the potential persistence in the difference of growth of a given industry in a state-pair (the value of the variable is closely related to the previous value). To overcome these econometric challenges, we use Arellano-Bond estimator (Arellano and Bover, 1995; Blundell and Bond, 1998) that relies on the generalized method of moments (GMM) and provides a solution for the efficient estimation of dynamic panels. This estimator corrects for the endogeneity of the lagged dependent variable (which is introduced to control for its persistence or mean-reversion) and provides consistent parameter estimates even in the presence of endogenous right-hand-side variables (in our case, the banking integration variable). It also allows for fixed effects, heteroskedasticity and autoregressive error terms.

The results are supportive of our hypothesis. We observe higher growth for less-specialized manufacturing industries in a given state when the state's banking system gets integrated with that of another state that is more-specialized in the same sector. Moreover, the larger the discrepancy in specialization in the industry between two states in a state-pair, the higher the impact of banking integration on the growth of that sector in the state that is less-specialized. The results are pronounced for high external finance dependent industries. Moreover, these findings are robust to changes in the sample, estimation period, estimation method (OLS with IV versus Arellano-Bond with IV), and the type of fixed effects included in the regression.

Our coefficient estimates exhibit reasonable magnitudes. We find that for states with less-specialized industries, the increase of banking integration from zero to 1.2% (the average for the estimation sample) with the more-specialized states' banks leads to a 1.05% increase in the differential growth of value added over and above a comparable benchmark of the same industry in the more-specialized states. We obtain similar results for the gross operating surplus (capturing the total remuneration of capital) (0.93%), total compensation (0.44%), total number of employees (0.17%), productivity (0.50%), and wages (0.21%). These findings are stronger when we split the sample into quartiles based on the difference of state-pairs' industry specializations. There is a monotonic increase in the coefficient estimates of interest as the state-pair industry specialization differential widens.

The implications of our work go beyond academic curiosity. Our results suggest that the origins of institutions acquiring or merging with another economic region's banks can exert important influences on the industrial structure of the latter: banks, given their previous industry exposure, can play a non-trivial role in shaping industry structure of the economies that they enter.

Interstate Banking Integration and Corporate M&As and Divestitures in the US

The third chapter entitled *Interstate Banking Integration and Corporate M&As and Divestitures in the US* examines whether financial integration across regions can have an effect on the market for corporate control. We draw upon two different strands of the literature to build our hypothesis. The first area of research that we rely on focuses on the commercial banks' role in facilitating within-industry mergers and acquisitions (M&As) between their borrower-clients, and another (larger) one on the real effects of financial integration across states in the US. Ivashina et al. (2009) show the probability that a firm will be a target increases with bank lending intensity. Further, firms that have lending relationships with banks that have more clients in the same industry are more likely to be subject to a takeover attempt. Ivashina et al. (2009) also find that firms that switch to a new relationship-bank are also more likely to acquire companies that are borrowing from this new institution. These findings suggest that banks not only collect information for monitoring their borrowers but also transfer this information to their other borrowers to help them find potential targets among the bank's client network.

Following the removal of interstate bank-entry barriers, commercial banks enter new states for the first time through the acquisition of local banks. A natural implication of Ivashina et al. (2009) findings is that higher bank-entry observed post-interstate banking deregulations should lead to higher activity in the market for corporate control across financially integrated state-pairs. We conjecture that these bank-entries will broaden the set of potential matches for non-financial sector M&As and divestitures (asset sales) for their client firms located in the banks' home and newly-entered markets. As a result, after controlling for the possibility that bank-entry after deregulation might be endogenous, we should observe more M&A and divestiture activity between state-pairs with higher banking integration compared to those with no or little integration.

More precisely, we use state-pair-year-level data and test whether the total number and value of M&A or divestiture transactions increase as banking integration grows across economic regions. Our identification strategy focuses on (within) state-pair variation across

time after controlling for year fixed effects and time-varying state fixed effects for each of the states in a pair separately. We define banking integration as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. However, such specification is prone to the valid criticism that banking integration may not have taken place in a random fashion. Banks from a state seeking higher lending opportunities may enter higher-growth states, which are likely to be the regions with naturally higher M&A or divestiture activity. To deal with such concerns of possible endogeneity, we rely on instrumental variables (IV) estimation and we make use of the staggered nature of state-pair bank-entry deregulations that took place in the US.

We find that one standard deviation increase in banking integration between a state-pair (which is 0.0108 and corresponds to roughly an increase of 1% in banking integration) leads to 0.38 more deals per year (44% of the mean value) and a $44.15 million increase in the total value of M&A deals. For divestitures, one standard deviation of increase in banking integration between the average state-pair leads to 0.12 more cross-border transaction (23% of the mean value). However, we observe no statistically significant increase in the total value of divestitures between state-pairs (even though the coefficient estimates are positive).

We also ask whether there is a differential effect of banking integration across non-financial industries. If the increase in M&As and divestitures is driven by the size of the client network of the banks, then the observed effect should be pronounced for the industries that exhibit high external finance dependency (as defined in Rajan and Zingales, 1998). Second, we presume that banks of a state that are specialized in an industry (relative to US average) tend to lend more to this sector resulting in a bigger industry-client network. Thus, following the entry into a new market in another state, a potential acquirer-target match is more likely to happen for firms in these industries. Our results show that the findings are pronounced for industries that are high external finance dependent and that are developed (relative to the US average) in at least one state.

The main contribution of the paper is to test the implications of Ivashina et al. (2009) and link their findings with the larger literature on financial integration and real economic activity across regions. Our results are consistent with information flows among (i) the banks and their borrower-clients (ii) affiliates of the multi-bank holding companies, which facilitate the matching process of acquirer and target firms. We provide evidence for one particular micro-channel through which economic integration takes place following financial integration: multi-regional banks' role as catalysts in the market for corporate control across regions.

Contents

Chapter 1

The Effect of Credit Supply on Political Connections: Evidence from the US

1.1. Introduction

The literature has seen an increased interest in understanding how political connections affect firm value. Several papers find a positive association between firms' political connections and access to bank finance (Faccio, Masulis, and McConnell, 2006; Claessens, Feijen, and Laeven, 2008; Houston et al., 2014). However, it has not been explored whether firms increase their political connections when their access to bank finance deteriorates. This relation became particularly interesting in light of the increased engagement of the non-financial sector to the political campaigns during the credit crunch in 2007 and 2008. The campaign contributions increased by 35% in the 2007-2009 election cycle compared to the average level between 2001-2007.

I hypothesize that firms increase their political activism when they face an adverse bank lending shock that is exogenous. A negative bank lending shock can generate an immediate financing constraint or an expectation of one in the near future. Political connections can help alleviate the financing constraints mainly through increasing access to government contracts and subsidies that generate a reliable cash flow. Further, government contracts may decrease credit risk and attract privately-owned bank financing. Christopher (2011) and Goldman, Rocholl, and So (2013) show that politically connected firms are more likely to receive government contracts. Goldman (2015) suggests that federal government is a remarkably stable customer and government contracts act as hedge during 2008 financial crisis. Houston et al. (2014) show that political connections decrease loan spreads and the link is stronger for firms in industries that are more reliant on government contracts.

In this paper, I establish a causal link between banks' syndicated loan supply and non-financial firms' campaign contributions for US elections during the 2007-2008 financial crisis. Next, I present empirical evidence supporting my conjecture. I show that campaign contributions are associated with increased likelihood to win a contract and larger contracts. I also find that firms' past contributions and contracts are associated with a reduction in loan spread and an increase in loan amount for the future loans.

My empirical approach relies on a key premise established in both theoretical and empirical banking literature: Banks and firms establish a relationship through multiple lending transactions

and switching lender become costly for borrowers.[1] Thus, a borrower of a distressed lender would have greater difficulty in obtaining bank financing compared to a borrower of a financially healthy lender during the crisis period.

To illustrate the identification strategy, compare the financial health of two banks and their clients' borrowing record and political activism during the crisis period. For example, U.S. Bancorp had relatively small exposure to mortgage-backed securities, and its financials remain relatively sound during turbulent times such that it was able to increase its loans in the fourth quarter of 2008. On the other hand, Lehman Brothers were heavily involved in securitization and issued a large amount of mortgage-backed securities. As investor confidence eroded during 2008, Lehman filed for Chapter 11 bankruptcy on September 15, 2008. During the nine-months-period between the third quarter of 2007 and the first quarter of 2008, U.S. Bancorp and Lehman Brothers cut lending by 21% and 45% respectively compared to the same period of the previous year. Between the two relatively similar firms operating in the same industry, Atmos Energy Corp. that had a pre-crisis lending relationship with U.S. Bancorp obtained a loan in the following quarters of 2008, whereas Covanta Energy Corp. that had a pre-crisis lending relationship with Lehman Brothers did not have a loan during the same period. Meanwhile, Atmos Energy increased its' campaign contributions from $59,950 in 2005-2007 election cycle to $83,900 in the 2007-2009 election cycle (a 40% increase) and Covanta Energy contributed $11,450 and $54,750 to political campaigns in the 2005-2007 and 2007-2009 election cycles respectively (a 378% increase).

I mainly contribute to the literature on the mechanisms through which political connections affect firm value. Previous research shows politically connected firms are more likely to be bailed out and to receive government assistance funds in case of distress (Faccio, Masulis, and McConnell, 2006; Adelino and Dinc, 2014). However, the sample firms in these studies may be financially and/or economically distressed, making it difficult to identify the role of each on firms' political activism.[2] Even if these firms turn out to be financially distressed; it is not clear whether firms' political activism is driven by financial distress itself rather than the firm-level factors that push the firm into the financial distress in the first place. My paper tackles this

[1]See the detailed discussion of the literature on relationship lending in Section 4.2.

[2]I follow the conceptual definition of Lemmon, Yung-yu, and Tashjian (2009) for economic and financial distress. Firms facing financial distress are viable but have difficulty repaying debts. In contrast, firms facing economic distress are characterized by low or negative operating profitability and have questionable viability even in the absence of leverage.

problem by identifying an exogenous credit supply shock through which firms face with financing constraints. I provide novel empirical evidence that firms "actively" increase their political connections as the financial health of their lenders worsen. Second, my paper uncovers the effect of credit market frictions on the level of firms' political investment. My findings are likely to be pronounced for the developing countries compared to the US that has a relatively well-functioning credit market with low government ownership.

To test my hypothesis, I exploit the fact that the 2007-2008 financial crisis originated from the real-estate market rather than the corporate sector. The financial crisis provides a banking shock that is exogenous to the (non-real estate) corporate sector, which allows me to isolate the effect of credit supply from demand. Apart from its exogenous character, the timing and the evolution of the financial crisis provide a unique setting for my analysis. The crisis began in the run-up to the 2008 congressional elections when campaign contributions matter most. Second, the lending behavior of the financial institutions was differentially affected by the crisis due to the different levels of exposures to the meltdown of the subprime mortgage market.[3] I exploit such variation to empirically identify the effect of lenders' health on their borrowers' campaign contributions.

I start by constructing a dataset with the campaign contributions and accounting data of non-financial firms for the 2005-2008 periods. Among these firms, I identify the ones that establish a pre-crisis lending relationship with at least one bank in the syndicated loan market. Following Chodorow-Reich (2014), I define credit supply at the firm-level as the average quarterly lending of the firm's relationship lenders to other borrowers (excluding the firm itself).[4] I adopt a difference-in-differences framework, and I explain the changes in the firms' campaign contributions before and during the financial crisis with the changes in firm-level credit supply.

My results show that a 10% decrease (one standard deviation of change in lending in my sample) in loan supply of a given firm by its pre-crisis relationship lenders during the early crisis period leads to a 9% increase in firm's campaign contributions in 2008. The contributions are tilted towards powerful candidates such as incumbent politicians and chairmen and ranking minority members that serve on congressional committees that have jurisdiction over the firm's industry. The contributions are also inclined towards politicians that may be more willing to help

[3]Santos (2011) find that firms paid higher loan spreads or took out smaller loans when they borrowed from banks that incurred larger losses.
[4]See Section 4.2. and 4.4 for a detailed discussion.

the firm such as home state candidates. The findings are robust to alternative measures of loan supply and different time periods. Consistent with my financing constraint hypothesis, the firms that have a loan due in the second half of 2008 increase their campaign contributions by an additional 13%. Further, the firms that issue corporate bonds at the onset of the crisis (relatively) decrease their contributions by 5%.

Next, I investigate the channels through which campaign contributions alleviate financing constraints. One standard deviation increase in the campaign contributions is associated with a 4% greater likelihood in being awarded a contract and a $6.8 million increase in the contract amount for the median firm in the next election cycle. Further, one standard deviation increase in past level of contributions is associated with a 6 basis points decrease in loan spread, and a $93 million increase in loan amount for the median firm. Being a government contractor is correlated with a reduction in loan spread by 7 basis points after controlling for contributions. My findings lend support to the idea that campaign contributions are an investment in political capital rather than merely a form of consumption good.

The paper proceeds as follows. Section 1.2 reviews the relevant literature. In Section 1.3 I explain the datasets used in the paper. Section 1.4 elaborates on the construction of the regression variables and the identification strategy. Section 1.5 presents the results Section 1.6 provides additional robustness checks. Section 1.7 concludes.

1.2. Literature review

My paper is broadly related to the literature on the relationship between political connectedness and firm value. Cooper, Gulen, and Ovtchinnikov (2010) find that campaign contributions are positively related to the future returns and act as a form of investment in political capital. Shon (2010) finds an economically significant positive (negative) relation between pre-election campaign contributions to Bush (Gore) and stock returns during the 37-day election recount period. Using off-cycle US congressional elections, Akey (2015) find that post-election abnormal equity returns are 3% higher for firms donating to winning candidates. Connections to politicians serving on powerful congressional committees such as appropriations and taxation positively impact contributing firms' value. An alternative theory suggests that firms gain relatively little political benefits from their donations. Ansolabehere, de Figueiredo,

and Snyder (2004) argue that the links from an individual campaign contribution to the election prospects of the candidate and the voting decision on a particular legislation are weak. In line with this theory, Aggarwal, Meschke, and Wang (2012) suggest that characteristics of these firms –large and low growth firms with more free cash flow, show consistency with the existence of agency problems. Similarly, Coates and Cogan (2012) find that politically connected firms trade at a lower value than a control group of firms that do not support election campaigns following Citizens United v. FEC – a Supreme Court decision that relaxed constraints on campaign contributions in 2010.[5]

My paper contributes to one particular strand of this literature that investigates the role of political connections on access to finance. Cross country studies like Faccio, Masulis, and McConnell, 2006; Faccio, 2010; Boubakri, Cosset, and Saffar, 2012 find that politically connected firms have more debt compared to non-connected peers. Studies of emerging markets provide evidence supporting the role of political connectedness on preferential treatment to access to finance. (Khwaja and Mian, 2005, for Pakistan; Lazzarini et al., 2011, for Brazil; Iftekhar et al., 2014, for Poland) Using Brazilian campaign contributions data, Claessens, Feijen, and Laeven (2008) find that contributing firms increase their leverage relative to a control group after each election cycle. They suggest that the increase in bank finance may be through preferential treatment of government-owned commercial banks and development banks since contributing firms do not exhibit asset growth that may serve as collateral for arm's-length debt finance. Contrary to these papers' findings, Bliss and Gul (2012) suggest that politically connected Malaysian firms are charged higher interest rates by lenders given their higher inherent risks.

The rents from political connections do not abolish even in a fully privatized banking market. Using data from Mexico, a country in which banking system is fully privatized in late 1980's, Agarwal et al. (2016) find that private banks offer favorable loan terms to politically connected firms in return for more government borrowings. Using US data, Ovtchinnikov, Hanouna, and Prabhat (2016) find that political contributions reduce the price of credit risk, which suggests that political contributions are valued by market participants. Using the political connections of S&P

[5]There are several papers that study the effect of political connections on firm value and measure political connectedness through other means. Please see Fisman, 2001; Faccio, 2006; Ferguson and Hans-Joachim, 2008; Goldman, Rocholl, and So, 2009; Acemoglu et al., 2013 for personal connections; Chen, Parsley, and Yang, 2015; Borisov, Goldman, and Gupta, 2016 for lobbying.

500 companies through board members with political ties, Houston et al. (2014) find that the cost of a syndicated loan is significantly lower for companies with such board members. This negative link is stronger for firms with more government procurement dependence and firms facing stronger foreign competition and weaker for firms with a longer relationship with the banks in the syndicate. I extend their work by identifying a causal link between a firm's bank lending relationships and political connectedness through an exogenous shock to the former in the financial crisis.

Another paper close to my work is Adelino and Dinc (2014) who study the effect of firm's financial health on firm's lobbying activity. They find that firms with rising Credit Default Swap (CDS) spreads in 2008, lobbied more in the first quarter of 2009 for the stimulus bill that allows public spending in the form of tax cuts, grants, loans, and federal contracts. For my purpose, CDS rates could be a less than perfect proxy. CDS capture the risk of default, thus rising CDS spreads can be an indication of financial distress and/or economic distress (or the existence of pre-crisis firm-level factors that can push the firm into distress).[6,7] My results complement their work by identifying a bank lending channel through which firms shape its' relationship with the government.

1.3. The data

The campaign contribution data come from the Federal Election Commission (FEC). Corporations are allowed to make a contribution to candidates by sponsoring corporate Political Action Committees (PACs). FEC provides data on the sponsoring firms, the date and amount of contribution and the identity of the receiving candidate.[8] For each receiving candidate, I also

[6]Please refer the definitions of financial and economic distress given in footnote 2.

[7]Transocean Inc. is an off-shore drilling contractor that regularly lobbies. In April, 2010, one of its drilling rigs caught fire, resulting in an oil spill at the Gulf of Mexico. On April 21, 2011, BP filed $40 billion worth of lawsuits against contractors, including Transocean. Following the cessation of drilling activities in the Gulf, the estimated amount of Transocean's 'at-risk' contract backlog reached $1.8 billion.(http://www.cbsnews.com/news/transoceans-problems-run-deeper-than-lost-revenue-from-gulf-spill-drill-ban/) According to CDS data provided by Markit, the 5-year CDS spread of Transocean was 66 bps at the beginning of April, whereas it reached a level of 558 bps on June, 2nd. Further, Transocean had an average lobbying expenditure of $107,500 between 2002 and 2009, but it climbed up to $540,000 in 2010 $470,000 in 2011 and $400,000 in 2012. It dropped back to a level of $90,000 - close to historic average- in 2013. This event, albeit out-of-sample, exemplifies how firm-specific distress factors that do not stem from a bank financing channel drive both CDS rates and political activism.

[8]PACs cannot be financed by the firm's own funds. Instead, contributions are made by corporate managers, employees and shareholders. However, the corporations are allowed to fund the overhead expenses of the PAC. The

obtain data from the FEC whether the candidate is racing for a seat in the Senate or the House of Representatives, the candidate's state, district, party affiliation and incumbency status. Next, for all elected officials, I collect data on their congressional committee assignments and their rankings in each committee for 109th (January 2005-January 2007) and110th (January 2007-January 2009) congress using Charles Stewart's Congressional Data Page.[9]

The syndicated loan data come from the Thomson Reuters Dealscan database. The data contain loan-level information on the deal amount, the role of each lender in the deal, the identity of the borrower and the lender and their ultimate parents. Unfortunately, the Dealscan data do not provide a borrower-level identifier that is used in standard databases. For this, I use the linking table between Dealscan and Compustat provided by Chava and Roberts (2008). I also manually verify the ultimate parents of the lender as of the loan origination date, since Dealscan periodically updates the ultimate parent of the lender following a merger or acquisition. I obtain merger and acquisition chronology of the banks in my sample from Federal Reserve's National Information Center database.

The contract data come from the USAspending.org website. The website provides data from the Federal Procurement Data System (FPDS) that tracks procurement contracts from several departments and agencies of the federal government. The dataset provides contract-level information such as signing date, dollar amount, and the name of the parent recipient firm.

I start with 1816 firms and professional associations that sponsor PACs in 2005-2007 and 2007-2009 election cycles.[10] I match companies by name to Compustat/CRSP merged

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contributions from corporate PACs are legally limited to $10,000 per candidate per election cycle. This cap binds "hard money" contributions whose data are provided by the FEC. The role of soft money contributions significantly decreased by the Bipartisan Campaign Reform Act in 2002. However, the constraints on campaign contribution were gradually lessened after the financial crisis. Citizens United v. FEC court ruling in 2010 relaxed constraints on the ability of corporations to spend money on political campaigns. It allowed for unlimited donations from corporations, unions and individuals to go to super PACs and nonprofits, which, in turn, could spend the money on advertisements or praising candidates. These expenditures cannot be coordinated with the spending of the contributions of the candidate. McCutcheon v. FEC ruling in 2014 struck down the aggregate limits on the amount an individual may contribute during a two-year period. (https://www.publicintegrity.org/2014/04/22/14611/mccutcheon-decision-explained-more-money-pour-political-process) However, the period of my study is earlier than these court rulings.

[9] http://web.mit.edu/17.251/www/data_page.html

[10] I focus on Senate and House elections. I exclude contributions to presidential candidates for two reasons: (i) contributions for presidential races compose a small part of firms' total contributions (average of $81,584 for House candidates, $35,203 for Senate candidates and $7,630 for presidential candidates conditional on contributing a positive amount in 2007-2009 election cycle) and relatively few firms contribute to presidential candidates (365 sponsoring firms in 2007-2009 election cycle) (ii) presidential elections take place in years divisible by four, my sample period includes only one presidential election which makes it less feasible for a difference-in-difference analysis.

database.[11] In cases where a firm is a subsidiary, I use the ultimate parent in my analysis. This matching gives me a sample of 766 firms. I exclude all finance, insurance and real estate firms and firms operating in public administration (SIC code greater than 5999 and lower than 7000, and higher than 8999). I also exclude firms whose ultimate parents are incorporated outside the US. The number of firms goes down to 553. I keep the firms with non-missing firm-quarter observations between 2004q3-2008q4 for my primary control variables (book value of assets, operating profit, sales). The number of firms reduces to 493, 421 (85%) of which signed at least one syndicated loan before the crisis (2002q2-2007q2). These 421 firms constitute the main sample.[12] Next, I use a string distance search algorithm to match the names of the contributing firms (493 firms) to the contract recipient firms. Out of 493 firms, I identify 380 firms (77%) as government contractor between 2007-2010.

The next section will provide an overview of the data by discussing the summary statistics and the characteristics of the banks that issue syndicated loans to the firms in my main sample.

1.3.1. Summary statistics

Panel A of Table 1.1 reports summary statistics for firms that have accounting data in Compustat/CRSP database and that contribute to political election campaigns in 2005-2007 and 2007-2009 election cycles. Columns 1 and 3 display the statistics for all firms (493 firms) and columns 2 and 4 show the statistics of firms with at least one syndicated loan deal between 2002q2-2007q2 (421 firms). The firms with a lending relationship in the syndicated loan market support more candidates and contribute more money per candidate. The mean contribution amount during the 2005-2007 cycle is around $140,000, increasing to approximately $170,000 in the next cycle for all contributing firms. These numbers are around $152,600 and $184,000 respectively for firms that borrow through syndicated loan market. The total number of candidates supported increases from 48 to 54 for all firms and from 51 to 58 for contributing firms that borrow in the syndicated loan market. For all columns, average contribution amount

[11]I thank Alexei Ovtchinnikov for generously providing CRSP identifiers of the companies that are used in Cooper, Gulen, and Ovtchinnikov (2010).

[12]My study does not appear to suffer from survivorship bias. There are 20 firms that do not take place in the sample of 493 firms (4%) since their accounting data series in Compustat stop at a quarter between 2004q3-2008q4. Within the same sample, there are 25 firms (5%) whose accounting data series in Compustat stop at a quarter in 2009 or 2010 (21 of them dropped due to M&As and 4 firms filed bankruptcy). My results are robust to exclusion of these firms.

per candidate is between $2,200 and $2,500 with a standard deviation of around $1000. This amount is well under the $10,000 limit imposed by the FEC. Similar numbers are reported in Cooper, Gulen, and Ovtchinnikov (2010) ($2,086 between 1979-2004 in 12/2004 dollars).

The firms with a lending relationship in the syndicated loan market are larger in asset size and sales volume with a mean value of assets of $15 billion and sales of $3 billion. They also have smaller market-to-book ratios compared to all contributing firms. However, the ratios of sales-to-assets, operating profit-to-asset, and debt-to-assets are similar.

In Panel B of Table 1.1, I examine the data of government contracts. The number of recipient firms (among 493 firms) is 333 and 371 in 2007-2009 and 2009-2011 election cycles, respectively. These numbers are proportionately similar for contributing firms with syndicated loans. Government contract amount is defined as the total dollar amount (in millions) of federal government procurement contracts awarded to a firm in a given election cycle. The mean is very high compared to the median as the data have an extremely right-skewed distribution (mean: 9,180 p50:13 p90: 3,872 p95: 22,697 for 2009-2011). Average contract amount is $13,6 billion in 2007-2009 and decreases to $9,2 billion (due to the decrease in contract amount for firms above the 99th percentile) in 2009-2011. However, the median firm receives $13 million-value contracts that are roughly 2.6 times higher than the levels in 2007-2009.

In Table 1.2, I list prevalent industries and headquarter states of the contributing firms. Similar to Table 1.1, column 1 is for all firms (493 firms), and column 2 is for the firms with at least one syndicated loan deal between 2002q2-2007q2 (421 firms). Contributing firms with syndicated loans is a representative sample of all contributing firms regarding industry and state distribution. Leading industries are utilities, chemicals, pharmaceuticals, transportation, communications, and energy and 19 industries comprise approximately 70% of the firms in columns 1 and 2. In both columns, almost half of the firms' headquarters are located in seven states: California, Texas, Illinois, Philadelphia, Ohio, Virginia and New York. Technology firms are clustered in California and energy firms are populated in Texas. Firms in Virginia operate in several industries, but the high number of contributing firms can be explained by the proximity to the Capitol. I provide more information about the characteristics of the firms based on their exposure to loan supply shock in Section 1.4.4.

Last, I examine the characteristics of the banks that have a lending relationship with the firms in my sample as of 2007q2. There are 54 banks, 23 of which are domestic bank holding

companies (BHC) and national financial holding companies (FHC) that are under the supervision of Federal Reserve Board. Eight of the 54 banks are domestic entities that are neither BHC nor FHC (e.g. Merrill Lynch).The remaining banks are headquartered outside the US (e.g. BNP Paribas, Barclays). These banks cover almost all of the syndicated loan market in the USA during the two election cycles between 2005 and 2009.[13]

Figure 1.1 presents the campaign contributions of the sample firms and the syndicated loan issuance to non-financial US firms of these 54 banks. The blue (red) solid line indicates the quarterly number (volume) of syndicated loans. I index the syndicated loan series to 2005q1. The decline of loan issuance starts in 2007q3 when the drop in demand to CLOs (collateralized loan obligations) affected the demand for new loans. The number of new loans in my sample falls by 80% in 2008q4 relative to the peak period in 2007q2. When banks are sorted based on the reduction in new loan issuance, a bank in the 25th percentile reduces lending by 87% whereas a bank in the 75th percentile reduces lending by 68% in 2008q4 compared to 2007q2. The dashed green line depicts the yearly average campaign contributions of my sample firms. As expected, campaign contributions show variation within the election cycle, the amount of contribution is less in odd years than in even years within the cycle. Comparison across the two election cycles shows that contribution amount is higher by 20% in the cycle where the financial crisis takes place.

1.4. Regression analysis

1.4.1. Construction of the campaign contributions variables

The richness of the FEC data allows me to construct several left-hand-side variables to test my hypothesis. I hypothesize that firms increase their contributions following an exogenous negative loan supply shock. I simply start with a firm's average quarterly campaign contribution in a given period through its PAC. Next, I examine the contribution data across different types of candidates. First, I measure contribution amounts received by candidates that run for office in the

[13]The ratio of the volume of loans issued by the sample banks (as the lead lender) to the total volume of loans issued by the lead banks for all non-financial US firms is 98%. The 31 US banks issue a major part of the loans, the ratio of the volume of loans issued by the 31 US banks (as the lead bank) to the volume of loans issued by the 54 banks is 77%.

House of Representatives and Senate separately. According to the Origination Clause of the US Constitution, all revenue-raising (e.g., bills regarding taxation) and revenue-spending (e.g., bills regarding appropriations) legislations must start in the House of Representatives. Firms may find more advantageous to support House members, where potentially firm value creating bills are drafted. Second, I compute the contribution amounts to candidates that run for office in the state in which firm is headquartered since home candidates might have more incentive to help the donating firm in case of distress.[14] In a similar vein, I compute the contribution amount for incumbent and challenger (non-incumbent) candidates as incumbent candidates are highly likely to be re-elected, better able to exert political influence and thus help the donating firm.[15]

I also expect that firms are more likely to support politicians that exercise authority over the industry of the firm via their position in the congressional committees. These politicians are not only able to bestow a favor to donating firms during their election campaign since they already hold an office, but also it is highly likely that these candidates are going to hold a similar position in the next Congress.[16] I link each firm in my sample to committees that are "economically relevant" for their industries.[17] There are committees that have no jurisdiction over a particular industry but are powerful and appealing to every firm (Appropriations, Budget, Ways and Means, and, Small Business Committees in the House of Representatives; Finance Committee in the Senate and the joint committee on Taxation). I assign these committees to every firm. Next, I identify the candidates that hold office at these committees in a given quarter during 109th (January 2005-January 2007) and 110th (January 2007-January 2009) congress. Among these candidates, I also identify the ones that hold chairman and ranking minority member positions in these congressional committees, since these politicians are most influential legislators in the House of Representatives and the Senate.

[14]I use Compustat database "state" variable which indicates the location of the headquarters of the firm instead of the "incorp" variable which indicates the state of incorporation. 58% of the firms in my sample are incorporated in Delaware.

[15]In 2008 elections the reelection rate of house candidates and senate candidates are around 90% and 85%, respectively. (Source: https://www.opensecrets.org/resources/dollarocracy/02.php)

[16]The committee assignment of senators and congressmen are made primarily on the basis of seniority in the committee. Continuous service largely insures a seat on a committee once the politician is re-elected for the next term. For example in the 109th congress appropriations committee comprises of sixty-six congressmen, fifty-four of which served in the same committee also in the 110th congress.

[17]I use the Table B1 in Appendix B of Ovtchinnikov and Pantaleoni (2012) that matches congressional committees to industries using Fama-French-48 industry definitions. However, Compustat provides industry classification based on only NAICS or SIC classification system. Thus, I also use the table from the Kenneth. R. French website that assigns 4-digit SIC codes to each of the Fama-French-48 industry groups. The link table can be found at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_48_ind_port.html

Last, I test whether firms support candidates that might have the ability to influence banks' lending decisions via their power on financial services industry. One mechanism to exert such authority can be through the directorship appointments of public banks that are controlled by the government.[18] The second channel can be the politicians' power to take regulatory or legislative actions affecting financial industry. I test for the second mechanism by measuring firms' campaign contributions to candidates who has jurisdiction over banking industry (Financial Services Committee in the House of Representatives, and Banking, Housing, and Urban Affairs Committee in the Senate).

1.4.2. Construction of the credit supply variable

The empirical strategy of this paper requires that bank-firm relationships are sticky.[19,20] Perfect measurement of bank-firm relationships requires the data of every loan granted to the firms in my sample, which are not available for the US. The second-best option is to use syndicated loans. Syndicated loans are a major source of finance for U.S. corporations. In 2007, they accounted for at least 26% and 36% of the total commercial and industrial loans on the balance sheet of federally supervised institutions and large foreign banks, respectively (Ivashina and Scharfstein, 2010).[21]

Three issues arise concerning the measurement of lending relationships through syndicated loan data. I follow Bharath et al. (2007) to tackle these empirical challenges. First, it is not

[18]This is ruled out in my analysis since the banks in my sample are not government-owned banks.

