Essays in Financial Economics

By

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Submitted to the Alfred P. Sloan School of Management
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

June 2016

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Abstract

This thesis consists of three empirical essays in financial economics that explore the role financial regulation can play in firm, household, and investor decision making.

In the first chapter for households with homes worth less than the mortgage I test the effect of "household debt overhang" on their labor supply decisions. I utilize a new transaction-level dataset with comprehensive information on assets, liabilities, and deposits for all customers of a major U.S. financial institution from 2010-2014. I then exploit plausibly exogenous variation in the timing of home purchases among households in the same region and time as an instrument for the probability of negative home equity and find that negative equity causes a 2%-6% reduction in household labor supply. These results are robust to the inclusion of time-varying national cohort fixed effects as well as using a life-event driven proxy for the timing of home purchase based on the date of college attendance. Income-contingent loss mitigation creates implicit marginal tax rates that provide a plausible channel by which household debt overhang acts. Consistent with this explanation I find that the labor supply decline is larger in regions where mortgage modifications are more prevalent, even if foreclosures occur less frequently. Taken together these results provide evidence that the moral hazard problem caused by mortgage debt overhang can exacerbate employment declines and highlights the potential unintended consequences of mortgage assistance programs.

In the second chapter I investigate whether restrictions on bank speculation can be costly for non-financial firms by examining the unexpected inception of federal rating-contingent investment restrictions in 1936 preventing banks from purchasing speculative grade securities. Immediately following the ruling I find a persistent 3-5% equity value decline for firms requiring speculative financing, concentrated in industries reliant on external financing, but no change in bond yields. Rather than face increases in default risk or direct interest costs these firms reduce debt issuances to improve ratings, leading to reduced investment and asset growth in the years following the ruling.

In the third chapter (co-authored with Eric Hughson and Marc Weidenmier) we explore the role clearinghouses play in global financial stability. Empirical identification of the effect of centralized clearing on counterparty risk is challenging because of the co-incidence of macro-economic turbulence and the introduction of clearinghouses. We overcome these concerns by examining a novel historical experiment, the establishment of a clearinghouse on the New York Stock Exchange (NYSE) in 1892. During this period the largest NYSE stocks were also listed on the Consolidated Stock Exchange (CSE), which already had a clearinghouse. Using identical securities on the CSE as a control, we find that the introduction of clearing reduced annualized volatility of NYSE returns by 90-173bps and increased asset values. Prior to clearing, shocks to overnight lending rates reduced the value of stocks on the NYSE, relative to identical stocks on the CSE, but this was no longer true after the establishment of clearing. We also show that at least ½ of the average reduction in counterparty risk on the NYSE is driven by a reduction in contagion risk – the risk of a cascade of broker defaults. Our results indicate that clearing can cause a significant improvement in market stability and value through a reduction in network contagion and counterparty risk.

Thesis Supervisor: Antoinette Schoar
Title: Michael Koerner '49 Professor of Entrepreneurial Finance

Acknowledgments

I am appreciative of everyone who helped me to receive my PhD. I am truly thankful to my thesis advisor Antoinette Schoar for all her support and advice. She helped me develop as a researcher and who, without, this thesis certainly wouldn't have been possible. I am incredibly grateful to my committee members, Jonathan Parker, Deborah Lucas, and Nittai Bergman for their time, feedback, and guidance as I developed the papers that became the chapters of this thesis. I would also like to thank Paul Asquith, Jean-Noel Barrot, Hui Chen, Randolph Cohen, Xavier Girourd, Andrew Lo, Erik Loualiche, Andrey Malenko, Jun Pan, Adrien Verdelhan, and Haoxiang Zhu for their support and so many helpful discussions throughout my time at MIT. I also wanted to give a special thanks to my co-authors and mentors Eric Hughson and Marc Weidenmier. It has been a pleasure learning from you and working with you and I look forward to continuing that in the future.

I was lucky enough to have brilliant and supportive group of colleagues at MIT who have helped me in too many ways to enumerate throughout this process including Winston Dou, Stephen Murphy, Hong Ru, Daan Struyven, Richard Thakor, and Nils Wernerfelt. Finally I also wanted to thank my family and my fiancé Ali. I am indebted to them for their tremendous support, love, and unwavering encouragement.

Contents

Chapter 1
Household Debt Overhang and Labor Supply*

1 Introduction

Following the historic decline in house prices during the recent financial crisis more than 15 million U.S. mortgages, or approximately 1/3rd of mortgaged properties, had negative home equity[1]. At the same time, labor markets experienced a severe and prolonged deterioration, with employment still below pre-recession levels for years after the crisis[2]. A number of recent theoretical papers (Mulligan (2008, 2009, 2010), Herkenhoff and Ohanian (2011), Donaldson et al. (2014)) have shown that this co-incident movement in housing and labor markets may have been partially driven by perverse labor supply incentives caused by the unprecedented decline in household home equity. They show that house price declines can cause a "household debt overhang" problem, similar to the problem faced by highly levered firms, but where negative home equity exacerbates employment declines. In particular, if household income is transferred to mortgage lenders via increased liability repayment, then this transfer would incentivize households to reduce their labor supply.

In this paper I empirically test the effect of mortgage debt overhang on labor decisions and find that negative home equity causes a substantial reduction in household labor supply. In particular, instrumented negative home equity is associated with a 2.3%-6.3% reduction in household income. This reduction in labor supply appears to be driven by large changes in household labor decisions, such as reductions in employment, rather than effort supplied at existing jobs. Income-contingent loss mitigation strategies by lenders, such as mortgage modifications, create implicit marginal tax rates that provide a plausible channel by which household debt overhang acts. Consistent with this explanation, I find that the

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* I am grateful to my advisors Antoinette Schoar, Jonathan Parker, Deborah Lucas, and Nittai Bergman for their feedback and guidance throughout this project. I would also like to thank Andrew Lo, Adrien Verdelhan, Xavier Giroud, Daan Struyven, Daniel Green, Stephen Murphy, Nils Wernerfelt, and MIT Sloan faculty seminar participants for helpful comments. A special thanks to the major U.S. financial institution that provided the data for this project. I was an unpaid intern at the institution during the time of this research, but any conclusions or errors herein are mine and do not represent the views of the data provider and any employees of that firm.

[1] According to First American CoreLogic as of June 30, 2009. In fact in some states more than half all mortgages were underwater.

[2] Bureau of Labor Statistics (BLS)

reduction in labor supply for households with negative home equity is amplified in regions where mortgages are modified at a higher rate, even controlling for delinquency and foreclosure rates in those regions. Despite the potential economic importance of such a mechanism, to the best of my knowledge, this is the first paper to establish the role mortgage debt overhang played in reducing household labor supply following the crisis.

Empirical identification of the effect of household debt overhang on labor supply faces a number of challenges which I address in this paper. First of all, few datasets have comprehensive household-level panel information on income, assets and liabilities. The few databases that do, such as the American Housing Survey (AHS), tend to be surveys that suffer from self-reporting biases and small sample sizes that confound clean identification[3]. Even with appropriate data, simple regressions of labor income on negative home equity are unlikely to provide causal interpretation. A number of omitted variables drive both house prices and labor income (ex. local labor demand shocks) and reverse causality could be problematic since wealthier households are likely to invest more in home improvements.

In this paper I overcome these challenges by utilizing a new transaction-level dataset with comprehensive information on assets, liabilities, and deposits for all customers of a major U.S. financial institution from 2010-2014, referred to hereafter as *MyBank*, and an empirical methodology based on variation in the timing of home purchases. The transaction-level deposit information allows me to generate accurate high frequency measures of household income, while the data on assets and liabilities lets me determine which households have negative home equity. Since I observe actual deposits rather than reported values any estimated effects represent actual changes in deposit behavior rather than changes in household reporting in response to eligibility criteria[4]. I then use information on the timing of home purchase, relative to households in the same region, as an instrumental variable for the probability a household has negative home equity. In this empirical strategy households are exposed to identical time-varying local house price shocks, but differ in their home equity based on when they happened to purchase their home.

Since variation in the timing of home purchases is not randomly assigned I address concerns that omitted variables could be related to the timing of purchase and future income in a way that violates the exclusion restriction of the instrumental variables methodology. First I show that for low levels of expected loan-to-value, house price shocks have little effect, but as the probability of having negative

[3] For example, Cunningham and Reed (2012) use AHS data, but only have 652 household-year observations over the course of 9 years with negative equity, which is a very limited sample for something as noisy as self-reported household equity and labor income.

[4] Chetty et al. (2013) have shown that in the context of household response to the EITC individuals manipulate self-employment reported income.

equity rises, labor supply falls, consistent with an explanation driven by negative home equity. I also show that the results are robust to including household fixed effects, controlling for national cohort trends, and including a number of time-varying non-parametric household-level controls for household characteristics that could be related to local demand shock sensitivity. There could still be a concern that even within a region the timing of purchase could be related to future house price movements and income shocks in an unobservable way[5]. To reduce even that concern I instrument for negative equity using the age of student loans as a proxy for life-event driven home purchases and find that results are robust to this specification. This alleviates concerns that omitted variables such as industry choice drive both local demand sensitivity and the timing of home purchases.

One final concern I address is that households with *MyBank* mortgages and negative equity could be systematically hiding income from the institution they owe money. Since I measure only deposit inflows at *MyBank,* households who also have mortgages at *MyBank* could be closing accounts or reducing payroll inflows at that institution in order to appear less able to pay and receive more assistance. To partially alleviate this concern throughout my analysis I use multiple restrictions to be sure households in the panel have active retail accounts, taking advantage of the inflow and level information I have for all retail accounts at *MyBank*. Results are robust to all choices of filter and measures of income. I also rerun the analysis for households with a *MyBank* retail and credit card account, but have a mortgage where *MyBank* does not own or service the mortgage. In this case the household has no incentive to hide deposits and I find that negative equity still reduces income. Overall these results are consistent with income shrouding playing little role in the observed decline in deposits, so that results represent actual declines in overall household deposits. This may not be that surprising since virtually all income-contingent loss mitigation programs require documentation of income, which would include income deposited at any institution.

These results complement a recent body of work that investigates how households respond to excess liabilities. A number of recent papers have looked at how indebtedness affects entrepreneurial activity (Adelino, Schoar, and Severino 2015), employment opportunities among impoverished households (Bos et al. 2015)[6], and labor income among bankrupt households (Dobbie and Song 2015b). Melzer (2015) has also shown that households with negative home equity reduce investments in their

[5] For example, if real estate brokers were more likely to purchase homes in Nevada during the peak of the crisis than they were in say Nashville, they would experience larger house price declines and their labor income could be more exposed to local housing demand shocks.

[6] The paper focuses on sample of households who were delinquent on a loan from a pawnshop within the last two years. Not surprisingly this sample population has very low income. Only 43% are employed and only 6% are homeowners. Credit constraints that prevent this population from finding employment, such as being unable to use a credit card to buy as suit, seem unlikely to extend to the average U.S. homeowner.

house, since they anticipate no longer being residual claimants. Mayer et al. (2014) found that households were aware of the announcement of a large scale mortgage modification program by Countrywide and responded by falling delinquent, despite the ability to pay. Taken together these results suggest that a significant number of households are aware of their home equity and loss mitigation programs, and are willing to respond strategically via their home investment and mortgage payment decisions[7]. This paper contributes to this literature by showing that households also reduce their labor supply in response to the incentives provided by negative home equity and mortgage assistance programs.

This paper also fits within a broader literature analyzing the relationships between household liabilities, assets, consumption, and labor decisions. This includes a broad and growing literature trying to understand how negative home equity interacts with labor mobility in the U.S. and abroad (Fredrick et al. (2014), Cohen-Cole et al. (2015), Demyanyk et al. (2013), Donovan et al. (2011), Goetz (2013), Modestino and Dennett (2013), Mumford and Schultz (2014), Schulhofer-Wohl (2012), Struyven (2014))[8], the effect of contract modifications including large scale loan modifications programs (Agarwal et al. (2010), Agarwal et al. (2012), Calomiris et al. (2011), Chang and Weizheng (2013), Collins and Urban (2015), Dobbie and Song (2015a), Dobbie and Song (2015b), Goodman et al. (2011), Goodman et al. (2012), Goodman and Woluchem (2014), Lucas et al. (2011), Mayer et al. (2014), McCoy (2013), Mulligan (2009), Schmeiser and Gross (2014), Gerardi and Li (2010)), and how liabilities alter household consumption and investment decisions (Baker (2015), Bhutta et al. (2010), Adelino et al. (2015), Cunningham and Reed (2013), Foote et al. (2008), Fuster and Willen (2013), Gerardi et al. (2013), Guiso et al. (2013), Melzer (2015)).

The remainder of the paper is organized as follows: Section 2 begins with a discussion of household debt overhang and the relationship with mortgage modification programs. Section 3 precedes with a description of the data. In Section 4, I present the empirical methodology. I discuss the empirical results in Section 5. Section 6 concludes the paper.

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[7] Even though the authors are unable to investigate the effects on income of the announcement of the countrywide program it is worth noting that settlement had debt-to-income targets of 34% for at least 5 years based on the previous 1 year of income, which like HAMP imply marginal tax rates in excess of 100%. A household willing to stop paying their mortgage and forgo an employment opportunity would be eligible for more than 100% of the forgone income in reduced monthly payments once they received a modification.

[8] In these settings households are financially constrained by negative equity which prevents them from moving, also known as "housing lock". Due to the effectively non-recourse nature of mortgages in the U.S. the effect of housing lock on mobility is unclear and empirical evidence is divided. Modestino and Dennett (2013) also point out that while non-pecuniary costs of immobility could be large, very few households in a given year have to move for employment, so the effect on aggregate labor supply is unlikely to be much larger than tenths of a percent, and certainly not the 2.3%-6.3% observed in this paper.

2 Debt Overhang and Mortgage Modifications

For highly levered firms a reduction in firm wealth reduces the marginal incentives for investment in positive net present value projects because the benefits accrue disproportionally to existing debt holders (Myers 1977). Highly levered households face a similar problem when deciding to invest in the effort needed to earn labor income. If a portion of any marginal income earned by an indebted household is transferred to a lender via increased liability repayment, then this transfer to debt holders acts just like an implicit tax that incentivizes households to reduce their labor supply (Mulligan (2008, 2009, 2010), Herkenhoff and Ohanian (2011), Donaldson et al. (2014)).

While in practice income-contingent repayment for foreclosed properties in deficiency judgments are rare (Ghent and Kudlyak 2011), income contingent mortgage modifications were ubiquitous following the crisis (Goodman et al. 2011) and likely provide a major channel through which household debt overhang problems occur. In response to the substantial rise in mortgage delinquencies during the crisis, lenders engaged in large scale mortgage modification programs to help distressed borrowers. In fact from January 2008-May 2011 51% of all non-performing or re-performing subprime mortgages received a mortgage modification (Goodman et al. 2011)[9]. While these modifications may have been optimal collection strategies by lenders they may have also provided perverse labor supply incentives. Mulligan (2009) has shown that in theory and in practice lenders are more likely to engage in loss-mitigation actions for delinquent borrowers if they demonstrate a reduced ability to pay their liabilities. These income-contingent loss mitigations result in implicit marginal tax rates with strong moral hazard incentives for households to reduce labor supply. In the case of the majority of public mortgage modification programs debt-to-income targets create implied marginal tax rates in excess of 100% for households with negative equity, which as noted by Mulligan (2009) "is significant even from a macroeconomic perspective" and likely to "produce distortions that are large enough to be visible in the national employment data".

These income-contingent loss mitigations mean that for many households with negative equity the majority of benefits from additional time and effort invested in employment income accrue to the debt holders rather than the household. For example, if an average negative home equity household with \$4,000/month in gross income and \$1,500 in monthly mortgage payments was seeking a mortgage modification via the Home Affordable Modification Program (HAMP) and worked to earn an extra \$500/month in income not only would all of the additional \$500/month in income accrue to the lender,

[9] For Prime, Alt A, and Option ARM, the modification rates were 23%, 31%, and 29% respectively.

the household would actually end up losing at least $3,271 over the next 5 years despite the additional time/effort[10]. Just like in the classic corporate debt overhang problem faced by firms "the gain in the market value of debt acts like a tax on new investment [and] if that tax is high enough, managers may try to shrink the firm" (Myers 2001), where in the case of this household debt overhang problem the borrower reduces the "firm" by reducing their labor supply. This could mean that a fall in housing wealth, which via a wealth effect would normally suggest a rise (weakly) in household labor supply, could actually cause a reduction in labor supply via a substitution effect coming from the implicit marginal tax of the income-contingent loss mitigation by the lender.

3 Data Description and Validation

The majority of my data comes from a major U.S. financial institution but I also merge in zip-code level income from the Internal Revenue Service (IRS) to validate my income measures and state-level judicial foreclosure law information.

3.1 *MyBank* Data

The data provider for this project is a major U.S. financial institution, who I refer to as *MyBank*, with transaction-level client account information on more than 1/4th of all U.S. households over the 5 years from 2010-2014[11]. For the purposes of this project I focus on households with sufficient *MyBank* relationships to estimate income and mortgage information and analyze income decisions at a monthly household level. Income is estimated using retail account deposit information and mortgage information is either derived from credit bureau data (only available for households w/ *MyBank* credit card accounts) or *MyBank* mortgage account information. In appendix A I detail how household information from multiple *MyBank* accounts are combined at a monthly frequency. Information on the change in sample size because of data requirements is shown in table A1.

3.1.1 Mortgage Accounts Data

For each mortgage account I have detailed information on the mortgage type (ex. fixed rate 30 year), characteristics at origination including the date, reported income, credit score, interest rate, appraised

[10] Calculations based on checkmynpv.com.
[11] According to census.gov from 2009-2013 there were about 116 million U.S. households and *MyBank* has client accounts covering more than 31 million households (see table A1 for details), which would be about 27% of all U.S. households. The coverage is lower when looking at individuals, which is likely because dependents are unlikely to have separate *MyBank* accounts (ex. children) and some households with multiple adults still may choose to list only one person in the account information.

loan-to-value, and ongoing monthly mortgage performance, characteristics, and actions, including delinquency status, current loan-to-value updated using internal LPS MSA-level HPI data, any loss mitigation actions taken, such as mortgage modifications, and current interest rates. Perhaps not surprisingly given the substantial coverage of this data provider, in figure 2 I show that the time series of delinquency rates for *MyBank* mortgage data matches closely with the levels and trends seen in national Federal Reserve economic mortgage data over the past 5 years.