[19]Presence of bank-firm relationships is documented in the literature. Sharpe (1990) theorizes that banking relationships occur as more information is collected about the customer compared to other firms during screening and monitoring stages of the lending process. The benefits for the lender include extending future loans and other investment banking business (Bharath et al., 2007; Yasuda, 2005; Drucker and Puri, 2005). For the borrower, it indicates lower cost of capital, greater credit availability and lower collateral requirements (Petersen and Rajan, 1994). However, repeated interaction can also lead to hold-up problem which allows lenders to charge higher interest rates (Sharpe, 1990). See also Ongena and Smith, 1998; and Boot, 2000 for a complete review of the literature.

[20]Chodorow-Reich (2014) provides empirical evidence for the presence of bank-firm relationships in the syndicated loan market. He finds that a bank that served as the lead lender of a borrower for the previous loan has a seventy-one percentage point greater likelihood of being the lead lender for the next loan. Moreover, pre-crisis borrowers of healthier banks receive either a favorable modification for their existing loan (e.g. extending maturity, relaxing covenants) or are more likely to obtain a loan during the financial crisis.

[21]The values are computed using Federal Reserve Board of Governors Survey of Terms of Business Lending, Deal Scan database and Shared National Credit (SNC) program (Ivashina and Scharfstein, 2010).

straightforward to identify the banks that establish a relationship with the borrower.[22] I exclude the banks that are identified as "participant" and the banks whose roles are "undisclosed." This conservative approach allows me to avoid eliminating any bank that may take an information-intensive role in the syndicate. The second issue arises because Dealscan does not provide the share of individual banks for most of the deals, which makes measuring the intensity of the relationship using the loan amount retained by each lender infeasible. Bharath et al. (2007) argue that lead banks establish a lending relationship by granting the loan rather than by keeping a particular fraction of the loan amount. Thus, I calculate my relationship measure based on the number of deals between a borrower and a bank rather than the amount retained by the bank.[23,24] The third issue emerges if the original relationship lender disappears due to a merger or acquisition. Similar to Bharath et al. (2007), I account for bank mergers and acquisitions by simply assigning the acquiring bank's ultimate parent company to the new bank. This method of assignment assumes that the relationship between target bank and the borrower is fully transferred to the acquiring bank. However, for the mergers and acquisitions that occur after 2007q2, I do not transfer the lending relationship to the acquiring bank. This treatment assumes a drastic decrease in the value of the firm's previous lending relationship with the target bank. Thus, the borrowers of the target bank do not immediately benefit from the acquiring bank's ability to supply credit even if the acquiring bank is healthy. This assumption is founded on the nature of the acquisitions in 2008.

For every borrower firm i and bank b', I construct a measure of relationship by using the borrower's last five-year borrowing record at the onset of the crisis. I choose this period since 75th percentile of the maturity of the loan facilities in my sample is equal to five years, and it is highly likely for a firm to sign a deal within this period. I define the start of crisis as the

[22]Syndicated loans are offered jointly at least by two lenders. However, the involvement of the lenders with the borrower varies depending on their role in the deal. Typically, the "lead arranger bank" initiates and maintains the relationship with the firm and negotiates the terms and guarantees a loan amount for a price range. Then, the lead arranger seeks "participant banks" to fund a part of the loan. It is also common for a syndicate to have more than one relationship bank, possibly under different title. Francois and Missonier-Piera (2007) suggests that competitive advantages of banks in specific duties lead to multiple lead arrangers within a deal. A number of descriptions such as '"Arranger", "Administrative agent", "Agent", or "Lead manager" correspond to the lead bank status of the lender. Further, the contents of the lenders' role change on par with the evolution of the syndicated loan market.

[23]A second reason why the loan amount retained by the bank might not be a better measure of bank-firm relationship is that as long as the loan agreement permits, the lender may choose to sell its' part immediately in the secondary market (Guner, 2006).

[24]I construct relationship variables using the lending volumes for the robustness checks of my regressions.

beginning of 2007q3. Thus the five-year window opens at 2002q2 and closes at 2007q2. I start with the following equation:

$$relation_{i,b',2007q2} = \frac{loan\ number_{\ i,b',2002q2\text{-},2007q2}}{loan\ number_{\ i,2002q2\text{-},2007q2}} \quad (1)$$

where the numerator is the number of syndicated loans granted to borrower i by bank b' and the denominator is the total number of loans of borrower i issued between 2002q2 and 2007q2.

Next, I normalize $relation_{i,b',2007q2}$ so that the summation across banks adds up to 1:

$$relation\ norm_{i,b',2007q2} = \frac{relation_{i,b',2007q2}}{\sum_b relation_{i,b,2007q2}} \quad (2)$$

Then, I begin to construct a proxy measure of loan supply. As discussed in Chodorow-Reich (2014), a bank's loan supply to a given firm is a function of the bank's internal cost of funds and observable and unobservable characteristics of the firm. The bank's internal cost of funds is not observable. An alternative observable measure should be correlated with the bank's internal cost of funds but not correlated with the unobservable firm characteristics that may also affect firms' campaign contributions. Else, the identification would suffer from omitted variable bias. Chodorow-Reich (2014) proposes a measure that reasonably satisfies these conditions. It is the overall lending of the bank to all other US borrowers excluding the firm itself. This variable is expected to be correlated with the bank's internal cost of funds. To satisfy the second condition, the health of banks must be uncorrelated with the unobserved characteristics of their borrowers that may affect both loan outcome and campaign contributions. I present the arguments on the validity of this condition for my sample in Section 1.4.4.

Formally, let j denote a borrower except for borrower i. $loan\ supply_{-i,b',t}$ indicates the overall lending of the bank b' to all other US borrowers excluding borrower i at quarter t. It is formalized as follows:

$$loan\ supply_{\ -i,b',t} = \sum_j loan\ number_{j,b',t} \quad (3)$$

Last, I compute the loan supply for a given firm *i*, by weighting the overall lending of each lender at time *t* by the *relation* $norm_{i,b,2007q2}$ defined by Eq. (2). I call this variable *relationship loan supply*$_{i,t}$ and compute as follows:

$$relationship\ loan\ supply_{i,t} = \sum_{b} (loan\ supply_{-i,b,t} * relation\ norm_{i,b,2007q2}) \quad (4)$$

Figure 1.2 displays the borrowing record of a firm in my sample and exemplifies how the variables in Eq. (1) to (4) are constructed in each step. Arcbest Corp., a freight transportation, and logistics company signed four syndicated loan deals between 2002q2-2007q2. The lead banks of the syndicates of these loan deals were composed of Bank of America, Wells Fargo, SunTrust Banks, Wachovia and Mitsubishi UFJ Financial Group (except that Mitsubishi did not participate in the deal in 2002). First, I compute the relationship measure of Arcbest with each of these banks. The normalized values for Bank of America, Wells Fargo, Suntrust Banks, Wachovia and Mitsubishi UFJ Financial Group are 0.21, 0.21, 0.21, 0.21, and 0.16 respectively. Using these numbers as weights, I compute relationship loan supply as the weighted average number of loans supplied by these banks (excluding the loans to Arcbest Corp.) for each quarter. Figure 1.2 presents the computation of the variable for 2006q1 and 2008q1 as an example.

1.4.3. Empirical strategy

The estimation strategy aiming to test my hypothesis should primarily deal with the problems of omitted variable bias and reverse causality. There might be unobservable firm or industry-level factors that may affect both firms' campaign contributions and loan supply. A spurious relationship can be established unless these factors are accounted for. Second, reverse causality could be at play. It might be the case that political connections expand business opportunities for the firm leading to an increase in loan demand which, in turn, affects loan supply. I attempt to solve these issues by studying how firms change their contributions to political campaigns around an exogenous shock on their lenders' financial health induced by the 2007-2008 crisis. The exogeneity of the loan supply shock alleviates the reverse causality problem. Further, the crisis provides a small window over which the shock has affected banks and hence borrowing

firms. Many unobserved firm characteristics that can be time-varying in the long run can be accounted for by using fixed effects.

The regression equation is as the following:

$$ln(Contributions)_{i,n,s,t} = \beta.ln(Relationship\ loan\ supply)_{i,n,s,t-1} + \theta.X_{i,n,s,t-1} \quad (5)$$

$$+ I_{n,\,t} + I_{s,t} + I_t + I_i + \varepsilon_{i,n,s,t} \qquad for\ t=0,1$$

where i, n, s, and t denote firm, industry, state, and quarter respectively. The dependent variable is the logarithm of the average quarterly contribution amount by firm i operating in industry n in state s as defined in Section 1.4.1.[25] The independent variable is $ln(Relationship\ loan\ supply)_{i,n,s,t-1}$ that is defined by Eq. (4). Control variables ($X_{i,n,s,t-1}$) are the logarithm of the total assets and the logarithm of the sales to account for size; the operating income scaled by assets to control for profitability. I also control for firm's market-to-book ratio for growth opportunities and debt scaled by assets to account for leverage. Industry-time fixed effects ($I_{n,t}$) absorb the variation that can originate from industry-specific events. Some states are more adversely affected by the financial crisis through shocks to the housing market that may lead to larger economic repercussions in these regions. This might be reflected in the campaign contributions of the firms that are headquartered in these states. I also include state-time fixed effects ($I_{s,t}$) to the regressions to alleviate this concern. While time fixed effects (I_t) absorb the differences across time, firm fixed effects (I_i) capture the unobservable time-invariant characteristics of firms.

I prefer to use a two-period model instead of a panel specification at the quarter-level. An ideal panel setting requires an exogenous measure of loan supply at every quarter. However, exogeneity of loan supply shock already became questionable through the end of 2008 as the crisis spilled over the corporate sector. If I were to use a quarterly panel, the causal link established between loan supply and campaign contributions would be problematic. A two-period "before-after" framework provides flexibility to select proper time windows for the shock.

[25]There are 35 firms that have zero contribution amounts either in 2005-2007 or 2007-2009 election cycle. I follow the approach of Hu (1972) to deal with the problem of zeros in the log-log specification. He states that " … adding a constant to all sample values in the regressand is not as good as the approach of adding a constant only to the sample observations having zero or negative values. It was shown that … the estimated parameter form the latter approach has a smaller deviation from the true parameter than the estimated parameter of the first approach." I use the first method and add a constant to the observations of these firms in both cycles. I choose the constant as the minimum positive contribution amount of the sample. My results are robust to both methods.

As a standard practice in two-period estimations with fixed-effects, I first-difference Eq. (5) and obtain Eq. (6). While this process eliminates firm fixed effects, state-time, industry-time and time fixed effects show up as state dummies, industry dummies, and constant respectively. The regression equation that I actually estimate is as the following:

$$\Delta ln(Contributions)_{i,n,s} = \beta.\Delta ln(Relationship\ loan\ supply)_{i,n,s} + \theta.\Delta X_{i.n,s} + I_n + I_s + Const. + \Delta\varepsilon_{i,n,s} \quad (6)$$

This specification corresponds to a difference-in-difference estimation with a heterogeneous treatment variable ($\Delta ln(Relationship\ loan\ supply)_{i,n,s}$). The coefficient ($-\beta$) estimates whether firms that experience larger drops in loan supply from their relationship lenders during the crisis have a stronger increase in their campaign contributions.

The timing of the exogenous loan supply shock is central for the selection of treatment period. I focus on the early period of the crisis and measure the loan supply during 2007q3-2008q1 for the treatment period.[26] I measure campaign contributions between 2008q2-2008q4 – until the end of the election cycle. I compute the control variables at the same time with the bank lending shock, i.e., over 2007q3-2008q1. For the pre-treatment period, I take the seasonality of the syndicated loan market as well as the biennial cycle of the campaign contributions into account.[27,28] The simplest approach to control for these patterns is to compare identical quarters in pre-treatment and treatment periods. I choose the pre-treatment period as 2005q3-2006q4, that is eight quarters prior to the treatment period. A timeline of my analysis is displayed in Figure 1.3.

Next, I investigate the channels through which campaign contributions alleviate financing constraints. My conjecture is that campaign contributions provide preferential access to government contracts that provide a stable cash flow. First I ask whether firms' campaign contributions are associated with receiving a government contract and larger contracts. I estimate the following two regressions:

[26]The syndicated loan lending dramatically fell starting from 2007q3. It was the time when the concern about the credit risk of the CLOs including securitized syndicated loans led to a drop in new syndicated loan issuance. The decline continued throughout 2008 but by the end of 2008, the crisis already impacted the corporate sector. In a similar vein, Duchin, Ozbas, and Sensoy (2010) suggest that the demand-side of the crisis became apparent after Lehman failure.

[27]Murfin and Petersen (2016) show that firms can borrow more and at a lower interest rate during late spring and fall compared to summer and winter.

[28]Politicians run their campaigns on a two-year cycle and national elections take place in even years. Candidates collect more contributions in even years than they do in odd years as the election date approaches.

$$Contract\ indicator_{i,n,s,T} = \beta.ln(Contributions)_{i,n,s,T-1} + \theta.X_{i,n,s,T-1} + I_{n,T} + I_{s,T} + I_T + \varepsilon_{i,n,s,T} \quad (7)$$

$$ln(Contract\ amount)_{i,n,s,T} = \beta.ln(Contributions)_{i,n,s,T-1} + \theta.X_{i,n,s,T-1} + I_{n,T} + I_{s,T} + I_T + \varepsilon_{i,n,s,T} \quad (8)$$

These regressions cover two election cycles (2007-2009 / 2009-2011) and time T indicates an election cycle. I take the average of all quarter-level variables over one election cycle. $Contract\ indicator_{i,n,s,T}$ is a dummy variable that takes the value one if a contract is awarded to the firm in a given election cycle. $ln(Contract\ amount)_{i,n,s,T}$ is the logarithm of the average quarterly dollar amount of contracts conditional on the firm being awarded at least one contract.[29] $ln(Contributions)_{i,n,s,T-1}$ is the logarithm of the average quarterly campaign contributions of the firm. I compute this variable for the (ex-post) winning candidates, as well. Firm controls include logarithm of the firm's sales, operating profit scaled by lagged assets and market-to-book ratio. All right-hand-side variables are lagged by one election cycle (2005-2007 / 2007-2009). I use a probit model to estimate Eq. (7) since the dependent variable represents a binary outcome. I run an OLS regression to estimate Eq. (8).

Last, I examine how firms' past contributions and contracts are associated with access to bank finance. Out of 421 firms, there are 406 firms with non-missing loan- and firm-level control variables that borrow from the syndicated loan market between 2005-2010. I estimate the following regression:

$$ln(L)_{l,i,n,s,t} = \beta.ln(Contributions)_{i,n,s,t-1} + \varphi.Contract\ indicator_{i,n,s,t-1} + \eta.Loan\ controls_{l,i,n,s,t} + \theta.X_{i,n,s,t-1} + I_n + I_s + I_t + \varepsilon_{l,i,n,s,t} \quad (9)$$

where l denote loan. The dependent variable $ln(L)_{l,i,n,s,t}$ is the logarithm of either the spread or the amount of loan l granted to firm i at quarter t. In line with the literature, I use "deal amount" and "all-in-drawn" variables provided by the Dealscan to measure loan spread and loan amount, respectively. This spread is a measure of the overall cost of the loan because it takes both one-time and recurring fees associated with the loan into account. $ln(Contributions)_{i,n,s,t-1}$ is the

[29]This variable is constructed only with positive values of contract amount in the sample. There are 291 and 320 firms that are awarded contracts in 2007-2009 and 2009-2011 election cycle, respectively.

logarithm of the average quarterly campaign contributions over the last eight quarters before the loan origination date and $Contract\ indicator_{i,n,s,t-1}$ is a dummy variable that takes the value one if a contract is awarded to the firm during the same period. I include loan-level ($Loan\ controls_{l,i,n,s,t}$) and firm-level ($X_{i,n,s,t-1}$) controls that are used in similar studies like Santos (2011) and Houston et al. (2014).[30] I control for the effect of unobservable factors of industry, state and time on through the fixed effects I_n, I_s, and I_t, respectively.

1.4.4. Analysis of the borrower characteristics

In this section, I examine the characteristics of the firms in my sample to substantiate the assumptions of my empirical strategy. My tests rely on the identification condition that cross-sectional variation in lending during crisis period reflects only credit supply factors. Any borrower characteristics captured by the independent variable will render the effect unidentified. Examples of possible concerns could be healthy borrowers switching to relatively healthy lenders or healthy banks supplying loans to borrowers that operated in potentially less-affected regions or industries in anticipation of a crisis. This might have led to either i) a higher credit supply of healthy lenders during the crisis period due to the pre-crisis borrowers' higher creditworthiness or ii) healthy lenders to establish a pre-crisis lending relationship with a disproportionately high number of firms operating in particular industries or regions compared to unhealthy lenders.

To alleviate these concerns, I present two pieces of evidence. First, the crisis did not originate from the banks' corporate loan portfolio, and the timing was not anticipated. Anecdotal evidence and the previous literature (Ivashina and Scharfstein, 2010; Santos, 2011) accommodate this argument and make the identification condition at least admissible. Second, I show that the sample is well-balanced along observable dimensions across lenders, which also favors the even distribution of unobservable factors. These pieces of evidence also lend support to the so-called parallel trend assumption that is required for identification in a difference-in-difference framework. Parallel trend assumption requires that the treatment and control groups show similar patterns during the pre-treatment period. In the absence of the treatment, average change in

[30]For the sake of brevity, I explain the control variables in the text preceding the Table 1.6.

outcome for the treatment group is expected to be equal to the average change in outcome for the control group.

I split borrowers into two quantiles based on the change in relationship loan supply between 2007q3-2008q1 (relative to 2005q3-2006q1). This gives me two balanced groups regarding industry and spatial distribution. [31,32,33] Next, I examine the characteristics of the group firms. Panel A in Figure 1.4 plots the difference in the average asset growth, sales growth, quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets and total debts scaled by assets between these two group of firms. A visual inspection can tell that the series follow similar trends before the financial crisis. Next, I regress each series in Panel A on a quarterly time trend variable and a constant. Panel B marks the coefficients of the time trend and plots 95% confidence intervals. For all regressions, 95% confidence intervals include zero supporting my identification assumption that the characteristics of firms with a pre-crisis lending relationship with a healthy lender is very much alike to that of firms with a pre-crisis lending relationship with a less healthy lender.

Last, I examine whether campaign contributions of sample firms exhibit a similar pre-treatment pattern. I divide the sample into two groups of firms that stay above and below the median change in loan supply of their relationship banks between 2007q3-2008q1 (relative to 2005q3-2006q1). Figure 1.5 shows the difference in average quarterly campaign contributions between these two groups. The series of each group are demeaned by industry, state, and time before differencing. The pattern supports my identification condition allowing me to infer that the difference in campaign contributions is a result of the difference in the change in loan supply by the relationship banks.

[31]Firms with less healthy lenders and with healthy lenders are headquartered in thirty-eight and thirty-seven states respectively. Thirty-two of these states exist in both groups. States that do not belong to both groups are relatively small states (Washington DC, Delaware, North Dakota, Rhode Island, South Carolina, South Dakota, Hawaii, Iowa, Idaho, New Mexico and Utah)and are represented with very few firms in the sample.

[32]The number of industries that is common to both groups is seventeen. The group with distressed lenders has three additional firms that operate in agriculture (code 11), educational services (code 61), and other services (code 81). The group with healthy lenders has seven additional firms (general merchandise stores) that operate under retail trade category (code 45)

[33]My results are robust to dropping industries or states that are not common to both groups of firms.

1.5. Main Results

I present the two-period difference-in-difference estimation results of Eq. (6) in Table 1.3. As discussed in Section 1.4.3, 2005q3-2006q4 and 2007q3-2008q4 are selected as the pre-treatment and treatment period respectively. The dependent variable, *ln(Contributions)*, is the logarithm of the average quarterly campaign contributions during 2008q2-q4 (relative to 2006q2-q4). My independent variable, *ln(Relationship loan supply),* is defined as the logarithm of the average quarterly number of the syndicated loans granted to nonfinancial US firms (excluding the firm itself) during 2007q3-2008q1 (relative to 2005q3-2006q1) by the pre-crisis relationship banks.

Across all specifications, I find that firms increase their campaign contributions, as their pre-crisis relationship banks cut lending during the crisis. The estimations are statistically significant and exhibit similar order of magnitudes that are given in the last row of Table 1.3. In column 1, the coefficient is -0.88 and is statistically significant at the 5%-level. A decrease in *ln(Relationship loan supply)* by one standard deviation leads to a 9% increase in campaign contributions. In column 2, I add market-to-book and debt-to-asset ratio as additional controls. The coefficient is -0.79 and statistically significant at the 5%-level. To keep as much firm as possible in my sample, I do not use market-to-book and debt-to-asset ratio in remaining regressions. In column 3, I use the classification of the lead arranger given by the Dealscan "Lead Arranger Credit" field to identify pre-crisis relationship banks.[34] The coefficient is -0.65 and significant at the 5%-level. One standard deviation decrease in the *ln(Relationship loan supply)* leads to a 7% increase in campaign contributions. In the last column of Table 1.3, I use the total amount of loans (in dollars) as a proxy for the loan supply of the firms' relationship banks during the crisis. The regression results have a similar order of magnitude and have a 5% statistical significance. A decrease in *ln(Relationship loan supply)* by one standard deviation leads to an 8% increase in campaign contributions.[35]

[34]This is a yes/no field indicating whether or not a particular lender will receive Lead Arranger League Table credit based on Reuters LPC's League Table guidelines. These data are also used in the literature to identify banks that take an information-intensive role in the syndicates (e.g. Sufi, 2007). This narrows down the broad definition of Bharath et al. (2007) that assign a relationship between a borrower and each bank in the syndicate except the banks with the "participant" role.

[35]As explained in Section 4.1, the fraction of the loan retained by each lender is largely missing in the data. One way to measure the loan supply of a given lender might be to sum up the dollar value of the estimated fraction of the lender over the deals. This would be mostly imprecise and misleading. I tackle this problem of missing allocation data as follows: I only keep the lead banks defined by Dealscan to narrow down the number of relationship banks

In Table 1.4, I report the results of the regressions where I investigate how contributions are distributed over different types of candidates. The dependent variable is the logarithm of the average quarterly campaign contributions for a given type of the candidate that is indicated in the header of each column. Apart from the usual control variables used in Table 1.3, the regressions in Table 1.4 also contains *ln(Contr. excluding the group)*. It is computed as the logarithm of the average quarterly campaign contributions to the candidates excluding the group indicated in the header. This variable allows controlling for the general trend in campaign contributions within the firm. Across all specifications, its coefficient is positive and statistically significant at the 1%-level, which underlines the need for inclusion of this variable for proper specification.

I conjecture that firms with a pre-crisis lending relationship with the less healthy lenders may be more inclined to support candidates with power and willingness to take the actions that benefit the firm. As discussed in Section 1.4.1 in detail, I expect an incremental effect on the contributions to the House candidates, home-state candidates, and incumbents. In a similar vein, I expect the politicians that exert authority over the firm's industry through their seats in the congressional committees to receive more contribution from the borrowers of less healthy lenders.

The results presented in the columns 1 to 6 of Table 1.4 are in line with this conjecture. For the first two columns, my findings suggest that one standard deviation decrease in *ln(Relationship loan supply)* leads to a 6% increase in campaign contributions to the House candidates when I control for the contributions to Senate candidates. Such an effect is not observed for Senate candidates when I control for the contributions to House candidates. In column 3 and 4, I find that one standard deviation decrease in *ln(Relationship loan supply)* leads to a 6% increase in contributions to the candidates that run for a seat in the firm's home state whereas no such effect is observed for the candidates that run for office for other states. Columns 5 and 6 examine incumbent and entrant candidates. My findings suggest an 8% increase in contributions to incumbent politicians when I control for the contributions to entrants whereas no incremental effect exists for non-incumbents. Column 7 of Table 1.4 refers to the candidates that serve on committees at the time of contribution. Column 8 of the same table refers to senior members that serve as chairmen and ranking minority member(s) of these congressional

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then ascribe the total loan amount of a given deal to these lenders. In case of multiple lead lenders within the syndicate, I divide the loan amount into equal parts.

committees on the date of contribution. My findings suggest that one standard deviation move in *ln(Relationship loan supply)* leads to a 5% and a 4% increase in campaign contributions to committee members and senior committee members respectively. Last, I test whether firms contribute to politicians that serve in Financial Services Committee in the House of Representatives, and Banking, Housing, and Urban Affairs Committee in the Senate. I present the results in columns 9 and 10 of Table 1.4. I do not find any increase in contributions to the candidates that serve on these committees. The findings suggest that firms do not prefer to channel their contributions to politicians that may influence banks' lending decisions through their authority in the financial industry.

In Table 1.5 and 1.6, I investigate the channels through which campaign contributions alleviate financing constraints. First, I ask whether the level of campaign contributions is associated with the allocation of government contracts. I present the estimation result of Eq. (7) and Eq. (8) in Table 1.5. In columns 1 and 2, I find that firms' likelihood of receiving a government contract is positively associated with the level of campaign contributions in the previous election cycle. I estimate a probit model and report the marginal effects when all the independent variables are held at their median. Economically, one standard deviation change (±1/2 s.d.) is associated with a 4% increase in likelihood to win a government contract. The result is slightly pronounced for contributions to winning candidates. While the economic effect increases to 5%, statistical significance rises from the 10% to the 5%-level. Then, I investigate whether firms' campaign contributions in the past election cycle are associated with larger contracts (conditional on being awarded a contract) in the next cycle. I report the results of all contributions and contributions to winning candidates in columns 3 and 4, respectively. The coefficients are 0.329 and 0.333 and statistically significant at the 1%–level. Economically, one standard deviation increase in the contributions is associated with $6.8 million more government contracts for the median firm (median value is $10 million). Similar to column 2, the result is pronounced for contributions to winning candidates. One standard deviation increase in the contributions to winning candidates is associated with $7 million more government contracts for the median firm.

Last, I examine how contributions and access to government contracts are associated with bank borrowing terms. I present regression results of Eq. (9) in Table 1.6. I find that past campaign contributions are negatively associated with loan spreads and positively associated

with loan amounts; while being a government contractor is negatively correlated with loan spreads. The effects are statistically significant and exhibit similar economic magnitude across columns. The coefficient signs of the control variables are also in line with similar studies. I find that larger size, higher profitability, and market-to-book ratio as well as better credit ratings, and past lending relationships are significantly negatively correlated with loan spreads. Larger size, better credit ratings, and past lending relationships also have a positive association with the loan amount.

My findings in column 1 of Table 1.6 indicate that after controlling for relevant firm and loan specific factors, as well as time, state and industry trends, one standard deviation increase in contributions is associated with a 6 basis points decrease in loan spread for the median firm (median value is 112 basis points). In column 2, I add *Contract indicator* that takes the value one if the firm is a recipient of a government contract for the last eight quarters. Being a government contractor is further associated with a reduction in loan spread by 6.5% (1- exp(-0.067)) or 7 basis points for the median firm. In columns 3 and 4 of Table 1.6, my dependent variable is loan amount. While one standard deviation increase in contributions of the median firm is associated with a $93 million increase in loan amount (median value is $775million), being a government contractor is not statistically associated with the loan amount.

1.6. Robustness Checks

In this section, I perform additional regressions to test the robustness of my main results.

1.6.1. Alternative pre- and post-treatment periods

In Table 1.7, I report two-period difference-in-differences estimates of Eq. (6) for alternative periods. This exercise allows me to check whether a particular period is driving the main results. The treatment periods are indicated in the header of each column. The second and third row of the header shows the sub-periods in which *ln(Relationship loan supply)* and *ln(Contributions)* are computed respectively. The treatment period proceeds the pre-treatment period by eight quarters.

In the first two regressions, I change the sub-periods where I measure the *ln(Relationship loan supply)* and *ln(Contributions)* within the baseline treatment period. In the first and the second columns of Table 1.7, the coefficient estimate of *ln(Relationship loan supply)* is -0.43 and -0.49, respectively. The magnitude of the effect decreases to 6% as I narrow down the period where I measure loan supply. This diminishing effect is reasonable since credit crunch became more apparent starting from 2008q1 and adversely affected banks had more drastic cuts in syndicated loan lending hereafter. For the third and fourth columns of Table 1.7, I extend the treatment period towards 2009 to see how long the effect continues. The statistical significance persists through 2009q2 with an average impact of 6% but disappears in 2009q3. I expect the effect to vanish within a year since these firms are relatively large companies with political ties thus can establish new lending relationships in a rather short period. In the last column, the variables for the pre-treatment period are computed as average values of the two periods that precede the treatment period by eight and sixteen quarters. The coefficient of *ln(Relationship loan supply)* is -0.59 and statistically significant at the 10%-level. The result remains significant with a slightly diminishing effect, which is reasonable as my bank-firm relationship measure becomes less precise as the date moves further away from 2007q2.

1.6.2. Bank financing substitutes and financing needs

If financing constraints drive the increase in campaign contributions as in my conjecture, then firms that can substitute bank loan with other external funding sources at the onset of the crisis may have less need for external finance in the short run. Thus, they may not increase their campaign contributions as much even though their relationship banks fared poorly. To test this conjecture, I construct two variables to measure corporate bond and equity issuances of the firm. I define *Bond issuance* and e*quity issuance* as the amount of issuance between 2007q3-2008q1 (2005q3-2006q1) scaled by lagged assets as of 2007q2 (2005q2) for the treatment (pre-treatment) period. Then, I run my baseline regression after including these variables as controls.[36]

[36]I use Thomson One Deals database for corporate bond issuance. The equity issuance variable comes from the Compustat (the Compustat items *sstky* and *prstkcy*). The number of sample firms in the regression that controls for equity issuance is subject to data availability.

The result in column 1 of Table 1.8 suggests that corporate bond issuance at the onset of the crisis decrease campaign contributions for the upcoming quarters. The coefficient is -0.21 and statistically significant at the 5%-level. This finding suggests that one standard deviation increase in corporate bond issuance during pre-crisis period leads to a decline by 5% in campaign contributions. The coefficient of *ln(Relationship loan supply)* is -0.63 and statistically significant at the 5%-level, the magnitude of the effect is around 6.5%. In column 2 of Table 1.6, I find that equity issuance at the onset of the crisis have no statistically significant effect on campaign contributions even though the sign of the coefficient is in line with the conjecture. The coefficient magnitude and statistical significance of *ln(Relationship loan supply)* are not affected, which suggests that my main independent variable is robust to the inclusion of these variables.

In Table 1.8, I also examine the link between campaign contributions and the firms' near-term financing needs. A considerable part of the syndicated loans is issued for debt repayment and refinancing purposes. If a syndicated loan matures in a period during which the conditions of refinancing are tight, this should have an effect on campaign contributions for the same period. For this purpose, I construct a dummy variable, *Is debt mature*, that takes value one if the maturity date of a firm's existing syndicated loan falls on a date between 2008q2-2008q4 (2006q2-2006q4) for the treatment (pre-treatment) period. I run my baseline regression after including *Is debt mature* as a control variable similar to the previous exercise. I present the results in column 3 of Table 1.8. I find that firms with a loan due during the second half of 2008 (compared to second half of 2006 of the pre-treatment period) increase their campaign contribution by 13% for the same period. The coefficient of *ln(Relationship loan supply)* is -0.89 and stays economically and statistically significant.

1.7 Conclusion

Access to adequate external financing is a vital issue for firms. The financial crisis of 2008 demonstrated how important external financing is, not only for small and medium enterprises but also for large firms. In this paper, I present supporting evidence on the idea that firms invest in political connections to alleviate financing constraints when they face a negative credit supply shock.

I examine the causal link between banks' syndicated loan supply and non-financial firms' campaign contributions during the 2007-2008 financial crisis. I examine how firms change their campaign contributions around an exogenous shock on their lenders' financial health induced by the crisis. My empirical approach relies on the existence of a lending relationship between a bank and a borrower that is established through multiple transactions. As the bank collects more information about the borrower, switching to a new lender become costly for borrowers. Second, I exploit the fact that the financial crisis originates from the real estate market. The financial health of the banks was differentially affected due to the different levels of exposures to the subprime mortgage loans. This variation across banks allows me to define a firm-level credit supply variable as the overall syndicated lending of the firm's pre-crisis relationship lenders to other borrowers. I adopt a difference-in-differences framework, and I explain the changes in the firms' campaign contributions before and during the financial crisis with the changes in firm-level credit supply.

I find that a 10% decrease in loan supply of a firm's pre-crisis relationship lenders leads to a 9% increase in campaign contributions in 2008. Next, I show that campaign contributions are associated with increased likelihood to win a federal government contract and larger contracts. Further, past level of campaign contributions and being a government contractor is linked to favorable borrowing terms. My findings lend support to the idea that campaign contributions are an investment in political capital and can be a tool to alleviate financing constraints.

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1.8. Tables and figures

Table 1.1. Summary statistics

This table reports summary statistics. *Contribution amount* is shown in dollars and refers to the total amount of contribution to political election campaigns by the firm per election cycle (eight quarters). *Number of supported candidates* is the number of candidates whose political election campaigns are supported by the firm through contributions. *Contribution amount per candidate* is *Contribution amount* divided by *Number of supported candidates*. *Relationship loan supply* for a given firm is the average number of the syndicated loans granted to non-financial US firms (excluding the firm itself) by the banks with which the firm has a pre-crisis lending relationship. It is computed as a weighted average of the overall quarterly lending of each lender by using the bank-firm lending relationship measure (as defined by Eq. (2)) as weights. *Total assets* are the book value of firm's assets in millions; *Sales* are the quarterly sales in millions. *Market-to-book* is the sum of the market value of the firm's equity and the book value of firm's debt, all divided by the firm's book value of assets. *Sales/Assets* is sales scaled by lagged assets. *Operating profit/Ass*ets is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. *Debt/Assets* is the total debt scaled by assets. Panel B reports summary statistics of government contract data for 2007-2009 and 2009-2011 cycles. *Government contract amount* is the total dollar amount of federal government procurement contracts awarded to a firm in a given election cycle.

Panel A:

		2005-2007 cycle		2007-2009 cycle	
		All Contributing Firms	Contributing Firms w/ syndicated loan	All Contributing Firms	Contributing Firms w/ syndicated loan
Number of firms		493	421	493	421
Contribution amount	Mean	139,957	152,620	169,579	183,851
	Median	42,200	49,250	55,500	61,500
	S.dev.	257,198	270,399	319,946	339,020
Number of supported candidates	Mean	48	51	54	58
	Median	22	25	27	30
	S.dev.	66	68	74	77
Contribution amount per candidate	Mean	2,268	2,310	2,426	2,461
	Median	2,127	2,188	2,316	2,380
	S.dev.	1,064	1,070	1,065	1,072
Relationship loan supply	Mean		147		97
	Median		139		88
	S.dev.		49		52
Total assets ($mil)	Mean	13,519	14,559	14,814	15,726
	Median	4,902	5,654	5,830	6,579
	S.dev.	30,202	31,755	28,057	28,967
Sales ($mil)	Mean	2,934	3,123	3,401	3,590
	Median	986	1,126	1,126	1,278
	S.dev.	7,117	7,301	8,338	8,507
Market-to-book	Mean	4.00	3.10	2.60	2.40

	Median	2.40	2.30	2.20	2.20
	S.dev.	52	12	33	34
Sales/ Assets	Mean	0.27	0.26	0.27	0.26
	Median	0.21	0.21	0.22	0.22
	S.dev.	0.20	0.19	0.22	0.19
Operating profit /Assets	Mean	0.03	0.03	0.02	0.03
	Median	0.02	0.02	0.02	0.02
	S.dev.	0.03	0.02	0.03	0.03
Debt/Assets	Mean	0.28	0.29	0.30	0.31
	Median	0.26	0.27	0.28	0.28
	S.dev.	0.23	0.19	0.26	0.24

Panel B:

		2007-2009 cycle		2009-2011 cycle	
		All Contributing Firms	Contributing Firms w/ syndicated loan	All Contributing Firms	Contributing Firms w/ syndicated loan
Number of firms with federal government contract		333	291	371	320
Government contract amount ($mil)	Mean	13,614	12,578	9,180	10,449
	Median	5	5.5	13	15
	S.dev.	100,348	94,672	70,990	76,324

Table 1.2. Industry and state distribution of firms

The table presents the distribution of firms across industries and across states of headquarters.

Industry	All contributing firms	Contributing firms w/ syndicated loan
Utilities	75	71
Chemicals & plastics	29	25
Pharmaceuticals	28	25
Transportation (rail, land, water, air)	25	21
Communications	25	19
Oil & natural gas	20	18
Computer Software	26	17
Hospitals & health care services	19	16
Primary & fabricated metal	14	13
Computer hardware & semi-conductors	18	11
Defense	17	11
Surgery & other medical instruments	13	9
Beverages	9	8
Motor vehicles	7	7
Aircraft & parts	7	7
Casinos & resorts	7	7
Restaurants	6	6
Detergents & cosmetics	5	4
Tobacco	3	3
TOTAL	**353**	**298**
	72%	71%
States		
California	52	36
Texas	50	47
Illinois	28	27
Philadelphia	30	26
Ohio	28	28
Virginia	26	22
New York	24	19
TOTAL	**238**	**205**
	48%	49%

Table 1.3. Main results

The table reports two-period difference-in-differences estimates of Eq. (5). The actual estimation is performed through Eq. (6) after first-differencing. The first (pre-treatment) period is 2005q3-2006q4 and the second (treatment) period is 2007q3-2008q4. The dependent variable is the logarithm of the average quarterly campaign contributions that is measured over 2006q2-2006q4 (2008q2-2008q4) for the pre-treatment (treatment) period. *ln(Relationship loan supply)* is the logarithm of the average number of the syndicated loans granted to non-financial US firms (excluding the firm itself) by the banks with which the firm has a pre-crisis lending relationship. It is computed as the weighted average of the overall quarterly lending of each lender over 2005q3-2006q1 (2007q3-2008q1) for the pre-treatment (treatment) period by using the bank-firm lending relationship measure (as defined by Eq. (2)) as weights. *ln(Assets)* is the logarithm of the book value of firm's assets in millions, *ln(Sales)* is the logarithm of sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. *Market-to-book* is the sum of the market value of the firm's equity and the book value of firm's debt, all divided by the firm's book value of assets. *Debt/Assets* is the total debt scaled by assets. All firm-level controls are measured over 2005q3-2006q1 (2007q3-2008q1) for the pre-treatment (treatment) period. Standard errors are corrected for heteroscedasticity. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable: ln(Contributions)	Baseline	Baseline w/ more control variables	Loan supply measured by the number of lead loans	Loan supply measured by the volume of lead loans
	(1)	(2)	(3)	(4)
ln(Relationship loan supply)	-0.88**	-0.79**	-0.65***	-0.23**
	(2.40)	(2.07)	(3.25)	(2.37)
ln(Assets)	0.80***	0.78***	0.80***	0.80***
	(3.34)	(3.09)	(3.35)	(3.35)
ln(Sales)	0.17	0.16	0.16	0.18
	(0.59)	(0.55)	(0.56)	(0.62)
Operating profit/Asset	6.00*	3.95	6.20*	6.19*
	(1.71)	(0.96)	(1.78)	(1.78)
Market-to-book		0.00		
		(0.11)		
Debt/Asset		-0.23		
		(0.74)		
N	421	415	421	421
R2	0.31	0.28	0.31	0.30
% change when ln(Relationship loan supply) move by one s.d.	9%	8%	7%	8%

Table 1.4. Campaign contributions across different types of candidates

The table reports two-period difference-in-differences estimates of Eq. (5). The actual estimation is performed through Eq. (6) after first-differencing. The first (pre-treatment) period is 2005q3-2006q4 and the second (treatment) period is 2007q3-2008q4. The dependent variable is the logarithm of the average quarterly campaign contributions that is measured over 2006q2-2006q4 (2008q2-2008q4) for the pre-treatment (treatment) period. The type of the candidate is indicated in the header of each column. The headers of the first and second columns indicate the candidates that run for the House of Representatives and the Senate respectively. The third column refers to candidates that run for a state seat where the company is headquartered, and the fourth column refers to candidates that run for a seat in any other state. The fifth and sixth columns refer the incumbents and entrant candidates respectively. The header of the column seven indicates the candidates that serve in congressional committees that have jurisdiction over the firm's industry. Column eight refers to senior members that serve as chairmen and ranking minority members of these congressional committees. In a similar vein, the ninth and tenth column indicates members and senior members that serve on Financial Services and Banking Housing and Urban Affairs Committees respectively. *ln(Relationship loan supply)* is the logarithm of the average number of the syndicated loans granted to non-financial US firms (excluding the firm itself) by the banks with which the firm has a pre-crisis lending relationship. It is computed as the weighted average of the overall quarterly lending of each lender during the pre-treatment (treatment) period by using the bank-firm lending relationship measure (as defined by Eq. (2)) as weights. *ln(Assets)* is the logarithm of the book value of firm's assets in millions, *ln(Sales)* is the logarithm of sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. All firm-level controls are computed over the quarters where *ln(Relationship loan supply)* is measured. *ln(Contr. excluding the group)* is the logarithm of the campaign contributions to candidates excluding the group indicated in the header. Standard errors are corrected for heteroscedasticity. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable: ln(Contributions)	House	Senate	Home	Out-of-home	Incumbent	Non-incumbent	Committee	Senior Committee	Bank Committee	Senior Bank Committee
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln(Relationship loan supply)	-0.64**	-0.32	-0.60*	-0.55	-0.79**	-0.36	-0.53*	-0.39*	-0.36	-0.10
	(2.06)	(0.83)	(1.82)	(1.34)	(2.01)	(1.08)	(1.78)	(1.82)	(1.38)	(0.32)
ln(Assets)	0.45*	0.46	0.28	0.55**	0.77***	0.12	0.16	-0.01	0.09	-0.19
	(1.87)	(1.50)	(1.15)	(2.34)	(3.23)	(0.48)	(0.71)	(0.05)	(0.51)	(0.90)
ln(Sales)	0.18	-0.07	0.20	-0.05	0.11	0.19	-0.07	-0.10	0.23	0.32
	(0.72)	(0.19)	(0.68)	(0.18)	(0.39)	(0.61)	(0.28)	(0.46)	(1.12)	(1.30)
Operating Profit/Asset	2.89	0.44	-2.71	2.76	5.44	-1.73	1.12	-0.89	-1.34	-2.58
	(0.90)	(0.10)	(0.65)	(0.83)	(1.59)	(0.59)	(0.36)	(0.40)	(0.51)	(1.12)
In(Contr. excluding the group)	0.29***	0.32***	0.26***	0.26***	0.11**	0.17***	0.37***	0.14***	0.15***	0.25***
	(5.57)	(5.28)	(4.73)	(4.40)	(2.59)	(2.92)	(6.41)	(3.32)	(3.72)	(4.28)
N	421	421	421	421	421	421	421	421	421	421
R2	0.34	0.31	0.32	0.31	0.34	0.25	0.24	0.39	0.22	0.22

Table 1.5. The effect of campaign contributions on the procurement of government contracts

The table reports the probit model estimates of Eq. (7) in column 1 and 2 and the OLS estimates of Eq. (8) in column 1 and 2. The regression covers two election cycles (2007-2009 / 2009-2011) and time unit is one election cycle. All quarter-level variables are averaged over the election cycle. *ln(Contract amount)* is the logarithm of the average quarterly dollar amount of contracts conditional on the firm being awarded at least one contract in a given election cycle. *Contract indicator* is a dummy variable that takes the value one if a contract is awarded to the firm. *ln(Contributions to candidates)* is the logarithm of the average quarterly campaign contributions. *ln(Contributions to winning candidates*) is measured similarly, but only for the contributions to (ex-post) winning candidates. *ln(Sales)* is the logarithm of sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. *Market-to-book* is the sum of the market value of the firm's equity and the book value of firm's debt, all divided by the firm's book value of assets. All right-hand-side variables are lagged by one election cycle (2005-2007 / 2007-2009). For probit specifications, the reported values are marginal effects when all independent variables are held at their median value. Standard errors are corrected for heteroscedasticity and clustered at the firm-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable:	Contract indicator		ln(Contract amount)	
	(1)	(2)	(3)	(4)
ln(Contributions to candidates)	0.026*		0.329***	
	(1.74)		(3.33)	
ln(Contributions to winning candidates)		0.032**		0.333***
		(2.13)		(3.44)
ln(Sales)	0.060***	0.056***	0.455***	0.449***
	(3.54)	(3.27)	(2.63)	(2.60)
Operating Profit/Assets	-0.649	-0.630	2.148	2.414
	(0.72)	(0.70)	(0.25)	(0.28)
Market-to-book	-0.004*	-0.004*	-0.008	-0.008
	(1.66)	(1.66)	(1.25)	(1.29)
Time fixed effects	Y	Y	Y	Y
State-time fixed effects	Y	Y	Y	Y
Industry-time fixed effects	Y	Y	Y	Y
Number of observations	773	773	609	609
Number of firms	420	420	327	327
R2			0.27	0.28
Pseudo-R2	0.14	0.14		

Table 1.6. The effect of campaign contributions and government contracts on loan spreads

This table reports the estimation results of Eq. (9). The dependent variable is *ln(Loan spread)* in columns 1, and 2. It is defined as the logarithm of the loan spread that is the interest spread over LIBOR plus loan arrangement fees. In columns 3, and 4, the dependent variable is *ln(Loan amount)* that is defined as the logarithm of the loan size in millions. *ln(Contributions)* is the logarithm of the firm's average quarterly campaign contributions over the last eight quarters before loan origination date. *Contract indicator* is a dummy variable that takes the value one if a contract is awarded to the firm during the same period. *Is relationship lending* is a dummy variable equal to one if the firm borrowed from the lender of the current loan over the last four quarter. *In(Sales)* is the logarithm of the sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. *Debt/Assets* is the total debt scaled by assets. *Market-to-Book* is the sum of the market value of the firm's equity and the book value of firm's debt, all divided by the firm's book value of assets. These variables are averaged over the last eight quarters before loan origination date, as well. *S&P credit rating index* is an index that assigns a value for each firm's S&P domestic long-term issuer credit rating ranging from one to twenty. The value of the index increases as the rating deteriorates (i.e. the index =1 for firms with an AAA rating). For the firms without a credit rating, the index is equal to twenty-one. I lag the index value by one quarter. *Loan controls* indicate whether the following loan-level controls is included in the regression: *ln(Maturity)* is the logarithm of the loan maturity in months. *Loan type* and *loan purpose* are dummy variables that control for different loan types and purposes (general corporate purposes, working capital, and acquisition) respectively. *Secured* and *Senior* take the value of one if the loan is secured and senior, respectively. *Dividend restriction* is a dummy variable indicating the loan has dividend payment restriction clause. *Performance pricing* indicates whether the loan has a pricing scheme that is contingent upon its performance. *ln(Loan amount)* and *ln(Loan spread)* is used as controls unless it is the dependent variable. Standard errors are corrected for heteroscedasticity and clustered at the industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable:	ln(L. spread)	ln(L. spread)	ln(L. amount)	ln(L. amount)
	(1)	(2)	(3)	(4)
In(Contributions)	-0.020**	-0.020**	0.046***	0.047***
	(2.66)	(2.59)	(3.16)	(3.22)
Contract indicator		-0.067*		-0.069
		(1.81)		(0.76)
Is relationship lending	-0.075*	-0.077*	0.149**	0.151**
	(1.73)	(1.77)	(2.22)	(2.30)
In(Sales)	-0.065**	-0.059**	0.438***	0.443***
	(2.65)	(2.57)	(7.33)	(7.33)
Operating Profit/Asset	-9.581***	-9.544***	-3.108	-3.090
	(6.76)	(6.60)	(0.94)	(0.94)
Debt/Assets	0.520***	0.513***	0.227	0.220
	(7.50)	(7.37)	(1.13)	(1.09)
Market-to-book	-0.005**	-0.005**	-0.001	-0.001
	(2.33)	(2.31)	(0.19)	(0.18)
S&P credit rating index	0.039***	0.039***	-0.021*	-0.020*
	(6.94)	(6.84)	(2.00)	(1.95)
Loan Controls	Y	Y	Y	Y
Quarter/industry/state fixed effects	Y	Y	Y	Y
Number of loans	1819	1819	1819	1819
Number of firms	406	406	406	406
R2	0.77	0.77	0.49	0.49

Table 1.7. Alternative periods

The table reports two-period difference-in-differences estimates of Eq. (5). The actual estimation is performed through Eq. (6) after first-differencing. The treatment periods are indicated in the header of each column. For the first four columns, the pre-treatment period precedes the treatment period by eight quarters. For the fifth column, pre-treatment values of all variables are computed as average values of the periods that precede the treatment period by eight and sixteen quarters. The second row of the header indicates the period where loan supply is measured, and the third row indicates the period where campaign contributions are measured. The dependent variable is the logarithm of the average quarterly campaign contributions. *ln(Relationship loan supply)* is the logarithm of the average number of the syndicated loans granted to non-financial US firms (excluding the firm itself) by the banks with which the firm has a pre-crisis lending relationship. It is computed as the weighted average of the overall quarterly lending of each lender for the pre-treatment (treatment) period by using the bank-firm lending relationship measure (as defined by Eq. (2)) as weights. *ln(Assets)* is the logarithm of the book value of firm's assets in millions, *ln(Sales)* is the logarithm of sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. All firm-level controls are computed over the quarters where *ln(Relationship loan supply)* is measured. Standard errors are corrected for heteroscedasticity. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable: ln(Contributions)	2007q3-2008q4 Loan Supply: 2007q3-2007q4 Contribution: 2008q1-2008q4	2007q3-2008q4 Loan Supply: 2007q3 Contribution: 2007q4-2008q4	2007q3-2009q2 Loan Supply: 2007q3-2008q1 Contribution: 2008q2-2009q2	2007q3-2009q3 Loan Supply: 2007q3-2008q1 Contribution: 2008q2-2009q3	2007q3-2008q4 Loan Supply: 2007q3-2008q1 Contribution: 2008q2-2008q4
	(1)	(2)	(3)	(4)	(5)
ln(Relationship loan supply)	-0.43***	-0.49***	-0.64*	-0.43	-0.59*
	(2.63)	(2.68)	(1.89)	(1.25)	(1.71)
ln(Assets)	0.48**	0.40*	0.47**	0.54***	0.52**
	(2.16)	(1.86)	(2.29)	(2.66)	(2.42)
ln(Sales)	0.33	0.36	0.12	-0.01	0.42*
	(1.45)	(1.50)	(0.47)	(0.04)	(1.72)
Operating profit/Asset	4.36*	3.45	3.50	4.02	4.33
	(1.71)	(1.41)	(1.14)	(1.46)	(1.20)
N	421	421	421	421	421
R2	0.25	0.24	0.20	0.19	0.26
% change when ln(Relationship loan supply) move by one s.d.	6%	6%	6%	-	5%

Table 1.8. The effect of corporate bond and equity issuance and debt to mature on campaign contributions

The table reports two-period difference-in-differences estimates of Eq. (5). The actual estimation is performed through Eq. (6) after first-differencing. The first (pre-treatment) period is 2005q3-2006q4 and the second (treatment) period is 2007q3-2008q4. The dependent variable is the logarithm of the average quarterly campaign contributions that is measured over 2006q2-2006q4 (2008q2-2008q4) for the pre-treatment (treatment) period. *ln(Relationship loan supply)* is the logarithm of the average number of the syndicated loans granted to non-financial US firms (excluding the firm itself) by the banks with which the firm has a pre-crisis lending relationship. It is computed as the weighted average of the overall quarterly lending of each lender over 2005q3-2006q1 (2007q3-2008q1) for the pre-treatment (treatment) period by using the bank-firm lending relationship measure (as defined by Eq. (2)) as weights. *ln(Assets)* is the logarithm of the book value of firm's assets in millions, *ln(Sales)* is the logarithm of sales in millions. *Operating profit/Asset*s is the quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets. All firm-level controls are measured over 2005q3-2006q1 (2007q3-2008q1) for the pre-treatment (treatment) period. *Bond issuance* and *equity issuance* are defined as the amount of issuance between 2005q3-2006q1 (2007q3-2008q1) scaled by lagged assets as of 2005q2 (2007q2) for the pre-treatment (treatment) period for corporate bond and equity respectively. *Is debt mature* is a dummy variable that indicates the loan maturity date of a firm's syndicated loan falls on a date between 2006q2-2006q4 (2008q2-2008q4) for the pre-treatment (treatment) period. Standard errors are corrected for heteroscedasticity. *t*-Stats are reported below coefficient estimates. *, **, *** denote the statistical significance at the 10%, 5%, and 1%-level, respectively.

Dep. Variable: ln(Contributions)	(1)	(2)	(3)
ln(Relationship loan supply)	-0.63**	-0.85**	-0.89**
	(2.25)	(2.11)	(2.41)
ln(Assets)	0.76***	0.64**	0.75***
	(3.64)	(2.54)	(3.09)
ln(Sales)	0.08	0.27	0.17
	(0.31)	(0.84)	(0.63)
Operating Profit/Asset	6.00	6.33*	6.02*
	(1.58)	(1.65)	(1.70)
Bond issuance	-0.21**		
	(2.52)		
Equity issuance		-0.50	
		(1.22)	
Is debt mature			0.13*
			(1.82)
N	421	360	421
R2	0.21	0.35	0.32

Figure 1.1. Firms' campaign contributions and syndicated loan issuance

This graph presents time series of the yearly average campaign contributions of the sample firms and quarterly number and volume of syndicated loan issuance to non-financial US firms of the 54 banks with which these firms have a pre-crisis lending relationship. The blue (red) solid line indicates the quarterly number (volume) of syndicated loans. I index both series to 2005q1. The dashed line depicts the yearly average campaign contributions of sample firms in US dollars.

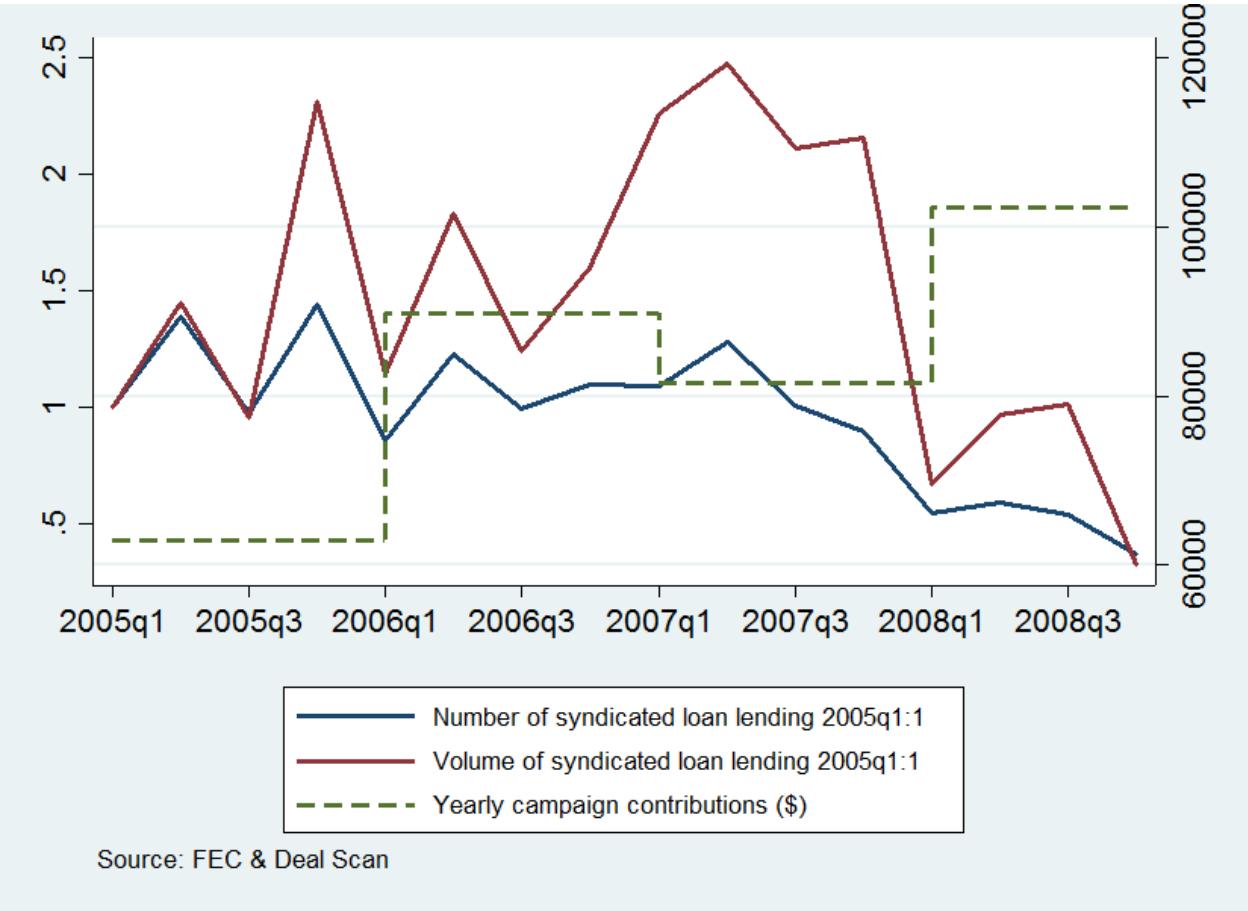
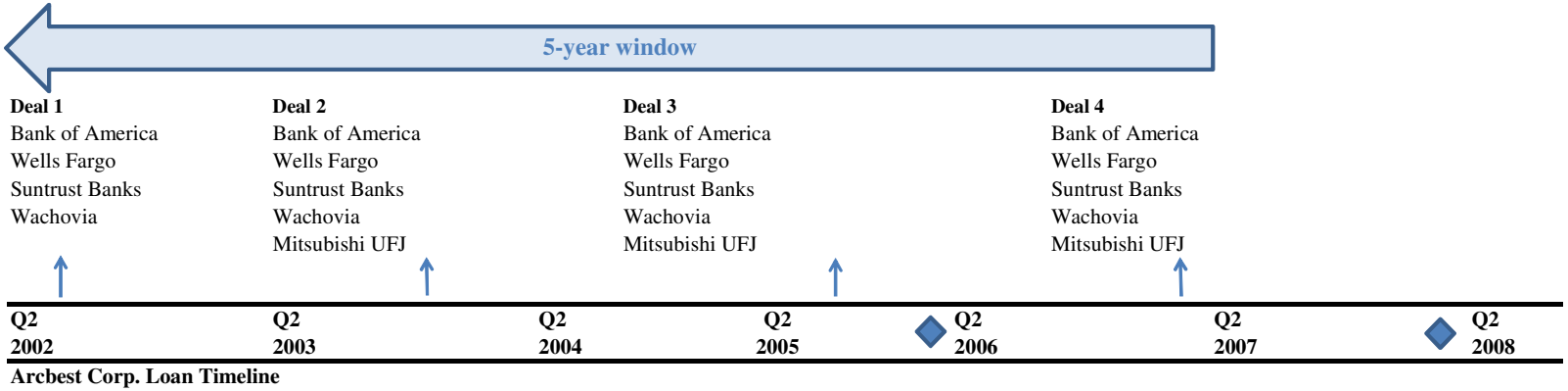


Figure 1.2. An example for the construction of the relationship loan supply variable for 2006q1 and 2008q1



relation $_{\text{Arcbest, Bank of America, 2007q2}}$ =4/4
relation $_{\text{Arcbest, Wells Fargo, 2007q2}}$ =4/4
relation $_{\text{Arcbest, Suntrust, 2007q2}}$ =4/4
relation $_{\text{Arcbest, Wachovia, 2007q2}}$ =4/4
relation $_{\text{Arcbest, Mitsubishi, 2007q2}}$ =3/4

relation norm $_{\text{Arcbest, Bank of America, 2007q2}}$ =1/4.75 =0.21
relation norm $_{\text{Arcbest, Wells Fargo, 2007q2}}$ =1/4.75 =0.21
relation norm $_{\text{Arcbest, Suntrust, 2007q2}}$ =1/4.75 =0.21
relation norm $_{\text{Arcbest, Wachovia, 2007q2}}$ =1/4.75 =0.21
relation norm $_{\text{Arcbest, Mitsubishi, 2007q2}}$ =0.75/4.75 =0.16

loan supply $_{\text{-Arcbest, Bank of America, 2006q1}}$ =247
loan supply $_{\text{-Arcbest, Wells Fargo, 2006q1}}$ =165
loan supply $_{\text{-Arcbest, Suntrust, 2006q1}}$ =85
loan supply $_{\text{-Arcbest, Wachovia, 2006q1}}$ =141
loan supply $_{\text{-Arcbest, Mitsubishi, 2006q1}}$ =321

relationship loan supply $_{\text{Arcbest, 2006q1}}$
=247x0.21+165x0.21+85x0.21+141x0.21+321x0.16
=185.4

loan supply $_{\text{-Arcbest, Bank of America, 2008q1}}$ =190
loan supply $_{\text{-Arcbest, Wells Fargo, 2008q1}}$ =111
loan supply $_{\text{-Arcbest, Suntrust, 2008q1}}$ =45
loan supply $_{\text{-Arcbest, Wachovia, 2008q1}}$ =122
loan supply $_{\text{-Arcbest, Mitsubishi, 2008q1}}$ =307

relationship loan supply $_{\text{Arcbest, 2008q1}}$
=190x0.21+111x0.21+45x0.21+122x0.21+307x0.16
=147.4

Figure 1.3. Timeline of the analysis

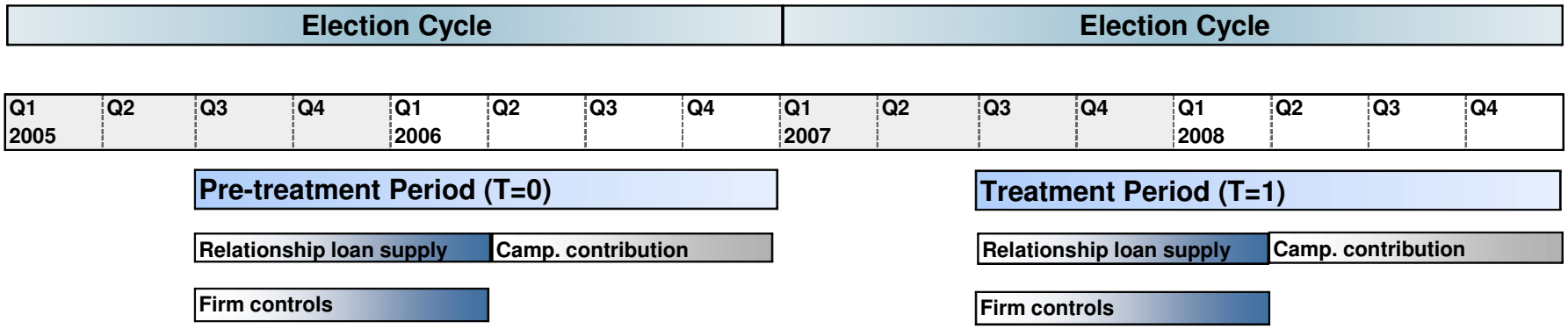


Figure 1.4. Borrower characteristics before the crisis

This figure is prepared by classifying the sample firms based on being above and below the median change in relationship loan supply between 2007q3-2008q1 (relative to 2005q3-2006q1). The graphs in Panel A plot the difference in the quarterly average values of several firm characteristics of these two groups of firms before 2007q2. These firm characteristics are asset and sales growth in the upper graph, quarterly earnings after interest, taxes, depreciation, and amortization scaled by lagged assets and total debts scaled by assets in the lower graph. Panel B is created by regressing each series in Panel A on a quarterly time trend variable. It marks the coefficients and plots 95% confidence intervals of the time trend variable for each firm characteristic.