3.1.2 Credit Card Accounts Data

By a substantial margin the largest population of households with a *MyBank* relationship are credit card customers. This should be expected since households very often only have one mortgage lender, but will have multiple credit cards. For each credit card account and month *MyBank* pulls credit bureau data on the associated customer liabilities. For the purposes of this paper this monthly frequency credit bureau data is the only information used from the credit card accounts. The credit bureau data includes comprehensive data on all customer liabilities across all lenders including mortgages, auto-loans, student loans, home equity lines of credit, credit cards, and installment credit as well as monthly updated credit scores. For each credit category the dataset includes information on the term, balance, monthly payments, and initial balance.

3.1.3 Retail Accounts Data

Retail accounts include any checking or savings accounts. The raw data includes every single transaction into these accounts (inflows and outflows) but to protect privacy include only the day a transaction occurred, the amount of the transaction, and very general transaction category types (ex. "ACH direct deposit"). The dataset includes billions of transactions over the period 2010-2014, but since my goal is to measure income I focus on the subset of transactions labeled as deposits, which include direct deposits, such as "ACH direct deposit", physical deposits including at the teller and ATM, and other deposit types including mobile RDC deposits. Since some of these accounts are not being used to deposit the majority of income I further restrict my analysis to households with active accounts[12] who appear to use their *MyBank* retail accounts to deposit the majority of their income[13].

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[12] A household is defined to have "active" accounts if across all accounts in a given month they deposit at least $100 or have $200 in financial assets.

[13] To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k.

To explore the validity of using deposits as an income measure I then focus on 376 million direct deposit transactions and utilize the fact that direct deposit paychecks tend to fall on a set of possible regular schedules. This allows me to explore to what extent my deposits are consistent with what would be expected and create a "jobs algorithm" to try and assign paychecks to specifics regularly paying jobs. I find that consistent with bureau of labor surveys my paychecks peak on the 1st, 3rd, 15th and last day of the month[14]. Direct deposits tend to pay on Fridays, as expected, while physical deposits tend to post on the following Mondays. After running the "jobs algorithm" 90% of account-month observations have at least one job associated with them, where the assigned jobs paychecks can explain 84% of all observed deposits. In fact according to the Social Security Administration[15] the average monthly benefits for a beneficiary of social security is $1,223.45/month, which matches favorably with the $1,267.5/month I see per social security recipient in my sample based on the algorithm. For more details on the algorithm see appendix B.

Given the importance of this income measure for my analysis I also confirm the validity of my income measure by comparing the average annual income based on my deposit data at a zip code-level with those reported by the IRS Statistics of Income (SOI) over the period 2010-2013. In figure 1A you can see a very strong correlation between these measures of income. Regardless of the type of income measure used and the subsample explored I find that zip code level correlations between my measure and the IRS SOI are very high and range from 0.736 all the way up to 0.911. The fact that the relationship is so strong between these two measures and one measure does not appear to be systematically higher suggests that for the subset of households analyzed deposits represent an effective measure of household income.

3.1.3 Merging *MyBank* Data

For the majority of my analysis I focus on households with retail deposits that let me measure income, and mortgages at *MyBank* that let me see their level of home equity or about 200k households in the final sample representing approximately 7.8 million household-month observations. For most of my analysis I focus on households with income at origination, loan origination date, and additional information which restricts that to approximately 5.4 million household-month observations. I also consider households with *MyBank* retail and credit card accounts and mortgages with any lender as robustness check, which

[14] The peak on the 3rd is due to social security payments. For more details see Appendix B as well as Stephens (2003).

[15] www.ssa.gov

increases the sample to about 20.1 million household-month observations. For more details on the data merging see appendix A.

I analyze a broad range of characteristics for each sub-sample of *MyBank* in table 1. From the tables we can see that the median household income for households with mortgages is about $5-6k/month and as expected the majority of household liabilities are mortgage related. The median level of income, non-housing financial assets, mortgage leverage, and mortgage interest rates are similar to self-reported information collected by the Survey of Consumer Finance (SCF) for households with at least $1,000 in active mortgage balance in 2010 consistent with the representative nature of the *MyBank* national coverage and lends credibility to the external validity of the conclusions of this paper. For more details on this comparison see table A2 in appendix A.

The *MyBank* mortgage data includes information on reported income at origination which provides a nice opportunity to test the validity of the cross-lines of business data matches as well as providing another check of the quality of my deposit based income measure. In figure 1B I plot the cumulative distribution function of income at origination and income based on deposits for a match sample of individual households who originated a mortgage in the same year when sufficient deposit information is available to estimate income. These distributions appear remarkably similar and the individual income correlations range from 0.378 to 0.449 depending on the measure of deposit in come used, all of which lend substantial credibility to the internal matches across *MyBank* lines of business as well as validating my income measure across the income distribution.

3.2 IRS Zip Code Level Income Data

For the purposes of income validation I utilize publicly available zip-code level income data from the IRS (Internal Revenue Service) Statistics of Income for 2010-2013. This data is based on administrative records of individual income tax returns (Forms 1040) from the IRS Individual Master File (IMF) system. More details about IRS SOI income data are available online at www.irs.gov.

3.3 State-Level Judicial Foreclosure Data

As noted by Mian et al. (2015) states that don't require judicial procedures for mortgage lenders to foreclose on delinquent borrowers are twice as likely to foreclose. The increased ease and likelihood of foreclosure reduces the likelihood that non-performing mortgages will receive a modification. For example, the documentation for the net present value tests for mortgage modifications under HAMP includes "state-level foreclosure timelines" and "state-level average foreclosure costs" as major determinants of whether or not a mortgage modification should be undertaken. To explore this source of variation I merge in state-level judicial foreclosure requirements based on RealtyTrac's website, just as was carried out in Mian et al. (2015).

3 Empirical Methodology

To understand the effect of negative household equity on labor supply I run an instrumental variables regression using variation in the likelihood of negative equity based on the timing of home purchase relative to households living in the same region at the same time. To build intuition for the instrumental variables approach though I start by running the following simple panel regression

$$y_{icrt} = \alpha_i + \gamma_{rt} + \sum_k \delta_{1k} \cdot 1_{\{l_k \leq LTV_{it} \leq \mathrm{h_k}\}} + X'_{it}\beta + \epsilon_{icrt} \quad (1)$$

where for household i in month t in region r that originated their mortgage on date c, this regresses household income, y_{icrt}, on a dummy variables which equals 1 only if the households loan-to-value ratio, LTV_{it} is greater than l_k and less than $\mathrm{h_k}$ for k loan-to-value buckets, region x time fixed effects, household-level fixed effects, and a number of time-varying household level controls, X'_{it}. The problem with a naïve regression of income on home equity is that reverse causality or omitted variables are not only possible, but are likely to prevent confidence in any causal interpretation of the effect of negative equity on labor supply. For example, time varying local demand shocks and initial credit quality could affect both income and home equity and households with higher income likely invest more in home maintenance. Since I compute changes in house prices at region level, the inclusion of region x time fixed effects precludes the possibility that results are driven by variation in local demand shocks or individual variation in home investment. I also include multiple loan-to-value indicator buckets to see if, as would be predicted by household debt overhang, declines in income occur only for high loan-to-value ratios. In this specification I also include household fixed effects to rule out any time invariant omitted variables, as well as time-varying household-level controls such as the amount of mortgage pre-payment as well as non-linear controls for credit score, origination home equity, and origination income interacted with time fixed effects.

Despite the inclusion of all these controls time-varying household level variation in LTV still has the potential to confound casual interpretation. In equation 2 I make this more transparent by decomposing the current household's LTV into three distinct components; (1) house prices changes, (2) changes in the balance of the mortgage, and (3) origination LTV.

$$LTV_{it} \equiv \frac{1}{\%\Delta HP_{rct}} \times \%\Delta Loan_{it} \times LTV_{ic} \quad (2)$$

Since households with improved income are more likely to prepay their mortgage, reducing the LTV, prepayment poses an empirical challenge for identification. To circumvent this rather than using actual changes in loan amount, I compute what the loan reduction would be if the mortgage was a 30-year (360 months = *T*) fixed rate loan paying the median national monthly mortgage rate, *r* (I use 6.75% based on my sample statistics).

$$\%\Delta \text{Synth}Loan_{ct} \equiv -\frac{(1+r)^{t-c}-1}{(1+r)^{T}-1} \tag{3}$$

The resulting formula in equation (3) varies across mortgages based on the age of the loan, but no longer depends on any other source of household-specific variation. An additional concern is that origination LTV could be a function of household specific characteristics, such as income or credit quality. Since I include household-level fixed effects in specification (1), time-invariant factors, like LTV at origination, are only a concern when interacted with a time-varying factor, as is the case here. In particular if high LTV at origination individuals are more sensitive to local demand shocks then this could be driving any simultaneous movement in income and household equity, rather than labor supply. To alleviate this concern I use the median national LTV at origination for each cohort for all households. Combining these I get the synthetic LTV, or *SLTV*, which only varies at the cohort-region-time level, and, controlling for all previously mentioned fixed effects, provides a plausible instrument for the probability of household having negative equity:

$$SLTV_{rct} \equiv LTV_c \times \frac{1}{\%\Delta HPI_{rct}} \times \%\Delta \text{Synth}Loan_{ct} \tag{4}$$

Variation in SLTV, after including all controls in equation (1), will be driven almost entirely by the timing of house purchase within a given region. Households that bought homes prior to relative local house price declines will have higher SLTVs relative to those who bought immediately afterward.

To formalize the instrumental variable approach define I run a 2SLS regression where the 1st stage is

$$U_{it} = \alpha_i + \gamma_{rt} + \phi_{ct} + \delta_1 \cdot 1_{\{SLTV_{rct} \geq 100\}} + X'_{it}\beta + \epsilon_{icrt} \tag{6}$$

,where I defined a household who has negative home equity (aka underwater) as $U_{it} \equiv 1_{\{LTV_{it} \geq 100\}}$, and the 2nd stage is[16]

$$y_{icrt} = \alpha_i + \gamma_{rt} + \phi_{ct} + \delta_2 \cdot \widehat{U}_{it} + X'_{it}\beta + \epsilon_{icrt} \tag{7}$$

[16] I run this using the 1st stage as a linear probability model using negative SLTV as the instrumental variable. For robustness I also show results using multiple loan-to-value bucket indicators in the 1st stage, but not probit or linear-linear models. As noted by many papers (ex. Greene 2004) probit estimates are inconsistent in a fixed effect panel regression as are purely linear models when the underlying treatment effect varies non-linearly.

The necessary assumption for the exclusion restriction is that after controlling for all fixed effects the synthetic LTV only affects income via the probability the house has negative home equity. To extent that all remaining variation in SLTV after all controls is driven by the timing of home purchases the exclusion restriction requires that the timing of home purchases is unrelated to other factors that could alter future income changes. To make this clear as a robustness check I also replace the 1st stage above with one that only includes house price changes at a region-cohort-time level explicitly.

This still leaves one possible confounding factor; the timing of house purchases within a region could violate the exclusion restriction. For example, if house price purchases by households with income more sensitive to local demand shocks could predict future house price declines then this could be potentially problematic. To address this I focus on life-event driven moves based on the time since a household attended college. In particular for each household rather than using the region-cohort-time percent change in house price I instead use the expected change in house price at the region-college attendance year-time as a proxy for the house price change.

4 Results

4.1 Negative Home Equity and Household Labor Supply

In this section I analyze the results of using variation in the timing of house purchases as a plausibly exogenous source of variation in the probability of having negative home equity among households living in the same region at the same time. I focus on the subset of households with sufficient deposit and mortgage information at *MyBank* to estimate current income, income at origination, and current loan-to-value. In table 2 column 1 I regress the % change in income, normalized by income reported at the time of mortgage origination, on indicators for varying loan-to-value ratio ranges, while including MSA x time fixed effects, household-level timing varying prepayment controls, income at origination, and 10% indicator buckets for original loan-to-value interacted with time fixed effects. Consistent with negative equity reducing labor supply I find that for low values of loan-to-value buckets income does not fall, but for high LTVs income falls by 4-5%. One potential concern is that income at origination and the additional other household time invariant controls may not capture all differences in characteristics across cohorts that could later reduce income via omitted variables. To address that concern in column 2 I rerun the analysis using household fixed effects. Though there is a small increase in the income reduction for a lower tier of loan-to-value ratios, for all high loan-to-value ratios results are largely unchanged. The non-linear nature of the effect of loan-to-value ratio on changes in income is illustrated clearly in figure 3. In this figure the x-axis are indicator dummies for each household-month that appears in a given 10% LTV bucket and the right hand side are the co-efficients from the regression run I just described for column 2. The only difference, besides more granular buckets, is that I normalize the fixed effect so buckets less than 100% sum to zero, allowing us to cleanly observe any changes that occur for high loan-to-value buckets. What we see is that for low loan-to-value ratios changes in loan-to-value do not have significant effects on labor income, but for high values, especially those above 100% LTV we see a large and consistent reduction in income. These results are consistent with household debt overhang causing a reduction in labor supply[17]. If we were concerned that variation in moving date is generally correlated with sensitivity to local demand shocks we would expect differences in income changes even for low loan-to-value buckets. Restricting the analysis to only direct deposits on the left hand side in column 3 yields almost identical results, lending credibility to the fact that changes in deposits are being caused by a reduction in wages rather than some other form of account inflows.

As was mentioned previously there could still be a concern with the above procedure that time-varying household specific factors, including income, could influence the loan-to-value ratio. To address this concern in table 3 I set a dummy variable equal to 1 if the synthetic loan-to-value ratio, which is not based any household specific time varying factors, is greater than 100%. In column 1 I run a reduced form regression using the negative equity synthetic LTV as an instrument, after controlling for MSA x time and household fixed effects, and I find that it is associated with a statistically significant reduction in household labor income. To quantify the size of this effect and the validity of the IV I run a formal 1st stage in column 2 and find that a negative SLTV is associated with a 36.8% higher chance of a household having negative equity, after controlling for MSA x time and household fixed effects, and reveals that this is a strong instrument. The formal result of this IV is shown in column 3 and shows that estimated average effect of negative equity is a 3.63% reduction in household income. When re-running the analysis using raw $ deposits per month instead of normalizing by origination income I find that it reduces income by -$366/month or about 4% of mean monthly income in my sample.

In columns 5-7 I show the results are robust to the choice of instrument. In particular in columns 5 and 6 I use a non-linear 1st stage based on 10% SLTV buckets and find that income falls 2.34% and $305/month respectively. As noted previously, you may still be concerned that even the SLTV could be providing some variation in current LTV not driven solely by the timing of moving. To alleviate this concern I use 10% buckets for MSA level house price changes since mortgage origination as an

[17] Note that for lenders the pertinent loan-to-value ratio would be the value after sale, including all costs. Since these house prices are computed at a region level and do not account for the costs of execution we would expect some reduction in income even for households with observed loan-to-value ratios just below 100%.

instrument, after controlling for MSA x time and household fixed effects. The reduced form of this IV regression is shown in figure 4. Just as was the case with loan-to-value, for low or positive differences house prices based on the timing of moving relative to households in the same region at the same time there is no change in income, but when house prices are significantly lower income falls. Since I am controlling for MSA x time fixed effects and computing changes in house price since origination at an MSA level the only source of variation here is based on the timing of home purchase relative to home owners in the same region at the same time. I run this IV formally in column 7 and find that as expected negative equity is associated with a decline in household income.

In tables 5-7 I show that these declines are robust to the choice of measurement of changes in income and liabilities, clustering of standard errors, observational frequency of the analysis, and are not driven by costs associated delinquency. If my measure of income based on deposits falls systematically relative to reported income as loan-to-value rises then this would negate the debt overhang interpretation of results. To alleviate these concerns in columns 1-3 of table 4 I show that results are largely unchanged when I use current deposits divided by mean deposits over my whole sample, rather than the reported income at origination, or the log of deposits. In table 4 column 4 and table 5 I show that the significance of results is not driven by an underestimate of standard errors due to the high frequency level of monthly observations. In column 4 I show that results are still significant when clustering at the MSA instead of MSA-month level and in table 5 I show that results are robust to running all analysis at the quarterly or yearly frequency, where home equity is computed as either the average or maximum over the sample period. In table 6 I rerun the analysis among the subset of households that also have a *MyBank* credit card account, which allows me to observe all their credit bureau liabilities. In this specification I show that results are robust to using a measure of negative equity based on all liabilities not just those associated with the primary mortgage balance. Since households with negative home equity are more likely to fall delinquent, if the costs of delinquency itself, such as explicit costs, stress, or employer background checks, affect income this would be problematic for my interpretation. I show in table 7 though that the results are significant even looking at only all households that are current on all mortgage payments and so don't face costs associated with delinquency.

Overall these results are consistent with negative home equity causing an average labor income decline of 2.34%-6.34%. With some additional assumptions I can estimate the labor supply elasticity with respect to the implicit tax rate of mortgage modifications. In my mortgage data households with negative equity are 21 percentage points more likely to receive mortgage modifications than those without negative equity. From Mulligan (2009) we know that national mortgage modification programs create a substantial implicit tax, but lost income occurs immediately while lost benefits occur over the following 5 years. We know that total benefits over those 5 years are 1.2-1.5 times larger than the loss in income, so an implicit present value tax rate of 100% is consistent with reasonable discount rate benchmarks. Combining these we can say that the average household with negative equity faces an expected implicit marginal tax rate of 21% and since they reduce their labor supply by 2.34%-6.34% this implies an elasticity of 0.11-0.30. These estimates are lower than the large elasticities of 0.94 estimated by Dobbie and Song (2015b) among bankrupt households, but compare favorably with estimates of Hicksian elasticity of labor supply in the microeconomic literature, which are on average approximately 0.25 (Chetty 2012).

Using the estimated labor supply declines for negative equity we can also get some estimates of the potential macro-economic effects. If the average unemployed household on average earns half of their employed level of income and all changes in labor supply occur via the extensive margin then a 2.3%-6.3% reduction in labor income is consistent with a 4.6%-12.6% rise in unemployment among negative equity households. CoreLogic estimates that approximately 15 million households had negative equity following the crisis. Combining these estimates and aggregating the partial equilibrium results suggest a 0.69-1.89 million decline in job-equivalent labor supply because of household debt overhang. From the peak of 2008 to the trough in 2010 non-farm payrolls fell by about 8.6 million jobs, so the estimated decline from household debt overhang would be 8%-21% the size of the total general equilibrium employment decline following the crisis[18].

4.2 Additional Robustness Checks

One potential concern with these results is that the timing of purchase might be correlated with factors related to future house price changes and labor income declines, which would violate the exclusion restriction of the instrumental variable used. I attempt to address these concerns in Table 8. In columns 1 and 2 I rerun the analysis in columns 3 and 4 of table 3, but now also include cohort x time fixed effects. If the concern is that national trends in the timing of home purchases around the time of the crisis could be related to labor demand shock sensitivity this should capture any variation coming from national cohort effects. I find that effects are essentially unchanged by the inclusion of cohort x time fixed effects where estimated declines in labor income due to negative home equity are 3.47% and $298/month. In column 3 of Table 8 I include purchase cohort x time fixed effects, but also a large range of non-parametric household-specific time varying controls that might be expected to be correlated with labor demand sensitivity. These include declines for origination income and property value, mortgage original

[18] The actual total amount of reduced labor participation following the crisis that can be explained by household debt overhang will depend critically on labor demand curve and in particular the stickiness of wages.

interest rate by percentage buckets, and original credit score in bins of 50 all interacted with time fixed effects. These results show a 4.94% decline in household income, again consistent with overall results.

Even with all these controls, there is still the potential I am missing some omitted variable which varies within region relative to national trends, but that predicts both future relative house price performance in a region and local demand sensitivity. One possible story could be the industries that are related to real estate, such as construction, could perform well in regions when house prices rise, encouraging employees in those industries to purchase properties just before local house price declines. Since workers incomes are more exposed to house price declines this could lead to a violation of the exclusion restriction. To address even this concern I use the time since a household attended college, as proxied by the average origination date of all student debt[19], as an instrument for the likelihood of a household having negative equity. The idea is that the only driver of the timing of home purchases is life-event driven, such as moving after graduating college, rather than something like occupational choice. Consistent with all the previous results I find that this IV regression estimates that negative equity is associated with a 3.78% reduction in household income.

One final concern with all the analysis up to this point could be that I measure deposits at only one institution and in particular I use deposits from the same institution that is their mortgage lender. If households hide income from their lender when they have negative equity this could mean that the reduction in deposits seen for households with negative equity is actually just movement of deposits to another institution rather than an actual decline in overall deposits from income. With this concern in mind throughout my analysis I use multiple restrictions to be sure households in the panel have active retail accounts, taking advantage of the inflow and level information I have for all deposit accounts at *MyBank* and results are robust to all choices of filter and measures of income. To be even more careful though in Table 9 I rerun my analysis focusing on *MyBank* retail customers with a mortgage from another lender. Since I no longer have detailed mortgage information I use the zip code households enter in their retail accounts[20] as a proxy for the MSA the property is located in and information from the credit bureau data on mortgage origination dates. I then utilize the same synthetic LTV computed in the previous analysis based on those households with *MyBank* mortgages, which varies only at the region-time-cohort level. Note that in this case these are reduced form regressions since current LTV is not available in credit bureau data to run the 1st stage. This method of computing the synthetic LTV is likely to reduce the power

[19] For a small subsample of households with credit cards I have information on when they graduated college. This sample is too small to use as an instrument, but has provided credibility that as would be expected, average origination date of student loans is highly correlated with the timing of college graduation.
[20] For households with multiple zip code I use the zip code of the largest account and the date closest to the origination of the most recently originated mortgage.

of the regression, but the reduced form regression still finds that negative SLTV is associated with lower deposits, after including all region x time, cohort x time, and household fixed effects. The result holds when analyzing households with mortgages at any lender or for the subset of households where *MyBank* is not a servicer or owner of the mortgage. Overall these results suggest that hiding income is unlikely to explain the reduction in monthly deposit inflows seen for households with negative equity.

4.3 Extensive vs. Intensive Margin

To understand potential drivers of the decline in labor supply for households with negative equity I investigate how households change their income. Do they alter their labor decisions via the extensive margin, such as labor market participation, or the intensive margin, such as altering hours worked at existing jobs? Unfortunately since I do not observe occupational choice I cannot test this directly, what I can test is to what extent changes in income are driven by households making large employment decisions or a many households making marginal changes. In Table 10 I test this in columns 2 and 4 by excluding cases where income changes by more than 25% relative to either the income at origination or the mean income estimated in sample. I find that when excluding large employment decisions there is no longer statistically significant relationship between negative equity and labor supply. This suggests that small changes driven by say reduced ability to wage bargain with a monopsonist among households whose labor mobility is reduced by negative equity[21] is less likely to provide an alternative explanation for the labor supply results shown in this paper. In columns 1 and 3 I show that these results are not driven by households systematically leaving the bank. I exclude only cases where households deposit $0 into their accounts and results are still significant. In column 5 I also show that households are make large reductions in labor income, such as going on unemployment, but are also more likely to leave the labor market entirely. In particular I show that households with negative home equity are more likely to receive social security, which suggests that they are either more likely to retire or move onto disability.

4.4 Effect of Mortgage Modifications Rates

The magnitudes of the decline in labor supply for households with negative equity shown in this paper suggest that household debt overhang induced by income-contingent loss mitigation likely represent the most plausible channel for the relationship between negative equity and labor supply. In figure 5 I show that households with negative equity are much more likely to fall delinquent on their payments and receive a mortgage modification. In fact among households with negative equity and who are 60+ days delinquent 44% receive a mortgage modification within the next 24 months. In figure 6 I take the

[21] See for example Cunningham and Reed (2012).

difference in the distribution of debt-to-income ratios for households with negative equity relative to those with positive equity. Rather than a consistent decline in income across the distribution I find evidence consistent with households shifting above the 31% DTI threshold used by many mortgage modification programs and bunching in the DTI ranges above that threshold. In figure 7 I also find that household income rises dramatically in the months after a household receives a mortgage modification. While there exist multiple possible explanations for the behaviors observed in these figures, they are consistent with income contingent modifications playing a significant role in explaining the labor supply response to negative equity.

To try and analyze the relationship between negative equity, labor supply, and mortgage modifications more formally I rerun the main regressions, but focus on how the average treatment effect of negative equity on labor supply varies across regions that experience more and less mortgage modifications. If households that become delinquent are more likely to receive mortgage modifications in a given region, then if the reduction in labor supply is being driven by income contingent loss mitigation we would expect these households to see a larger reduction in income. In column 1 of Table 11 I test this explicitly by interacting an MSA's modification rate, relative to the average in the sample, with the negative equity dummy. An MSA's modification rate is a time invariant metric computed as the number of mortgages that ever receive a modification in a given MSA divided by the number of all mortgages ever in a region. I find that a one-standard deviation increase (1.54%) in the modification rate is associated with 0.72% larger reduction in labor supply for households with negative equity. If we assume that this increased modification rate holds for negative equity households then this would suggest a point estimate for the elasticity of labor supply with respect to mortgage modifications of 0.47.

One concern with this analysis could be that areas with more modifications could also have more delinquencies, foreclosures, and generally worse economic conditions which could perhaps effect heterogeneity in the average treatment effect. To reduce this concern I show in column 2 that results are robust to the inclusion of the percent of all mortgages that are ever at least 60 days delinquent as a control. In column 3 I also show the effect of negative equity on labor supply is larger if the modification rate is higher among only mortgages that are at least 60 days delinquent. Similar to in column 1 I find that a one-standard deviation change in this measure of modification rate (3.91%) is associated with a 0.801% decline in labor supply. In column 3 I go one step further to reduce concerns about regional omitted variables driving heterogeneity in the treatment effect, rather than mortgage modification rates. In column 4 of Table 11 I include a dummy variable for the MSA being in a state that has judicial foreclosure requirements. Mian et al. (2015) and Ghent (2012) convincingly argue that state foreclosure laws differ based on historical path dependent exogenous events and there exists no significant differences in a number of characteristics for states with and without judicial foreclosure requirements. States with judicial foreclosure requirements though are twice as likely to foreclose on delinquent borrowers. The increased ease and likelihood of foreclosure reduces the likelihood that non-performing mortgages will receive a modification. For example, the documentation for the net present value tests for mortgage modifications under HAMP includes "state-level foreclosure timelines" and "state-level average foreclosure costs" as major determinants of whether or not a mortgage modification should be undertaken. Consistent with the higher likelihood of modification increasing the effect of negative equity on labor supply I find that states that require judicial foreclosure requirements are associated with larger declines in labor supply for negative equity. Another nice feature of this methodology is that Mian et al. (2015) find that the reduced foreclosures in states with judicial foreclosure requirements leads to smaller aggregate demand shocks. Therefore the larger response of labor supply, in partial equilibrium, that I find for households with negative equity in states with judicial foreclosure requirements exist despite the lower likelihood of foreclosure and improved local labor demand.

5 Conclusion

In this paper I investigate the effect of mortgage debt overhang, in particular negative home equity, on household labor supply. I use a new comprehensive dataset with information on household-level liabilities, assets, and all deposit transactions for all customers of a major U.S. financial institution from 2010-2014 and variation in home equity based on the timing of home purchases among households in the same region. Instrumenting for home equity, I find that negative equity causes an average reduction of 2.3%-6.3% in household income, consistent with households responding to the incentives created by negative equity and income-dependent mortgage assistance programs by reducing their labor supply. These declines are driven by large employment decisions, such as labor force participation. I also find that the labor supply decline is larger in regions where mortgage modifications are more prevalent, even if foreclosures occur less frequently, highlighting potential unintended consequences of mortgage assistance programs.

These results shed new light on the role mortgage-induced debt overhang played in exacerbating employment declines following the crisis. Mulligan (2008, 2009, 2010) has shown that negative home equity acts like an implicit tax on household labor income that provides strong incentivizes for them to reduce their labor supply. Mian and Sufi (2012) have examined how house price shocks affect equilibrium employment via local labor demand, but this is the first paper to demonstrate the role house price declines, and subsequent household debt overhang, play in reducing labor supply. Herkenhoff and

Ohanian (2011) and Donaldson, Piacentino, and Thakor (2014) have modelled the implications of these incentives and shown that household debt overhang can raise equilibrium unemployment and could explain some of the sluggish recovery of labor markets after debt-driven financial crises. While identifying the aggregate general equilibrium response to household debt overhang is beyond the scope of this paper, my results do suggest that debt overhang has a role to play in understanding how household balance sheets can exacerbate financial crises.

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Table 1A. Summary Statistics

To be included in the panel all households must have at least 12 months with deposits across all accounts of >=$100 & <=$50k and a mean and median level of deposits across all accounts of >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorised at 99th percentile. Group A look at only households that have retail and credit card accounts at *MyBank* and a mortgage with any lender. Group B examines only the subset of households with mortgages either owned or serviced by *MyBank* from 2010-2014.

	Mean	Median	Std. Dev	#Obs (mil)	#HHs (mil)
A. Households w/ *MyBank* Retail & Credit Card Accounts & Any Bank Mortgage 2010-2014					
Retail Data					
Income (All)	$7,856	$5,525	$8,547	24.42	0.622
Income (Dir. Dep.)	$6,632	$5,358	$5,305	7.81	0.195
Savings	$33,440	$9,782	$58,140	24.42	0.622
Bank Card/Credit Bureau Data					
All Liabilities	$294,600	$258,600	$204,585	21.74	0.568
MTG Balance	$250,900	$222,600	$165,344	20.94	0.554
MTG Interest Rate	6.96%	6.75%	3.33%	21.60	0.565
Has Autoloan	30.4%			21.74	0.568
Has *MyBank* MTG	32.1%			24.42	0.622
Bal Used/Available All Credit	21.9%	7.0%	29.3%	20.49	0.550
FICO Bank Credit Score	768	782	73.1	21.74	0.568
B. Households w/ MyBank Mortgage					
Mortgage Data (@ origination)					
MTG Balance (000s)	169.7	139.5	113.0		
MTG Interest Rate (%)	5.88	5.75	1.30		
Income @ Origination	7,054	5,730	5,025		
Combined Loan-to-Value	73.1	77.47	19.9		
Is Fixed Rate	91.2%				

Table 1B. Summary Statistics (cont.)

To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$50k and a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorised at 99th percentile. This sample includes only households that have retail and mortgage accounts at *MyBank* from 2010-2014.

	Mean	Median	Std. Dev	# Obs (mil)	# HHs (mil)
C. Households w/ *MyBank* Retail & *MyBank* Mortgage 2010-2014					
Retail Data					
Income (All)	$7,663	$5,315	$8,439	7.835	0.200
Income (Dir. Dep.)	$4,142	$2,826	$4,742	7.835	0.200
Income (Dir. Dep. w/ Filter)	$6,470	$5,172	$5,226	2.291	0.058
Savings	$35,370	$10,100	$60,626	7.835	0.200
Card/Credit Bureau Data (w/ *MyBank* Credit Card Account)					
All Liabilities	$266,300	$225,000	$210,610	5.158	0.144
Has Autoloan	30%			5.158	0.144
Bal Used/Available All Credit	20%	10%	29.3%	5.158	0.144
FICO Bank Credit Score	767	782	74.4	5.158	0.144
Mortgage Data					
Primary MTG Balance	$199,900	$170,700	$137,130	7.835	0.200
MTG Interest Rate @ Origination	5.373	5.375	1.227	7.835	0.200
MTG Age (Months)	64	58	49	7.835	0.200
Income @ Origination	$7,494	$6,237	$5,171	5.419	0.147
Origination Loan-to-Value (%)	64	68	22.1	7.835	0.200
Current Loan-to-Value (%)	58	58	31.5	7.835	0.200
Is Owner Occupied	92.0%			7.835	0.200
Is Fixed Rate	83.9%			7.835	0.200

Table 2. Income vs. LTV

This table shows the relationship between income and current household mortgage loan to property value (LTV) after controlling for household specific factors and local demand shocks. Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on dummies for various ranges of current (LTV) ratios (where house price is computed using original property value and changes in LPS MSA-level house price indices used by *MyBank* internally), region x time fixed effects, origination buckets interacted with time fixed effects, controls for household level mortgage pre-payments, mortgage age, and income at origination. Column 2 is the same as column 1, but instead of a variety of household specific controls includes household fixed effects. Column 3 is the same as 2, but the numerator in the dependent variable proxy for income is direct deposit inflows rather than all deposit inflows. All standard errors clustered at the MSA x Cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDeposits	(2) %ΔDeposits	(3) %Δ Direct Deposits
50 < LTV < 90	-0.83	-2.60***	-3.97**
	(0.91)	(0.45)	(0.32)
90 < LTV < 100	-4.15***	-4.48***	-4.55***
	(1.40)	(0.64)	(0.42)
100 < LTV < 110	-4.98***	-5.46***	-5.01***
	(1.69)	(0.75)	(0.48)
110 < LTV	-4.46***	-4.15***	-5.51***
	(2.08)	(0.88)	(0.60)
Region x Time FE	Yes	Yes	Yes
Orig LTV x Time FE	Yes	No	No
Prepay/Amort Control	Yes	No	No
HH FE	No	Yes	Yes
Loan Age FE	Yes	No	No
Income @ Origination	Yes	No	No
Adjusted R^2	0.124	0.486	0.686
Observations (mil)	5.375	5.375	5.375

Table 3. Income vs. Synthetic LTV: An IV Approach

This table shows the average change in household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probably of having negative equity. Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on a dummy which equals 1 if my synthetic loan to value ratio (SLTV) is greater than 100%, region x time fixed effects, and household fixed effects. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. Column 2 is the same as 1 but includes a dummy equal to 1 if a household's current loan to value is greater than 100%. This is the 1st stage estimate of the IV regression. In column 3 I present the results of using the IV in column 2 on the % change in deposits normalized by origination income. Column 4 is the same as 3 but includes raw monthly deposit inflows as the dependent variable, without any normalization. Column 5 is the same as 3 but uses dummies for SLTV 10% bandwidth buckets as an IV. Column 6 is the same as 5 but looks at raw deposits. Column 7 is the same as 5, but uses 10% buckets of MSA level house price changes since mortgage origination as non-linear IV. All standard errors clustered at the MSA x Cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	%ΔDep	LTV>100	%ΔDep	$ΔDep	%ΔDep	$ΔDep	%ΔDep
LTV>100			-3.63***	-366.4***			
(IV: SLTV>100)			(0.55)	(58.1)			
LTV>100					-2.34***	-305.3***	
(IV: SLTV 10% Bkts)					(0.51)	(50.4)	
LTV>100							-6.34***
(IV: HPI 10% Bkts)							(1.36)
SLTV>100	-1.34***	0.368***					
	(0.20)	(0.007)					
Region x Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.488	0.783	0.623	0.377	0.623	0.377	0.623
Observations (mil)	5.375	5.375	5.375	5.375	5.375	5.375	5.375