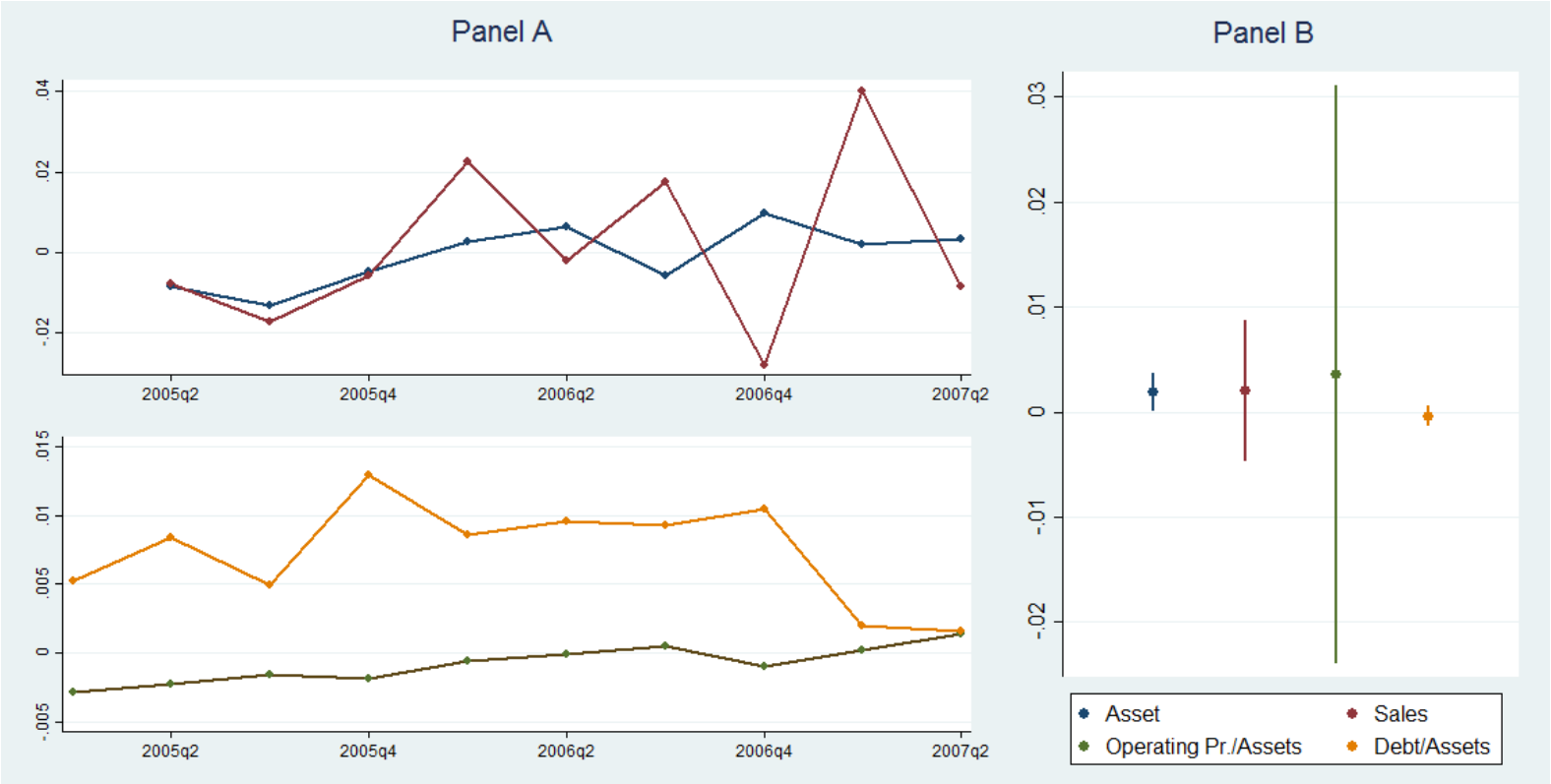


Figure 1.5. Parallel trends in campaign contributions

The graph displays the difference in quarterly average campaign contributions between two groups of firms that are constructed by dividing the sample firms based on being above and below the median change in relationship loan supply between 2007q3-2008q1 (relative to 2005q3-2006q1). The series are demeaned by industry, state, and time before differencing.

Chapter 2

The Financial Integration and Growth: Banks' Previous Industry Exposure Matters

2.1. Introduction

Over the past four decades, states (countries) have become much more integrated financially, in many instances through out-of-state (foreign) bank-entry. For example, banking deregulations in the US have led to the emergence of financial conglomerates that can now operate unhindered within the 50 states of the Union. A similar trend is also observed for the EU-member countries.[1] There is evidence suggesting that the effects of financial integration go beyond the simple provision of additional capital. For example, Morgan, Rime, and Strahan (2004) find that there is synchronization of states' output fluctuations following integration through the banking sector. In fact, a number of papers point to a reallocation of capital across industries following financial integration (see Fisman and Love, 2004, for international evidence; Acharya, Imbs, and Sturgess, 2011, for the US; and Bekaert et al., 2013, for the EU). However, we know little about the micro mechanisms behind the macro-level evidence of the observed economic convergence that follows financial integration. The contribution of this paper is to explore the role of a particular channel in this reallocation process: industry-specific information collection and processing by financial institutions when providing capital to firms located in different markets that they enter. In other words, we examine whether financial integration can affect the growth of various industries differently given the market-entrant financial institutions' previous exposure to the same industry.

More specifically, we test for a channel that works through commercial banks' exposure to more prevalent industries in their "home" state. Our conjecture is that financial integration with out-of-state banks that are more knowledgeable about an industry should lead to faster growth in that sector. We test this hypothesis using a series of quasi-natural experiments: staggered bank-entry deregulations at the state-pair-level during the 1980s and 1990s. We proceed as follows. First, we define the specialization of a manufacturing industry in a state as the ratio of that sector's share of manufacturing output (i.e., value added) to its share of overall US manufacturing output.[2] Second, we presume that banks in a given state that is more-specialized in an industry would naturally lend more to that sector on average (compared to banks in states in which the same sector is less-specialized). Hence, before

[1]Evidence indicates that interregional banking integration leads to more firm formation (e.g., Cetorelli and Strahan, 2006), higher industry turnover (Kerr and Nanda, 2009), more interregional trade (Michalski and Ors, 2012), and higher industry growth (e.g., Bruno and Hauswald, 2014).

[2]Our index adapts that of revealed-comparative advantage proposed by Balassa (1965) to the context of U.S. state industrial production, a standard approach in regional economics or international trade studies. An under-specialized (over-specialized) industry would have a ratio less (higher) than one.

entering new markets banks in states that are more-specialized in an industry would have, on average, more information about the functioning and prospects of that sector, compared to institutions operating in states that are less-specialized.[3]The information collected and processed by the banks in their home state's more prominent (more-specialized) industries would be reflected in their ability to screen and monitor loans in that sector (for ex., through specialization of lending officers or the use proprietary credit scoring systems). Third, we conjecture that when these same banks enter a new market in another state for the first time (typically through the acquisition of a local bank in their "host" state post bank-entry deregulation), their home state industry exposure would give these lending institutions a natural advantage in screening loans. This informational advantage would arise, for example, through the sharing (with the acquired bank) of lending officers who know of a particular industry, or proprietary credit scoring models. We justify these steps using the related evidence from the literature (see Section 2.2 below). Finally, using state-pair-industry-level data, we test differential growth rates of less-specialized industries in a state-pair following the less-specialized state's banking deregulation and financial integration with the more-specialized state for a given sector.

To conduct our tests, we rely on US data that have a number of clear advantages over cross-country studies. First, banking integration is shown to affect the real economy in the US (e.g., Morgan, Rime, and Strahan, 2004; Cetorelli and Strahan, 2006; Kerr and Nanda, 2009; Rice and Strahan, 2010; Michalski and Ors, 2012). Moreover, during the years that we study, the banking sector forms roughly one-fifth to one-third of the US financial sector. So any effect that we observe is unlikely to be economically negligible. Second, US manufacturing firms operate in a single and fairly homogeneous economic and legal environment. As such, we do not have to worry about confounding effects (for example, differences in legal systems as documented in La Porta et al., 1997 and 1998, among others) with which cross-country studies have to deal. Third, we concentrate our study on manufacturing industries that typically face US-wide competition, can organize their activities easily anywhere in the Union, are not subjected state-level barriers to entry, have (in principle) access to the same technology and inputs with similar quality, and whose output

[3]Comparative advantage of local lenders is examined both theoretically and empirically in the literature. For example, in the Dell'Ariccia, Friedman, and Marquez (1999) model, asymmetric information between incumbent and entrant banks arises thanks to the information processing that is involved in granting prior loans to borrowers in the local market. Consistent with the hypothesis that local banks have lower information asymmetries, Bofondi and Gobbi (2006) find that Italian banks entering a new market have higher default rates than incumbents.

data are fairly homogenous across different sub-industries.[4] Finally, and very importantly, the use of US data allows us to control for the endogeneity of lending institutions' entry: we can instrument banking integration thanks to the staggered interstate bank-entry deregulations that took place at different points in time for different state-pairs. Our empirical setup allows us to control for existing economic conditions prior to deregulation (for example, neighborhood effects or geographic distance for a state-pair, their pre-existing industry compositions or natural endowments) as well as state- and industry-level confounding factors that vary over time.

The results are supportive of our hypothesis. First, we check whether industries in states that are classified as being less-specialized in those sectors grow faster than the same industries in states that are classified as being more-specialized: We observe no difference between the growth measures for industries located in states that are less versus more-specialized in them. This general observation holds true even when we examine quartiles of the data that we create defined by the differences in sector-level specialization between state-pairs. The growth of sectors in less versus more-specialized states does not differ even when the difference in specialization is at its highest (as defined by the fourth quartile of difference in specialization). These observations are important, because we would like to rule out the possibility that the results that we find are driven by differential growth measures across less-specialized versus more-specialized states. Then, we conduct sets of regressions, using different test variables and estimators. In these regressions, we control for a very large set of confounding factors explicitly by including state-year effects, industry-year effects, state-pair-industry effects, and implicitly including state-pair-industry-years effects (since our dependent variable is the differential growth of a sector between a pair of states).[5] Consistent with our hypothesis, we observe higher growth for less-specialized manufacturing industries in a given state when the state's banking system gets integrated with that of another state that is more-specialized in the same sector. These findings are driven by cases in which the

[4]This is not necessarily true for agriculture, mining or some service industries (e.g. electricity generation or shipping) where the natural endowment is decisive for the location choices. It is also not true for service industries (e.g. real estate, retail) where the local demand is important or various laws might limit industry growth (financial services being an example). Moreover, the capital intensity of the services sector is typically lower than that of manufacturing. Such considerations prevent conducting proper testing for the effects that we study in this paper for industries other than manufacturing.

[5]As described further below, our state-pair-industry-level dependent variable is constructed such that the growth of a given industry in the less specialized state is always benchmarked on the growth of the same industry in the more specialized state of the pair. This approach allows us to refine our tests: if our conjecture holds true, we should observe an effect that increases with higher difference in sector-specialization between a state-pair (as of the date of deregulation).

difference in industry specialization in a state-pair is higher, which is consistent with a re-allocation of capital.

Our coefficient estimates exhibit reasonable magnitudes. We find that for states with less-specialized industries, the increase of banking integration from zero to 1.2% (the average for the estimation sample) with the more-specialized states' banks leads to a 0.83% increase in the differential growth of value added over and above a comparable benchmark of the same industry in the more-specialized states. We obtain similar results for the sector-level gross operating surplus (capturing the total remuneration of capital), total compensation, total number of employees, and productivity (i.e., value added by employee). These findings are stronger when we split the sample into quartiles based on the difference of state-pair's industry specializations: the coefficient estimates of interest are larger and more statistically significant in the fourth quartile (where state-pair industry specialization difference is at its highest). Moreover, these findings are robust to changes in the sample, estimation period, estimation method (OLS with IV versus Arellano-Bond with IV), and the fixed effects included in the regression.

Our results provide evidence consistent with a micro-level channel for the macro-level evidence on industrial convergence provided by Kim (1995), and Dumais, Ellison, and Glaeser (2002) in general, and as a result of bank branching deregulation by Acharya, Imbs, and Sturgess (2011) in particular. To the best of our knowledge, there are no papers on the sector-specific exposure of financial institutions and their industry-level impact following entry. An exception is Bernstein et al. (2016) that provide international evidence of country-level industry growth following private equity firms' entry (we detail the differences between their paper and ours in Section 2.2 below).

The implications of our work go beyond academic curiosity. Our results suggest that the origins of institutions acquiring or merging with another economic region's banks can exert important influences on the industrial structure of the latter: banks, given their previous industry exposure, can play a non-trivial role in shaping industry structure of the economies that they enter. An acquirer from an economic region (state or country) that specializes in the automobile industry would have a potentially different and lasting imprint on the industrial structure (hence its future economic growth and industrial development) than an acquirer from an economic region (state or country) that specializes in the food industry.

The paper proceeds as follows. In Section 2.2 we review the literature important for our hypothesis. In Section 2.3 we detail the empirical approach and the data that we use. In

Section 2.4 we present the main results. In Section 2.5 we discuss the robustness of our empirical findings and their economic relevance and consistency. Section 2.6 concludes.

2.2. Literature review

Our paper is related with different strands of the literature on financial integration and growth. First, our work is linked to the research on the growth of industries given the financial development of countries. Rajan and Zingales (1998) show that external finance dependent industries grow faster in economies with higher financial development. Wurgler (2000) finds that there is more (less) investment in growing (declining) industries in countries with more developed financial markets compared to states with a less developed financial sector. Fisman and Love (2004) find that industry growth across countries is more correlated for country-pairs with more developed financial sectors, which suggests that the financial sector, given its level of development, leads to similar shock responses across different countries. Following US interstate banking deregulations Cetorelli and Strahan (2006) find that the resulting higher banking competition is associated with the growth of small firms at the expense of large ones, whereas Kerr and Nanda (2009) document that small firm entry and exit (the so-called "churning" effect) increases. Bruno and Hauswald (2014) provide evidence that foreign bank-entry can have a positive effect on external finance dependent industries; whereas Behn et al. (2014) report that post financial liberalization industry growth depends on the interaction of domestic and foreign banks given the competitiveness of the local banking system prior to foreign bank-entry. One channel through which capital reallocation is taking place appears to be through improvements in firm productivity. Beck, Levine and Loayza (2000) find that country-level total factor productivity (TFP) growth is higher for countries that experience increases in private credit. Bertrand, Schoar, and Thesmar (2007) document that credit in France went to more productive firms following the 1985 removal of lending directives imposed on banking institutions, with deregulation leading to a change in allocations in the real economy. Krishnan, Nandy, and Puri (2015) find that TFP of small firms' increases following higher branching deregulation in the US. In contrast to these papers, we show a given industry's post-deregulation growth, including the growth of its productivity per worker, is affected by entrant-banks' prior exposure to the sector.

Our paper is also closely related with a smaller strand of the literature that examines the effects of financial integration across countries or states. Morgan, Rime, and Strahan (2004) find that banking integration across states helps smooth regional output fluctuations in the US while the risk of transmission of macroeconomic shocks across states increases.[6] Acharya, Imbs, and Sturgess (2011) observe that following the removal of interstate bank branching restrictions not only did the states' output volatility decreased, but that states' industrial portfolios started to converge towards a common US benchmark, with the effect being driven by sectors with a larger share of young, small and external finance dependent companies. In a similar vein, Bekaert et al. (2013) observe reductions in European intra-sector growth differentials following this economic region's financial (albeit through equity market) integration. Michalski and Ors (2012) show that integration of the real sector across regions follows financial integration: they find that the state-pairs that experience higher integration following pairwise interstate banking deregulations trade more compared to non-integrated states. The above-cited results on the reallocation of capital across sectors and regions (states or countries), suggest that banks' lending policies can affect the industrial landscape, especially so after important bank-entry deregulations. Little is known so far, however, as to the micro-channels through which financial integration is affecting the industrial composition of economic areas.

One exception is Bernstein et al. (2016) who study the impact of private equity firms' entry into a country on the growth of industries the former specialize in. These authors examine growth rates of productivity, employment, and capital formation at the country-industry-level with international data covering 20 sectors in 26 large economies between 1991 and 2007. They find that following PE investment in a country, the industries in which these institutions specialize enjoy higher total production, value added, total wages and employment growth. While our results complement theirs, our paper differs from Bernstein et al. (2016) in many dimensions. First, we use US interstate banking deregulations as a series of quasi-natural experiments to identify the industry growth effects of (potentially endogenous) financial integration through the banking sector. In our case financial integration between pairs of states could not increase before interstate banking deregulations became effective. This allows us to use a clear identification scheme that varies over time and state-pairs. In contrast, pinning down identification is much harder in an international setting

[6]Goetz and Gozzi (2013) use finer state-pair-industry-level data and interstate bank-entry deregulations for identification (as in Michalski and Ors, 2012; and Goetz, Laeven, and Levine, 2013). They find results that are similar to Morgan, Rime, and Strahan (2004) who rely on state-level data.

as it is very difficult, if not impossible; to find exogenous changes that would generate strong instrumental variables. Without exogenous deregulatory events similar to ours, it is also more difficult in cross-country studies to account for the possible effects of other developments in the financial sector.[7] Second, during the period covered in our study the commercial banks' role in the US remains very important: 21.1% to 34.5% of the financial sector total assets in 1994 and 1985, respectively (Financial Accounts of the United States, 2014). Other segments of the financial industry were less influential during these years (and remain so in international settings even today). Importantly, in the US setting that we rely on, other segments of the financial sector (for example, investment banking) did not exhibit similar patterns of entry and integration for the same state-pairs during the same years. As such, we can clearly establish a causality running from banking integration to industry growth. Third, our US setting allows us to conduct counterfactual exercises by examining the growth of less-specialized sectors when banking integration takes place with states that are also less-specialized in the same industries. Such exercises allow us to rule out the possibility that our results are merely driven by statistical artifacts. Finally, we conduct a series of additional regressions and observe that our empirical results are robust. Moreover, a simple calculation exercise based on a Cobb-Douglas production model allows us to check the consistency of our various estimates with respect to each other. In the next section we review our approach to identify the impact of banking integration on industry growth, define the empirical specification that we use, and provide information on the data and their sources.

2.3. Identification, empirical specifications, and the data

2.3.1. Identification

We first elaborate on the economic channels that are behind our hypothesis. We conjecture that less-specialized industries in a state would grow faster if their state experiences banking integration with other states in which the same sector is more-specialized.

Our conjecture requires that industry-specific information (for example, in the form of proprietary credit scoring models, or transferring loan officers) is shared among banks

[7]For example, Behn et al. (2014) use international data and find evidence of industry-level growth after major financial deregulations, which are typically followed by foreign bank entry. However, they do not examine whether foreign banks' pre-entry industry exposure plays a role in that sector's growth in the host country.

belonging to a multi-bank holding company (MBHC).[8] It means that the sector-specific information flows from a member bank located in a state that is more-specialized in a particular industry, to another affiliated bank operating in a state that is less-specialized in the same industry. MBHCs play a central role in our story because following interstate banking deregulations, which we use to identify the effect of banking integration; bank-entry took place through the acquisition of deregulating states' banks by out-of-state banking conglomerates.[9] In this setting, a natural way for information to flow within the expanding MBHC would be the sharing of proprietary credit scoring systems of previously separate banking entities.[10] Such information flows between banks of the same financial conglomerate are to be expected given evidence in the literature indicating that information sharing does occur across bank and non-bank subsidiaries of the same MBHC. For example, Gande et al. (1997) show that during securities issuance, MBHCs fulfill a certification role in a way that is consistent with a flow of information from the commercial banks to investment banking (the so-called Section 20) subsidiaries of the same financial conglomerate. Similarly, Massa and Rehman (2008) examine the portfolio choices of mutual funds that are proprietary to MBHCs and find that mutual funds significantly increase their investments in firms borrowing larger amounts from MBHC-affiliated banks, which is consistent with information flows from the banking subsidiary to the mutual fund subsidiary. Newer evidence on mutual funds by Luo, Manconi, and Schumacher (2014) suggests that target (acquirer) funds start investing in sectors that the acquiring (targeted) fund used to invest in before the acquisition. More pertinently for our conjecture, Schumacher (2015) finds that when investing abroad international mutual funds overweight the largest industry segments of their home countries (i.e., the sectors they are more exposed to in their home country).

There is also another strand of the literature (Winton, 2000; Stomper, 2006) that makes theoretical arguments for the sector-level specialization of banks in their lending.[11] However,

[8]MBHCs were a common form of banking conglomerate in the US during the 1980s and 1990s.

[9]Banks were able to open new branches across state lines (if the host state allowed it) after the adoption of the 1994 Interstate Bank Branching and Efficiency Act (IBBEA, also known as the Riegle-Neal Act), which become effective in 1995. As the data available to us do not extend beyond 1997, we cannot exploit this legislative change, which, for example, Krishnan, Nandy, and Puri (2015) use to examine the effect of more bank finance on firms' TFP.

[10]For the role and importance of credit scoring systems in bank lending in the US refer to Frame, Srinivasan and Woosley (2001); Akhavein, Frame, and White (2005); and Berger, Frame, and Miller (2005), among others.

[11]Winton (2000), studying the costs and benefits of lending diversification, provides theoretical arguments suggesting Modern Portfolio Theory-based lending may not be the optimal strategy if monitoring is costly and loans have important downside risk (i.e., it may pay off to specialize under certain conditions). Stomper (2006) suggests that industry-expert banks may extract rents that are proportional to the sector-specific risks that they take: this would lead to a banking market equilibrium in which certain banks specialize in lending to certain sectors, leading to a sector-level concentration in lending.

the related empirical evidence to date is mixed.[12] That said, for our conjecture to go through we do not need banks coming from states that are more-specialized in certain industries to be specialized (or focused) in lending primarily to these sectors. The fact that these banks would have more information about these sectors (in which their state is more-specialized) *relative* to banks in their newly entered markets would suffice. In our story, the newly acquired bank would improve its lending with better screening through the additional sector-specific information provided by the acquiring-MBHC that operates in states that are more-specialized in the same sector. The information channel is especially pertinent for states that are less-specialized in an industry, which are the focus of our paper. Our setup allows us to account for the size of the difference in industry-specific specialization "gap" between any two state-pairs. We find that when the difference in the specialization of states in a given industry is small, banking integration has no effect on differential industry growth. The effects that we observe are driven by cases in which a state-pair has a large difference in its specialization in a given industry. Next, we provide a discussion of the problem of endogeneity that we face in conducting our analysis.

Ideally, a direct test of our hypothesis would involve data on the sector composition of US banks' loan portfolios before and during the integration process. Post-acquisition by MBHCs from states that are more-specialized in a sector, we should observe an increase in the segment-level lending by the (acquired) banks in the state that is less-specialized in the same industry.[13] Unfortunately, such industry-level decomposition of bank lending is not available in the financial statements (the so-called, Call Reports) of the US commercial banks that have to file with the federal regulators. Instead, we rely on state-industry-year-level data and regress the annual growth rates of less-specialized industries on, among other variables, a test variable that captures state-and-industry-specific bank-integration with more-specialized states (more detail is provided in Section 2.3.2). However, such regressions would be biased

[12]Using Italian data Acharya, Iftekhar, and Saunders (2006) find that diversification of banks' industrial lending does not guarantee higher portfolio performance, suggesting that there may be benefits to specialization. Hayden, Porath, and van Westernhagen (2007) find that lending to certain sectors generally increases loan portfolio performance, but not necessarily in the way anticipated by Winton (2000) or found by Acharya, Iftekhar, and Saunders (2006). More recently, Tabak, Fazio, and Cajuerio (2011) use Italian data and find that industry-specialization leads to higher portfolio returns and lower risk. In a similar vein, Böve, Düllmann, and Pfingsten (2010) observe that specialization leads to better monitoring by German banks, whereas Jahn, Memmel, and Pfingsten (2013) find that these institutions' specialization reduces loan write-offs. In contrast, Beck and De Jonghe (2013) examine an international sample of large banks and find that sector-level specialization generates higher volatility and lower returns.

[13]We know of no evidence to date on post bank-acquisition portfolio convergence for commercial and industrial loans at the industry level. That said, there is limited anecdotal (e.g., Wall Street Journal, 1996) and empirical (e.g., Zarutskie, 2013) evidence of portfolio harmonization across loan categories for banks (i.e., business loans, real-estate loans, personal loans, etc.).

and inconsistent if bank-integration would be endogenous to industry structure in general and industry growth potential in particular.

From one point of view, endogeneity is not likely to be a major concern: existing evidence on the political economy of interstate banking deregulation does not attribute a role to lobbying by non-financial industries (Kane, 1996; Kroszner and Strahan, 1999). Even if non-financial industries were to play a role in interstate banking deregulations, it is improbable that the industries in which a state is less-specialized (i.e., smaller), and on which we focus, would be the driving lobbying force for interstate bank-entry deregulation at the state legislature. Nevertheless, even if the deregulation process is not likely to be endogenous to the growth of less-specialized industry segments, some banks' entry decisions might be endogenous: at least some MBHCs' entry may have been driven by opportunities in lending growth. If so, our banking integration might be endogenous to the growth of industry segments.

This is where the staggered series of interstate banking deregulations provide us with a powerful identification tool at the state-industry-level through the use of instrumental variables approach similar to Morgan, Rime, and Strahan (2004), Michalski and Ors (2012), and Goetz, Laeven, and Levine (2013). Because both our bank integration variable and the IVs vary at the state-industry-year-level, we can identify the impact of integration of a state's banking system with those located in states that are more-specialized in an industry.

Finally, interstate banking deregulations also allow us to come up with the proper counterfactuals to rule out the possibility that our regressions are merely picking up spurious correlations. If the information channel we have in mind would hold true, then we should observe *no* effect when a state that is less-specialized in certain industry segments would find its banking system integrated with banks of other states that are also less-specialized in the same industry. This is what we exactly find: if a state ends up with more banking links with another state that is similarly specialized in a given industry, that sector does not experience higher growth. Put differently; such integration provides no additional benefits regarding information flows, loan screening and monitoring for the concerned industry.

2.3.2. Empirical specifications

In this section, we introduce the regression specifications and variables that we use and detail the empirical challenges that we face. We first calculate the annual state-level

specialization for each of the 19 two-digit SIC manufacturing industries.[14] As mentioned earlier, specialization is defined as the ratio of a sector's share of state's manufacturing output (i.e., value added) to the same sector's share of overall US manufacturing output.

Then, we use the following regression equation to examine changes in relative sector-level growth at the state-pair-level after interstate banking deregulation:

$$\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 L1.DEREGULATED_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t} \qquad (2.1)$$

where, $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (Y) of sector s in state i and year t *relative* to the growth of the same sector s in state j and year t, with i (j) being the less (more)-specialized state of the pair in sector s as of the date of effective interstate deregulation for state pair i-j; [15] $DEREGULATED_{i,j,t}$ is an indicator variable that is equal to 1 starting with the year *after* (and including all the subsequent years) the state-pair i-j effectively opens their markets to each other's banks, and 0 otherwise; $\delta_{i,j,s}$ is the state-pair-industry fixed effect, $\delta_{i,t}$ is the state-year fixed effect for state i, $\delta_{j,t}$ is the state-year fixed effect for state j, $\delta_{s,t}$ is the sector-year fixed effect, and δ_t is a year fixed effect; $e_{i,j,s,t}$ is the error term. The six output variables (Y) used in the analysis are defined in Section 2.3.3 below. It should be noted that this is a very demanding specification. The annual differencing of industry growth rates at the state-pair-level takes out the effects of any shock that affects a particular industry at the state-pair-level in a given year. Furthermore, $\delta_{i,j,s}$ fixed effect soaks up any unobservables that are state-pair-industry specific and that remain constant over time. As such, any sector-specific differences in initial endowments, or geography related advantages for the state-pair (such as proximity) are accounted for. As such, the initial tendency of small sectors (these would be among the less-specialized ones in a state) to grow faster and large ones to grow slower, something that could otherwise drive our results, would be absorbed by $\delta_{i,j,s}$. Put differently, $\delta_{i,j,s}$ fixed effect accounts for any observable or unobservable *pre-conditions* (such as sector-specific endowments, or lack thereof) that might have an impact on sector specific growth. State-year fixed effects ($\delta_{i,t}$ and $\delta_{j,t}$) account for state-level changes in economic factors (for example, economic growth at the state-level, the

[14] As explained in Section 2.3.3 below, the number of manufacturing industries (19) with which we can work is imposed on us by the publicly available version of the Census data as provided by the Bureau of Economic Analysis (BEA).

[15] $\Delta\ln(Y_{i,s,t})$ is the growth of sector s in state i and year t, i.e., $\Delta\ln(Y_{i,s,t}) = \ln(Y_{i,s,t}) - \ln(Y_{i,s,t-1})$. The order of growth terms is fixed as of the date of effective deregulation of the state-pair and does not change over time, irrespective of changes in specialization of states i and j in sector s over the years.

effects of state-wide legislation, for example about minimum wages, etc.) Industry-year fixed effects ($\gamma_{s,t}$) account for time-varying developments in sector *s* at the US-level that could exacerbate the growth of more or less-specialized industries (our differenced specification implicitly takes care of industry-year growth that is common for all states).We also have year fixed effects, δ_t, to account for the growth of the US economy (of course, one of the many fixed effects that each of $\delta_{i,t}$, $\delta_{j,t}$ and $\delta_{s,t}$ involve is dropped to avoid multicollinearity with δ_t).

In Eq. (2.1) the dependent variable ($\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$) is at the state-pair-industry-time-level whereas the test variable ($DEREGULATED_{i,j,t}$) varies at the state-pair-level. This is likely to render identification difficult, as our hypothesis suggests that entry by banks from states that are more-specialized in sector *s* into states that are less-specialized in the same industry should lead to higher growth of *s* in less-specialized states. One way to improve identification is to take into account the discrepancy in specialization in a given industry. The larger the difference in specialization in *s* in a deregulating state-pair is, the higher the effect that we hypothesize should be. The largest differences would typically correspond to cases in which state *i* is under-specialized and state *j* is over-specialized. Small differences in sector-specific specialization of state-pair *i-j* would amount to comparing growths of sectors in which both states are similar in terms of specialization. That is, small differences in specialization would be akin to comparing growth of industry *s* across a deregulating state-pair *i-j* when both states are similarly under-, over- or not particularly specialized, thus conveying no specific informational advantages to banks of state *j* entering state *i*.