Table 4. Robust to Normalization

This table shows that the negative effect of mortgage loan-to-value (LTV) on labor supply is robust to the choice of normalization and method of clustering standard errors. Just as in the main specifications Column 1 regresses the % change in deposits on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. The numerator is still the monthly deposit inflows, but in this case the denominator is the households average monthly deposit inflows over the entire sample period. Column 2 is the same as column 1, but includes direct deposits instead of all deposits. Column 3 is the same as column 1 but the dependent variable is the log of all monthly deposit inflows, with nothing in the denominator. For households with 0 deposits in a given month, but with a still active account \$1 was included instead. Column 4 is the same as column 3 of table 3, but standard errors are clustered at the MSA instead of MSA x cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDirDep	(3) log(Dep)	(4) %ΔDep
LTV>100 (IV: SLTV>100)	-4.87*** (0.73)	-2.23** (1.10)	-4.50** (1.85)	-3.69*** (0.84)
Region x Time FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.027	0.087	0.572	0.619
Denominator	Mean Dep	Mean DirDep	N/A	Orig Income
SE Clustering-Level	MSA-Mo	MSA-Mo	MSA-Mo	MSA
Observations (mil)	5.375	4.788	5.375	5.375

Table 5. Robust to Observational Frequency

This table shows that the negative effect of mortgage loan-to-value (LTV) on labor supply is robust to the choice of observational frequency. Just as in the main specifications Column 1 regresses the % change in deposits on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. In this case though deposits are the average deposits over an entire quarter (3 months) negative equity is actually the % of times a mortgage has negative equity over that 3 month period. Column 2 is the same as column 1, but negative equity is not the % of the time a mortgage has negative equity over the period, but just a dummy equal to 1 if it ever has negative equity over the 3 month period. Column 3 is the same as column 1 but aggregated over calendar year (12 months) instead of quarterly (3 months). Column 4 is the same as column 2 but aggregated over calendar year (12 months) instead of quarterly (3 months). All standard errors are clustered at the MSA x cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDep	(3) %ΔDep	(4) %ΔDep
LTV>100	-6.02***	-5.39***	-6.33***	-5.28***
(IV: SLTV>100)	(0.80)	(0.72)	(1.08)	(0.90)
Region x Time FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
Negative Equity	Period Mean	Period Max	Period Mean	Period Max
Frequency	Qtrly	Qtrly	Yrly	Yrly
Adjusted R^2	0.042	0.042	0.026	0.026
Observations (mil)	1.867	1.867	0.558	0.558

Table 6. Income vs. LTV: Current vs. Delinquent Borrowers

This table shows that the effect of negative equity on household income is driven by households that are not delinquent on their mortgage payments. Just as in the main specifications Column 1 regresses the % change in deposits on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. The dependent variable is all deposits each month normalized by the reported income at origination. The sample analyzed is restricted to only mortgages that are current on all payments. Column 2 is the same as column 1, but run on the sample of households who are delinquent or foreclosed on their mortgage. All standard errors clustered at the MSA x Cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDep
LTV>100	-3.97***	2.44
(IV: SLTV>100)	(0.57)	(1.53)
Region x Time FE	Yes	Yes
HH FE	Yes	Yes
Delinquency Status	Current	Delinquent
Adjusted R^2	0.624	0.623
Observations (mil)	4.957	0.247

Table 7. Income vs. LTV: All Liabilities

This table shows that the effect of negative equity on household income is robust to including all liabilities as reported by the credit bureau. Similar to the main specifications Column 1 regresses the % change in deposits on an instrumented dummy equal to one if all outstanding liabilities divided by the home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. The dependent variable is all deposits each month normalized by the reported income at origination. The sample analyzed is restricted to only households that have *MyBank* mortgage, credit card, and retail accounts. Column 2 is the 1st stage of the instrumental variable regression run in column 1. All standard errors clustered at the MSA x Cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) LTV>100
LTV>100 (IV: SLTV>100)	-6.67*** (1.61)	
SLTV>100		0.159*** (0.004)
Region x Time FE	Yes	Yes
HH FE	Yes	Yes
Measure of Equity	All Liabilities	All Liabilities
Adjusted R^2	0.623	0.798
Observations (mil)	3.555	3.555

Table 8. Controlling for Cohort Effects

This table shows the decline household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probably of having negative equity, is not driven by differential cohort sensitivity to local demand shocks. Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on an instrumented dummy equal to one if current mortgage loan to property value is greater than 100%, region x time fixed effects, household fixed effects, and purchase date cohort x time fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. Column 2 is the same as 3 but includes raw monthly deposit inflows as the dependent variable, without any normalization. Column 3 is the same as 1 but also includes time varying non-parametric household-level controls. These include deciles for origination income and property value, mortgage original interest rate by percentage buckets, and original credit score in bins of 50 all interacted with time fixed effects. Column 4 uses the time since a household attended college, as proxied by the average origination date of all student debt as an instrument for the likelihood of a household having negative equity. All standard errors clustered at the MSA x Cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)
	%ΔDeposits	$ΔMonthly Deposits	%ΔDep	%ΔDep	%ΔDep	%ΔDep
LTV>100	-3.47***	-298.1***	-4.94***		-5.63**	-3.26***
(IV: SLTV>100)	(1.18)	(61.3)	(1.03)		(2.97)	(0.56)
LTV>100				-3.78**		
(IV: College Grad Yr)				(1.77)		
MSA x Time FE	Yes	Yes	Yes	Yes	Yes	No
Zip Code x Time FE	No	No	No	No	No	Yes
HH FE	Yes	Yes	Yes	Yes	Yes	No
Cohort x Time FE	Yes	Yes	Yes	No	No	No
HH Time Varying Controls	No	No	Yes	No	No	No
Region x Time x College Grad Yr FE	No	No	No	No	Yes	No
Adjusted R^2	0.490	0.380	0.492	0.547	0.550	0.623
Observations (mil)	5.375	5.375	5.219	0.665	0.665	5.271

Table 9. Mortgages at Non-*MyBank* Lenders

This table shows the relationship between income and current household mortgage loan to property value (LTV) after controlling for household specific factors and local demand shocks is not driven by households who deposit and lend at the same institution hiding income. I do this by using credit bureau data to look at households with *MyBank* retail and credit card accounts but who get mortgages from another lender. Column 1 monthly deposit inflows on an dummy equal to one if my synthetic loan to value ratio (SLTV) measure is greater than 100%, region x time fixed effects, household fixed effects, and home purchase cohort date x time fixed effects. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. Column 2 is the same as column 1 but restricts the analysis to only households with mortgages not serviced or owned by *MyBank*. P-Values: * 10%; ** 5%; ***1%.

	(1) $ΔMonthly Deposits	(2) $ΔMonthly Deposits
SLTV>100	-48.8***	-65.0***
	(10.4)	(15.0)
Region x Time FE	Yes	Yes
HH FE	Yes	Yes
Cohort x Time FE	Yes	Yes
Mortgage Servicer/Owner	All	Not *MyBank*
Adjusted R^2	0.344	0.348
Observations (mil)	20.113	15.018

Table 10. Extensive vs. Intensive Margin

This table explores the drivers of the negative effect of mortgage loan-to-value (LTV) on labor supply. Just as in the main specifications Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. In this case though cases with 100% decline in deposits are completely excluded from the analysis. Column 2 is the same as column 1 but excludes any changes larger than 25%. Column 3 is the same as column 1, but the dependent variable is the average of all monthly deposits over the whole time period for each household rather than the income at origination. Column 4 is the same as column 3 but excludes any declines larger than 25%. Column 5 is the same as column 1, but does not exclude any deposits and the dependent variable is a dummy equal to 1 if the jobs algorithm identifies that the household receives any social security checks. All standard errors are clustered at the MSA x cohort level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDep	(3) %ΔDep	(4) %ΔDep	(5) %GetSS
LTV>100	-3.28***	0.09	-4.83***	-0.23	0.65**
(IV: SLTV>100)	(0.54)	(0.25)	(0.71)	(0.17)	(0.31)
Region x Time FE	Yes	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes	Yes
Normalization	Orig Inc	Orig Inc	Mean Income	Mean Income	No
%ΔDep Range	>-100%	>-25%	>-100%	>-25%	N/A
Adjusted R^2	0.621	0.597	0.042	0.188	0.549
Observations (mil)	4.794	3.888	4.961	3.076	5.375

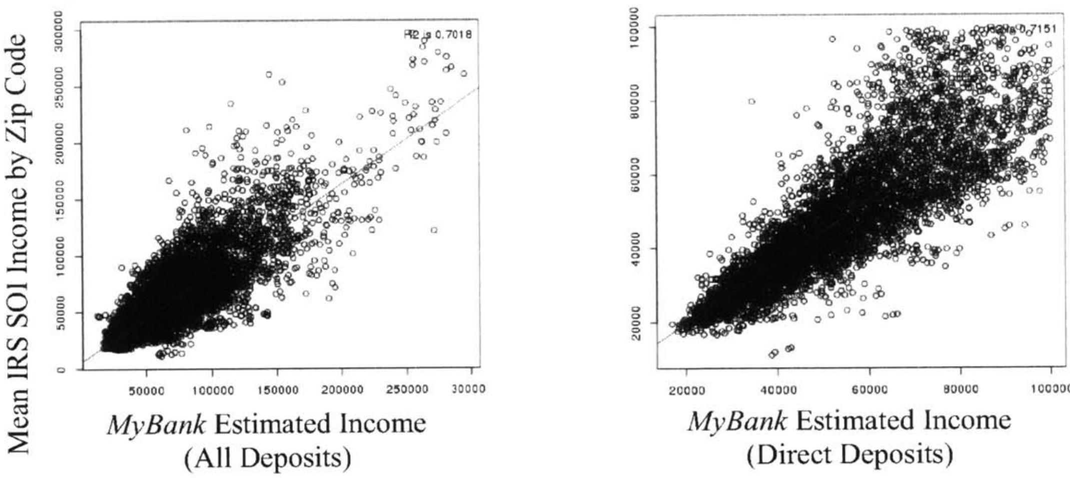
Table 11. Income vs. Negative Equity: Effects in High Modification Regions

This table shows how the relationship between income and current household mortgage loan to property value (LTV), after controlling for household specific factors and local demand shocks, varies in regions when mortgage modifications are more likely. Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time fixed effects, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. This is also interacted with the level of excess modifications per mortgage in a given MSA. This modification rate is the number of mortgages ever modified from 2010-2014 divided by the number of all outstanding mortgages over the same time period. The excess modification rate is the rate in a given MSA minus the average rate for all MSAs in the sample, weighted by the number of observations in the sample. Column 2 is the same as column 1, but also interacts the excess delinquency rate with the instrument for having negative home equity. The delinquency rate is the number of mortgages ever 60 or more days past due from 2010-2014 divided by the number of all outstanding mortgages over the same time period. The excess delinquency rate is the rate in a given MSA minus the average rate for all MSAs in the sample, weighted by the number of observations in the sample. Column 3 is the same as column 1, but instead of the modification rate for all mortgages I use just the modification rate among delinquent mortgages. The modification rate in this case is the number of mortgages ever modified from 2010-2014 divided by the number of mortgages ever 60 or more days past due over the same time period. The excess modification rate is again the rate in a given MSA minus the average rate for all MSAs in the sample, weighted by the number of observations in the sample. Column 4 is the same as column 1 but includes a dummy variable equal to 1 if the state the property is located in requires judicial foreclosure requirements interacted with the instrumented dummy for negative equity. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDep	(3) %ΔDep	(4) %ΔDep
LTV>100	-3.63***	-3.63***	-3.63***	-3.20***
	(0.55)	(0.55)	(0.55)	(0.46)
LTV>100 x MSA Excess Mod Rate (%)	-0.721**	-1.123**	-0.205**	
	(0.322)	(0.521)	(0.090)	
LTV>100 x MSA Excess DQ Rate (%)		0.161		
		(0.261)		
LTV>100 x Jud Foreclosure State				-1.72*
				(1.01)
Region x Time FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
IV	SLTV>100	SLTV>100	SLTV>100	SLTV>100
MSA Excess Rate	Ever Mod /MTG	Ever DQ60+ /MTG	Ever Mod / Ever DQ60+	N/A
1SD Excess Mod Rate	1.54%	4.05%	3.91%	N/A
Adjusted R^2	0.619	0.619	0.619	0.619
Observations (mil)	5.375	5.375	5.375	5.375

Figure 1. Validity of Income Measure
1A. Zip-Code Level Mean Income IRS SOI vs. MyBank (2010-2013)

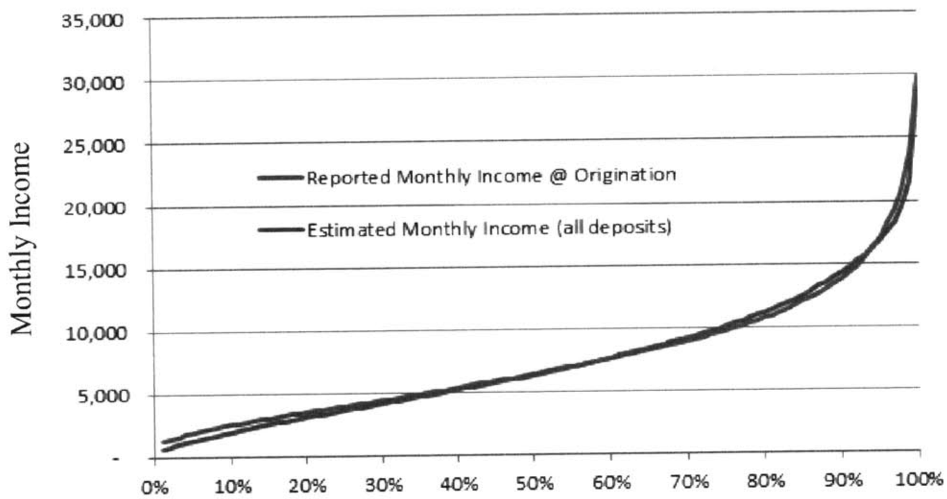
These figures compare the mean incomes by zip code from 2010-2013. To be included there must be at least 4,000 IRS SOI returns and at least 1,000 *MyBank* observations per zip-code year w/ filters applied. To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel.



Correlations	All Deposits	All Direct Deposits	All Jobs
MyBank Retail Acct	0.832	0.886	0.911
MyBank RTL, CC, & Any MTG	0.838	0.777	0.736

1B. Estimated Income vs. MyBank @ Origination Distribution

This figure compares the cumulative distribution of reported income at mortgage origination for *MyBank* mortgages with the estimated income based on retail deposits for all households in the same calendar year for all households with data available for both, who meet the filter requirements. To be included in the panel all households must have at least 12 months with deposits across all accounts and years >=$100 & <=$25k, a mean and median level of deposits across all accounts and years >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. The table below includes the pair-wise individual correlations for each household for all three measures of income.



Correlation	All Deposits	Direct Deposits	Job Direct Deposits
MyBank RTL & CC & Any MTG	0.378	0.511	0.449

Figure 2. Validity of Delinquency Measure

This figure compares a time series of mortgage delinquency rates for households with mortgage at *MyBank* using *MyBank's* internal mortgage data with national seasonally adjusted quarterly mortgage delinquency rates published by Federal Reserve Economic Data (FRED) from 2009-2014. Quarterly data from are interpolated between quarters to provided monthly estimates. The green and blue top lines for both FRED and *MyBank* represent the percent of all mortgages that are at least 30 days past due. The red bottom line represents all *MyBank* mortgages that are at least 90 days past due.

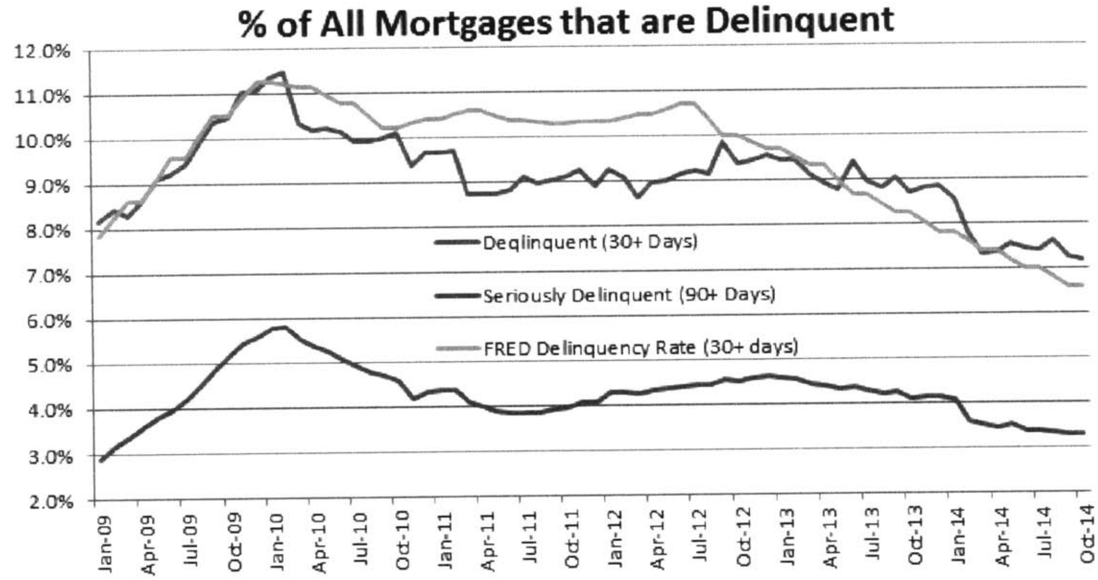


Figure 3. LTV vs. Income: Identification Based on Timing of Moving

This figure shows the relationship between income and current household mortgage loan to property value (LTV) after controlling for household specific factors and local demand shocks. This figure shows the coefficients of regression where I regress the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on dummies for various ranges of current (LTV) ratios, where house price is computed using original property value and changes in LPS MSA-level house price indices used by *MyBank* internally, region x time fixed effects, and household fixed effects. In this figure the x-axis indicator dummies for each household-month that appears in a given 10% LTV bucket and the right hand side are the co-efficients from the regression (shown in red). I normalize the fixed effect so buckets less than 100% sum to zero, allowing us to cleanly observe any changes that occur for high loan-to-value buckets. 95% confidence intervals computing standard errors clustered at the MSA x cohort level, are plotted with dotted lines on either side.

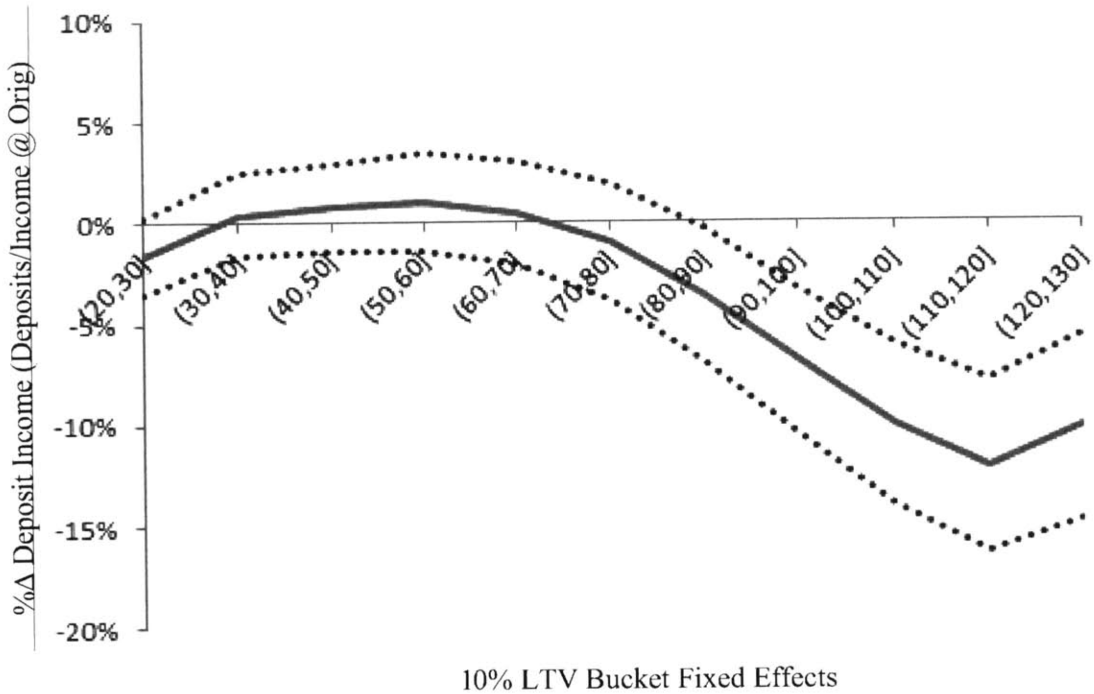
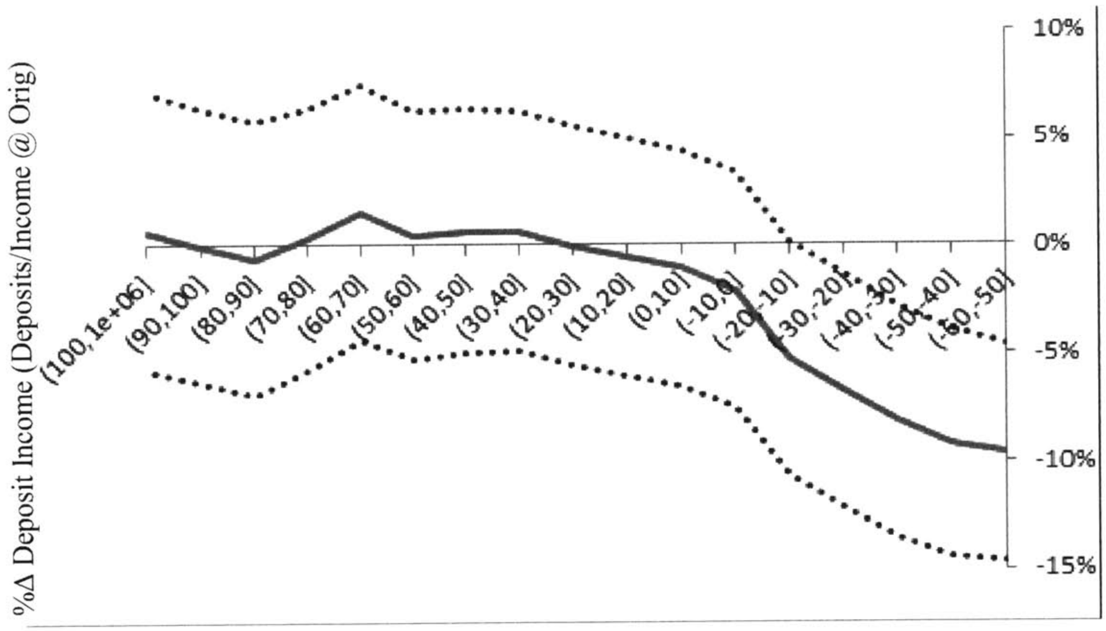


Figure 4. LTV vs. Income: Identification Based HPI IV Reduced Form

This figure shows the average change in household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probably of having negative equity. This figure shows the coefficients of regression where I regress the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on dummies for various ranges of MSA-level house price index changes since mortgage origination, where house price is computed using original property value and changes in LPS MSA-level house price indices used by *MyBank* internally, region x time fixed effects, and household fixed effects. In this figure the x-axis are indicator dummies for each household-month that appears in a given 10% HPI change bucket and the right hand side are the co-efficients from the regression (shown in red). I normalize the fixed effect so buckets greater than 0% sum to zero, allowing us to cleanly observe any changes that occur for negative house price differences. 95% confidence intervals computing standard errors clustered at the MSA x cohort level, are plotted with dotted lines on either side.



%Δ 10% HPI Bucket Fixed Effects

Figure 5. Modification and Delinquency Rates vs. LTV

This figure shows how delinquency and modification rates vary with a household's mortgage loan to home value (LTV) ratio by 10% LTV buckets over the time period 2010-2014. Each unit of observation is at the household month level. The black line represents the % of households with a LTV ratio in a given month with the 10% range that will receive a mortgage modification in within the next year. The red dashed line is the percent who are ever at least 60 days past due on any mortgage interest payments.

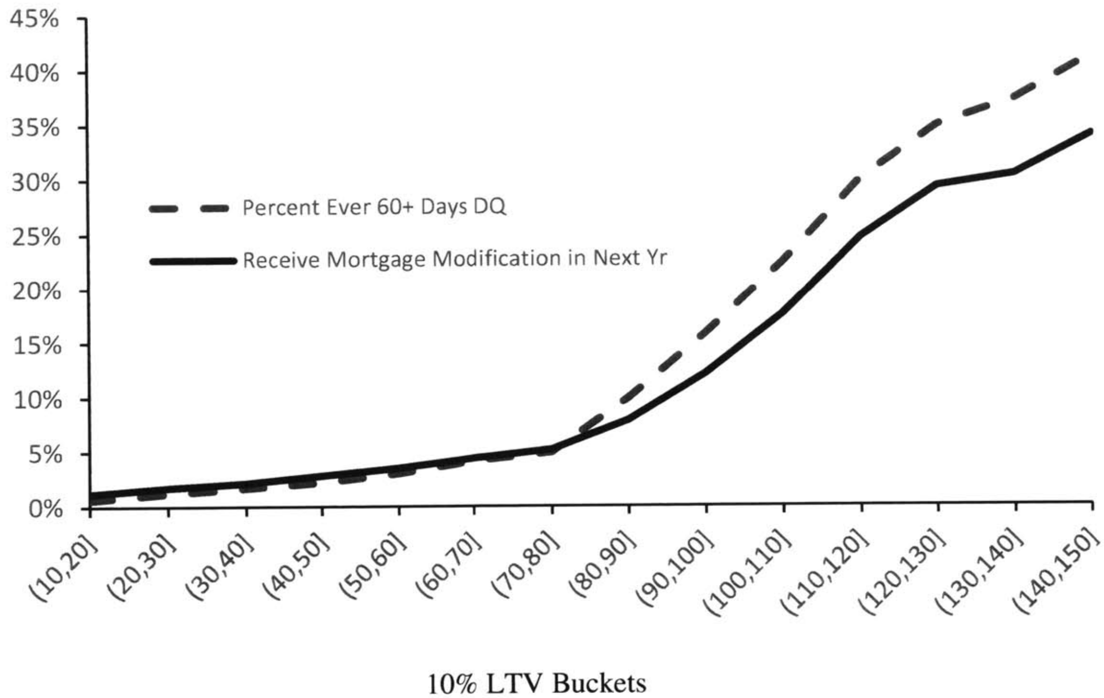


Figure 6. Bunching at Program Kink Points

This figure shows to what extent it appears that distribution of households with negative equity is consistent with bunching above the typically 31% debt-to-income (DTI) threshold of many mortgage modification programs. For all households with retail, credit card, and mortgage account at *MyBank* I compute the probability distribution function of the front end DTI ratio for each household-month observation from 2010-2014 for those households with and without negative home equity. I then take the difference between these distributions and plot tem here as a function of 1% DTI buckets.

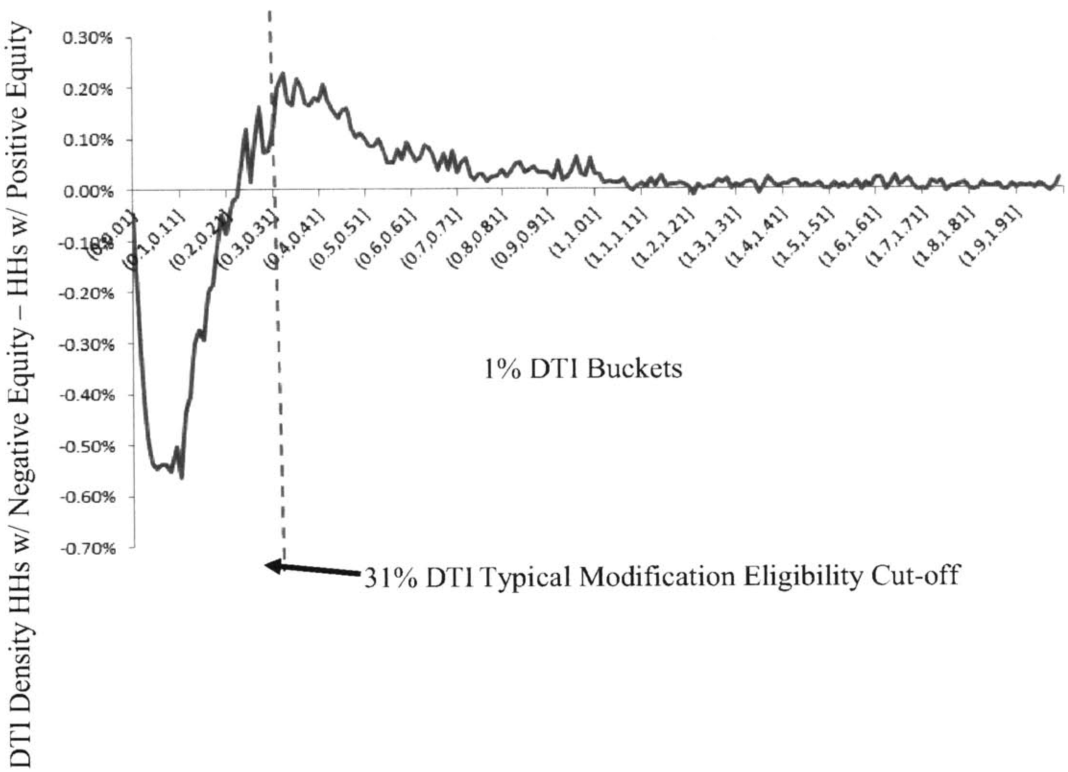
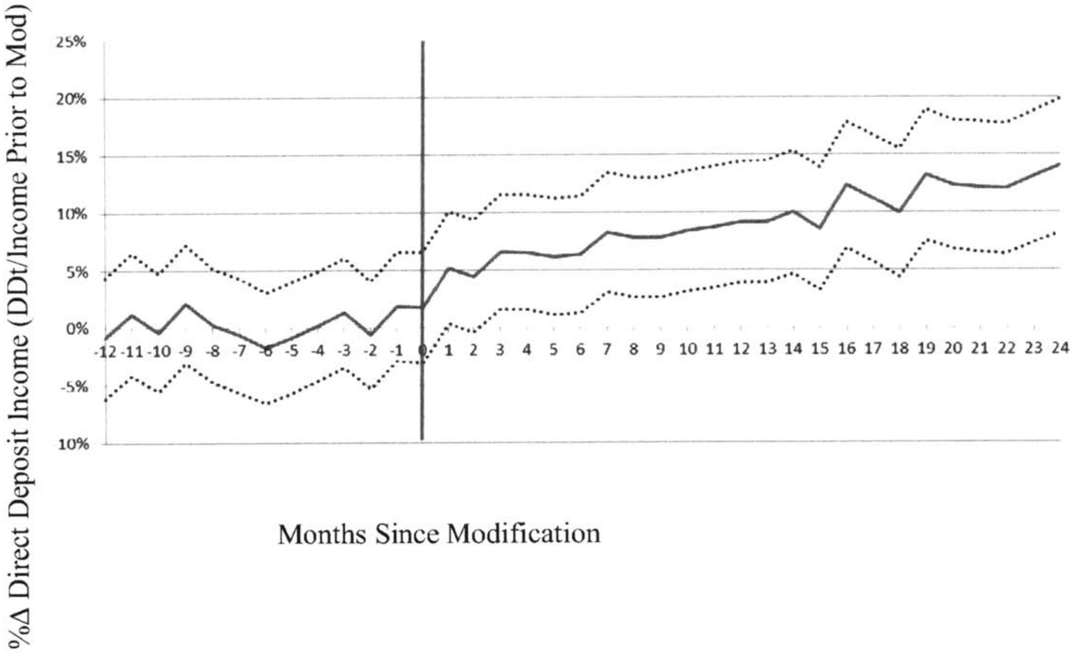


Figure 7. Mortgage Modifications and Labor Supply Event Study

In this figure I look at how household income changes for households who receive mortgage modifications around the dates they receive modifications. This figure plots the results from a regression of the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, on dummies for event time relative to the month a mortgage is modified, with time, loan, and household fixed effects. The red line is the estimated coefficients from the event time dummies, normalized to zero for the pre-event period, and the dotted lines represent 95% confidence internals for these estimates, using standard errors clustered at the household level.



Appendices

Appendix A: Panel Data Construction

The data provider for this project is a major U.S. financial institution, who I refer to as *MyBank*, with transaction-level client account information on more than 1/4th of all U.S. households over the 5 years from 2010-2014. For the purposes of this project I focus on households with sufficient *MyBank* relationships to estimate income and mortgage information and analyze income decisions at a monthly household level. Income is estimated using retail account deposit information and mortgage information is either derived from credit bureau data (only available for households w/ *MyBank* credit card accounts) or *MyBank* mortgage account information. In table A1 I detail the effect on sample size and household characteristics when multiple *MyBank* accounts are combined at a monthly frequency. In table A2 I also compare simple summary statistics from this primary sub-sample of households with *MyBank* mortgages and retail accounts with self-reported information collected by the Survey of Consumer Finance (SCF) for households with at least $1,000 in active mortgage balance in 2010. I find that my sample of households has similar levels of income, non-housing financial assets, mortgage leverage, and are charge comparable mortgage interest rates consistent with the representative *MyBank* national coverage and lending credibility to the external validity of the conclusions of this paper.

Table A1. Effect of Panel Data Construction on Sample Size

Merging is done at HH-level. To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k. To be "active" a HH must have at least $200 aggregated across all accounts in a month or at least $100 in deposits across all accounts. For direct deposits and assigned to jobs direct deposits the same restrictions apply as with deposits, but for direct deposits and assigned direct deposits only respectively, and >=75% of all deposits must be via the channel of interest. 1st row includes no filters, but all others that include retail include the filter.

	Median Ann. Deposits	Median MTG Bal	#HH-Mo Obs (mil)	#Accts (mil)	#Custs (mil)	# HHs (mil)
MyBank Retail Acct (*Raw*)	$23,556					
MyBank Retail Acct	$37,166					
MyBank Credit Card Acct		$152,268				
MyBank Mortgage		$116,255				
MyBank RTL & MTG	$63,780	$170,726	7.83	1.40	0.70	0.20
MyBank RTL & CC & Any MTG	$66,301	$222,626	24.42	4.84	1.99	0.62
MyBank RTL & CC & No MTG	$39,982	$0	30.13	6.22	2.43	0.96
MyBank RTL, CC, MTG	$73,011	$177,631	4.36	1.32	0.49	0.13
MyBank RTL, CC, & Non-*MyBank* MTG	$67,506	$228,569	16.58	4.30	1.75	0.54
MyBank RTL & CC & Non-*MyBank* & Direct Deposit Req.	$72,587	$224,421	5.52	1.14	0.45	0.17
MyBank RTL & CC & Non-*MyBank* & Assigned Direct Deposit Req.	$63,837	$210,748	0.88	0.15	0.06	0.03

Table A2. *MyBank* Summary Stats vs. Survey of Consumer Finance

To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$50k and a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorised at 99th percentile. This sample includes only households that have retail and mortgage accounts at *MyBank* from 2010-2014. Data from Survey of Consumer Finance (SCF) comes from 2010 and includes all households with a primary mortgage outstanding balance of at least $1,000 (13,580 households).

	SCF Median (2010)	*MyBank* Median	*MyBank* Std. Dev
Households w/ *MyBank* Retail & *MyBank* Mortgage 2010-2014			
Retail Data			
Income (All)	$5,083	$5,315	$8,439
Income (Dir. Dep. w/ Filter)	--	$5,172	$5,226
Savings	$7,850	$10,100	$60,626
Mortgage Data			
Current Loan-to-Value (%)	58.6	58.0	31.5
MTG Interest Rate	5.39	5.38	1.23
Is Fixed Rate	87.4%	83.9%	

Appendix B. Jobs Algorithm

Of the billions of transactions from 2010-2014 there are 731,219,999 transactions into accounts at MyBank which are labeled as "deposits". Of these ~376m (51%) are direct deposits (denoted by "ACH direct deposit"), 327m (45%) are physical deposits, which include teller and ATM deposits, and the remaining ~28m (4%) include other deposit types such as "Mobile RDC Deposits". Excluded from these transactions are all transfers, outflows, et al. Besides a broad classification the dataset also includes account IDs and the date they were made. Table B1 below illustrates a hypothetical set of deposits in the data files provided[22].