To functionalize this improvement in identification, we define $\Delta SPECIALIZATION_{i,j,s} = |SPECIALIZATION_{i,s} - SPECIALIZATION_{j,s}|$ where specializations are defined as of the year of effective banking deregulation of state-pair *i-j*. There are different ways to incorporate this difference (or lack thereof) in specialization in a state-pair into our tests. One possibility is to run Eq. (2.1) after having classified all observations pertaining to state-pairs per industry by the difference in specialization in that sector (as of the interstate banking deregulation) and run separate regressions. We do so after classifying all observations in state-pair in a given industry *s* into quartiles of $\Delta SPECIALIZATION_{i,j,s}$. Another possibility is to run a modified version of Eq. (2.1) in which we interact $L1.DEREGULATED_{i,j,t}$ with $\Delta SPECIALIZATION_{i,j,s}$:

$$\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t}) = \beta_1 L1.DEREGULATED_{i,j,t} +$$

$$\beta_2 L1.DEREGULATED_{i,j,t} \times \Delta SPECIALIZATION_{i,j,s} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t} \qquad (2.2)$$

where all of the variables are as defined above.[16]

One weakness of Eq. (2.1) and Eq. (2.2) is that $DEREGULATED_{i,j,t}$ cannot take into account the actual banking integration that takes place. To remedy this problem, in the second set of regressions we replace $DEREGULATED_{i,j,t}$ with the actual banking integration ($INTEGRATION_{i,j,t}$) between a state-pair over time:

$$\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t}) = \beta_1 L1.INTEGRATION_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t} \quad (2.3)$$

where, $INTEGRATION_{i,j,t}$ is defined as the sum of common banking assets belonging to MBHCs headquartered in either of the two states i and j in a given year t divided by the total of all banking assets in both states in the same year (banking assets of either state's MBHCs that are located in other states are not taken into account in this calculation). As in the case of Eq. (2.2), to improve identification, we also interact $INTEGRATION_{i,j,t}$ with $\Delta SPECIALIZATION_{i,j,s}$:

$$\begin{aligned} \Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t}) &= \beta_1 L1.INTEGRATION_{i,j,t} \\ &+ \beta_2 L1.INTEGRATION_{i,j,t} \times \Delta SPECIALIZATION_{i,j,s} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t} \end{aligned} \quad (2.4)$$

where, all of the variables are defined as above.

However, equations above could still suffer from a number of problems. First, as explained above in Section 2.3.1, banking integration can be endogenous to manufacturing sectors' growth differentials. To deal with this potential problem, we run versions of Eq. (2.3) and Eq. (2.4) using Instrumental Variables (IV) estimation. As an instrument, we use the average number of years since the effective deregulation (*YEARS_SINCE*) between each state-pair that we lag by one year in actual estimation.[17] This instrument has the benefit of capturing succinctly the dynamics of different types of deregulatory processes for interstate bank-entry that were put in place in the US between 1977 and 1995. In some instances, state i and j permitted entry only based on reciprocity, in which case the effective date of opening is

[16]Note that $\Delta SPECIALIZATION_{i,j,s}$ term that should stand alone is absorbed into the state-pair-industry fixed-effect since $\Delta SPECIALIZATION_{i,j,s}$ is fixed as of the date of effective state-pair deregulation and does not change over time.

[17]In the estimation we use *L1.YEARS_SINCE* as an IV, since the instrumented variables are themselves lagged one year to avoid simultaneity. As an alternative IV, we also use the square root of the years since effective deregulation (but without taking the average, which could make a difference in case of non-reciprocal deregulations, as we explain in the text). Our IV-regression results are not affected by the choice of the instrument.

that of the state that allows (reciprocal entry) the latest. For example, if state *i* does (reciprocal) entry deregulation in the year 1986, and state *j* does the same but only on December 31st, 1990, *YEARS_SINCE* will be equal to 0 in all years before 1991 and 1 for all years after 1990. In this case, the average years since deregulation will be equal to 1 for 1991, 2 for 1992, 3 for 1993 and so on: this is because the number of years since effective deregulation goes up by the same increment of 1 by each for both states *i* and *j*. In other instances, some states decided to open up their banking markets in a non-reciprocal way (i.e., irrespective of the regulatory stance of the counterparty state). Suppose that state *i* non-reciprocally deregulated in 1986 but *j* allows interstate bank-entry in a reciprocal way as of 1990. In this case the average number of years since deregulation would be equal to 0 prior to 1987, it will be equal to 0.5 for 1987, 1 for 1988, 1.5 for 1989, 2 for 1990 (the year of reciprocal opening of state *j*), and be equal to 3 in 1991, 4 in 1992, and so on.

Besides endogeneity, we face two additional and related empirical challenges. One potential concern is mean-reversion in our dependent variable (difference in state-pair-industry growths). Relatively smaller industries in a state (i.e., the ones in which the state is more likely to be less-specialized) are likely to grow much faster than the larger ones (i.e., sectors in which the state is more likely to be more-specialized). More established industries might eventually stagnate and experience slower or even negative growth. One way to account for the potential mean-reversion, which is mainly associated with the different growth cycles of the same industry in different states, is to use another (contemporaneous or lagged) variable that is indicative of the segment's size in the state's economy. One such control variable is the value added share of the industry (as in Cetorelli and Gambera, 2001; Cetorelli, 2004), another is its labor share (as in Cetorelli and Strahan, 2006). However, in our case the dependent variable is the difference state-pair-industry-level growths, which is likely to be affected by the state-pair differences in value added or labor share of the sector.[18] Put differently, industry value added or labor share are likely to be endogenous to the growth of that segment, and this even if we take differences of these variables across state-pairs for a given industry. The second concern that we face is the potential persistence in the difference of growth of sector in a state-pair. For example, introducing lagged state-pair differences in labor share of the segment as a control variable to handle mean reversion would provide little

[18]This issue is not a primary concern for the cited papers. The empirical analysis in Cetorelli and Gambera (2001) is cross-sectional (and does not have a time-series component). In Cetorelli (2004) and Cetorelli and Strahan (2006) the dependent variable is the (level of) number of firms or average firm size in an industry: it is not obvious that a (relative to the rest of the economy) stagnating industry's number of firms or average firm size would shrink as the overall economy continues to expand on average.

relief if the sector-level growth measures are persistent. In other words, we could face concerns that are due to the dynamic panel nature of our study. As a result, in some of our regressions we use the lags of our dependent variables to control for mean-reversion and persistence to assure ourselves of the robustness of our results.

The final issue that we need to take into consideration in this dynamic panel setting is the fact that we would also like to control for the unobservables with industry-time and state-time fixed effects. The problems cited in the previous paragraph would be exacerbated by the fact that including a large number of fixed effects in dynamic panel models can lead to biased and inconsistent estimators, especially for "small *T*, large *N*" panels (Nickell, 1981). Judson and Owen (1999) state that the bias is inversely related to panel length *T*, since the effect of idiosyncratic shocks will decay overtime. Given that our data panel has moderately few time periods (*T*=17) but large *N* (with a maximum of 21,342 observations in each year for 19 manufacturing industries in (48×47)/2 = 1,128 state-pairs) our regressions are potentially prone to "dynamic panel bias". Under such conditions, the Arellano-Bond (AB) estimator (following Arellano and Bover, 1995; Blundell and Bond, 1998), which relies on the generalized method of moments (GMM), provides a solution for the efficient estimation of dynamic panels. This estimator corrects for the endogeneity of the lagged dependent variable (which is introduced to control for its persistence or mean-reversion). The numbers of lags that are introduced (say, in Within regressions), which depend on the dependent variable, are determined by the AB serial autocorrelation tests. AB estimator also provides consistent parameter estimates even in the presence of endogenous right-hand-side variables (in our case, the bank-integration variable). It also allows for fixed effects, heteroskedasticity and autoregressive (AR) error terms.

Our dynamic panel exhibits all of these characteristics, thus we use the system version of the AB estimator (Blundell and Bond, 1998) in some of our regressions.[19] We do this because system version of the AB estimator (system-AB estimator hereafter) involves first-differencing of the regression equation of interest and building a system of two equations -- the original equation and the transformed one -- an approach that provides more suitable instruments (e.g., Roodman, 2009) for our lagged dependent (difference of growth) variables.[20, 21, 22]

[19]Due to the problems associated with AB-estimator, we rely on a set of estimators to check the robustness of our results to estimator choice.

[20]For a similar application of system-GMM proposed by Blundell and Bond (1998) to country-level growth rates see Beck, Levine, and Loayza (2000) as well as Levine, Loayza, and Beck (2000), and to (external finance dependent) industries' growth rates see Bruno and Hauswald (2014).

2.3.3. The data

To construct our database we rely on two separate sources. First, we use annual Bureau of Economic Analysis (BEA) estimates of state-and-industry output variables. The benefit of the BEA data is that they help us assess the overall economic impact of banking integration on 19 industrial segments (as opposed to the overall state-level output growth).[23] The downside is that state-industry-level value added, which is equal to state-industry-level Gross State Product (GSP),is a BEA estimate based on industry-level US Census Bureau data.[24] Nevertheless, we use BEA's manufacturing segment-level aggregate data, as they are the only publicly available state-industry-year-level data that can be obtained. Second, we use BHC and commercial bank financial statements to calculate the banking integration variable across state-pairs. These data come from the financial statements (the so-called Call Reports and Y-9 forms) that all US banks and BHCs have to file with their federal regulators.[25]

We use 1972-1997 BEA data to estimate our regression equations over 1981-1997 (the difference is due to the lags that we introduce in some regressions, especially those estimated with the system-AB estimator). We start in 1981for two reasons. First, we do not have BHC structure (i.e., membership) data prior to 1981.[26] Second, even though Maine was the first state to deregulate bank-entry into its market in 1978, its actual (effective) deregulation did not start until 1982 when New York reciprocated. We take into account the IBBEA, which took effect in September of 1995 and leveled the playing field in interstate banking at the

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[21]In a horse race of methods used in estimating dynamic panel models used in corporate finance research with panel data, Flannery and Hankins (2013) recommend for practical applications a system-GMM over alternative estimators.

[22]When using the system-AB estimator, we need to (i) select the autoregressive lag structure J and (ii) decide on the number of instruments to use for the lagged dependent variable. The different output measures that we use as dependent variables exhibit empirically different autoregressive patterns. To accommodate such differences we make use of the AB serial autocorrelation tests applied to the residuals in the differenced equations. As a rule, we use the specifications with the minimum number of lags and with AB autocorrelation test p-values that do not reject the null hypothesis of no serial correlation at least at the 10%-level for up to second-order serial correlation.

[23]An alternative source of data, available from the Annual Survey of Manufacturing (ASM), and containing the more data, proved to be unsuitable for our investigation. First, the publicly available version of ASM contains too many zeros (due to non-disclosure rules that require that data be suppressed if it were to reveal or hint at the identity of the participating firms) introducing gaps in a panel setting, something that severely limits the sample size that we could investigate. Second, the ASM data start in 1982 (in contrast to BEA data that start in 1963). These two features matter crucially when the estimation requires dynamic panel techniques with lagged variables as instruments.

[24]GSP is the state-level equivalent of the country-level Gross Domestic Product (GDP).

[25]These are the Federal Reserve System, the Office of the Comptroller of the Currency, and the Federal Deposit Insurance Corporation.

[26]Even though the individual bank financial (the so-called Call Report) data are publicly available since 1978, the BHC (Y-9) data are publicly available starting with 1986 only. We supplement the latter with the so-called BHC structure (membership) data for 1981-1985 that we obtained from the Federal Reserve Board of Governors. We could not find BHC structure data for years prior to 1981.

federal-level (i.e., for all states) by allowing banks to consolidate their activities into a single corporate charter and allowing them to enter new markets by opening new branches (if the states allowed such branching entry). It should be noted that we cannot go beyond 1997 because of changes in the industry classification standards.[27]

In Table 2.1, we provide information on the manufacturing industries, their distribution as under- and over-specialized sectors of activity across states, as well as their external finance dependence status for the whole sample. The first three columns of Table 2.1 list the names of the 19 manufacturing industries covered in the study, their BEA identifiers as well as the corresponding two- or three-digit SICs. In the fourth column of Table 2.1 we indicate the nine industries that we classify as more external finance dependent as they are the median of the measure proposed by Rajan and Zingales (1998).[28] In column five (six) of Table 2.1, we observe that an industry is classified as under-specialized (over-specialized), i.e., with a specialization index below (above) one, in 31.1 (16.7) states on average. There is variation in this dimension across industries: an industry can be under-specialized (over-specialized) in 24 to 40 (8 to 24) states.

Table 2.2 provides the summary statistics for the variables that we use. The average of $SPECIALIZATION_{i,s}$ is equal to 0.59 with a standard deviation of roughly 0.35 while that of $-SPECIALIZATION_{j,s}$ where is equal to 1.19 with a standard deviation of roughly 0.58. The average of $\Delta SPECIALIZATION$ is equal to 0.60 and has a standard deviation of 0.5: at the state-pair-industry-level there is a lot of variation in industry specialization, which is important for us to be able to conduct the tests of our hypotheses. We don't want our empirical results to be driven by accentuated growth patterns of extremely less-specialized industries in some states (for example, a 50% increase in the output by the sole producer in the state would lead to a 50% growth in that sector) or extremely specialized industries in other states (these are more likely to be small and economically undiversified states). To avoid such cases we trim the data based on specialization: we leave out 5% of most- and least-specialized state-industries on either end of *SPECIALIZATION*.[29] To have a proper

[27]In 1997 the US Census Bureau (and hence the BEA) have switched from the Standard Industry Classification (SIC) to the North American Industrial Classification System (NAICS). Even though there is a concordance table between the two systems at the four-digit level, there is no way to match these two classifications at the two-digit level, which is the detail level for the publicly available version of the BEA data that we use.

[28]To do this, we use firm-level variables in COMPUSTAT universe and compute the average value of each firm's external financing needs for 1982-1995, which is calculated by subtracting cash flows from operations from total capital expenditures and then dividing it by total capital expenditures. Next, we aggregate the firm-level ratios of external financial dependence using the median value for all firms in each BEA industrial classification category.

[29]Note that we do *not* trim data based on output growth, something that could bias our results.

panel without missing observations, we keep only state-pair-industry observations for which we have no missing values over 1981-1997.[30]

In Table 2.2 we provide two sets of statistics for output measures. First, we provide state-industry-level output growth for each state in a pair *i-j*: we do this to check whether output growth measures differ systematically across the state-pairs *i-j*. Second, we provide statistics on our dependent variables. We have six dependent variables as measures of state-industry-level growth. Value added (*VA*) is equivalent to state-industry-level GSP. Gross Operating Surplus (*GOS*) is the return to the capital employed in the industry at the state-level. Compensation of employees (*COMP*) is the total of disbursements to industry's employees (including wages plus retirement and similar contributions made by the employers). It should be noted that *GOS* and *COMP* are the two main components of *VA*.[31] The number of employees (*EMP*) at the state-industry-level includes both full- and part-time employees (without a full-time equivalent adjustment unfortunately). Productivity (*PROD*) is measured as value added per employee at the state-industry-level. Similarly, wages (*WAGE*) is gross compensation per employee at the state-industry-level. In Section 2.5.2, we provide a simple Cobb-Douglas production model and show how these six variables are linked with each other.

In Table 2.2, we observe little difference in annual output growths at the state-industry-level. For example, the average for $\Delta ln(VA_{i,s,t})$ is equal to 0.056 (i.e., 5.6%) and so is the average for $\Delta ln(VA_{j,s,t})$. For other variables, there are slight differences in the growth rates for *GOS* (with averages of 7.0% and 7.6% for *i* and *j*, respectively), *COMP* (averages of 4.95% and 4.66%), *EMP* (0.8% and 0.35%), *PROD* (4.7% and 5.3%), and *WAGE* (4.1% and 4.3%). The somewhat sizeable standard deviations observed in Table 2.2 for some of these growth rates are because we are dealing with relatively small industries (in which their state is less-specialized) whose growth can change by large values year-to-year if (relatively) few establishments are launched or closed. Unsurprisingly, the averages of our dependent variables, the differential output growths ($\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$) are close to zero: the average for *VA* is equal to 0.0003, for *GOS* –0.0064, for *COMP* 0.0029, for *EMP* 0.0049, for *PROD* –0.0058, and for *WAGE* –0.0020. However, we only cannot reject the hypothesis that the difference in *VA* growth is not different from zero at the 10%-level. This means that although in all the less-specialized industries grew roughly at the same pace as the more-specialized ones (as defined by the ordering in our pairs) their increases in employment that were driving

[30]The gaps in the data are due to zeros or values that are unreported by the BEA for various reasons.
[31]Other items like subsidies for industries are typically negligible parts of VA.

the compensation component of the *VA* were faster than those of more-specialized industries. The opposite was true for the *GOS*. Next, we discuss our results, and present them in Tables 2.3 through 2.7.

2.4. Main results

In Table 2.3, we provide the estimates of Eq. (2.1). In Panel A, we present the coefficient estimates for *L1.DEREGULATED* of Eq. (2.1) using the Within estimator with each output measure in a given column representing a different regression. For *VA* the estimate of β_1 is equal to 0.0118 that is statistically significant at the 1%-level. This finding suggests that after interstate bank-entry deregulation, less-specialized industries in states denoted *i* grew 1.18% faster than relatively more-specialized industries in states denoted *j*, on average. Similar results are obtained across the columns for all of the output variables, except one. For *GOS* the estimate of β_1 is equal to 0.0262, for *COMP* to 0.0062, for *EMP* to 0.0052 and for *PROD* to 0.0059, all of which are statistically significant at the 1%-level. Only *WAGE* does not indicate differential growth after state-pair bank-entry deregulation when we examine states that are less-specialized in the sector versus those that are more-specialized: the coefficient estimate is equal to 0.0007 and not statistically significant. These results suggest that banking deregulation (that potentially leads to bank-entry) affects the growth of sectors in states that are less-specialized compared to states that are more-specialized: after deregulation gross operating surplus increases by 2.62%, aggregate sector-level compensation by 0.62%, employment by 0.52% and productivity (as measured by value added by employee) by 0.59%.

We first would like to make sure that these results are not an artifact of the dynamic panel that might be present in our data series. To do so, in Panel B of Table 2.3, we use system-AB estimator to estimate versions of Eq. (2.1) that include lags of the dependent variable (difference in growth across states for the same sector), with the lag structure being determined by AB serial correlation tests (which are not reported to conserve space, but are available upon request). For the coefficient of interest, the results of Panel B are very similar to those of Panel A: For *VA* the estimate of β_1 is equal to 0.0123, for *GOS* to 0.0265, for *COMP* to 0.0059, for *EMP* to 0.0049 and for *PROD* to 0.0064. All coefficients are statistically significant at the 1%-level; whereas the estimate for *WAGE* is equal to 0.0008 and not statistically significant.

These results suggest that interstate bank-entry deregulation has a differential impact. However, they do not clarify whether the observed effect is due to less-specialized (most likely smaller) industries having access to more finance or whether the said deregulations lead to a redistribution of lending given the informational advantages some banks coming from states that are more-specialized in the industry might have. We address these questions in a number of steps.

First, we examine whether the observed effect of interstate bank-entry deregulation (which could potentially lead to banking integration) increases as the difference in sector specialization between two states is larger. This can be addressed in two ways: estimating Eq. (2.1) for different subsamples given the quartiles of *ΔSPECIALIZATION* or by interacting this variable with the deregulation indicator variable. If the story is one about an increase in the provision of loans (without a sector-specific role for banks' prior exposure to the industry), then in either approach we should observe that deregulation should lead to an increase in growth, irrespective of the discrepancy in sector-related specialization between state-pairs that deregulate bank-entry.

In Panel A of Table 2.4, we present estimates for the coefficient of *L1.DEREGULATED* in Eq. (2.1) that are estimated without any lags, for different samples of the data by quartiles of *ΔSPECIALIZATION*. For *VA* the estimate of β_1 is equal to 0.0022 (not statistically significant) in the first quartile, 0.0067 (and statistically significant at the 10%-level) in the second quartile, 0.0204 (statistically significant at the 1%-level) in the third quartile, and 0.0108 (statistically significant at the 15%-level) in the fourth quartile. Similar patterns for the estimate of β_1 are also observed for productivity: for *PROD* β_1-estimates are 0.0006, 0.0063, 0.0083, and 0.0078 across quartiles 1 through 4, respectively, with the latter three estimates being statistically significant at the conventional levels. For other measures of output, the pattern is still there, even if less clearly. For example, for *GOS* β_1-estimates are 0.0185, 0.0373, 0.0316, 0.0192 for quarters 1 through 4, respectively, but only the second and third quarter results are statistically significant (at the 1%-level).

To further examine the issue, we estimate Eq. (2.2), without any lags, in which *L1.DEREGULATED* is interacted with *ΔSPECIALIZATION* and present the results in Panel B of Table 2.4. The β_2-estimate for the *L1.DEREGULATED*×*ΔSPECIALIZATION* interaction is positive for *VA* (0.0067), *COMP* (0.0046), and *EMP* (0.0053) and statistically significant at the 1%-level. The β_2-estimates for *GOS*, *PROD* and *WAGE,* are small and not statistically significant.

These results provide the first evidence for our hypothesis that banks' previous exposure to a sector matters when the same institutions enter a new market. Value added is impacted differentially for less-specialized industries; and it is driven by increases in employee compensation that, in turn, is affected by an increase in employment. However, this does not give the full picture. *DEREGULATED* accounts bank-entry liberalization of state-pairs but does not account for *actual* bank entry, something we take into account in the next set of tables.

In Panel A of Table 2.5, we present β_1-estimates for *L1.INTEGRATION* when Eq. (2.3) is estimated using IV approach (but without adding any lags of the dependent variable). The coefficient estimate for *VA* is equal to 0.6944, which is statistically significant. This suggests that one standard deviation increase (0.0112) in banking integration across state-pairs leads to a 0.78% (=0.6944×0.0112) differential growth for industries across the same state-pairs. Similarly, one standard deviation increase in banking integration leads to a 0.42% (=0.3554×0.0117) differential growth for *COMP*, 0.22% (=0.1878×0.0117) differential growth for *EMP*, 0.36% (=0.3117×0.0117) differential growth for *PROD*, and 0.18% (=0.1878×0.0117) differential growth for *WAGE*. The coefficient estimate for *GOS* is equal to 0.4190 but not statistically significant.

The results are stronger in Panel B of Table 2.5 when we estimate Eq. (2.3) with system-AB estimator including the proper number of lags for each growth measure so as to take into account the autocorrelation in the dynamic panels that we work with. The coefficient estimate for the interaction terms is statistically significant either at the 5% or 1%-level for all of our output measures: β_1-estimate is equal to 0.9391 for *VA*, 0.7600 for *GOS*, 0.3779 for *COMP*, 0.1475 for *EMP*, 0.4340 for *PROD*, and 0.1802 for *WAGE*. For one standard deviation increase in bank integration, these results suggest differential growth rates of 1.05% for value added, 0.93% for gross operating surplus, 0.44% for compensation, 0.17% for employment, 0.50% for productivity, and 0.21% for wages.

Of course, these results do not necessarily corroborate our hypothesis that banks' prior exposure to an industry matters for the industry's growth when these banks enter a new market. For a better test of our hypothesis, we re-estimate our IV-regressions using different quartiles of the data according to differences in specialization. In Panel A of Table 2.6, there is a clear monotonic increase in the coefficient estimate of *L1.INTEGRATION* with increasing quartiles of *ΔSPECIALIZATION*. For example, for value added β_1-estimates are 0.1299 (not statistically significant) for the first quartile, 0.5315 (and statistically significant at the 1%-level) for the second quartile, 1.1835 (and statistically significant at the 1%-level)

for the third quartile, and 1.6907 (and statistically significant at the 1%-level) for the fourth quartile. A similar monotonic increase in β_1-estimates is now observable for all of the output variables. For example, going from the first through quartile for compensation the β_1-estimates are equal to -0.0474, 0.2853, 0.7179, 1.0894, and all of which are statistically significant at the conventional levels, except the very first estimate. These findings that also hold for other output measures indicate that the higher the difference in state *i*'s specialization in sector *s* compared to state *j*, the higher the impact of *actual* banking integration. This evidence is consistent with our hypothesis that banks' prior industry exposure matters.[32]

In Panel B of Table 2.6, we present the estimates of Eq. (2.4), in which *L1.INTEGRATION* is interacted with *ΔSPECIALIZATION*. The β_2-estimates for the interaction are positive and always statistically significant at the conventional levels (at the 10%-level for *WAGE*). These results are supportive of the findings we had in Panel A of the same table.

2.5. Checks on the consistency and robustness of the results

To check the robustness of our results we conduct two additional exercises. First, in Section 2.5.1, we repeat our estimates of Eq. (2.3) after separating the data at our disposal into two subsamples based on industries' external finance dependence (as in Rajan and Zingales, 1998). In Section 2.5.2 we further check on the internal consistency of our estimates using a simple calculation exercise.

2.5.1. Checks on the robustness of the empirical estimates

If our conjecture holds true, the effects that we observe in Section 2.4 should be more pronounced for the external finance dependent (EFD) industries. Given Rajan and Zingales (1998) findings, it is natural to think that industries with higher EFD might benefit more from the industry-specific information flow induced by the banking integration across state borders. Put differently, if our hypothesis is true, we should observe stronger results for high-

[32]More direct tests would involve repeating the exercise in Table 7 (or Table 5) after limiting the sample to cases in which state *i* is under-specialized in sector *s* (i.e., $SPECIALIZATION_{i,s} < 1$) while state *j* is over-specialized in the same sector(i.e., $SPECIALIZATION_{j,s} > 1$). These are being prepared for the next version of the paper.

EFD manufacturing industries and weaker results for low-EFD industries. To test for these possibilities, we use the industry-level measure of external finance needs developed in Rajan and Zingales (1998) and divide our sample into two mutually exclusive subsets. The first subset contains nine industries that exhibit higher EFD in our sample, while the latter contains ten industries that have relatively low EFD in our dataset.

The results for Eq. (2.3) with IV-estimation are presented in Table 2.7. Panel A presents the low-EFD subsample results, and Panel B presents the high-EFD subsample results.[33] The coefficient estimate for the differential growth of value added for the low-EFD sample is equal to 0.4211 (statistically significant at the 1%-level), whereas the comparable estimate for the high-EFD subsample is equal to 1.0031 (statistically significant at the 1%-level). Results for *PROD* and *WAGE* exhibit a similar pattern. For the low-EFD sample, the coefficient estimate for *PROD* is equal to 0.1773 (statistically significant at the 10%-level). For the high-EFD sample; the coefficient has an estimate of 0.4894 (statistically significant at the 1%-level). For *WAGE* the low-EFD sample coefficient estimate is equal to 0.1124 (statistically significant at the 5%-level). For the high-EFD sample the coefficient has an estimate of 0.2088 (statistically significant at the 1%-level). Similar results also hold for *GOS*, *COMP,* and *EMP*, with the exception that low-EFD coefficient estimates are not statistically significant. For *GOS* the Eq. (2.3) β_1 coefficient estimate is equal to -0.1326 (statistically insignificant) for the low-EFD, 0.9571 (statistically significant at the 5%-level). For *COMP* the Eq. (2.3) β_1 coefficient estimate is equal to 0.0391 (statistically insignificant) for the low-EFD, 0.7347 (statistically significant at the 1%-level). Similarly, for *EMP* the Eq. (2.3) β_1 coefficient estimate is equal to -0.0733 (statistically insignificant) for the low-EFD, but equal to 0.4897 (statistically significant at the 1%-level).The fact that high-EFD results are always positive, statistically significant, and roughly twice the size of the low-EFD estimates (when the latter are statistically significant) is further evidence that is consistent with our conjecture.

2.5.2. Consistency checks through a simple calibration exercise

Finally, to frame the findings of Sections 2.4 and 2.5.1, we conduct a simple, partial equilibrium, calculation exercise relying on a representative production function. The model is kept purposefully simple. Our goal is not to conduct detailed output decomposition, but to

[33]We also replicated Tables 4 and 6 for low- and high-EFD subsamples and observed a pattern similar to the one described in this paragraph.

have an intuitive benchmark with which we can assess the relative sizes of our coefficient estimates with respect to each other. With this objective in mind, we define the following constant-returns-to-scale Cobb-Douglas function with capital and labor as the only factors of production:

$$Y=A(K)^{\alpha}\ (L)^{1-\alpha} \tag{2.5}$$

where, Y is the output (i.e., value added), A is TFP, K is the capital stock, α is the capital intensity (share) parameter, and L is the labor employed. Imposing equilibrium conditions that marginal products of capital and labor are going to be equal to the return on capital (r) and wages (w), respectively, we can rewrite Eq. (2.5) as:[34]

$$Y=rK + wL \tag{2.6}$$

Substituting value added for Y, gross operating surplus (i.e., remuneration of capital) for rK, and compensation of labor for wL, Eq. (2.6) becomes:

$$VA= GOS + COMP \tag{2.7}$$

with direct links to our dependent variables. We further note that $w = WAGE$, $L = EMP$, and $Y/L = PROD$ (notice that we do not have a measure of TFP since we do not observe K). Now, assuming that we start from some equilibrium and treating banking integration as an exogenous shock, we can frame and interpret the coefficient estimates given the structure imposed by Eq. (2.6) and Eq. (2.7). We work with our preferred estimates of the effects of integration on our variables of interest for the fourth quartile of $\Delta SPECIALIZATION$ shown in Table 2.6.

Let us first frame our basic estimates for *VA, GOS,* and *COMP*. For this exercise, first we fix the capital intensity parameter α equal to 0.36 (the average for the U.S. in the period 1981-1997 as given by the Penn World Tables 8.1), and that is standard in the growth accounting literature (e.g., Barro and Sala-i-Martin, 2003). Differentiating Eq. (2.7) with respect to time and dividing by Y both sides, and imposing from equilibrium conditions that $GOS = \alpha Y$ and $COMP = (1- \alpha)Y$ we obtain that $\gamma_{VA} = \alpha\ \gamma_{GOS} + (1- \alpha)\ \gamma_{COMP}$. We find outright

[34]Under the constant-returns-to-scale Cobb-Douglas production function, in equilibrium $r = \partial Y/\partial K = \alpha\, Y/K$ and $w = \partial Y/\partial L = (1-\alpha)\ Y/L$.

by estimating GDP growth differences that the less-specialized industries grow faster by 1.69% than their more-specialized counterparts if integration increases from 0 to 0.01. Running the estimations separately for *GOS* and *COMP* and making a similar calculation we would obtain 0.36×2.5553+0.64×1.0894=1.617, which is close to the GDP estimate of 1.69%. Eq. (2.7) suggests that the observed statistically significant increase in γ_{VA} as banking integration increases is due to both positive γ_{GOS} and γ_{COMP} differentials between less- and more- specialized industries.

The Cobb-Douglas production framework in Eq. (2.5) through Eq. (2.7) suggests that an increase in *GOS* could have four sources. *GOS* could go up due (i) an increase in capital employed *K*, (ii) an increase in *r*, the demanded return on physical capital, (iii) an increase in *A,* i.e., TFP, or (iv) an increase in α, the capital intensity (or share) of the production process. Put differently, the observed increase in γ_{GOS} is due to increase either in capital, its return, its productivity or intensity, or a combination thereof. In our context of increasing banking integration, changes in all of these are plausible. Unfortunately, the macro data at our disposal do not allow us to discern which component is more likely to be the source of higher γ_{GOS} given the increases in banking integration.[35] However, some of the findings in the literature are supportive of at least some of these possibilities. For example, Krishnan, Nandy, and Puri (2015) find that the TFP of small firms increases following interstate bank branching deregulations. Correa (2008) finds that the internal cash flow sensitivity of investments decreases for debt financing dependent firms following US banking deregulations. Rice and Strahan (2010) use the Survey of Small Business Finance data and find that (in a cross-sectional regression that forms a counterfactual as they focus on interstate branching deregulations) borrowing costs go down by 23 basis points for firms with higher return on assets but also by the same amount for larger small firms.[36] However, none of these studies examine the industry dimension of banking integration as we do here.

Other consistency checks on our results that the Cobb-Douglas model imposes are the following. Since $COMP = wL$ this means that $\gamma_{COMP}=\gamma_{WAGE} + \gamma_{EMP}$. Our estimate for the difference in the growth of *COMP* following integration is 1.0894 while those for *WAGE* and *EMP* respectively 0.4138 and 0.6659. First, this suggests that our estimates are consistent

[35]Data on capital stock are publicly available either at the sectoral level for the entire US or for each state but only at for all manufacturing industries combined. Even if there would be state-industry level statistics available for *K*, separating out new investments, existing capital stock and depreciation from each other would not be trivial.

[36]In the Cobb-Douglas framework this would be consistent, in equilibrium, with a lower marginal product of capital and higher capital employed by firms (holding TFP constant). More banking competition that would lower lending margins could therefore lead to an increase in investment.

with one another as 0.4138 + 0.6659 = 1.0797. Second, we conclude that banking integration leads to both higher employment and wage growth in the less-specialized industries relative to the more-specialized ones.[37] Next, as and *PROD = Y/L* this means that $\gamma_{PROD}=\gamma_{VA} - \gamma_{EMP}$. Here our estimate of the difference in growth of productivity due to banking integration is 0.6740 while that of *VA* and *EMP* is respectively 1.6907 and 0.6659. Since 1.6907 - 0.6659 = 1.0248, which means our productivity per worker growth may be underestimated. This may be because our employment measure does not perfectly capture the actual number hours worked, but the fact that we compare results without the proper lag structure as shown by system-AB estimators may also weigh in. Finally, since *WAGE = (1- α)Y/L=(1- α)PROD* we have $\gamma_{WAGE} =\gamma_{(1-\alpha)}+\gamma_{PROD}$. For the U.S. the parameter α grows according to the Penn World Tables v.8.1 from 0.346 in years 1980-1982 to 0.361 in the years 1996-1998, which implies a 0.18% fall in (1- α) parameter yearly over the sample period. Then obtained estimates lead us to calculate $\gamma_{(1-\alpha)}+\gamma_{PROD}$ = -0.18 + 0.674 = 0.494, close to our estimate for γ_{WAGE}.

2.6. Conclusion

We examine whether interregional banking integration could affect industry structure. Identifying the effect of banking integration on the real sector at the industry-level is empirically difficult for a number of reasons. First, typically it is not possible to observe the industry composition of the banks' loan portfolios. Second, a change that is exogenous to the industry exposure of banks is needed, as cross-sectional variation is unlikely to be convincing for pinning down the effect of banks' industry-exposures on sector-level growth: many confounding effects would get in the way of a proper identification. Third, even with exogenous changes in regulation, endogeneity is a major challenge, as financial institutions actual entry decisions in new markets might not be separated from their growth opportunities.

Interstate bank-entry deregulations in the US provide a series of exogenous shocks that we exploit to overcome these difficulties. The staggered state-pair interstate banking deregulations allow us to identify the effects of banking integration, as they permit instrumenting for our test variable. Because it is impossible to measure directly banks' industry expertise in lending with the macro-level data that are available to us, these sets of

[37]Demyanyk, Ostergaard, and Sørensen (2007) find that the personal income insurance (the ability of personal income to absorb state-level shocks) increases over the years post-interstate banking deregulations whereas Demyanyk (2008) finds that self-employed income increased over the years after interstate branching deregulations. Both studies relate their findings to the availability of more small business finance post-deregulation, but neither of them has an industry dimension.

deregulations allow us to proxy for industry knowledge by the banks' higher exposure to certain industries in their home markets prior to entry into new markets (something that was not possible before state-pair deregulations).

We find a series of evidence that are consistent with our conjecture that stipulates that banking integration affects states' industry structures: following interstate bank-entry deregulation, as MBHCs (that were over-exposed to certain industries in which their home state is more-specialized) acquired banks in other states for the first time, the resulting integration among banks led to an increase in the growth of sectors located in states that are less-specialized in them compared to the growth of the same sectors in those states that are more-specialized. Our evidence is based different sets of estimations (Within regressions, IV estimates, AB estimates) in some of which we also take into account the dynamic panel nature of our data. The observed effect is more accentuated in industries that are more external finance dependent.

Our results are robust in the series of checks that we conduct and indicate a channel through which the industrial landscape is shaped by banks' lending choices. As banking organizations make use of the information that they have accumulated in their home market when they enter the new markets (states) for the first time, the industries that were under-developed in the latter markets benefit. We do not know whether this effect is due to higher amount of sector-specific lending, or better pricing, as our data do not contain such refined information. The policy dilemma is obvious: banking regulators' decision for foreign bank-entry can have implications beyond the stability of the financial system: new banks can affect industrial structure in a way that depends on their country of origin and as a result can affect sector-specific development.

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2.7. Tables and figures

Table 2.1. Under- and over-specialized industries

Specialization is defined as the ratio of that sector's share of manufacturing output (i.e., value added) in a given state to that same sector's share of overall US manufacturing output. An under-specialized (over-specialized) industry would have a ratio less (higher) than one.

Industry Name	BEA ID	2-Digit SIC	High External Finance Dependent Sectors	Number of States in which the industry is among the under-specialized sectors	Number of states in which the industry is among the over-specialized sectors	Name of states in which the industry is among top-3 over-specialized sectors
Lumber and wood products	14	24	0	24	24	AR, ID, ME, MS, MT, OR, VA, VT, WA, WY
Furniture and fixtures	15	25	0	33	15	MI, MS, NC, VA
Stone, clay, and glass products	16	32	1	24	24	NV, PA, OK, WV
Primary metal industries	17	33	0	32	16	IN, MD, OH, PA, WV
Fabricated metal products	18	34	0	36	12	CT, IL, MI
Industrial machinery and equipment	19	35	1	30	18	IA, NH, WI
Motor vehicles and equipment	21	371	0	40	8	DE, IN, KY, MI, OH
Other transportation equipment	22	372-379	0	34	14	AZ, CT, FL, KS, MO, WA
Miscellaneous manufacturing	24	39	1	31	17	MA, NJ, NV, RI, SD
Food and kindred products	26	20	0	25	23	IA, ID, ND, NE
Textile mill products	28	22	1	40	8	AL, GA, NC, RI, SC, VA
Apparel and other textile products	29	23	0	32	16	NC, NY
Paper and allied products	30	26	0	30	18	AL, GA, ME, MN, OR, WA, WI
Printing and publishing	31	27	0	29	19	FL, NV, NY
Chemicals and allied products	32	28	1	33	15	DE, LA, NJ, WV
Petroleum and coal products	33	29	1	33	15	LA, MS, MT, OK, TX, WY
Rubber and misc. plastics products	34	30	1	26	22	IA, OK
Leather and leather products	35	31	1	30	18	CO, MA, ME, MO, NH, RI, WI
Electronic equip. and instruments	76	36 & 38	1	33	15	AZ, CA, VT
Average				31.3	16.7	

Table 2.2. Descriptive Statistics

The data come from the BEA between 1981 and 1997 and cover 48 contiguous US states (excluding Alaska, Hawaii, and the DC) and 19 manufacturing industries at two-digit SIC-level (excluding tobacco). *SPECIALIZATION* is defined as the ratio of that sector's share of manufacturing output (i.e., value added) in a given state to that same sector's share of overall US manufacturing output. We trim 5% of state-industry observations on either end of the specialization. $DEREGULATED_{i,j,t}$ is an in indicator variable that is equal to 1 starting with the year of (and including all the subsequent years) the state-pair *i-j* effectively opens their markets to each other's banks, and 0 otherwise. $INTEGRATION_{i,j,t}$ is defined as the sum of common banking assets belonging to MBHCs headquartered in either of the two states *i* and *j* in a given year *t* divided by the total of all banking assets in both states in the same year. The instrumental variable $YEARS\_SINCE_{i,j,t}$ is the average number of years since the effective deregulation between each state-pair. The growth of industry-level output measure *Y* is defined as $\Delta ln(Y) = ln(Y_{s,t}) - ln(Y_{s,t-1})$. The dependent variable ($\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$) is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less- (more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i-j*. The industry-level output measures are: Value Added (*VA*), Gross Operating Surplus (*GOS*), Compensation of Employees (*COMP*), Employment (*EMP*), Productivity (*PROD*) and Worker Remuneration (*WAGE*). *VA* is the contribution of industry to gross state product. *GOS* is the surplus accrued to capital from production. *COMP* consists of wages, salaries and social benefits paid to employees. *EMP* is the total employment in the industry. *PROD (=VA/EMP)* and *WAGE (=COMP/EMP)* are measures of productivity and compensation per worker, respectively. Superscript [a] denotes the hypothesis that the mean of the variable is not different from zero could not be rejected at the 10%-level.

	Obs.	Mean	Std. Dev.	Min	Max
$SPECIALIZATION_{i,s}$	298,690	0.59410	0.35674	0.08455	2.66082
$SPECIALIZATION_{j,s}$	298,690	1.19544	0.57939	0.09014	2.81462
$\Delta SPECIALIZATION_{i,j,s}$	298,690	0.60133	0.51414	0.00004	2.69707
$L1.DEREGULATED_{i,j,t}$	298,690	0.40575	0.49103	0	1
$L1.INTEGRATION_{i,j,t}$	298,690	0.00195	0.01118	0	0.22763
$L1.YEARS\_SINCE_{i,j,t}$	298,690	1.47357	2.47521	0	13.71370
$\Delta ln(VA_{i,s,t})$	295,647	0.05641	0.1747	-1.44691	1.78397
$\Delta ln(VA_{j,s,t})$	295,647	0.05607	0.15605	-1.44691	1.78397
$\Delta ln(GOS_{i,s,t})$	144,211	0.07024	0.43867	-4.81218	5.24174
$\Delta ln(GOS_{j,s,t})$	144,211	0.07668	0.39452	-4.81218	5.24174
$\Delta ln(COMP_{i,s,t})$	240,516	0.04955	0.10166	-1.22377	2.01490
$\Delta ln(COMP_{j,s,t})$	240,516	0.04664	0.07989	-1.22377	2.01490
$\Delta ln(EMP_{i,s,t})$	240,567	0.00839	0.09113	-1.48160	1.74216
$\Delta ln(EMP_{j,s,t})$	240,567	0.00351	0.07190	-1.19987	1.68243
$\Delta ln(PROD_{i,s,t})$	240,567	0.04775	0.14045	-1.64686	1.77875
$\Delta ln(PROD_{j,s,t})$	240,567	0.05352	0.13099	-1.64686	1.77875
$\Delta ln(WAGE_{i,s,t})$	240,516	0.04116	0.05783	-0.64350	0.78845
$\Delta ln(WAGE_{j,s,t})$	240,516	0.04313	0.04560	-0.57367	0.6663
$\Delta ln(VA_{i,s,t}) - \Delta ln(VA_{j,s,t})$	295,647	0.00034[a]	0.21282	-2.56495	2.02401
$\Delta ln(GOS_{i,s,t}) - \Delta ln(GOS_{j,s,t})$	144,211	-0.00643	0.54408	-6.26760	7.41034
$\Delta ln(COMP_{i,s,t}) - \Delta ln(COMP_{j,s,t})$	240,516	0.00290	0.11688	-2.22034	2.08295
$\Delta ln(EMP_{i,s,t}) - \Delta ln(EMP_{j,s,t})$	240,567	0.00487	0.10252	-2.02021	1.84219
$\Delta ln(PROD_{i,s,t}) - \Delta ln(PROD_{j,s,t})$	240,567	-0.00577	0.17522	-2.20636	1.92428
$\Delta ln(WAGE_{i,s,t}) - \Delta ln(WAGE_{j,s,t})$	240,557	-0.00197	0.06423	-0.76398	0.78005

Table 2.3. Effect of pairwise interstate banking deregulation on differential output growth at the state-pair-industry-level

Panel A of this table presents *Within* regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta . L1. DEREGULATED_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$. Panel B reports the results of the same equation (with proper number of lags of the dependent variable) using system-AB estimator. $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less- (more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i-j*; $DEREGULATED_{i,j,t}$ is an in indicator variable that is equal to 1 starting with the year (including all the subsequent years) in which the state-pair *i-j* effectively opens their markets to each other's banks, and 0 otherwise; all regressions include state-pair-industry, state i-year, state j-year, and sector-year, and year fixed effects. *Y* is one of Value Added (*VA*), Gross Operating Surplus (*GOS*), compensation of employees (*COMP*), number of employees (*EMP*), productivity as measured by output per employee (*PROD*), or wage measured as compensation per employee (*WAGE*). *Lt* represents the t^{th} lag. The standard errors are clustered at the state-pair-industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

PANEL A: no lags						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.DEREGULATED	0.0118***	0.0262***	0.0062***	0.0052***	0.0059***	0.0007
	(6.28)	(4.40)	(4.92)	(4.67)	(3.71)	(1.22)
Number of observations	295,647	144,211	240,516	240,567	240,567	240,516
Number of clusters	17,391	8,483	14,148	14,151	14,151	14,148

PANEL B: with lags						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.DEREGULATED	0.0123***	0.0265***	0.0059***	0.0049***	0.0064***	0.0008
	(5.90)	(3.90)	(4.95)	(4.86)	(3.54)	(1.32)
L1.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.1180***	-0.2716***	0.1154***	0.1430***	-0.2264***	-0.1818***
	(35.27)	(65.06)	(30.20)	(42.30)	(74.81)	(45.67)
L2.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.0695***		-0.0419***			
	(25.01)		(11.62)			
L3.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.0844***		-0.0289***			
	(32.01)		(9.14)			
L4.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$			-0.0715***			
			(15.64)			
Number of observations	295,519	144,147	240,019	240,567	240,567	240,500
Number of clusters	17,391	8,483	141,48	14,151	14,151	14,148

Table 2.4. Effect of pairwise interstate banking deregulation on differential output growth with specialization difference

Panel A of this table presents *Within* regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 L1.DEREGULATED_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$ using subsamples defined by the quartiles of differences in industry specializations at the state-pair-level. Panel B of this table presents *Within* regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 L1.DEREGULATED_{i,j,t} + \beta_2 L1.DEREGULATED_{i,j,t} \times \Delta SPECIALIZATION_{i,j,s} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$. The variables are defined as follows: $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less- (more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i-j*; $DEREGULATED_{i,j,t}$ is an in indicator variable that is equal to 1 starting with the year (including all the subsequent years) in which the state-pair *i-j* effectively opens their markets to each other's banks, and 0 otherwise; $\Delta SPECIALIZATION_{i,j,s}$ equals $|SPECIALIZATION_{i,s} - SPECIALIZATION_{j,s}|$ with *SPECIALIZATION* defined as the ratio of that sector's share of manufacturing output (i.e., value added) in a given state to that same sector's share of overall US manufacturing output (specializations are defined as of the year of effective banking deregulation of state-pair *i-j*); all regressions include state-pair-industry, state i-year, state j-year, and sector-year, and year fixed effects. *Y* is one of Value Added (*VA*), Gross Operating Surplus (*GOS*), compensation of employees (*COMP*), number of employees (*EMP*), productivity as measured by output per employee (*PROD*), or wage measured as compensation per employee (*WAGE*). *Lt* represents the t[th] lag. The standard errors are clustered at the state-pair-industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Panel A: Within regressions (no lags)						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.DEREGULATED	0.0022	0.0185	0.0042*	0.0046**	0.0006	-0.0008
(1st Quartile of *ΔSPECIALIZATION*)	(0.59)	(1.54)	(1.69)	(2.03)	(0.21)	(0.75)
L1.DEREGULATED	0.0067*	0.0373***	0.0025	0.0020	0.0063**	0.0005
(2nd Quartile of *ΔSPECIALIZATION*)	(1.84)	(3.21)	(1.07)	(0.95)	(2.09)	(0.47)
L1.DEREGULATED	0.0204***	0.0316***	0.0095***	0.0093***	0.0083***	0.0001
(3rd Quartile of *ΔSPECIALIZATION*)	(5.60)	(2.70)	(3.84)	(4.19)	(2.64)	(0.08)
L1.DEREGULATED	0.0180***	0.0192	0.0079***	0.0049**	0.0078**	0.0027**
(4th Quartile of *ΔSPECIALIZATION*)	(4.38)	(1.60)	(2.90)	(2.08)	(2.21)	(2.25)
Panel B: Within regressions (no lags)						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.DEREGULATED	0.0080***	0.0268***	0.0035**	0.0023*	0.0059***	0.0010*
	(3.83)	(4.08)	(2.44)	(1.72)	(3.44)	(1.66)
L1.DEREGULATED×ΔSPECIALIZATION	0.0067***	-0.0010	0.0046***	0.0053***	-0.0000	-0.0006
	(4.27)	(0.21)	(3.38)	(4.27)	(0.02)	(1.21)

Table 2.5. Effect of pairwise interstate banking integration on differential output growth at the state-pair-industry-level

Panel A of this table presents *Instrumental Variables* (IV) regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta\, L1.INTEGRATION_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$. Panel B reports the results of the same equation (with proper number of lags of the dependent variable) using system-AB estimator. $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less-(more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i-j*; $INTEGRATION_{j,t}$ is the sum of common banking assets belonging to MBHCs headquartered in either of the two states *i* and *j* in a given year *t* divided by the total of all banking assets in both states in the same year; all regressions include state-pair-industry, state-year, sector*s*-year, and year fixed effects. *Y* is one of Value Added (*VA*), Gross Operating Surplus (*GOS*), compensation of employees (*COMP*), number of employees (*EMP*), productivity as measured by output per employee (*PROD*), or wage measured as compensation per employee (*WAGE*). *Lt* represents the t^{th} lag. The standard errors are clustered at the state-pair-industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

PANEL A: no lags						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	0.6944***	0.4190	0.3554***	0.1878**	0.3117***	0.1525***
	(5.69)	(1.43)	(4.25)	(2.48)	(3.84)	(4.34)
Number of observations	295,647	144,211	240,516	240,567	240,567	240,516
Number of clusters	17,391	8,483	14,148	14,151	14,151	14,148

PANEL B: with lags						
	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	0.9391***	0.7600**	0.3779***	0.1475**	0.4304***	0.1802***
	(6.31)	(2.14)	(4.55)	(2.25)	(4.43)	(4.41)
L1.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.1181***	-0.2716***	0.1154***	0.1430***	-0.2264***	-0.1818***
	(35.34)	(65.31)	(30.25)	(42.39)	(74.98)	(45.79)
L2.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.0697***		-0.0419***			
	(25.10)		(11.66)			
L3.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$	-0.0846***		-0.0290***			
	(32.15)		(9.19)			
L4.$[\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})]$			-0.0716***			
			(15.70)			
Number of observations	295,519	144,147	240,019	240,567	240,567	240,500
Number of clusters	17,391	8,483	141,48	14,151	14,151	14,148

Table 2.6. Effect of pairwise interstate banking integration on differential output growth with specialization difference

Panel A of this table presents *Instrumental Variables* (IV) regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 \, L1.INTEGRATION_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$using subsamples defined by the quartiles of differences in industry specializations at the state-pair-level. Panel B of this table presents IV regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 \, L1.INTEGRATION_{i,j,t} + \beta_2 \, L1.INTEGRATION_{i,j,t} \times \Delta SPECIALIZATION_{i,j,s} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$. Variables are defined as follows: $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less- (more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i-j*; $INTEGRATION_{j,t}$ is the sum of common banking assets belonging to MBHCs headquartered in either of the two states *i* and *j* in a given year *t* divided by the total of all banking assets in both states in the same year; $\Delta SPECIALIZATION_{i,j,s}$ equals $|SPECIALIZATION_{i,s} - SPECIALIZATION_{j,s}|$ with *SPECIALIZATION* defined as the ratio of that sector's share of manufacturing output (i.e., value added) in a given state to that same sector's share of overall US manufacturing output (specializations are defined as of the year of effective banking deregulation of state-pair *i-j*); all regressions include state-pair-industry, state *i*-year, state *j*-year, sector *s*-year, and year fixed effects; *.Y* is one of Value Added (*VA*), Gross Operating Surplus (*GOS*), compensation of employees (*COMP*), number of employees (*EMP*), productivity as measured by output per employee (*PROD*), or wage measured as compensation per employee (*WAGE*). *Lt* represents the t[th] lag. The standard errors are clustered at the state-pair-industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Panel A: IV regressions (no lags)

	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	0.1299	-0.1260	-0.0474	-0.1162	-0.0934	0.0189
(1st Quartile of *ΔSPECIALIZATION*)	(0.59)	(0.24)	(0.33)	(0.83)	(0.65)	(0.30)
L1.INTEGRATION	0.5315***	-0.0522	0.2853**	0.1752	0.2021	0.1089**
(2nd Quartile of *ΔSPECIALIZATION*)	(2.67)	(0.11)	(2.15)	(1.52)	(1.55)	(2.03)
L1.INTEGRATION	1.1835***	0.3862	0.7179***	0.4402***	0.6185***	0.2098***
(3rd Quartile of *ΔSPECIALIZATION*)	(4.68)	(0.69)	(4.01)	(3.22)	(3.71)	(2.98)
L1.INTEGRATION	1.6907***	2.5553**	1.0894***	0.6659**	0.6740**	0.4138***
(4th Quartile of *ΔSPECIALIZATION*)	(3.87)	(2.31)	(3.50)	(2.52)	(2.30)	(3.01)

Panel B: IV regressions (no lags)

	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	-0.6770***	-0.7500	-0.7831***	-0.8324***	-0.0415	0.0156
	(2.71)	(1.28)	(4.23)	(5.00)	(0.26)	(0.21)
L1.INTEGRATION×ΔSPECIALIZATION	3.1480***	2.7976**	2.6332***	2.3614***	0.8175**	0.3166*
	(5.76)	(2.21)	(6.44)	(6.55)	(2.45)	(1.95)

Table 2.7. Effect of pairwise interstate banking integration on differential output growth: High versus low external finance dependent industries

This table presents *Instrumental Variables* (IV) regressions: $\Delta \ln(Y_{i,s,t}) - \Delta \ln(Y_{j,s,t}) = \beta_1 L1.INTEGRATION_{i,j,t} + \delta_{i,j,s} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + e_{i,j,s,t}$ where, $\Delta ln(Y_{i,s,t}) - \Delta ln(Y_{j,s,t})$ is the differential growth of output variable (*Y*) of sector *s* in state *i* and year *t relative* to the growth of the same sector *s* in state *j* and year *t*, with *i* (*j*) being the less- (more-) specialized state of the pair in sector *s* as of the date of effective interstate deregulation for state-pair *i*-*j*; $INTEGRATION_{j,t}$ is the sum of common banking assets belonging to MBHCs headquartered in either of the two states *i* and *j* in a given year *t* divided by the total of all banking assets in both states in the same year; all regressions include state-pair-industry, state *i*-year, state *j*-year, and sector *s*-year fixed effects. *Y* is one of Value Added (*VA*), Gross Operating Surplus (*GOS*), compensation of employees (*COMP*), number of employees (*EMP*), productivity as measured by output per employee (*PROD*), or wage measured as compensation per employee (*WAGE*). Panel A presents β_1 estimates obtained with the low-external finance dependent (EFD) sample, whereas Panel B those obtained with high-EFD, with EFD being defined as in Rajan and Zingales (1998). *Lt* represents the t^{th} lag. The standard errors are clustered at the state-pair-industry-level. *t*-Stats are reported below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Panel A: IV regressions (no lags) – Low external finance dependent industries

	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	0.4211***	-0.1326	0.0391	-0.0733	0.1773*	0.1124**
	(2.71)	(0.36)	(0.35)	(0.74)	(1.66)	(2.38)

Panel A: IV regressions (no lags) – High external finance dependent industries

	VA	*GOS*	*COMP*	*EMP*	*PROD*	*WAGE*
L1.INTEGRATION	1.0031***	0.9571**	0.7347***	0.4897***	0.4894***	0.2088***
	(5.28)	(2.07)	(5.77)	(4.20)	(4.05)	(4.12)

Chapter 3

Interstate Banking Integration and Corporate M&As and Divestitures in the US

3.1. Introduction

We examine whether financial integration across economic areas can have a positive effect on the market for corporate control of non-financial industries across the same regions. We draw upon two different strands of the literature to build our hypothesis. The first area of research that we rely on focuses on the commercial banks' role in facilitating within-industry mergers and acquisitions (M&As) between their borrower-clients, and another (larger) one on the real effects of financial integration across states in the US. Following the removal of interstate bank-entry barriers, commercial banks enter new states for the first time through the acquisition of local banks. We conjecture that these bank-entries will broaden the set of potential matches for corporate (i.e., non-financial sector) M&As and divestitures (asset sales) for their client firms located in the banks' home and newly-entered markets.[1] As a result, after controlling for the possibility that bank-entry after deregulation might be endogenous, we should observe more M&A and divestiture activity between state-pairs with higher banking integration compared to those with no or little integration.

Next, we ask whether there is a differential effect of banking integration across non-financial industries. If the increase in M&As and divestitures is driven by the size of the client network of the banks, then the observed effect should be pronounced for the industries that exhibit high external finance dependency (as defined in Rajan and Zingales, 1998). Second, we presume that banks of a state that is specialized in an industry (relative to US average) tend to lend more to that sector resulting in a bigger client network operating in that industry. Thus, following the entry into a new market in another state, a potential acquirer-target match is more likely to happen for firms in that industry. We conjecture that financial integration of these states through their banking systems leads to an increase in the number and value of cross-border within-industry M&As and divestitures for these industries after controlling for time-varying industry and state fixed effects.

The main argument and empirical evidence in support of our working hypothesis are provided by Ivashina et al. (2009). Using SDC data for 1,454 corporate acquisitions between 1992 and 2005, these authors show that the probability that a firm will be a target increases with bank lending intensity. Further, they find that firms that have lending relationships with banks that have more clients in the same industry are more likely to be subject to a takeover

[1]Interstate banking integration via the acquisition of local banks allows us to conjecture a specific channel through which a potential acquirer and target firm is matched. The entrant bank would immediately access the client portfolio of the target bank, facilitating the matching process.

attempt. Ivashina et al. (2009) also provide evidence that firms that switch to a new relationship-bank are also more likely to acquire companies that are borrowing from this new institution. These findings suggest that banks not only collect information for monitoring their borrowers but also transfer this information to their other borrowers to help them find potential targets among the bank's client network. A natural implication of Ivashina et al. (2009) findings is that higher bank-entry observed post-interstate banking deregulations should lead to higher activity in the market for corporate control across financially integrated state-pairs. We test this implication by examining M&As and divestitures for non-financial firms between 1982-1995, a period during which states of the Union have opened their banking markets, typically for the first time, to entry by banks from other states in a staggered fashion. During this period, bank-entry took place through the acquisition of local banks. Acquired banks became parts of the multi-bank holding companies (MBHCs) that now operated across state borders. In this context, we believe that it is not unrealistic to expect that banks belonging to these conglomerates (but operating in different states) share information among themselves and with their clients in a way that is consistent with Ivashina et al. (2009). The information sharing hypothesis among affiliates of a MBHC is also based on other papers that find evidence of information flows between bank and non-bank affiliates of financial holding companies in the US (e.g., commercial and investment banking affiliates in Gande et al. 1997; commercial banks and mutual fund affiliates in Massa and Rehman, 2008).[2] We complement Ivashina et al. (2009) work in two dimensions. First, we examine within as well as across industry M&As and test whether banks' proven role for horizontal transactions extends to vertical or diversifying ones. Second, we also analyze corporate divestitures since the latter are found to be related to industry restructurings, which also involve M&As (Mulherin and Boone, 2000).

Another aim of this paper is to link Ivashina et al. (2009) findings with the larger literature on financial integration and real economic activity across regions. Earlier work by Morgan, Rime, and Strahan (2004) examines state-level data and finds that banking integration across states helps smooth regional output fluctuations in the US while increasing the possibility of importing economic shocks from other states.[3] Cetorelli and Strahan (2006) look at the state-level size distribution of US firms and find that higher banking competition

[2]Luo, Manconi, and Schumacher (2014) provide evidence consistent with information flows across international affiliates after mutual fund M&As.

[3]Goetz and Gozzi (2013), who use finer state-pair as well as industry level data and deregulations for identification (as in Michalski and Ors, 2012; and Goetz, Laeven, and Levine, 2013), find results that are similar to Morgan, Rime, and Strahan (2004) who rely on state level deregulations.

following interstate banking deregulations is associated with the growth of small firms at the expense of larger ones. Kerr and Nanda (2009) document that small firm entry and exit (the so-called "churning" effect) increases following the same bank-entry liberalization events. More recently, and from a different perspective, Michalski and Ors (2012) provide evidence that within-US trade flows between state-pairs increase following interstate bank-entry deregulation between the same states. Finally, Karakaya, Michalski, and Ors (2014) find that a state's underdeveloped industries (with respect to US average) grow faster in terms of value added, gross operating surplus and productivity following higher banking integration with states that are over-developed in the same sectors. Karakaya, Michalski, and Ors (2014) findings are consistent with the view that banks that are exposed to certain industries are better at screening loans in the same sector when they enter new banking markets. By linking Ivashina et al. (2006) findings with those of this larger literature, we provide evidence for one particular micro channel through which economic integration takes place following financial integration: multi-regional banks' role as catalysts in the market for corporate control across regions. As such, we expand the literature by drawing attention to the role banks maybe playing in regional convergence found, for example, in Acharya, Imbs, and Sturgess (2011).

Our paper is also related to the recent work of Frésard, Hege, and Phillips (2014) who examine the relation between international M&As and countries' industry specialization. These authors find that firms from over-specialized countries in a given industry are more likely to acquire targets in under-specialized countries. Even though we also analyze, in a second step, the effect of states' over-specialization in an industry on M&As and divestitures with our state-pair-industry-year-level regressions, our focus is very different. In contrast to Frésard, Hege, and Phillips (2014), who analyze the role of countries' industry over or under specialization in corporate acquisitions across states in an international setting, we primarily examine the role of commercial banks' integration in shaping the US market for corporate control across state-pairs. As such, our work complements their evidence on sectoral specialization in M&A activity, by emphasizing the matching role played by banks across US regions with different specialization levels in a particular industry segment.

More precisely, we examine whether the number and the value of corporate M&As and divestitures were higher across state-pairs whose banking sectors integrated following interstate banking deregulation, compared to state-pairs that did not deregulate entry into their banking sector (i.e., compared to state-pairs with non-integrated banking systems). We define banking integration for a state-pair *i-j* as the ratio of banks' total assets in *i* owned by MBHCs located in *j plus* banks' total assets in *j* owned by MBHCs located in *i divided by* the

combined total assets of banks in both states. In OLS regressions we find a positive impact of banking integration on both the total number and the value of M&As and divestitures. However, such findings are prone to the valid criticism that banking integration may not have taken place in a random fashion. Another unobserved factor (for example, state- and or industry-level growth opportunities that are not fully accounted for in our specifications) may lead to a spurious correlation between our dependent variable (number or value of M&As and divestitures) and our test variable (banking integration). Banks from a state seeking higher lending opportunities may enter higher-growth states, which are likely to be the regions with naturally higher M&A or divestiture activity. To deal with such concerns of possible endogeneity, we rely on instrumental variables (IV) estimation in our main set of results. We make use of the staggered nature of state-pair bank-entry deregulations that took place in the US to follow an IV-estimation approach (similar to the ones used in Morgan, Rime, and Strahan, 2004; Michalski and Ors, 2012; and Goetz, Laeven, and Levine, 2013). IV-estimates suggest stronger effects of interstate banking integration on corporate sector M&As and divestitures (consistent with the idea that *within* (i.e., OLS) estimator provides downward-biased estimates due to endogeneity).