Table B1. Hypothetical Example of Transaction Dataset

Date	Acct ID	Description	Amount
1/15/2011	1032101	ATM Deposit	130.00
1/15/2011	1032101	ACH Direct Deposit	652.21
1/30/2011	1032101	ACH Direct Deposit	652.21
1/3/2011	2031411	Mobile RDC Deposit	78.32

Since no information is provided on the reason for the transaction or the provider of the funds, to determine the number of jobs associated with an account and the $/job I focus only on direct deposit transactions >=$100 and <=$25,000, leaving ~333m transactions, and utilize the fact that direct deposit paychecks tend to fall on a set of possible regular schedules.

As noted by the Bureau of Labor Statistics (BLS)[23] employers can be characterized as weekly, bi-weekly, semi-monthly, and monthly payers. Adjusting for holidays, weekly payers pay every week on the same day, bi-weekly payers pay every two weeks on the same day, semi-monthly pay on the 1st and 15th or 15th and 30th, and monthly payers tend to pay on the last or first day of the month. Only one major employer type is absent from the BLS characterization, the U.S. government. About 1/4th of households have a social security recipient and depending on the type of program, the date filed, and birthdate of the individual social security checks are paid on either the 3rd of each month, or the 2nd, 3rd or 4th Wednesday of each month[24]. As can be seen in figures B1 and B2 the majority of direct deposits tend to fall on

[22] These are for illustration purposes only to show data structure. All values are fabricated for this example and do not depict actual transactions in the database.
[23] http://www.bls.gov/opub/btn/volume-3/how-frequently-do-private-businesses-pay-workers.htm
[24] www.ssa.gov

Fridays and on 1st, 3rd, 15th, or end of the month, where the exact day depends on the length of the month and any holidays.

Figure B1. Deposits by Type and Day of the Month

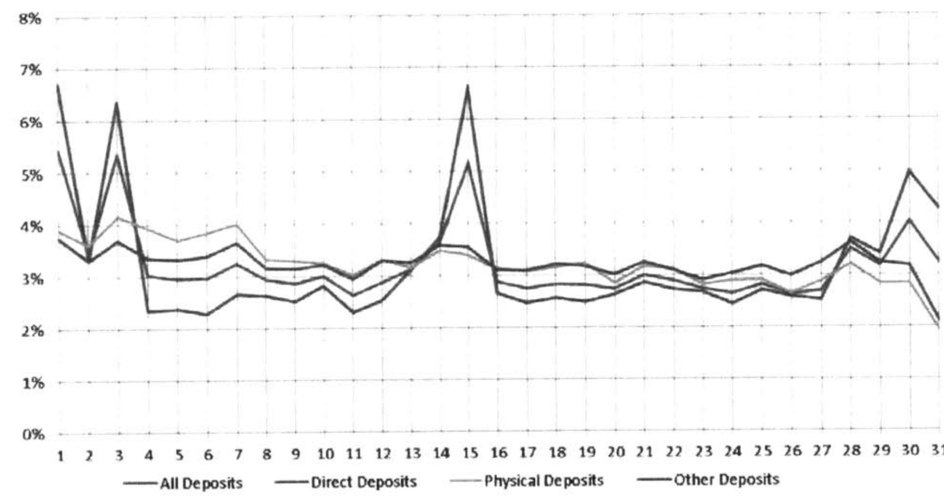
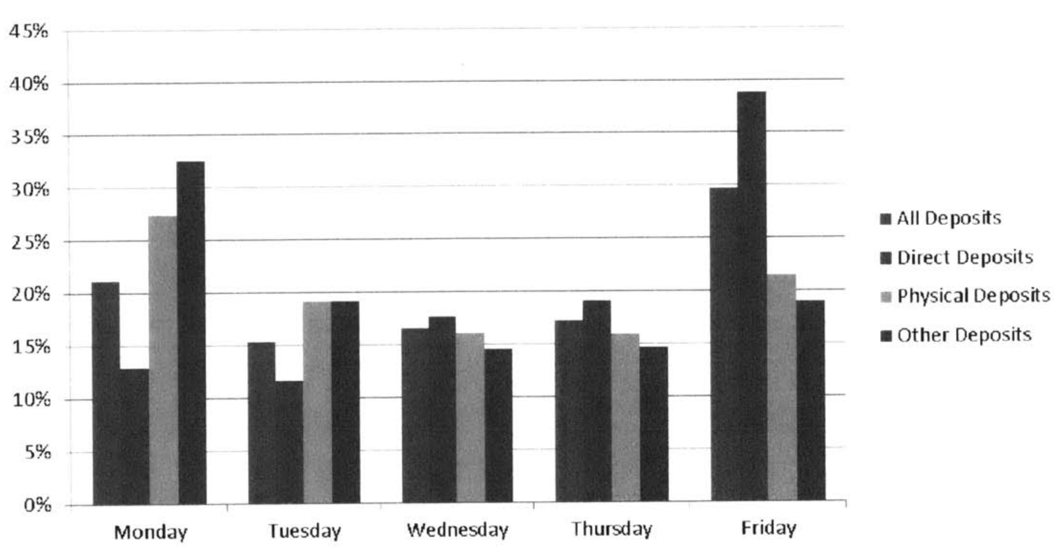


Figure B2. Deposits by Type and Day of the Week



Using this information I assign paychecks to jobs using a *minimum jobs* algorithm, which assumes that the smallest number of jobs is likely to explain the paychecks seen. For example, if a HH receives 4 checks in a month, one say each Weds, and there are only 4 Weds in the month, then this is assigned as a weekly paycheck, rather than 2 bi-weekly paychecks or 2-3 social security paychecks. To achieve this I first assign all possible paychecks to weekly paying jobs, then bi-weekly, semi-monthly, monthly, and social security payments in order respectively, adjusting for holidays and non-business days (ex. If 15th is a Saturday and HH is paid semi-monthly then HH is paid on the 14th instead). Any paychecks which don't fit into one of these pay schedules is then left "unassigned". If multiple paychecks for a given account appearing on the same day the amounts are assigned equally to each job associated with those dates[25].

Since my analysis is run at a monthly frequency one final concern is that some payment schedules pay unequal number of times each month. For example, if a HH receives weekly paychecks every Friday, then in July-2015 they would have received 5 paychecks while in August-2015 they would only have received 4. This could add a lot of noise to the income estimates since ceterus paribus this would make it appear that the HHs income fell 20% in August. The recently created JP Morgan Chase Institute also uses in-house proprietary deposit data to estimate and analyze income and in their inaugural report they noted that one of their three major findings was that one of the "drivers of monthly [income] volatility includes months with five Fridays, when employees may be paid three times instead of two". Since this is an artifact of the panel construction rather than fundamental changes income I create an adjusted income measure which takes the raw total income from all paychecks for a job each month and multiplies it by (# of paychecks per year)/ ((# paychecks received this month) x (12 months per year)) which creates an annualized income measure, adjusting for differences in the number of paychecks, and then divided by 12 to get a monthly measure. This adjusted measure only works for paychecks which have been assigned to a job and pay schedule, but for this subset it should prevent noise caused by difference in the number of paychecks per month.

Though any algorithm based solely on pay schedules is going to miss a few job-related paychecks[26] this methodology is able to assign most paychecks to regularly paying jobs. More than 90% of account-month observations have at least one job associated with them, and as can be seen in table B2, the assigned to jobs paychecks represent 84% and 61% of all direct deposit paychecks by number and dollar amount received respectively[27]. According to the Social Security Administration[28] the average monthly

[25] Note that since all analysis is done at the average $/job level the precise allocation among jobs doesn't affect the data analyzed substantively.
[26] For example if the HR office accidentally pays checks a day late because of operational issues, the company has a non-standard holiday schedule, et al.
[27] Non-reoccurring payments, like bonuses or yearly incentives, are more likely to be excluded by the jobs algorithm, which may explain why the # of paychecks picked up by the algorithm is smaller than the amount ($).
[28] www.ssa.gov

benefits for a beneficiary of SS is $1,223.45/month, which matches favorably with the $1,267.5/month I see per SS recipient in my sample.

Table B2. Breakdown of Deposits from Jobs Algorithm

	All Deposits	Direct Deposits	Direct Deposits (Assigned)	Direct Deposits (SS)
# >0 HH-Mo Obs (mil)	35.64	35.31	24.49	12.47
Total $ (bil)	205.01	175.45	107.74	20.66
# Paychecks (mil)		105.91	88.86	
$/Paycheck (mean)		$1,657	$1,212	
# Households	757,205	757,205	757,205	757,205
$/Job (mean)			$3,506.4	$1,267.5
$/Job Adj. (mean)			$3,502.1	

In table B3 I breakdown the assigned jobs by pay schedule type. Consistent with Bureau of Labor Statistics (BLS) Current Employment Statistics (CES) surveys[29] the majority of jobs are associated with bi-weekly pay schedules, while semi-monthly pay schedules are associated with the highest paying jobs[30].

[29] http://www.bls.gov/opub/btn/volume-3/how-frequently-do-private-businesses-pay-workers.htm. About 36% of businesses pay on a bi-weekly basis and this is even higher among large businesses (1,000+ employees) where it is upwards of 70%, so the majority of jobs are paid bi-weekly.

[30] Based on the CES survey monthly payers are concentrated among very small businesses (<10 employees), which may explain, at least partially, the lower $/job seen for the monthly paycheck receivers.

Table B3. Job Assigned Direct Deposits by Pay Schedule Type

Direct Deposits (Assigned)	Weekly	Bi-Weekly	Semi-Monthly	Monthly
# Obs (mil)	3.34	14.75	4.89	6.99
Total $ (bil)	11.54	61.05	25.14	15.93
$/Job (mean)	$3,228.6	$3,603.1	$4,700.1	$1,671.0
$/Job Adj. (mean)	$3,254.8	$3,612.9	$4,627.2	$1,671.0

Overall these results are consistent with the jobs algorithm effectively assigning direct deposits to jobs in a manner that captures most direct deposit income.

Chapter 2
More Than Just Speculation: The Costs of Restrictions on Speculative Investing*

1 Introduction

In the aftermath of the recent financial crisis there has been renewed interest in the regulation of speculative investing by banks. For example, former Federal Reserve chairman Paul Volcker, who has been a leading proponent of restrictions on proprietary trading by commercial banks, asserted that "proprietary trading of financial instruments – essentially speculative in nature –[is] engaged in primarily for the benefit of limited groups of highly paid employees and of stockholders"[31]. In fact, a section of the recently implemented Dodd-Frank Act that bans proprietary trading by banks is referred to colloquially as the "Volcker Rule". In a report on the effects of the Volcker Rule, Duffie (2012) raises concerns that "firms would face higher costs for raising new capital", while Thakor (2012) laments that it is "likely to lead to higher costs of capital for businesses and potentially lower capital investments by these borrowers". Even the head of the House Financial Services Committee, Congressman Jeb Hensarling, argued in a letter to U.S. Treasury Secretary Jack Lew, as recently as July of 2014, that a lack of liquidity caused by the ruling could make it "more expensive for businesses to grow"[32]. Despite the heated debate there is still limited empirical evidence of the costs to non-financial firms of the Volcker Rule's investment restrictions.

In this paper I use a natural historical experiment to explore potential costs to non-financial firms of restricting speculative trading by banks. On February 15th, 1936 the Office of the Comptroller of the Currency unexpectedly announced that member banks of the Federal Reserve System, one of the largest buyers of corporate bonds, were no longer allowed to purchase securities rated as "speculative grade" by rating agencies. Running an event study following the announcement I find that financing constraints induced by the shock to demand for speculative debt causes a persistent 3-5% negative cumulative

* I would like to thank Antoinette Schoar for financial support as well as Walter Friedman, Peter Temin, Adrien Verdelhan, Xavier Giroud, Randolph Cohen, Carola Frydman, Eric Hilt, Felipe Severino, Stephen Murphy, Nils Wernerfelt, Daan Struyven, and seminar participants at the MIT finance lunch for helpful comments.

[31] "Commentary on the restrictions on proprietary trading by insured depository institutions", Paul Volcker, February 13th, 2012

[32] "Lew Challenged over Volcker rule impact", July 10, 2014. Financial Times.

abnormal return in the equity value of firms requiring speculative financing. The reduced market value is driven by firms in industries that are reliant on external financing. Since associated corporate bond yields do not change, the decline in the value of these firms is not driven by an increase in perceived default risk or a rise in direct costs of borrowing. Instead, I find that firms who initially require speculative financing reduce the size of their debt issuances to improve their credit rating. These firms subsequently have less long-term debt, fewer investments, and slower asset growth in the years following the ruling.

While direct evidence on costs of the Volcker Rule are limited[33], my results complement a growing literature that examines how shocks to the supply of credit in the segmented speculative corporate bond market can alter firm behavior. In modern financial markets regulatory investment restrictions and contracts which refer to the investment grade barrier, as defined by credit rating agencies, prevent large segments of the market from investing in bonds deemed "speculative" by rating agencies. Whether in surveys (Graham and Harvey 2001) or through revealed behavior (Kisgen 2006) firms consider their debt rating when determining their capital structure and this is especially true at the investment grade cut-off. Chernenko and Sunderam (2010) use a regression discontinuity approach and find that high-yield mutual fund flows differentially affect issuance and investment rates of matched firms just above and below the investment grade border. Lemmon and Roberts (2009) find that the collapse of Drexel Burnham Lambert, the passage of the Financial Institutions Reform, Recovery, and Enforcement Act of 1989, and regulatory changes in the insurance industry caused a contraction in the supply of speculative grade debt after 1989 which was matched one-for-one with a decline in investment by speculative grade firms. While studies of capital structure and investment decisions are important in understanding how firms respond to credit demand shocks, what we can't get from these studies is if shocks to speculative grade debt issuance actually caused a loss in firm value. In a classic corporate finance setting, where capital structure does not affect firm value, firms could alter issuance behavior in response to ratings or investor demand and still have a perfectly elastic credit demand curve consistent with Modigliani and Miller (1958). In fact, the finding of Lemmon and Roberts (2009) that substitution into non-bond financing is limited so that investment changes almost one-for-one with the decline in debt issuance, is suggestive of a very elastic credit demand curve. Similarly, Cherneko and Sunderam (2010) find that changes in investment by mutual fund flows are driven by changes in issuance quantity rather than changes in the cost of capital. It is hard to tell from these results to what extent, if any, shocks to the supply of credit alter firm value in the speculative grade bond market.

Ideally researchers would look at firm stock price response to unexpected and exogenous shocks to investor demand for speculative grade corporate debt, but that is challenging in the modern period. For

[33] Full compliance was not required before 2015, so the recent implementation makes such analysis difficult.

example, in Lemmon and Roberts (2009) the collapse of Drexel occurred over the course of 2 years and it would be anticipated well in advance of its complete collapse[34]. There is an expansive literature looking at how the downgrade of a firm's debt from investment grade to speculative grade alters firm value, but downgrades are also anticipated by the market and endogenously determined by firm risk. It is probably not surprising then that while some studies find that rating downgrades alter firm value (ex. Holthausen and Leftwich (1986), Dichev and Piotroski (2001)), Jorion and Zhang (2008) find that after controlling for rating fixed effects there is no abnormal return following downgrades at the investment grade barrier relative to any other downgrade and Vassalou and Xing (2005) find that after adjusting for default risk there is no abnormal equity return following bond downgrades at any level. This is a fundamental difficulty with analyzing the effect of investment restrictions caused by ratings in the modern period since the investment grade cut-off is well established and ratings are paid for by issuers. An inability to reach a rating threshold could provide a negative signal about firm management or operations (Kisgen 2006, Opp et al. 2010) which could reduce firm value independently of any credit demand effects. In addition, any analysis, even if it identified an exogenous shock to ratings, would have difficulty disentangling changes in firm value caused by rating-contingent triggers in contracts from the effects of the shock to clientele[35]. In addition to being a plausibly exogenous, unexpected, and large shock to investor credit supply in speculative debt the announcement I study in this paper ostensibly established the official "investment grade" barrier reducing the risk that firm clustering at the border could confound the regression discontinuity analysis. Also since at this time investors rather than issuers paid for ratings it simplifies the complexity of identifying the effects since the firm value functions did not enter the optimization of the rating agencies and subsequently alter ratings (Opp, et al 2010). This period also predated the use of rating-contingent covenants allowing me to more cleanly identify the effects of shocks to credit supply in speculative financing.

Even with these promising features, the announcement of the bank investment restrictions is not a true random experiment so I address concerns that other unobserved factors could drive changes in firm value around the time of the announcement. First of all, I show that the stock prices of firms using investment and speculative grade financing follow parallel trends in the days prior to the announcement, but while the stock value of firms requiring speculative financing falls dramatically in the days following the announcement, the same is not true for the average firm able to issue investment grade debt. These results are robust to controlling for Fama-French factor portfolio returns and industry x event fixed effects. I also find similar results using a fuzzy regression discontinuity approach where I compare firms

[34] This also presents a challenge for direct analysis of the Volcker Rule, since it was originally scheduled to be implemented as part of the Dodd-Frank Act in 2010, but mandatory compliance was not required till 2015.
[35] Standard and Poor's (2002) survey around 1,000 investment grade issuers in the U.S. and Europe and found that nearly half have borrowing arrangements that include credit rating contingent triggers.

just above (Baa) and below (Ba) the investment eligibility criteria. By contrast comparing firms with higher and lower rated bonds within investment and speculative grade produces no relative decline in value. As another pseudo placebo test I find that firms without debt, even if they are high risk, see no differential decline in stock market value, relative to firm's with investment grade debt, following the announcement. Any time we analyze historical events there is also a concern about external validity. While that concern will always remain to some degree, one of the nice features of this period is that the high corporate bond market liquidity and expansive rating agency coverage are comparable to what they are now and far and above anything that existed for most of the intervening decades (Biais and Green 2007).