Next, to examine the differential effect of banking integration across industries, we reconstruct our state-pair-year sample at the state-pair-industry-year-level. Then we divide our sample into mutually exclusive subsets to test each of our conjectures above: i) industries with high/low external finance dependency (defined in Rajan and Zingales, 1998), ii) over/under-specialized state-pairs for a given industry. Then, we ask whether there are more horizontal M&As and divestitures for a given industry across state-pairs whose banking systems have experienced a higher integration, compared to state-pairs with no such integration for each subset. Then we check whether the effect of banking integration is stronger for the first group of observations than for the second group.

We find that financial integration of regions through the banking sector increases M&A and divestiture activity, showing that banking integration affects the real economy not only through a lending channel but also through a market for the corporate control channel. Further, we find that banking integration is more effective in facilitating transactions for a particular industry within a given state-pair, when the industry is external finance dependent, and developed (relative to US average) in at least one of the states of the given state-pair at the time of deregulation. This is a novel finding that expands our understanding of the role financial integration in influencing the industrial landscape through the market for corporate control. While we focus on the US, our findings have wider policy implications. Bank M&As

across borders can have a positive effect on corporate M&As and divestitures across countries that may also have a differential effect across non-financial industries. Apart from the decision to integrate, the question of with whom to integrate also become central for policy-makers.

The paper proceeds as follows. In Section 3.2 we detail our specifications, the identification problem that we face, and the data that we use. In Section 3.3, we provide our primary results. In Section 3.4 we conduct a series of robustness checks. Section 3.5 concludes.

3.2. Empirical specifications, the data, and summary statistics

3.2.1. Empirical specifications

We use state-pair-year-level data, and test whether the total number and value of M&A or divestiture transactions increase as banking integration grows across economic regions after bank-entry deregulation. Our identification strategy focuses on (within) state-pair variation across time after controlling for unobservables through time-varying state fixed effects for each of the states in a pair separately. Using state-pair-year-level data allows us to control for the time-invariant state-pair characteristics (e.g. distance), and time-varying state-level variables such as the economic growth of the states in a given pair. Year fixed effects absorb other potential time-varying factors at the US-level. We further investigate whether these findings are driven by horizontal or vertical transactions. During the estimation process of all regressions, we partial out the (large number of) fixed effects and heteroskedasticity-robust standard errors are clustered at the state-pair-level.

We rely on linear regression models despite the fact that we have limited dependent variables (the number or value of M&As and divestitures), which have a natural lower bound of zero. We prefer not to use Tobit regressions because our dependent variables are not truly censored (i.e., an observation of zero suggests no M&A or divestiture for that state-pair in that year in the data). As noted by Angrist and Pischke (2009, pp. 104-107), linear regression models (which make relatively few distributional assumptions) with limited dependent variables (which is our case here), typically produce very similar marginal effects compared to nonlinear models such as Tobit (which requires more stringent distributional assumptions). Moreover, because our focus is on IV estimates, we would like to be able to conduct standard

identification tests to assess the validity of our instruments. These latter are available for through 2SLS-IV, GMM-2S-IV or LIML estimators (for example, in Stata), but are not available, to the best of our knowledge, for Tobit-IV or Poisson-IV estimators for panel data. Finally, the data on the number of M&As and divestitures contain many zeros (71% of the M&As and 77% of the divestitures have zero state-pair-year observations.) and are highly dispersed (i.e., their standard deviations are much higher than their means). This dispersion further complicates the use of non-linear IV estimation techniques that are already prone to the convergence issues due to the large number of fixed effects in our model (1301 dummy variables are created for state-pair, year, state-year fixed effects for our estimation equation). Because of these concerns, we limit ourselves to linear regression models.[4]

Using the state-pair-level data, we estimate the following regression model:

$$Y_{i,j,t} = \alpha_0 + \beta.INTEGRATION_{i,j,t-1} + \delta_{i,t} + \delta_{j,t} + \delta_t + \delta_{i,j} + \varepsilon_{i,j,t} \qquad (3.1)$$

where, $Y_{i,j,t}$ is the total number or value of M&As or divestitures for a state-pair in a given year among firms headquartered in state i and j; $INTEGRATION_{i,j,t-1}$ is the bilateral banking integration between the state-pair i-j in year t-1.$\delta_{i,t}$ ($\delta_{j,t}$) is a time-varying state i (j) fixed effect; δ_t is a year fixed effect. $\delta_{i,j}$ is a state-pair i-j fixed effect that controls for unobservables that remain constant over time; $\varepsilon_{i,j,t}$ is the error term. It should be noted that $Y_{i,j,t}$ is the total number or value of corporate M&As or divestitures in a given year between a state-pair i-j. As an example, suppose that, in a given year, a firm headquartered in i acquires a firm headquartered in j, but no firm in i acquires any firms in k, and two firms headquartered in j acquire one firm in i and one firm in k. Then, for that year, the number of M&As would be two for the state-pair i-j, zero for i-k, and one for j-k. We lag *INTEGRATION*, the instrumented endogenous test variable by one year for two reasons. First, we don't want the left-hand-side variable (Y) and the right-hand-side endogenous test variable (*INTEGRATION*) to be contemporaneously determined. Lagging the test variable minimizes the potential

[4]At the risk of leaving many state-level unobservable factors uncontrolled, I replace state-year fixed effects with the logarithm of the annual state GDP to achieve convergence in a Poisson regression. I use a moment evaluator program developed for *gmm* command in Stata by Timothy Simcoe at Boston University (http://people.bu.edu/tsimcoe/data.html). The advantage of this program is that it performs a *Within* regression and correct for endogeneity of the right-hand-side variables (in our case, the banking integration variable) while using Poisson specification. I use the same instruments that I use in the linear regressions. I have statistical significance only when the dependent variable is the number of M&As. One standard deviation change in integration (0.0108) corresponds to an increase in incidence rate ratio by a factor of exp(0.0108*12.8049)=1.15.

problem of endogeneity in the *Within* (panel fixed effects) regressions that we run. Second, intuitively it is less likely that bank acquisition taking place within year t would have an impact on corporate transactions in the same year. This is all the more so, as we measure banking integration as of year-end t. State-pair fixed effects ($\delta_{i,j}$) control for unobservables that remain constant over time for *i-j*, such as the adjacency (corporate activity is more likely to happen between states sharing a common border); regional location (in many instances states opened their banking markets to states in the same region first, typically leading to higher within-region integration initially);the distance between states (takeovers and asset sales may be less likely between further away states as information asymmetries are likely to increase with distance). State-specific year fixed effects ($\delta_{i,t}$ and $\delta_{j,t}$) control for observables (such as the economic growth of each of the states in a pair) as well as unobservables (such as changes in legislation or corporate taxes that are hard to track at the state-level) that vary over time.[5]

A major benefit of the state-specific year fixed effects ($\delta_{i,t}$ and $\delta_{j,t}$) is to control for the banking integration of the states in the state-pair with the remaining 46 states. The increase in total banking integration might proxy for the increase in bank finance, and our findings might be driven by the increase in availability of funding for M&As rather than bank affiliates sharing information across regions and facilitating potential acquirer-target matches. Using state-year fixed effects alleviate this concern by accounting for the effect of deregulation on bank finance availability in a given year in each state in the state-pair.[6]

We estimate regression Eq. (3.1) using both the *Within* as well as a 2S-GMM-IV estimators.[7] In the IV-regressions we use a combination of (i) average number of years since effective bank-entry deregulation between the states in a pair towards each other as well as its square or square-root, (ii) an indicator variable that takes the value of zero before effective bank-entry deregulation date, and of one for the years post deregulation (choice of these instruments is similar to those made by Morgan, Rime, and Strahan, 2004, or Michalski and

[5]An important concern that is addressed with the state-year fixed effects is the passage of anti-takeover laws (laws aimed to prevent hostile takeovers, see Atanassov(2013)) during our sample period. Similar to bank-entry deregulations these laws were adopted by states at a staggered fashion.

[6]A major concern could be state-pair integration is highly collinear with total integration if a state opens up to very few states. However, the correlation between state-pair integration and total integration of a given state is 0.21. Further VIF (variance inflation factor) test used for collinearity gives a value of 1.05. As a rule of thumb, a variable whose VIF values are greater than 10 may merit further investigation.These values are also in line with the fact that most states opened up their banking system nationally reciprocally (33.8%) and the second common mode of deregulation was national-reciprocal (21.6%).

[7]In this case, GMM-2S estimator is preferred to 2SLS because our dependent variables (the number or the value of M&As or divestitures at the state-pair or state-industry levels) are heteroskedastic, in which case 2SLS is still consistent, but GMM is more efficient asymptotically(e.g., Cameron and Trivedi, 2005, p. 747).

Ors, 2012). The first instrument has the benefit of following the dynamics of different types of deregulatory processes for interstate bank-entry that were put in place in the US between 1977 and 1995. In some instances, state *i* and *j* permitted entry only based on reciprocity, in which case the effective date of opening is that of the state that allows (reciprocal entry) the latest. For example, if state *i* does (reciprocal) entry deregulation in the year 1986, and state *j* does the same but only on December 31st, 1990, *YEARS_SINCE* will be equal to 0 in all years before 1991 and 1 for all years after 1990. In this case, the average years since deregulation will be equal to 1 for 1991, 2 for 1992, 3 for 1993 and so on: this is because the number of years since effective deregulation goes up by the same increment of 1 by each for both states *i* and *j*. In other instances, some states decided to open up their banking markets in a non-reciprocal way (i.e., irrespective of the regulatory stance of the counterparty state). Suppose that state *i* non-reciprocally deregulated in 1986 but *j* allows interstate bank-entry in a reciprocal way as of 1990. In this case the average number of years since deregulation would be equal to 0 prior to 1987, it will be equal to 0.5 for 1987, 1 for 1988, 1.5 for 1989, 2 for 1990 (the year of reciprocal opening of state *j*), and be equal to 3 in 1991, 4 in 1992, and so on. Our instruments are highly unlikely to be correlated with corporate M&A activity in a given state, yet it is highly correlated with banking integration: first, there cannot be increases in banking integration without bank-entry deregulation. Second, the more the time passes by after deregulation, the higher is the potential for integration.

In the second set of regressions, we focus on within-industry transactions:

$$Y_{i,j,s,t} = \alpha_0 + \beta.INTEGRATION_{i,j,t-1} + \delta_{i,t} + \delta_{j,t} + \delta_{s,t} + \delta_t + \delta_{i,j,s} + \varepsilon_{i,j,s,t} \quad (3.2)$$

where *i-j*, *s* and *t* denote state-pair, industry and time respectively. The dependent variable is the total number or the total value of horizontal state-pair M&As or divestitures in a given industry *s*. *INTEGRATION* is the state-pair banking integration in year *t-1*. $\delta_{i,t}$, $\delta_{j,t}$ are time-varying state fixed effects that capture state-level observables and unobservables. $\delta_{s,t}$ and $\delta_{t,}$ control for the industry-wide and US-wide time-varying factors respectively. Last, we put a panel fixed effect, $\delta_{i,j,s}$ so our identification strategy relies on the remaining variation at state-pair-industry-level. The instruments of the previous analysis are used for the IV estimation.

We further check the validity of the IVs with different identification tests. In the first identification test, the null hypothesis is that the regression equation is under-identified: a *rejection* shows that the underlying variance-covariance matrix is of full rank, i.e. the regression equation is identified. In the second identification test, we check whether our IV

model suffers from weak instruments problem. With endogenous regressors, OLS is downward biased. IV regression can alleviate this concern but the estimates as well as the standard errors still remain biased: the question is how much relative to the bias of the OLS estimator. This can be tested with the Kleibergen-Paap rk and Wald F-statistics whose value can be compared with Stock and Yogo (2005) critical values. The null hypothesis of Stock and Yogo's test is that the set of instruments is weak. The hypothesis is rejected/not rejected based on the largest relative bias of the IV estimator one is willing to tolerate. Instead of choosing a rejection threshold value, we report the bias of the IV estimator relative to the OLS estimator for each regression. In the third and final identification test, the joint null hypothesis is that the IVs are uncorrelated with the error term of the IV-regression and that the exclusion restrictions are valid: a rejection suggests that the IVs used may not be valid. We report the results of these three tests at the bottom of each of our regressions.

3.2.2. The data

To conduct our empirical tests we combine four separate databases. To calculate the banking integration variable across state-pairs, we use the financial statements (the so-called Call Reports) that the US banks and BHCs are required to file with the federal banking regulators. The publicly available version of the BHC Call Report (also known as Y-9) data starts in 1986. We complement these with the so-called *structure* data files of the National Information Center (NIC) for banking institutions for the year-ends 1981 to 1985. Given the one-year lag that we impose on the endogenous integration variable (so as to avoid contemporaneous co-variation with the dependent left-hand-side variable to minimize problems associated with endogeneity), our estimation sample begins in 1982, the year during which bank-entry deregulation became effective for the first time between certain state-pairs.[8] Lagging integration variable also makes economic sense as it is unlikely that entry in the form of bank acquisitions will affect M&A activity immediately, as information flow is unlikely to happen immediately among banks that are members of the MBHC following an M&A. Our sample ends in 1995 mainly due to IBBEA that took effect in September of 1995, and allowed unrestricted interstate bank-entry.[9] The implementation of

[8]Maine-New York was the first state-pair in 1982 followed by Maine-Connecticut, Maine-Massachusetts and Connecticut-Massachusetts in 1983.

[9]A second reason that we cannot expand the sample beyond 1997 is the fact that BEA industry classification changes from 2-digit SIC to 2-digit NAICS in 1997. Meaningful matches between the two classification systems can only be made at the four digit levels. As a result we cannot create a consistent data series to examine M&A activity post-IBBEA using interstate branch-entry restrictions as an identification scheme.

IBBEA weakens our identification strategy beyond 1995 in a number of ways. First, while some states implemented this act in its entirety (allowing entry through acquisitions but also through de novo bank branches), others restricted or forbade interstate entry via branches (for details see Rice and Strahan, 2010). We would like our identification scheme, hence our tests, not to be affected by these differences in the implementation of IBBEA. Second, IBBEA allowed MBHCs to simplify their corporate structure by merging banks in their conglomerate structure and getting rid of additional layers of top management and board of directors. However, the Call Reports are filed at the level of the emerging banking institution (in which some of the pre-IBBEA banking affiliates are turned into branches), which may span multiple states post-IBBEA. As a result, our *INTEGRATION* variable, which is based on total banking assets, would no longer provide an adequate picture of the actual banking integration as the sample extends beyond 1995.[10] We collect interstate bank-entry deregulation dates from Amel (2000) to create the effective dates of deregulation that we use to instrument banking integration.

We construct an annual panel of interstate M&As and divestitures using Thomson One Banker database over 1982-1995. More specifically, we drop recapitalizations, repurchases, restructurings, self-tenders, privatizations as well as deals involving government agencies from the sample. Our sample contains transactions in which acquirer and target firms in an M&A, or asset seller and buyer firms in a divestiture, are headquartered in different states. In other words, we exclude *within* state transactions, as well as those involving firms headquartered outside of the US. Thomson One Banker provides two-digit SIC for all acquirers and targets in the case of M&As, and for all sellers and buyers of assets in the case of divestitures. This allows us to examine horizontal versus vertical transactions separately.

We use annual Bureau of Economic Analysis (BEA) estimates of state-industry output tables to measure the specialization level of states in a given industry. There might be two concerns regarding BEA data. First, annual state-industry-level value added is a BEA estimate based on US Census conducted once in five years. Second, BEA and SIC industry segmentation do not perfectly match and in few cases BEA segmentation is very coarse covering up to four two-digit SIC category. Table 3.1 presents the two-digit SIC correspondence for each BEA category. Nevertheless, we use BEA data, as they are the only publicly available state-industry-year-level data.

[10]One way to get around this problem is to presume that bank assets (or loans) and deposits are highly correlated. Under this assumption one can use FDIC's Summary of Deposits dataset that provides branch-level deposit information and calculate integration based on deposits of a bank at the state level.

Finally, we use Compustat data to compute industry-level external finance dependency as defined by Rajan and Zingales (1998). Using Compustat data, we compute the value of each firm's external financing needs, which is calculated by subtracting cumulative cash flows from operations from cumulative capital expenditures and then dividing it by the capital expenditures over the period 1982-1995. Calculating the measure over a period, rather than on annual basis helps reduce the effect of an outlier firm-year observation. Next, we select the value of the median firm in each industry and rank the industries accordingly.[11]

3.2.3. Summary statistics

We present the summary statistics in Tables 3.1 and 3.2. Table 3.1 provides detailed information on the dispersion of M&As and divestitures over industries and time. As already documented by Mitchell and Mulherin (1996), the market for corporate control exhibits industry and time-series clustering. More restructuring and takeover activity is observed towards the end of the sample period. This increased activity is mostly driven by particular industries (e.g. communications, wholesale trade, retail trade) that respond to industry specific deregulations and technological advancements. Mitchell and Mulherin (1996) note this as a striking difference from the wide-spread takeovers in 1980's.

Table 3.2 presents the summary statistics of the same data at the state-pair, state-pair-industry-level in Panel A and B respectively. The median of total number or value of transactions is zero at all levels of data since we have more than 50% zero observations both for M&As and divestitures. Furthermore, the standard deviations of dependent variables are three to four times larger than their means at the state-pair-level (even ten times larger for observations at state-pair-industry-level), complicating the use of non-linear IV estimation methods.

We report the descriptive statistics of M&As and divestitures at the state-pair-level in Panel A. More horizontal transactions occurred compared to vertical consolidations during our sample period. On average, 0.87 M&A is realized in a state-pair per year of which 0.49 is horizontal, and 0.38 is vertical. The average of total M&A deal volume realized in a state-pair in our period of analysis is around $51 million of which $33 million is horizontal, and $18million is vertical. For divestitures, these numbers are smaller, being $13.5 million for horizontal divestitures and $7.5 million for vertical divestitures.

[11]Among 42 industries, Compustat has only one firm in "membership organizations" (BEA ID: 69) category so we exclude this industry and run our regressions on 41 sectors.

In Panel B of Table 3.2, we present the statistics at the state-pair-industry-level that categorizes each within industry state-pair transaction into industry groups defined by BEA segmentation. For a given state-pair, 0.01 M&A is realized on average for a particular industry per year, and the average of total deal value is around $790,000. For divestitures, these values are smaller, 0.008 and $320,000 respectively.

Figure 3.1 plots state-pair banking integration and number of M&As and divestitures over the period of analysis. While the average number of M&As and divestitures increase for integrated state-pairs as average banking integration increase, this pattern is not as strong for the M&As that take place in non-integrated state-pairs.

3.3. Findings

Our identification relies on within state-pair variation of the total number or value of M&A deals across time. We first examine whether increases in banking sector integration between two states leads to higher M&A activity (measured both as the number of deals but also the value of such deals across state-pairs). It should be noted that we focus on the total number or value of transactions in a state-pair and we do not make a distinction between acquisitions of state *i*'s firms in state *j* and vice versa.

In the first column of Table 3.3, we examine the number of M&A deals between state-pairs in a Within regression. The coefficient estimate for *INTEGRATION* is equal to 10.1497, which is statistically significant at the 1%-level. One standard deviation increase in banking integration between a state-pair (which is 0.0108 and corresponds to roughly an increase of 1% in banking integration) leads to 0.11 more deals per year between them. However, this *Within* estimator based result is downward biased if banking integration is endogenous. In column two of Table 3.3 we present the GMM-2S-IV regression results. The coefficient estimate of *INTEGRATION* is equal to 34.8380, which is statistically significant at the 1%-level. One standard deviation increase banking integration leads to 0.38 more deals per year. This low value in absolute terms is not surprising given the very large number of zeros contained in the state-pair M&A deals data. However this corresponds to an increase of 44% of the mean value. It should also be noted that the estimate of the column is well identified. The null hypothesis of under-identification is rejected (i.e., the variance-covariance matrix is of full rank). The null hypothesis of weak instruments is also rejected at the 5%-level. Finally, the over-identification test, i.e., the null hypothesis of valid instruments, cannot be

rejected. A similar set of identification test results is obtained in all of the remaining regressions, except where otherwise noted.

In Table 3.3 columns three and four, we look at the changes in the value of M&A deals following banking integration between state-pairs. In column three, the coefficient estimate for *INTEGRATION* is equal to -248.25 but not statistically significant. In the last column of Table 3.3, the same coefficient estimate with panel GMM-2S-IV regression is equal to 4,088.085, which is statistically significant at the 5%-level. One standard deviation increase in banking integration leads to a $44.15 million increase in the value of M&A deals between an average state-pair. Again, this coefficient estimate does not suffer from apparent identification problems.

These first set of results suggest that compared to state-pairs whose banking systems are not linked, those with integrated banking sectors have higher M&A deals and higher M&A values, on average. These results are consistent with the flow of information across members of MBHCs leading to a more efficient market for corporate control across state-pairs. In the next step, we examine whether the increase in M&A activity between state-pairs with more banking integration is due to horizontal (within the industry) or vertical (across industry) consolidation.

In Table 3.4, we focus on horizontal and vertical M&As (as in Ivashina et al., 2009) separately. In the first column, in which we present the Within regression results, the coefficient estimate for *INTEGRATION* is equal to 7.48, which is statistically significant at the 1%-level. One standard deviation increase in banking integration between the average state-pair (which is equal to 0.0108) leads to a 0.08 more deals. In column two, with panel GMM-2S-IV regressions, the coefficient estimate for *INTEGRATION* becomes equal to 26.96, which is statistically significant at the 1%-level. One standard deviation rise in banking integration between a state-pair leads to 0.29 more interstate horizontal M&A deals. Although the coefficient estimate is lower compared to the one in column 2 of Table 3.3, this estimate corresponds to an increase of 87% of the mean value which is higher than 44% (as computed for Table 3.3). In the third column, the OLS (i.e., Within) estimate for *INTEGRATION* is equal to 82.66 but not statistically significant. In the fourth column of Table 3.4, with panel GMM-2S-IV regressions the coefficient estimate for *INTEGRATION* is equal to 3607.21, which is statistically significant at the 5%-level. One standard deviation increase in banking integration between the average state-pair leads to $38.96 million more horizontal M&A deals between these states on an annual basis. These results corroborate the findings of Ivashina et al. (2009). In the last four columns of Table 3.4, we examine vertical

M&As. In the fifth column, the coefficient estimate for *INTEGRATION* is equal to 2.666, which is not statistically significant. Similarly in the next column, with the GMM-2S-IV estimator, the same coefficient estimate becomes equal to 7.1193, which is also marginally statistically significant at the 10%-level but the regression fails to pass the over-identification test at the 5%-level. In the last two columns, when we examine the values of vertical M&A deals, we find not statistically significant results. These findings suggest that most of the higher M&A activity that we observe between state-pairs whose banking systems have experienced higher integration is due to horizontal transactions, rather than across industry deals. For the latter, we have only weak evidence, despite the fact that IV-regressions are well identified.

In Table 3.5, we examine the total number and value of divestitures between state-pairs. To the best of our knowledge, we are the first ones to examine whether information flows within MBHCs following bank-entry deregulation leads to higher asset sales (i.e., divestitures). In the first column of Table 3.5, we observe that the coefficient estimate for *INTEGRATION* is equal to 4.88, which is statistically significant at the 1%-level. However, without IVs, this estimate is biased toward zero if *INTEGRATION* is endogenous to corporate transactions between state-pairs. Indeed, in column two of Table 3.5, estimating the same regression with the GMM-2S-IV estimator, the coefficient for *INTEGRATION* is equal to 11.34, which is statistically significant at the 5%-level. One standard deviation of increase in banking integration between the average state-pair leads to 0.12 more cross-border asset sales transaction (23% of the mean value). However, we observe no statistically significant result in columns three and four when we examine the total value of divestitures between state-pairs (even though the coefficient estimates are positive).

In Table 3.6, we look at the horizontal and vertical divestitures separately. In column one, the coefficient estimate for *INTEGRATION* is equal to 4.99, which is statistically significant at the 1%-level. In column two, with the IV estimation, the coefficient estimate for *INTEGRATION* becomes equal to 11.23, which is statistically significant at the 1%-level. One standard deviation increase in bank integration leads to 0.12 more deals that equal to an increase of 37% of the average number of cross-border divestitures. In column three, in which the dependent variable is the total value of horizontal divestitures within the state-pair, the coefficient estimate of *INTEGRATION* is equal to 178.10 but not statistically significant. In the fourth column of Table 3.6, when re-estimate the same regression equation using IVs, the coefficient estimate for *INTEGRATION* is equal to 978.44, which is marginally statistically significant at the 10%-level. One standard deviation increase in banking

integration leads to $10.57 million more in the value of transactions between state-pairs, on average. In the last four columns, we examine, inter-industry divestitures, but none of the coefficient estimates for *INTEGRATION* is statistically significant. These findings suggest that within-industry divestitures increase faster among 5,214 that occurred between 1982 and 1995, following banking integration. However, bank integration has no effect on the 2,948 across industry asset sales that took place in the same period.

If banking integration facilitates the matching process of potential targets and acquirers, it should do more so if targets and acquirers are located at a distance apart and information collection is more costly. We expect the banking integration to be more effective in matching firms as the distance between acquirer and target increases. We test this conjecture for the sub-sample of state-pairs whose states are apart from each other above the distance between the median state-pair. Table 3.7 displays the results for M&As for the first four columns; and divestitures for the last four columns for this sub-sample. In column one, the coefficient estimate for *INTEGRATION* is equal to 90.02 and is statistically significant at the 1%-level. With the IV estimation, the coefficient estimate for *INTEGRATION* becomes equal to 662.70, which is statistically significant at the 1%-level. One standard deviation increase in banking integration for this sub-sample (0.0029) leads to 1.92 more deals between state-pairs, on average. This effect is around five times stronger than the effect measured for the main sample. A similar conclusion can be attained for the divestitures. IV estimation results suggest that one standard deviation increase in banking integration leads to 0.78 more divestitures between state-pairs, which is 6.7 times more than the effect measured for the main sample. While the value of state-pair M&As also increases in proportion to the increase in number, we do not observe such an effect on the value of divestitures.

Next, we test our hypothesis for a sub-sample of deals in which both acquirer and target firms are publicly traded. We expect the effect of banking integration to be smaller for these deals for two reasons. First, information collection about public firms is relatively easy compared to private firms. Second, public firms are less dependent on bank finance. Thus the probability of a public firm being in the portfolio of a bank is smaller than a private firm. Table 3.8 displays the results for M&As for the first four columns; and divestitures for the last four columns for this sub-sample. The coefficient of *INTEGRATION* in columns 2 and 6 is about one-eighth and one-twentieth of the coefficients in full sample displayed in Table 3.3 and Table 3.5. We also observe a smaller effect on the total value of deals.

In Table 3.9, we focus on the estimation results of Eq. (3.2) with the whole sample at the state-pair-industry-year-level.[12] In column one, we look at the changes in the number of horizontal M&A deals for a given industry following banking integration between state-pairs. The coefficient estimate for *INTEGRATION* is equal to 0.5963, which is statistically significant at the 1%-level. An increase by one standard deviation in banking integration leads to 0.007 more deals per year. This very low value in absolute terms should be expected given the very large number of zeros contained in the state-pair-industry M&A deals dataset, however, this corresponds to an increase of 60% of the mean number of deals within a state-pair in a given industry. When the dependent variable is the total value of within-industry state-pair M&As, the coefficient estimate of *INTEGRATION* is 64.0566 and statistically significant at the 5%-level. One standard deviation increase in banking integration leads to a $0.69 million increase in the total value of M&A deals for a particular industry. The coefficients also exhibit reasonable magnitude when we compare the results with column one and column two of Table 3.4. The coefficients for Table 3.4 estimate that one standard deviation increase in banking integration between the average state-pair leads to 0.29 more deals and $38.96 million more deal volume for all industries. For a given industry these values should be divided by forty-two (the number of industries in our sample), which is equal to 0.007 and $0.93 million respectively.[13] For the divestitures, the estimates are smaller, but in the close range of economic magnitude. One standard deviation increase in banking integration leads to an increase of %38 of the mean number of the state-pair divestitures in a given industry. The effect of banking integration on the total value of state-pair divestitures at the industry-level is statistically significant at the 10%-level. The coefficient in column four corresponds to an increase of $0.2 million increase in the total value of divestiture deals for a particular industry within integrated state-pairs compared to non-integrating ones.

Table 3.10 displays the results for two mutually exclusive subsets after we split the whole sample into two and classify twenty (twenty-one) industries as low (high) external finance dependent (EFD). In the first four columns where the results for high EFD industries are displayed, the effect of banking integration is more pronounced in every regression compared to the results in Table 3.8. An increase of one standard deviation in banking integration within a state-pair leads to an increase in within-industry M&A deals per year by 0.013 for a

[12]Starting from Table 9, we only present the results of GMM-2S-IV estimations for the sake of brevity. The OLS results can be provided upon request.

[13]A close magnitude is reasonable but the exact numbers may not be achieved since Eq. (2) also controls for industry year fixed effects.

given industry. In terms of total deal volume, the coefficient of *INTEGRATION* in column two corresponds to an increase of $2.4 million. We also see an elevated effect on divestitures. The coefficient of *INTEGRATION* in column three and four is 0.5394 and 41.6249 respectively as opposed to the coefficients of 0.2615 and 18.5181 in Table 3.8. Further, they are more statistically significant. The last four column reports the estimation results for low EFD industries. There is a statistically significant effect only in column five and to a much lesser extent regarding economic magnitude. An increase of one standard deviation in banking integration within a state-pair leads to an increase in within-industry M&A deals per year only by 0.001. All regressions are well identified except the one in column 7; the regression fails to pass over-identification test at the 5%-level.

In Table 3.11, column one, we look at the changes in the number of horizontal M&A deals for a given industry following banking integration between state-pairs where at least one state is over-specialized. The coefficient estimate for *INTEGRATION* is equal to 1.0937, which is statistically significant at the 1%-level. One standard deviation of increase in banking integration leads to 0.012 more deals per year corresponding to an increase of 83% of the mean number of deals within a state-pair in a given industry. When the dependent variable is the total value of within-industry state-pair M&As, the coefficient estimate of *INTEGRATION* is 182.6564 and statistically significant at the 5%-level. The finding exhibits reasonable magnitude. One standard deviation increase in banking integration leads to a $1.97 million increase in the total value of M&A deals for a particular industry. For the divestitures, the coefficients are displayed in column three and four. The estimates are smaller but in the close range of economic magnitude. One standard deviation increase in banking integration leads to an increase of %53 of the mean number of the state-pair divestitures in a given industry. The effect of banking integration on the total value of state-pair divestitures at the industry-level is statistically insignificant, albeit positive. In the last four columns of Table 3.11, we replicate the regressions for Eq. (3.2) for state-pair-industry observations where both of the states are under-specialized in a given industry. The only significant result is in column one, the coefficient of *INTEGRATION* is 0.2109 which is smaller than the coefficient in column one. One standard deviation increase in state-pair banking integration leads to 0.002 more within-industry deal per year in a given industry that corresponds to 53% of the mean. For the rest of the regressions, we have positive coefficients but statistically insignificant results, despite the fact that IV-regressions are well identified.

One opposing argument could be that the correlation is spurious, and it is obvious that the M&As and divestitures are driven by over-specialized states because there are potentially

more firms in these states. We try to alleviate this concern with the state-year fixed effects. Further, our panel fixed effect at state-pair-industry-level helps us compare the integrated state-pairs where at least one of the state is over-specialized at the time of bank-entry deregulation with the non-integrated state-pairs where at least one of the state is also over-specialized.[14]

3.4. Conclusion

In this paper, we examine the effect of higher financial integration of regions through their banking systems and the market for corporate control. Ivashina et al. (2009) findings suggest that banks can play an important role in matching borrower's clients for within-industry M&A transactions. Based on state-level US data, our findings corroborate Ivashina et al. (2009) findings at the aggregate level. We measure the banking integration of state-pairs across time following the removal of bank-entry restrictions. We test whether banking integration has any impact on the state-pairs' M&A and divestiture transactions when compared with the transactions among state-pairs whose banking systems are not integrated. Using IV estimation, we find evidence of higher M&A and divestitures for state-pairs with integrated banking systems. The observed increase is overwhelmingly due to within-industry transactions that follow banking integration.