This paper also fits within a broader literature analyzing the relationships between credit ratings, investor demand, market segmentation, costs of external financing, and firm behavior. This includes a broad and growing literature trying to understand how firms respond to credit ratings (Holthausen and Leftwich (1986), Graham and Harvey (2001), Dichev and Piotroski (2001), Vassalou and Xing (2005), Kisgen (2006), Tang (2006), Jorion and Zhang (2007), Sufi (2009), Kisgen and Strahan (2010), Kisgen (2012)), the effect of rating contingent regulation (Harold (1938), Hickman (1957), West (1973), Moreau (2008), Partnoy (2010), Flandreau (2010)), how market segmentation can alter asset prices and firm behavior (Faulkender and Petersen (2006), Mitchell, Pedersen, and Pulvino (2007), Lemmon and Roberts (2009), Duffie and Strulovici (2012), Chernenko and Sunderam (2012)), and the costs of constraints on external financing (Modigliani and Miller (1961), Miller (1977), Myers and Majluf (1984), Rajan and Zingales (1992), Hubbard and Calomiris (1994)).

The analysis begins in Section 2 with a brief background on the introduction of rating-contingent regulation and the economic context in which it affected banks and firms. Section 3 describes the data used in the project. Section 4 presents the empirical predictions and methodology. Section 5 contains the empirical results. Section 6 concludes.

2 Historical Background

2.1 The Introduction of Rating-Contingent Regulation

In 1909 John Moody was inspired by the success of credit ratings used by mercantile credit report agencies in the 19th century and contemporaneous corporate bond rating systems in Vienna and Berlin to publish his first "Moody's Manual" with ratings of the securities of railroad companies[36]. Moody's had

[36] John Moody had also just recently sold the manual business he established in 1900 to Roger Babson and Freeman Putney Jr. following its bankruptcy in the face of the panic of 1907. His original manuals had no ratings, just financial information on firms, so the ratings in his 1909 manual may have been in part an attempt to get around his

also settled on a set of ratings which he would not significantly alter until the 1980s[37], with Aaa constituting the highest rated securities followed by Aa, A, Baa, Ba, B, Caa, Ca, and C respectively. The volumes on railroads were so successful in 1914 he started publishing ratings for the securities of utility and industrial companies. Poor's Publishing Company who had been successfully selling comprehensive manuals on firms for more than a half century quickly joined the ratings business in 1916, followed soon afterwards by Standard Statistics in 1922, and Fitch Publishing Company in 1924. Thus by the mid-1920s the names of the credit rating agencies who still constitute the largest players in the industry had been established: Moody's, Poor's, Standard, and Fitch[38]. By 1928 Hickman (1957) estimates that over 98% of all corporate debt was rated by at least one of these firms. In fact ratings were so comprehensive in the mid-1920s to find another period with as many firms with rated debt you would have to wait 70 years until the latter half of the 1990s (Fons 2004)[39].

Though rating agencies were already a large business by the 1920s, they did not become a part of regulation until the 1930s. In the 1931 Gustav Osterhus noted that Federal Reserve began using bond ratings in the 1930s in their examination of banks' portfolios for the first time, but the first explicit rating-contingent regulation occurred in the fall of 1931. On September 11th, 1931 the Office of the Comptroller of the Currency (OCC) specified that banks with bonds rated Baa or higher would be carried at cost while those below that level would require fractional write-offs for capital requirements. In 1932 insurance regulation followed suit, but specified that all bonds rated Ba or higher would be marked at cost, while those lower rated would be marked-to-market. Thus as suggested by the analysis of Flandreau (2010) this established the first instance of national rating-contingent regulation, but did not definitively establish the "investment grade" barrier at the Baa level or prevent investment in securities below any specific threshold.

The clear establishment of what we now know as the investment grade barrier at "Baa" occurred in the spring of 1936. On February 15th, 1936 the OCC issued a ruling stating that national federal reserve member banks could not invest[40] in "speculative" securities as indicated by at least 2 (out of 4) rating agency manuals, where speculative was interpreted by Moody's in their weekly release to

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non-compete agreement with Babson and Putney as much as it was a response to a market demand for a simple rating system for corporate debt.

[37] In the 1980s the ratings were refined to include a "+" and "-" next to each rating category thus effectively doubling the number of rating buckets (Tang 2006).

[38] Standard Statistics and Poor's Publishing would merge in 1941 to become the name we associate now Standard & Poors.

[39] Gilbert Harold (1938) even noted that there was trader who was nicknamed "Triple-A James" because he would only buy securities with the highest rating of "Aaa".

[40] It is worth noting that the ruling applied only to the purchase of speculative corporate bonds, not bonds already held on the balance sheet of banks. This is critically important since the passage of this ruling did not require a mass selling of speculative grade bonds on the part of the banks.

constitute all bonds rated "Ba" (or the equivalent for the other rating agencies) or lower[41]. The Securities Tabulation Corporation of New York in response to this ruling released a report showing that about half of all bonds traded on the NYSE would no longer be eligible for purchase by member banks and more than half of all non-NYSE listed bonds would no longer be eligible. Unlike the ruling in 1931 this announcement was followed by multiple editorials in the *Wall Street Journal* and *New York Times* which were critical of the ruling in addition to numerous complaints by bankers[42].

Contemporaneous accounts also began to take note of the effect the regulation had on firm behavior. In particular, the *New York Times* noted just a month after the announcement that a firm avoided issuing bonds they knew would be designated as "speculative" by the rating agencies.

> *A conspicuous example of pre-offering rating occurred with the proposed issue of $40,000,000 of Jones & Laughlin Steel Corporation 4 per cent bonds...Two leading agencies rated these bonds just below the 'line' of eligibility as investments for member banks. While it is not held that these ratings were solely responsible for the original postponement of the offering, some observers strongly believe they played an important part in it.*
>
> New York Times, March 22, 1936

It appears that Jones & Laughlin Steel Corporation may have postponed its offering after it discovered it would be rated just below the eligibility line for investment grade. Consistent with this interpretation, Jones & Laughlin Steel still made the offering a month later in April of 1936 but was only able to issue $30 million instead of the original $40 million, but in doing so was able to attain a Baa, or investment grade, rating. It is reasonable to suppose that the additional $10 million may have been invested in positive NPV projects by the firm which were foregone, because of the inability (or perhaps the unobserved counterfactual cost) of issuing debt designated as "speculative" grade. It is perhaps not surprising then that in 1938 Gilbert Harold noted that "it is unanimously asserted by the ratings agencies that the use of bond ratings today is greater than ever before and that the use and reliance on the ratings is growing year by year".

2.2 The Importance of Institutional Investors

Just as they do today institutional investors constituted the majority of investors in corporate bonds[43]. Goldsmith (1958) shows that in 1939 about 65% of all corporate debt was held by institutional investors, almost all of which was held by commercial banks, life insurance companies, and trust departments. In the market for the primary issuance of corporate debt, institutions, and especially banks,

[41] This ruling was quickly extended formally to state member banks as well in a letter sent February 26th, 1936.
[42] "Banks oppose eligibility rules for investments", Wall Street Journal, March 13, 1936; "Security regulations opposed by bankers", Wall Street Journal, June 25, 1936
[43] Based on estimates from the Flows of Funds Accounts in the United States.

played an even bigger role. About a month after the Comptroller announced restrictions on investment in speculative bonds by Reserve Member banks the *New York Times* made a special note of the importance of banks in the primary issuance market for corporate bonds.

> *The importance of banks as outlets for new securities has seldom been more pronounced than now. The greatest proportion of almost all the new bond issues marketed in the last six months has found its way into the vaults of banks, insurance companies or other institutional buyers. It is estimated that <u>85 to 90 per cent of recent bond offerings has been absorbed by those buyers, of which Reserve Bank members have accounted for the largest part.</u>*
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> New York Times March 22, 1936

Primary issuance placement information is limited, but based on this excerpt we can ascertain that in the mid-1930s banks likely constituted the plurality and perhaps even the majority of demand for primary issuance of domestic corporate bonds. Therefore, their exclusion from investing in speculative corporate bonds, an area where as sophisticated investors they held even more importance than corporate bonds in general, was a large shock to the demand for speculative grade bonds.

While over the 1930s insurance companies and trust companies became larger investors in all asset classes, even in 1939 Moody's noted that the movement of banks out of bonds could not be easily matched by movement in by existing institutional investors.

> *It may be that some banks could successfully shift bonds to insurance companies and other non-bank buyers. Considering the volume of bonds held by all banks, it is unlikely that all the banks could successfully shift any considerable amount of bonds to nonbank buyers.*
>
> Moody's Investor Services (1939)

As Moody's noted non-bank buyers were unlikely to be able to easily move into the bonds held by banks. Harold (1938) notes that while insurance and trust companies were not usually officially restricted from investing in speculative securities they were oftentimes strongly discouraged in the form of increased reserve requirements and "suggested" guidelines[44]. For example New York State required speculative corporate bonds to be entered at market values, while lower risk bonds could be held at face value for reserve requirements.

The hesitation on the part of non-bank institutions was an artifact of not only explicit regulations and contracts, but also a response to the demands of their depositors and the market crash in the early part

[44] Even in the 1920s investment trusts used ratings to reassure investors of the quality of their portfolios (Flandreau 2010). For instance Robinson (1929) points out that the trust company Untied States Shares Corporation in 1927 signaled the soundness of its investment policy when it was initially created by stating that no securities held would be rated below Moody's B, at most 10% securities would be below Moody's Ba, at most 50% would be below Moody's Baa, and at least 20% would be above A.

of the 1930s. In 1905 the Hughes/Armstrong Law passed in New York State[45] restricted insurance companies from purchasing stocks or unsecured corporate debt[46]. While larger numbers of banks failed during the early 1930s very few insurance companies failed and of those practically all paid back deposits in full. The growth in life insurance company assets was spurred in large part by the view of individual investors that they constituted as safe haven for savings outside of the traditional banking system. Insurance company holdings were published annually in reports readily available to the public, which provided additional incentives for them to keep "speculative" trading to a minimum[47]. The inability of other large non-bank investors to enter the speculative bond space was responsible for substantial market segmentation in speculative bonds, so that the shock caused by the 1936 ruling had a long-run effect on aggregate demand for speculative bond issues.

I would expect this reduction in credit demand to be particular difficult for firms reliant on external financing, especially corporate debt placements with banks, which at the time varied substantially by industry. Most manufacturing firms financed themselves using internal cash flows, while transportation companies, such as railroads, and utilities were highly dependent on external financing. According to Koch (1943) manufacturing companies retained 58% of their savings from 1930-1933 to finance operations, while transportation and public utilities retained only 37%. Also while data is not available for transportation companies he finds that from 1921-1929 and 1934-1939 for large manufacturing firms 89% and 81% respectively of all financing was generated internally. He also shows that from 1900-1934 almost all net corporate debt issued by railroads was purchased by banks, while for utility companies this was about 53% and for other industrial companies it was only 19%. Calomiris and Hubbard (1995) also look at the revealed preference for internal financing by looking at the response of firms to undistributed profits taxes in 1936 and 1937 and find that manufacturing firms were likely to rely heavily on internal financing, even in the presence of large incentives to reduce their retained earnings. Based on the variation in reliance on external financing by industry I would expect non-manufacturing firms, and especially those in transportation or utilities industries to be more affected by the ruling restricting investment by banks.

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[45] Insurance companies were regulated at a state level, but New York State required all insurance companies offering insurance in the state to meet New York State regulatory requirements. The result was that a large percent of all insurance companies were required to meet the regulations in New York. For a sample collected in 1937 from the New York Insurance Report of the Superintendent of Insurance 47% of all insurance companies were headquartered in NY and more than 2/3rds were subject to New York State regulations.

[46] While these policies would be relaxed in 1928 to allow for investment in preferred stock and unsecured debt, common stock was still restricted and insurance companies generally held far less risky asset holdings than banks in the time leading up to the 1929 market crash.

[47] For example, one of the most sophisticated and by far the largest life insurance company, Metropolitan Life Insurance, kept active trading to a minimum. Based on author computations, in the 2 years from 1935 to 1937 over 80% of all corporate bond positions were identical.

2.3 Liquidity of 20th Century Bond Markets

In the modern period bonds are traded predominantly in opaque over-the-counter (OTC) markets, while stocks are traded on organized exchanges. The lack of transparency and liquidity in corporate bond prices makes it difficult to carry out high frequency analysis of bond price movements looking back even two or three decades[48]. This was not always the case. Until the mid-1940s the majority of trading in stocks *and* bonds occurred on organized exchanges with most listed on either the New York Stock Exchange (NYSE) or the New York Curb Exchange (NYCE)[49]. Based on Hickman (1957) we know that in 1936 approximately 78% of all corporate bonds were listed on a major exchange and from the *New York Times* in February 1936 average daily trading volume for U.S. stocks and corporate bonds on the NYSE were $2.6 million and $15.0 million respectively. Since bonds, like stocks, traded in large volumes on organized exchanges there was substantial transparency and liquidity in prices. Despite the enormous technological advances that have occurred over the last half-century Biais and Green (2007) find that because bonds were trading on exchanges trading costs for corporate bonds in the 1940s were as low or lower than they are even today. Therefore in some ways analysis of the current movement of corporate bond prices might have more in common with the 1930s than much more recent history.

3 Data Description

3.1 Credit Ratings

For all firms with bond prices in 1936 any new bonds issued, old bonds dropped, or ratings changes were entered at an annual frequency from *Moody's Industrial Manual, Transportation Manual,* and *Utilities Manual* and all ratings changes (included new and withdrawn ratings) at a weekly frequency from *Moody's Investment Weekly*[50]. Moody's issued bond ratings not firm ratings so there is some discretion in how to assign the firm rating associated with a given equity security. I need to assign one rating to each firm which can be used to match to the stock price. The objective is to measure the rating a

[48] The Lehman Brothers Fixed Income Database and similar databases which go back to the 1970s are only available at a monthly frequency (Acharya, et al. 2010)

[49] The New York Curb Exchange was the precursor to the modern American Stock Exchange.

[50] In figure 1 I show the distribution of corporate bond credit ratings given by Moody's Investors Services taken from the 1932 and 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* for 1,632 bonds actively traded on the New York Stock Exchange and New York Curb Exchange. Based on these ratings 30-35% of these bonds would not have been eligible for investment following the 1936 ruling by the Office of the Comptroller of the Currency (OCC) ruling restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade).

firm would receive if it tried to issue a bond after the event date. Since new bonds are typically issued subordinate to existing debt a firm's lowest bond rating is a good proxy for the best rating they could expect to receive if they issued new bonds, so I use this as the measure of a firm's rating[51].

3.2 Market Prices

All equity market data comes from the Center for Research in Securities Prices (CRSP) for all New York Stock Exchange-listed stocks for 1935-1936. Summary statistics on the matched sample of CRSP with Moody's manual ratings can be seen in table 1 for all 721 matching firms. Of these 211 are rated and 98 can finance themselves with investment grade debt. As we would expect firms with speculative grade debt tend to be smaller and have more volatile stock returns than firms able to issue investment grade debt. They also have similar market betas, but speculative firms tend have higher loadings on SMB and HML, which would be consistent with investment grade firms being large value firms, while speculative firms tend to be smaller high growth firms. This is confirmed by running a pooled regression of portfolio returns on Fama-French Factor excess returns in 1936 where we can see in table 2 that just as suggested by Davis, Fama, and French (2000) all 3-factors, market excess returns, size, and value are significant in the first half of the 20th century just as they are in the second.

Since almost all corporate bonds were traded on exchanges in the 1930s transactions on the two major exchanges, the New York Stock Exchange (NYSE) and New York Curb Exchange (NYCE), were published on a daily basis in the financial section of the *New York Times*. Comparing a sample of entries between the *New York Times* and a number of other periodicals confirmed at least the consistency across periodicals of the quoted values. From these pages I manually collected company names, bond prices, changes, volumes, and descriptions for the time period surrounding the event date. Data was generally collected at a monthly frequency based on week-end data[52], except for February 1936 where data was collected at a daily frequency.

3.2 Balance Sheet Information

To look at the long-run real effects of the comptroller's ruling I hand collect data at an annual frequency on the book value of total assets, long-term debt, and net property, plant and equipment (PP&E) from 1932-1940 for 422 firms that appear in the 1935 *Moody's Industrial Manual, Moody's*

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[51] While this is one intuitive definition few firms have multiple ratings that straddle the investment grade barrier so the specific assignment method, such as using the maximum rating instead, does not cause any significant variation in results.

[52] The week-end data means that all bonds with any transactions in the week are included even if transactions did not occur on the specific day collected.

Transportation Manual, or *Moody's Utilities Manual* and have NYSE stock price information available for the same period in CRSP. From table 3 we can see that the book value of long-term debt and net PP&E constitute around half of all total firm book value in 1935.

3.3 Insurance Company Portfolio Holdings

Insurance companies were the fastest growing institutional investor class in the 1930s in addition to being one of the most transparent. *The Annual Report of the Superintendent of Insurance of New York State* published every asset held by every insurance company headquartered in that state at the end of year. In addition to legal requirements on the accuracy of the positions there was also a publication at the time called *The Institutional Holdings of Securities* which published and sold the positions of all insurance companies all over the country. Since this book was used by traders to find institutions they could buy bonds from there were strong incentives to have accurate information. Therefore it is comforting to note that all positions cross-checked across all years and books between *Institutional Holdings* and the superintendent match. The other convenient feature of insurance companies at the time was the high concentration of assets in just a few companies. For example, just Metropolitan Life and New York Life held about 1/3rd of all U.S. insurance company holdings. For these two firms I collected every corporate bond held at the end of 1935 and 1937 from the *Annual Report of the Superintendent of Banking*. These bonds were then matched by company and bond information to the other previous bond and firm-level data sources.