Next, we examine the effect of banking integration on the states' cross-border M&As and divestitures at the industry-level. We find that the increase in cross-border transactions within the integrating state-pairs is pronounced compared to non-integrating state-pairs for external finance dependent industries. We also find that more cross-border horizontal M&A and divestiture is observed between the integrating states compared to non-integrating ones if the given industry is developed at least in one of the regions. However, this effect is not observed among state-pairs in which the given industry is under-developed in both states.

Our findings suggest that foreign bank-entry into a host country is likely to be followed by higher M&A and divestitures between the two countries. Further, the effect is differential across non-financial industries. As such, our paper provides additional evidence on the link between financial integration (in our case through banking systems) and the real economic integration.

[14]A more thorough regression should include time-varying industry state fixed effects ($\delta_{i,s,t}$, $\delta_{j,s,t}$) but we are not able to run this regression because total number of fixed effects to be created exceeds the maximum number of variables that can be generated by the software.

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3.5. Tables and Figures

Table 3.1. Industry composition of M&As and divestitures

Panel A presents the number of M&A transactions that occurred in 48 states (Alaska, Hawaii, and the District of Columbia are excluded) between 1982 and 1995 by industry. We use Bureau of Economic Analysis (BEA) industry segmentation to classify industries. The sample contains 42 manufacturing and services industries and the tobacco industry is excluded. 2-digit SIC correspondences of each BEA category are given in the next column. Panel B displays the same data for divestitures.

Panel A:

Industry Name	BEA ID	2-Digit SIC Correspondence	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	TOTAL
Lumber and wood products	14	24	9	5	4	3	6	4	3	3	4	5	3	2	11	17	**79**
Furniture and fixtures	15	25	4	9	7	7	20	12	5	4	4	3	6	14	12	9	**116**
Stone, clay, and glass products	16	32	5	10	13	4	10	6	4	8	5	6	3	4	11	7	**96**
Primary metal industries	17	33	9	11	11	4	5	6	10	12	7	10	7	10	19	28	**149**
Fabricated metal products	18	34	14	21	29	11	28	10	21	22	19	22	20	19	27	29	**292**
Industrial machinery and equipment	19	35	34	40	60	29	51	47	47	70	61	52	47	67	73	78	**756**
Motor vehicles and equipment	21	371	3	1	12	3	9	14	7	6	6	4	9	5	16	22	**117**
Other transportation equipment	22	372-379	7	14	13	11	7	9	6	8	5	4	9	6	6	11	**116**
Miscellaneous manufacturing	24	39	16	17	12	4	5	7	3	9	9	17	12	19	22	23	**175**
Food and kindred products	26	20	22	15	30	18	37	22	32	26	24	31	29	23	38	47	**394**
Textile mill products	28	22	9	7	8	3	9	5	8	4	7	9	7	7	11	19	**113**
Apparel and other textile products	29	23	5	11	16	6	6	10	5	11	11	7	9	14	18	17	**146**
Paper and allied products	30	26	2	9	4	6	7	11	12	9	9	7	10	12	12	30	**140**
Printing and publishing	31	27	8	14	27	12	31	39	27	35	28	23	24	25	48	50	**391**
Chemicals and allied products	32	28	27	38	29	22	21	46	24	44	37	54	45	54	72	89	**602**
Petroleum and coal products	33	29	3	2	2	1	1	1	2	5	3	4	2	1	1	2	**30**
Rubber and misc. plastics products	34	30	8	11	19	10	11	13	16	13	10	14	22	24	20	31	**222**
Leather and leather products	35	31	3	4	4	3	1	1	1	2	1	1	3	1	4	3	**32**

Railroad transportation	38	40	3	5	2	0	0	4	3	3	2	2	3	1	5	2	**35**
Local and interurban passenger transit	39	41	0	1	1	0	1	2	1	0	0	0	5	4	28	8	**51**
Trucking and warehousing	40	42	4	11	11	3	4	5	5	9	7	4	9	7	17	25	**121**
Water transportation	41	44	2	1	0	0	1	3	3	3	0	3	2	4	4	3	**29**
Transportation by air	42	45	4	3	14	14	16	11	5	10	4	8	5	2	4	10	**110**
Pipelines, except natural gas	43	46	1	0	0	2	0	0	0	0	1	2	0	1	0	2	**9**
Transportation services	44	47	1	0	1	4	2	1	4	4	2	7	4	7	12	20	**69**
Communications	45	48	23	42	46	14	31	40	54	57	55	67	59	107	133	150	**878**
Electric, gas, and sanitary services	46	49	9	10	20	8	12	14	20	24	52	41	53	58	43	53	**417**
Wholesale trade	47	50 & 51	38	80	83	28	52	49	53	93	75	72	94	144	220	239	**1320**
Retail trade	48	52-59	41	50	52	36	30	39	57	47	42	39	72	109	127	142	**883**
Hotels and other lodging places	58	70	3	9	4	6	8	7	6	2	3	2	4	37	24	19	**134**
Personal services	59	72	4	5	4	0	0	2	2	3	4	7	7	11	10	6	**65**
Business services	60	73&84&87&89	60	102	109	43	74	102	112	166	166	156	219	290	397	576	**2572**
Auto repair, services, and parking	61	75	0	3	3	3	4	2	1	8	5	4	4	3	8	6	**54**
Miscellaneous repair services	62	76	1	0	1	0	0	0	3	4	0	2	6	1	5	8	**31**
Motion pictures	63	78	2	2	9	6	16	10	14	17	9	8	8	27	26	39	**193**
Amusement and recreation services	64	79	2	3	6	2	2	0	5	1	0	4	10	24	19	36	**114**
Health services	65	80	16	36	48	11	17	14	9	30	47	67	166	149	178	176	**964**
Legal services	66	81	1	0	0	0	1	0	5	9	4	3	3	2	1	4	**33**
Educational services	67	82	0	4	3	0	2	2	1	4	3	0	5	3	11	6	**44**
Social services	68	83	1	0	0	1	1	1	1	1	2	1	5	0	1	5	**20**
Membership organizations	69	86	0	2	2	0	0	0	0	0	0	0	0	0	0	2	**6**
Electronic equip. and instruments	76	36 & 38	58	101	102	53	88	94	100	91	87	90	135	145	181	226	**1551**
TOTAL			**462**	**709**	**821**	**391**	**627**	**665**	**697**	**877**	**820**	**862**	**1145**	**1443**	**1875**	**2275**	**13669**

Panel B:

Industry Name	**BEA ID**	**2-Digit SIC Correspondence**	**1982**	**1983**	**1984**	**1985**	**1986**	**1987**	**1988**	**1989**	**1990**	**1991**	**1992**	**1993**	**1994**	**1995**	**TOTAL**
Lumber and wood products	14	24	0	1	7	0	3	2	2	3	2	3	3	6	4	2	**38**
Furniture and fixtures	15	25	0	1	1	1	3	0	1	8	5	4	1	2	6	6	**39**
Stone, clay, and glass products	16	32	5	11	11	4	3	3	12	7	5	3	6	9	20	10	**109**

Primary metal industries	17	33	4	14	10	10	12	5	8	11	8	7	7	9	22	10	**137**
Fabricated metal products	18	34	5	15	12	10	8	10	16	18	24	17	17	16	25	21	**214**
Industrial machinery and equipment	19	35	15	24	31	24	32	31	30	42	35	52	44	52	59	57	**528**
Motor vehicles and equipment	21	371	6	3	8	4	9	7	8	4	5	4	7	10	7	13	**95**
Other transportation equipment	22	372-379	4	6	0	2	7	11	8	11	5	5	16	11	12	9	**107**
Miscellaneous manufacturing	24	39	2	4	4	6	4	5	8	6	5	8	6	7	7	25	**97**
Food and kindred products	26	20	6	12	22	13	30	30	37	32	23	18	21	41	35	33	**353**
Textile mill products	28	22	1	3	2	1	3	3	6	6	2	3	8	10	11	6	**65**
Apparel and other textile products	29	23	0	1	2	2	2	1	3	8	3	4	4	10	13	4	**57**
Paper and allied products	30	26	4	6	10	18	9	9	7	7	9	9	12	13	10	22	**145**
Printing and publishing	31	27	9	8	8	13	21	14	32	35	33	28	19	38	25	58	**341**
Chemicals and allied products	32	28	11	33	37	22	40	33	29	47	41	65	57	72	61	73	**621**
Petroleum and coal products	33	29	3	1	6	1	5	1	3	3	1	1	5	7	5	8	**50**
Rubber and misc. plastics products	34	30	6	2	10	7	14	7	8	15	14	13	12	17	17	17	**159**
Leather and leather products	35	31	0	2	3	0	1	2	0	1	0	0	1	1	3	3	**17**
Railroad transportation	38	40	0	3	3	4	1	2	0	2	4	4	6	7	6	5	**47**
Local and interurban passenger transit	39	41	0	0	0	1	0	0	1	0	1	1	1	1	1	3	**10**
Trucking and warehousing	40	42	0	2	5	2	1	1	4	4	2	0	4	10	7	7	**49**
Water transportation	41	44	1	0	2	1	0	1	1	3	1	0	1	5	2	7	**25**
Transportation by air	42	45	1	0	1	4	11	5	4	9	17	36	6	12	9	4	**119**
Pipelines, except natural gas	43	46	0	0	1	1	2	0	2	0	0	3	3	5	1	2	**20**
Transportation services	44	47	1	0	1	0	1	0	2	3	1	2	2	3	4	1	**21**
Communications	45	48	19	35	37	44	47	37	61	83	37	45	46	115	135	189	**930**
Electric, gas, and sanitary services	46	49	0	6	4	6	8	4	3	12	18	17	18	28	28	22	**174**
Wholesale trade	47	50 & 51	14	52	36	18	16	14	25	39	43	39	49	68	73	94	**580**
Retail trade	48	52-59	13	21	34	27	34	30	47	54	36	36	59	83	73	71	**618**
Hotels and other lodging places	58	70	1	3	0	2	6	6	5	4	8	9	1	12	14	13	**84**
Personal services	59	72	0	1	0	0	1	0	1	2	3	1	2	8	1	6	**26**
Business services	60	73&84&87&89	9	27	14	19	32	15	28	76	59	74	97	113	152	139	**854**
Auto repair, services, and parking	61	75	2	0	1	1	4	3	5	1	1	5	5	1	5	6	**40**
Miscellaneous repair services	62	76	1	0	1	2	0	0	1	6	0	0	3	1	2	1	**18**

Motion pictures	63	78	1	3	0	1	2	4	3	9	7	7	13	9	13	12	**84**
Amusement and recreation services	64	79	2	3	2	1	2	2	0	2	1	3	3	7	14	15	**57**
Health services	65	80	4	6	11	5	13	7	4	20	15	25	40	51	56	81	**338**
Legal services	66	81	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**0**
Educational services	67	82	0	0	0	0	2	0	0	1	2	1	0	2	1	0	**9**
Social services	68	83	0	0	0	0	0	1	0	0	0	0	0	0	1	3	**5**
Membership organizations	69	86	1	0	0	0	0	0	0	0	0	0	0	0	1	1	**3**
Electronic equip. and instruments	76	36 & 38	25	41	39	26	52	42	44	76	62	61	95	104	100	112	**879**
TOTAL			**176**	**350**	**376**	**303**	**441**	**348**	**459**	**670**	**538**	**613**	**700**	**976**	**1041**	**1171**	**8162**

Table 3.2. Summary Statistics

The table presents the summary statistics for the variables used in the paper. The sample contains M&As and divestitures from 48 contiguous US states (Alaska, Hawaii, and the District of Columbia are excluded) and 42 manufacturing and services industries that are classified using Bureau of Economic Analysis (BEA) industry segmentation (tobacco industry is excluded). *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The left-hand-side variables are defined at state-pair-level in Panel A, and state-pair-industry-level in Panel B. At the state-pair-level, *TOTAL NUMBER (VALUE) OF M&As* is the total number (value) of transactions in a state-pair *i-j* where the acquirer is headquartered in state *i* and the target is headquartered in state *j* or vice versa. The transaction is considered as *HORIZONTAL* if the acquirer and the target belong to the same BEA industry category and *VERTICAL,* if otherwise. At the state-pair-industry-level, *TOTAL NUMBER (VALUE) OF M&As* is the total number (value) of horizontal state-pair transactions for a particular industry.

	Number of Observations	Mean	Std. Dev.	Median	Min	Max
INTEGRATION	15792	0.0018	0.0108	0.0000	0.0000	0.2050
Panel A:State-pair-level:						
TOTAL NUMBER OF M&As	15792	0.8656	2.6104	0	0	60
HORIZONTAL	15792	0.4882	1.5790	0	0	31
VERTICAL	15792	0.3774	1.2283	0	0	29
TOTAL NUMBER OF DIVESTITURES	15792	0.5168	1.4591	0	0	25
HORIZONTAL	15792	0.3302	0.9934	0	0	15
VERTICAL	15792	0.1867	0.6391	0	0	11
TOTAL VALUE OF M&As (MM $)	15792	51.2556	499.4613	0	0	33919.8880
HORIZONTAL	15792	33.1707	391.3275	0	0	29607.0000
VERTICAL	15792	18.0849	266.3397	0	0	18894.0117
TOTAL VALUE OF DIVESTITURES (MM $)	15792	21.0305	150.3091	0	0	6838.5100
HORIZONTAL	15792	13.5146	112.5299	0	0	6830.5100
VERTICAL	15792	7.5159	90.8505	0	0	5000.0000
Panel B: State-pair-industry-level:						
TOTAL NUMBER OF M&As	663264	0.0116	0.1447	0	0	14
TOTAL NUMBER OF DIVESTITURES	663264	0.0079	0.1023	0	0	8
TOTAL VALUE OF M&As (MM $)	660061	0.7936	59.6507	0	0	29607.0000
TOTAL VALUE OF DIVESTITURES (MM $)	660732	0.3230	16.8336	0	0	6755.5000

Table 3.3. State-pair-level M&As and banking integration

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1). The dependent variable, is either the total number of state-pair M&As in a given year or the total value of state-pair M&As in a given year. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of State-Pair M&As		Value of State-Pair M&As	
	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	10.1497***	34.8380***	-248.2523	4088.0850**
	(3.44)	(3.63)	(0.50)	(2.12)
Number of observations	15792	15792	14076	14076
Number of state-pair clusters	1128	1128	1128	1128
Number of excluded IVs		3		3
State-pair fixed effects	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Regression F-statistic	11.82***	12.13***	0.25	4.12**
Under-identification test (Kleibergen-Paap rk LM statistic)		35.51***		34.00***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		15.53↯5%		14.37↯5%
Relative IV bias compared to OLS				
Over-identification test (Hansen's J-statistic)		3.66		2.57

Table 3.4. State-pair-level horizontal and vertical M&As and banking integration

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1). The dependent variable is either the total number or the total value of state-pair horizontal M&As for the first four columns; and the total number or the total value of state-pair vertical M&As for the last four columns. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of Horizontal State-Pair M&As		Value of Horizontal State-Pair M&As		Number of Vertical State-Pair M&As		Value of Vertical State-Pair M&As	
	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	7.4837 ***	26.9580 ***	82.6565	3607.2125 **	2.6660	7.1193 *	-261.6393	113.6020
	(3.82)	(3.89)	(0.22)	(2.50)	(1.60)	(1.80)	(1.33)	(0.12)
Number of observations	15792	15792	14371	14371	15792	15792	14560	14560
Number of state-pair clusters	1128	1128	1128	1128	1128	1128	1128	1128
Number of excluded IVs		2		4		2		2
State-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic	14.58 ***	13.99 ***	0.05	5.71 **	2.55	2.99 *	1.76	0.01
Under-identification test (Kleibergen-Paap rk LM statistic)		33.31 ***		34.68 ***		33.21 ***		31.13 ***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		21.73		10.62		21.80		19.38
Relative IV bias compared to OLS		↯10%		↯10%		↯10%		↯15%
Over-identification test (Hansen's J-statistic)		0.99		5.55		5.57 **		1.28

Table 3.5. State-pair-level divestitures and banking integration

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1). The dependent variable, is either the total number of state-pair divestitures in a given year or the total value of state-pair divestitures in a given year. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of State-Pair Divestitures		Value of State-Pair Divestitures	
	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	4.8769***	11.3404**	69.4823	753.0878
	(2.66)	(2.31)	(0.40)	(1.31)
Number of observations	15792	15792	14257	14257
Number of state-pair clusters	1128	1128	1128	1128
Number of excluded IVs		3		3
State-pair fixed effects	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Regression F-statistic	7.07***	4.93**	0.16	1.56
Under-identification test (Kleibergen-Paap rk LM statistic)		33.35***		27.87***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		14.54↯10%		11.60↯20%
Relative IV bias compared to OLS				
Over-identification test (Hansen's J-statistic)		3.25		1.05

Table 3.6. State-pair-level horizontal and vertical divestitures and banking integration

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1). The dependent variable is either the total number or the total value of state-pair horizontal divestitures for the first four columns; and the total number or the total value of state-pair vertical divestitures for the last four columns. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of Horizontal State-Pair Divestitures		Value of Horizontal State-Pair Divestitures		Number of Vertical State-Pair Divestitures		Value of Vertical State-Pair Divestitures	
	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	4.9877 ***	11.2300 ***	178.1034	978.4350 *	-0.1108	0.0090	178.1034	-392.4365
	(3.08)	(2.80)	(1.08)	(1.81)	(0.18)	(0.00)	(1.22)	(1.52)
Number of observations	15792	15792	14543	14543	15792	15792	14543	14857
Number of state-pair clusters	1128	1128	1128	1128	1128	1128	1128	1128
Number of excluded IVs		2		2		2		2
State-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic	9.50 ***	7.25 ***	1.17	3.00 *	0.03	0.00	1.17	2.13
Under-identification test (Kleibergen-Paap rk LM statistic)		33.21 ***		28.89 ***		33.31 ***		32.62 ***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		21.80 ↯ 10%		17.49 ↯ 15%		21.73 ↯ 10%		20.15 ↯ 10%
Relative IV bias compared to OLS								
Over-identification test (Hansen's J-statistic)		1.78		0.39		0.73		0.27

Table 3.7. State-pair-level M&As and divestitures for state-pairs located at above-median distance apart

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1) for the sub-sample of state-pairs whose states are apart from each other at a distance above the median value of the main sample. The dependent variable is either the total number or the total value of state-pair M&As for the first four columns; and the total number or the total value of state-pair divestitures for the last four columns. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of State-Pair M&As		Value of State-Pair M&As		Number of State-Pair Divestitures		Value of State-Pair Divestitures	
	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	90.0185 *	662.7056 ***	-285.1188	141421.60 *	34.8322	270.0864 **	-431.5337	10666.18
	(1.73)	(2.99)	(0.08)	(1.68)	(1.63)	(2.22)	(0.46)	(0.78)
Number of observations	7896	7896	7193	7193	7896	7896	7339	7339
Number of state-pair clusters	564	564	564	564	564	564	564	564
Number of excluded IVs		3		3		3		3
State-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic		7.57 ***		2.37		4.17 **		0.51
Under-identification test (Kleibergen-Paap rk LM statistic)		6.73 *		7.43 *		6.73 *		8.20 **
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		2.02 ↯>25%		2.22 ↯>25%		2.02 ↯>25%		2.30 ↯>25%
Relative IV bias compared to OLS								
Over-identification test (Hansen's J-statistic)		0.62		3.43		1.94		3.55

Table 3.8. State-pair-level M&As and divestitures among public firms

This table presents *Within* (panel fixed effects) and GMM-2S-IV regression results of Eq. (1) for the sub-sample of deals in which acquirer and target firms are both publicly traded. The dependent variable is either the total number or the total value of state-pair M&As for the first four columns; and the total number or the total value of state-pair divestitures for the last four columns. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair fixed effects, time-varying state fixed effects for states i and *j* that form the state-pair *i-j* and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. Identification tests are reported for IV-GMM2S-FE regressions: the null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of State-Pair M&As		Value of State-Pair M&As		Number of State-Pair Divestitures		Value of State-Pair Divestitures	
	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV	Within	GMM-2S IV
INTEGRATION	0.9526	4.3159 **	-214.2214	3208.7455 *	0.0908	0.5927 **	-11.4652	-107.3902
	(1.19)	(2.02)	(0.63)	(1.87)	(0.59)	(2.11)	(0.23)	(0.57)
Number of observations	15792	15792	15630	15630	15792	15792	15771	15771
Number of state-pair clusters	1128	1128	1128	1128	1128	1128	1128	1128
Number of excluded IVs		2		2		3		3
State-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic		3.76 **		3.22 *		4.11 **		0.30
Under-identification test (Kleibergen-Paap rk LM statistic)		33.31 ***		33.06 ***		35.51 ***		36.04 ***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)		21.73 ↯10%		21.53 ↯10%		15.53 ↯5%		15.76 ↯5%
Relative IV bias compared to OLS								
Over-identification test (Hansen's J-statistic)		0.09		2.83		2.76		1.54

Table 3.9. State-pair-industry-level M&As and divestitures

This table presents GMM-2S-IV regression results of Eq. (2). The dependent variables for the first two columns are either the total number or the total value of horizontal state-pair M&As in a given industry. The last two columns display the results for divestitures. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair-industry fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j*, time-varying industry and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. The null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-industry-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

Dependent variable:	Number of horizontal M&As	Value of horizontal M&As	Number of horizontal divestitures	Value of horizontal divestitures
INTEGRATION	0.5963***	64.0566**	0.2615***	18.5181*
	(4.49)	(2.20)	(3.05)	(1.79)
Number of observations	663264	660061	663264	660732
Number of state-pair clusters	47376	47376	47376	47376
Number of excluded IVs	3	3		
State-pair-industry fixed effects	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Regression F-statistic	20.14***	4.84**	9.25***	3.19*
Under-identification test (Kleibergen-Paap rk LM statistic)	1400.80***	1397.99***	1400.80***	1396.85***
Weak-identification test (Kleibergen-Paap rk Wald F statistic)	660.07↯ 5%	656.92↯ 5%	660.07↯ 5%	656.59↯ 5%
Over-identification test (Hansen's J-statistic)	2.31	2.56	2.38	0.21

Table 3.10. State-pair-industry-level M&As and divestitures for high/low external finance dependent industries

This table presents GMM-2S-IV regression results of Eq. (2). The dependent variable is either the total number or the total value of horizontal state-pair M&As/divestitures in a given industry. The first (last) four columns displays the results for industries that are above (below) median among 41 manufacturing and services industry on external finance dependency (EFD) that is computed following Rajan and Zingales (1998). *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair-industry fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j,* time-varying industry and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. The null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-industry-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

	INDUSTRIES W/ HIGH EFD				INDUSTRIES W/ LOW EFD			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Dependent variable:	Number of hor. M&As	Value of hor. M&As	Number of hor. divestitures	Value of hor. divestitures	Number of hor. M&As	Value of hor. M&As	Number of hor. divestitures	Value of hor. divestitures
INTEGRATION	1.1318***	224.0781***	0.5394***	41.6249**	0.1370*	-4.5670	-0.0046	-3.0461
	(3.88)	(2.78)	(3.50)	(2.34)	(1.89)	(0.20)	(0.06)	(0.30)
Number of observations	331632	328988	331632	329654	315840	315281	315840	315286
Number of industry clusters	23688	23688	23688	23688	22560	22560	22560	22560
Number of excluded IVs	2	2	3	3	2	2		
State-pair-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic	15.00***	7.71***	12.20***	5.44**	3.54*	0.04	0.00	0.09
Under-identification test	676.56***	673.53***	700.40***	696.79***	666.18***	666.57***	667.05***	666.79***
Weak-identification test	437.04 ↯ 5%	433.27 ↯ 5%	329.43 ↯ 5%	326.42 ↯ 5%	468.85 ↯ 5%	468.63 ↯ 5%	313.69 ↯ 5%	313.25 ↯ 5%
Over-identification test	1.65	0.66	0.47	1.52	0.08	0.00	6.10**	4.34

Table 3.11. State-pair-industry-level M&As and divestitures for industries in which states are specialized/not specialized

This table presents GMM-2S-IV regression results of Eq. (2). The dependent variable for the first four columns are the total number or the total value of horizontal state-pair M&As/divestitures in a given industry in which at least one of the states in the state-pair is over-specialized. The last four columns display the results for state-pairs in which both of the states are under-specialized. *INTEGRATION* is the banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states within the pair, divided by the sum of the banking assets of the two states. The equation includes state-pair-industry fixed effects, time-varying state fixed effects for states *i* and *j* that form the state-pair *i-j*, time-varying industry and year fixed effects. IVs involve a combination of the average number of years since the state-pair *i-j* have effectively deregulated interstate bank-entry towards each other, as well as its square or square root, and an indicator variable that equals one if state-pair *i-j* has deregulated entry towards each other and zero otherwise. The null hypothesis for the under-identification test is that the matrix of reduced-form coefficients has rank k-1; the null hypothesis for the weak identification test is that the equation is weakly identified; the null hypothesis for the over-identification test is that all instruments are valid. Standard errors are robust and clustered at the state-pair-industry-level. *t*-Stats are reported within parentheses below coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1%-level, respectively.

	AT LEAST ONE STATE IS OVER-SPECIALIZED				BOTH STATES ARE UNDER-SPECIALIZED			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Number of hor. M&As	Value of hor. M&As	Number of hor. divestitures	Value of hor. divestitures	Number of hor. M&As	Value of hor. M&As	Number of hor. divestitures	Value of hor. divestitures
INTEGRATION	1.0937***	182.6564**	0.4836***	15.8875	0.2109**	5.2025	0.0873	8.7571
	(4.33)	(2.47)	(3.49)	(1.04)	(2.13)	(0.46)	(1.14)	(0.99)
Number of observations	454902	452305	454902	452808	208362	207756	208362	207924
Number of industry clusters	32493	32493	32493	32493	14883	14883	14883	14883
Number of excluded IVs	2	2	2	2	2	2	2	2
State-pair-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression F-statistic	18.69***	6.10***	12.12***	1.08	4.50**	0.21	1.29	0.97
Under-identification test	728.17***	726.48***	745.87***	742.81***	581.79***	580.08***	646.62***	645.84***
Weak-identification test	463.85↯5%	460.98↯5%	520.12↯5%	515.59↯5%	413.93↯5%	412.53↯5%	475.57↯5%	474.53↯5%
Over-identification test	0.70	0.02	0.58	1.23	1.34	1.93	1.49	0.18

Figure 3.1. State-pair M&As and divestitures and banking integration

This figure plots state-pair banking integration and number of M&As and divestitures over the period of analysis. The blue dashed line is the average state-pair banking integration measured as the sum of the cross-state banking assets owned by the banks of the two states, divided by the sum of the banking assets of the two states within the pair. The green line shows the average number of transactions for state-pairs in which banking integration has not yet started, while the red line shows the average number of transactions for state-pairs in which banking sectors are integrated as of the end of the year indicated in the y-axis.

Conclusion

This dissertation is structured around two different topics that share a common underlying theme: The role of information in bank lending.

In the first chapter, the empirical approach relies on a key premise that a relationship is established between a bank and a borrower during the screening and monitoring stages of the lending process. As the bank collect information about the borrower, switching lenders become costly for the borrower-firm. This assumption allows to define a firm-level credit supply measure and to identify the effect of credit supply on firms' political activism. This chapter provides novel empirical evidence that firms "actively" increase their political connections as the financial health of their relationship lenders worsen.

In the second chapter, the focus is on industry-level information that banks collect during the lending process. More specifically, it tests for a channel that works through commercial banks' exposure to more prevalent industries in their "home" state. The findings support the conjecture that financial integration with out-of-state banks that are more knowledgeable about an industry leads to faster growth in that particular sector.

The third chapter investigates the effect of banking integration on the market for corporate control. The findings suggest that there are more M&A and divestiture activities between state-pairs with higher banking integration compared to those with no or little integration. The results are consistent with information flows among (i) the banks and their borrower-clients (ii) affiliates of the multi-bank holding companies, which facilitate the matching process of acquirer and target firms located in the banks' home and newly-entered markets.

This dissertation is a step towards better understanding the macro- and micro- level implications of the flow of information in the credit markets. The mechanisms highlighted in this paper have important policy implications and may be insightful in the context of other finance topics regarding the flow of information.

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ÉCOLE DOCTORALE
Sciences de l'homme et de la société (SHS)

Titre: Essais dans le Secteur Bancaire et de la Finance d'Entreprise

Mots clés: relations politiques, la crise financière, l'intégration bancaire, structure de l'industrie

Résumé: Cette thèse se compose de trois chapitres distincts. Le premier chapitre examine le lien de causalité entre l'offre de prêts syndiqués par des banques et les contributions de campagnes électorales par des entreprises non-financières aux États-Unis au cours de la crise financière de 2007–2008. Les résultats indiquent qu'une diminution de 10% de l'offre de prêt à une entreprise donnée par ses prêteurs pré-crise pendant la première période de crise entraîne une augmentation de 9% des contributions de campagne par cette entreprise en 2008. De plus, le niveau de contributions de campagne par des entreprises dans le passé est positivement associé à des conditions de prêt favorables dans l'avenir. Les résultats appuient l'idée que les contributions de campagne sont un investissement dans le capital politique plutôt qu'une simple forme de bien de consommation. Le deuxième chapitre identifie l'effet d'exposition industrielle de banques avant leur entrée sur le marché sur la croissance de production des secteurs de fabrication. Les résultats indiquent que plus grande est la différence de spécialisation dans un secteur entre deux états, plus grand est l'impact d'intégration bancaire sur la croissance de ce secteur dans l'état qui est moins spécialisé. Le dernier chapitre examine si l'intégration bancaire dans plusieurs régions a un impact sur le marché de contrôle des entreprises entre elles. Les résultats indiquent qu'il y a plus de fusions, acquisitions et cessions dans les paires d'états dont les systèmes bancaires ont connu une plus grande intégration, par rapport à des paires d'états sans une telle intégration. Les résultats dans les deux derniers chapitres indiquent un canal bancaire qui façonne le paysage industriel d'états.

Title: Essays on Banking and Corporate Finance

Keywords: political connections, financial crisis, banking integration, industry structure

Abstract: This dissertation is made of three distinct chapters. The first chapter examines the causal link between banks' syndicated loan supply and non-financial firms' campaign contributions for US elections during the 2007-2008 financial crisis. The results indicate that a 10% decrease in loan supply of a given firm by its pre-crisis relationship lenders during the early crisis period leads to a 9% increase in firm's campaign contributions in 2008. Further, firms' level of past campaign contributions is positively associated with favorable loan terms for the future. The findings lend support to the idea that campaign contributions are an investment in political capital rather than merely a form of consumption good. The second chapter identifies the effect of banks' industry exposures prior to market-entry on the output growth of manufacturing sectors through US bank-entry deregulations. The findings indicate that the larger the discrepancy in specialization in an industry between a state-pair, the higher the impact of banking integration on the growth of that sector in the state that is less-specialized. The last chapter examines whether banking integration across regions has any impact on the market for corporate control between them. The results show that there are more M&As and divestitures across state-pairs whose banking systems have experienced a higher integration, compared to state-pairs with no such integration. The findings in the last two chapters indicate a banking channel that shapes the states' industrial landscape.