3.4 Aggregate Bond Quantity Data

In 1937 the National Bureau of Economic Research (NBER) commissioned a study of the effects of the 1936 ruling entitled "The Investment Experience of Banks in Selected Cities, 1926-1936“. After checking with the archivist for the NBER it appears that this study was either never completed or has been lost. In that spirit but as part of a different NBER study Braddock Hickman continued the work of Harold Fraine's 1937 dissertation and collected an incredibly comprehensive database on bond issuance and default from the early 1900s to the 1940s covering over 90% of all issued bonds with detailed data on contract details, par amounts, ratings, state legality, et al. This data was aggregated and summarized in a number of papers, but unfortunately all the original data was lost. The data collected by Hickman includes all bonds rated and unrated, listed and unlisted, and, as far as I am aware, represents the most comprehensive data on debt issuance broken down by rating that exists for the period. I complement this data to understand the quantity issuance response of new debt for all firms that issued debt at either a rating of Baa or Ba after 1936 so I also hand collect information on issuance size from 1936-1940 taken from *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* for all bonds that had initial ratings of Baa or Ba.. More details on this data is available in summary table 9.

4 Empirical Predictions and Methodology

4.1 Predicted Short-run Response to Announcement

Firm equity value should decline in the days just after the announcement of the investment restrictions in 1936 if the following are true:

1) the announcement is an unexpected shock
2) market participants believe the shock to bank demand will alter the aggregate demand for speculative bonds
3) the change in demand decreases the financing ability of firms who previously used speculative financing in the bond market
4) the reduced external financing ability either constrains investment in positive NPV projects, increases risk/cost of default, or increases the explicit cost of debt capital.

If all four conditions hold then we would expect that firms that finance themselves with speculative debt will have negative abnormal cumulative adjusted returns (CARs) in the days following the announcement. We might also expect that as the information is incorporated into prices there would be a jump in the trading volume on the exchange. If the first 3 points above hold for equities and the inability to issue debt increases the default likelihood or reduces the expected recovery of debt holders[53] then we would expect bond yields to rise (or equivalently prices to fall). If bond prices do not change, but equity prices fall then this would be consistent with firms missing out on positive NPV investment opportunities because of the constraints on debt financing following the ruling.

For intuition I first run separate pooled regressions by category (ex. equally weighted average stock returns of just investment grade firms) and plot the cumulative residual from the following specification

$$R_t = \alpha + \beta_{\mathrm{Mkt}} R_{Mkt,t} + \beta_{\mathrm{HML}} R_{HML,t} + \beta_{\mathrm{SMB}} R_{SMB,t} + \epsilon_{i,t} \tag{1}$$

where R is the excess returns for the specified portfolio, on day t, after adjusting for the Fama-French factor controls[54], excess market returns, *Mkt*, high minus low book-to-market, *HML,* and small-minus-big market capitalization firms, *SMB*. Regressions coefficients are estimated based on daily data from

[53] It could also alter the covariance of these terms with the stochastic discount factor.

[54] Factor returns are taken from Ken French's website and are based on the factors in the seminal work by Fama and French in 1993.

1/17/35-1/17/36 and all cumulative residuals are based on out of sample tests beginning one-month before the event date. As noted by Kolari and Pynnonen (2010) the standard deviation of portfolio returns can be used to assess the significance of the event-window average abnormal return, since the cross-sectional dependence that exists among returns on individual events is incorporated in the time series variation.

To control for variation at the firm-level I rerun the following panel regression of the same event study,

$$R_{i,t} = \alpha_i + \kappa\, \mathrm{E_t} + \delta\, \mathrm{S_i}\, \mathrm{E_t} + \beta_{\mathrm{Mkt}}\, R_{Mkt,t} + \beta_{\mathrm{HML}}\, R_{HML,t} + \beta_{\mathrm{SMB}}\, R_{SMB,t} + \epsilon_{i,t} \tag{2}$$

This specification allows me to include firm fixed effects to control for any time invariant difference across securities in expected returns, prior to the ruling. The only component that separates this specification from a classical event study is that I allow for an event study dummy in addition to the event study interacted with speculative grade dummy. This is to allow for the announcement to have a fixed effect, potentially beneficial, on the non-speculative grade market. In other words I look at the relative cumulative abnormal returns and determine if they are significantly different during the event period, relying on the common trends assumption of a difference-in-difference regression[55]. In one additional robustness exercise I also include in (2) 2-digit SIC code industry fixed effects interacted with event fixed effects. I also rerun the specification in equation (2) using a fuzzy regression discontinuity approach where I only compare firms whose debt ratings are just above and below the investment grade rating cut-off.

As in Fama and French (1993) I include a proxy for default risk, *DEF*, when rerunning the event study for corporate bond returns. As in the original paper I compute *DEF* in my period based on the difference between the mean return of the bonds in my sample and the risk-free rate. The specification then becomes

$$R_{i,t} = \alpha_i + \kappa\, \mathrm{E_t} + \delta\, \mathrm{S_i}\, \mathrm{E_t} + \beta_{\mathrm{DEF,i}}\, R_{DEF,t} + \epsilon_{i,t} \tag{3}$$

where *R* is the excess returns for bond *i,* on day *t*, for post-ruling dummy *E*, speculative-grade dummy *S*, and of the five original Fama-French factor controls, default risk, *DEF*. Just as with stocks if the event was significant we should expect cumulative abnormal returns for speculative bonds following the announcement which would result in δ being significantly negative.

4.2 Predicted Long-run Response to Announcement

[55] Empirical results are all robust to excluding the event study dummy and using the cumulative abnormal returns and a null of 0% rather than the difference-in-differences framework.

If the speculative bond markets are segmented or arbitrage capital is slow moving (Mitchell, Pedersen, and Pulvino (2007), Duffie and Strulovici (2011)) so that the restrictions on bank investment shift aggregate demand for new speculative corporate bond issues then market clearing suggests that speculative bond prices should decline and/or quantities should fall. The extent of the reaction of each depends on the elasticity of the supply of new corporate debt or equivalently the elasticity of credit demand. While it might be difficult to alter the amount of outstanding debt in the short-run, in the long-run firms can easily alter the supply of debt by altering their issuance size and/or frequency. If non-debt external financing costs are high though this could prevent firms from investing in positive NPV projects. Therefore if the long-run elasticity of credit demand is very elastic we would expect to see a large change in the quantity of speculative debt issued and only a small change in yields. If instead firms demand for credit is fairly inelastic in the long-run we would expect very little response to the aggregate amount of debt issuance. Whether through prices or quantities in either case we would expect the reduced amount/benefits of debt financing to slow investment and asset growth of firms requiring speculative financing. I test these explicitly in the paper by comparing the amount and size of debt issuance for speculative vs. investment grade firms, before and after the ruling, as well as the growth rate of investment and total assets.

5 Results

5.1 Short-Run Response to Announcement

5.1.1 Equity Market Value

The comptroller announced that banks would be restricted from investing in speculative grade debt in a memo sent to banks on Saturday February 15th, 1936[56]. If the news was unexpected we might expect the stock market volume to trade based on the information and volume to spike and that is exactly what we see. In fact, the first full trading day following the announcement on February 17th, 1936 is the largest daily volume on the NYSE in the two years surrounding the date (figure 2) and that week, even excluding the 17th , is the highest trading volume week in that two year period as well. In figure 3 I explore the cumulative abnormal stock returns following the announcement. Since firm ratings are likely to pick up risk differences between firms by definition we will have to be very careful throughout the analysis to test the assumption of common trends. One encouraging result is that in figure 3 prior to the announcement there is no clear evidence of statistically significant deviations between the investment and

[56] Though at this time markets were open on Saturdays it is unclear if it was announced before or after markets closed so for my analysis I include the 15th as the event date, but all analysis is robust to the use of Feb 17th instead.

speculative grade firms. Then as we would expect if this was bad news for speculatively financed firms, that week was associated with a sudden statistically significant -4% negative cumulative abnormal return for speculative grade (Ba-C) firms relative to investment grade (Aaa-Baa) firm. The decline in speculative grade (Ba-C) firms is significant whether comparing to investment grade (Aaa-Baa) firms or with the null of 0% cumulative abnormal returns. By contrast investment grade (Aaa-Baa) firms rise slightly after the announcement but the gains are not statistically significant at conventional levels.

The decline in the value of firms requiring speculative financing is confirmed in the results of the panel regression specification 2 shown in table 4. Column 1 estimates a 69 basis point *per day* abnormal return for firms requiring speculative debt financing over the first six days following the event, giving a cumulative decline of 4.1%. Consistent with figure 3 there is no statistically significant change in stock market value for firms able to finance themselves with investment grade debt. In columns 2-6 I show that results are robust to the choice of the method of clustering standard errors, the choice of Fama-French factors as a control, and industry x event fixed effects. It seems reasonable to assume that since the event had a large effect on the market overall it might make sense to cluster errors by day to account for time variation in the residual variance. In table 4 columns (2) and (3) we can see that standard errors are robust to firm, day, or no clustering. We also know from Thompson (2010) that since double clustered standard errors are equal to $\hat{V}_{firm} + \hat{V}_{time,0} - \hat{V}_{white,0}$ clustering by firm in this case is a more conservative method than either clustering by time or by both firm and time, so I use that as a baseline for all other specifications. There could also be some concern that the choice of risk-adjustment could be driving results. In table 4 columns (4) and (5) I address this by considering no risk adjustment and using only the market excess return as a factor. In both cases speculative grade firms continue to underperform and the coefficient estimate is within error. There is of course still a concern that there are latent risk factors not being correctly adjusted for, which happen to move coincidentally in the same week as the comptroller ruling. As even another robustness check to avoid concerns about industry specific risk news in the week following the announcement[57] I re-run the baseline regression with 2-digit SIC industry dummies interacted with the event fixed effects so that the regression is only looking at return differences between investment grade and speculative grade firms in the week following the announcement within industry. Again speculative grade firms continue to underperform in the following week.

If the results are driven by the discontinuous difference at the investment grade barrier rather than unobserved risk differences between investment grade and speculative grade firms then we should expect a difference in returns for firms near the investment grade border who have similar risk profiles, but different ratings. These predictions are consistent with the results we observe in in table 5 column 1 and

[57] For example Monday February 17th, 1936 included the announcement of a Supreme Court case which affected utilities companies.

figure 4 using a fuzzy regression discontinuity approach where I compare firms just above (Baa) and below (Ba) the investment eligibility criteria. Ba (speculative grade) firms have negative cumulative abnormal returns of ~2% while Baa (investment grade) firms have positive cumulative abnormal returns of ~3%. The difference between them is statistically significant as is the speculative grade returns relative to the null of 0%. Since all rating differences should contain latent risk information, but only the investment grade barrier should matter for the ruling, I consider a placebo test where I compare buckets within investment grade and within speculative grade to see if they yield statistically significant event returns. In columns (2) and (3) we can see that despite the differences in ratings, and factor loadings as we can see in table 1, the event interacted with speculative grade dummies are not significant, while for column (1) which compares just across the investment grade barrier they are significant. We should also expect that for firms without debt, even with high risk, should also be less likely to be affected since they don't use debt financing. In figure 5 and table 7 columns 3 and 4 we can see that whether we consider all firms without debt or the riskiest quartile of firms without debt, *No Debt High Vol*, the firms who do not use debt financing do no significantly underperform either investment grade firms or the 0% benchmark following the comptroller announcement[58]. In columns 1 and 2 of table 6 I show that these results are robust to restricting the choice of pre-event period to 2 months before the announcement or using the maximum instead of the minimum rating of all firm debt issues. In columns 5 of table 6 and figures 3-5 I show that these declines are persistent and significant after the announcement, rather than showing signs of mean reversion after the initial decline in stock market value.

Since non-manufacturing firms, and especially those in transportation or utilities industries, were more reliant on external financing at the time I would also expect to see that the decline in firm value is largest among these firms. In fact, in table 7 column 1 I show that all of the decline in firm value following the announcement is being driven by non-manufacturing firms that require speculative financing. Since most non-manufacturing firms were either in the transportation or utilities sector it is not surprising that interacting the event with dummies for being in either of these industries yield similar results. Declines in equity market value for firms requiring speculative bond financing following the restriction of bank investment is concentrated in industries reliant on external financing.

5.1.3 Explicit Cost of Debt Financing

In contrast to the stock market there is no spike in trading volume on the New York Stock Exchange following the announcement (figure 6). We also see little relative price movement among

[58] Results plotting unrated bonds decline following the announcement just like speculative bonds, but even though the magnitude is similar the decline is not statistically significant given the small number of observations.

speculative relative to investment grade bonds in figure 7 or figure 8. This is confirmed in table 6 where I run the formal event study regression and find no significant drop in speculative grade bond prices relative to either 0% or investment grade. Since we observe a fall in equity values, but no rise in associated bond yields, this suggest that the ruling did not alter the risk of default or the explicit costs of debt financing, but rather firms requiring speculative finance were missing out on positive NPV investment opportunities because of the constraints on debt financing following the ruling. If this is true then we would expect to also see a decline in debt issuance among these firms.

5.2 Long-run Effects of Ruling

5.2.1 Bond Issuance Size

From figures 9 and 10 we can see evidence consistent with banks slowly moving out of their speculative grade bonds, while insurance companies slowly moved in. To the extent that insurance companies were able to completely fill the gap left by banks we would not expect to see any quantity response from firms. If on the other hand, as we would predict insurance companies were only able to slowly move into the speculative bond space and firms supply of debt is not perfectly inelastic we would expect to see a decline in debt issuance. Consistent with this prediction I show in figure 11a that aggregate investment grade issuance increased relative to speculative grade in the years after the 1936 ruling. For example, Aaa and Ba issuance moved almost in lockstep in the years from 1930-1935, but Aaa issuance rose more than 3 times faster from 1936-1940. From figure 11b and 12 we can see that this was not driven by a wedge in the number of issues but by the average issuance size. Just as was the case for Jones and Laughlin Steel Corporation it is likely the firms initially only able to issue speculative grade reduced their issuance sizes to become eligible for investment by banks. Investment grade bonds average issuance size increased after the ruling, with 3 out of the 4 increasing more than 75% , while speculative grade firms on average experienced much smaller growth and Ba bonds even decreased in size. The Hickman (1957) data doesn't have sufficient information on standard deviation to allow for a formal test of this difference, but based on the standard deviation in the sub-sample of issuances I hand collected shown in table 9, a difference-in-differences in the Hickman data would be statistically significant at conventional levels.

5.2.2 Real Effects

Based on the decline in equity market values, but lack of movement in bond yields in the event study analysis surrounding the ruling we would expect that firms were losing out on potential positive net present value investment opportunities. This was further confirmed by anecdotal and empirical evidence that firms requiring speculative financing reduced the size of their debt issuances following the announcement in order to order to become investment grade. In tables 10 I show additional evidence consistent with real long-term cost to firms requiring speculative financing, since even controlling for firm and industry fixed effects following the ruling, firms requiring speculative financing experience slower growth rates of book debt, assets, and investment. Based on table 10 these firms issue 21% less debt and grow net PP&E and assets 6.4% and 7.7% slower, respectively, over the years 1936-1940. Since in column 4 we see no change in the book debt to total assets ratio it suggests that the entire reduction in relative investment and asset growth can be explained by the fall in long-term debt financing. This large decline in long-term credit supply is consistent with previous results shown (Lemmon and Roberts (2009) and Chernenko and Sunderam (2012)), and when combined with the event study results are suggestive of persistent long-term costs to non-financial firms of restricting bank investments. Unlike the event study in the days immediately following the ruling, these long-term estimates are more likely to be confounded by coincident changes in the macro-economic environment in the years following the ruling. To alleviate this concern I rerun the analysis in table 11 using the same fuzzy regression discontinuity approach as before, where I compare firms just above (Baa) and below (Ba) the investment grade cut-off. Again I find firms requiring speculative financing have significantly lower growth rates of long-term debt and assets[59].

7 Conclusion

In this paper I explore the costs to non-financial firms of regulations intended to curb excess speculation by restricting propriety trading by banks. I use an unanticipated 1936 ruling preventing banks from purchasing securities rated as speculative by ratings agencies, as a plausibly exogenous shock to participation of banks in the speculative grade corporate debt market. I find a 3-5% negative cumulative abnormal return in the equity value of firms requiring speculative financing in the week following the announcement. This decline is concentrated among firms in industries that relied heavily on external financing at the time, but I find no commensurate decline in bond prices. Taken together these results suggest that firms that require speculative financing miss out on potentially beneficial future investment opportunities because of the inability to place bonds with banks. Consistent with this explanation I find evidence that these firms had smaller debt issues, less long-term debt, and slower investment and asset growth in the years following the ruling.

[59] The growth rate of net PP&E is measured with substantially more noise than either debt or total assets, so it may not mean much that the results are no longer significant in this specification. This is especially true since results in columns 3 of tables 10 and table 11 are no statistically different from each other and asset growth, of which PP&E is the largest component still has a statistically significant decline.

Overall my results are consistent with previous research which document the significant role institutional investors, such as banks, can play in the credit market and the influence their absence can have on non-financial firm financing and investment behavior. I also build on this literature by showing that the exclusion of banks from speculative investing can prevent firms from engaging in positive net present value investment opportunities and cause a substantial persistent downturn in growth among firms who finance their operations using financial markets. In a letter to US Treasury Secretary and chairman of the Financial Stability Oversight Council, Jack Lew, about the bank investment restrictions imposed by the Volcker Rule Congressman Jeb Hensarling insisted that Lew "ensure that regulation does not imperil, impeded or disrupt the US capital markets"[60]. Based on the historical evidence presented in this paper such market dislocations are certainly possible. Regulators may need to reconsider the costs to non-financial firms of policies that attempt to curb speculative trading by banks and academic research quantifying the trade-offs between bank risk taking and non-financial firm financing costs are likely to continue to be an important area for future work.

[60] "Lew Challenged over Volcker rule impact", July 10, 2014. Financial Times.

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Figure 1. Bond Rating Distribution for 1932 and 1935

This is the distribution of corporate bond credit ratings given by Moody's Investors Services taken from the 1932 and 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* for 1,632 bonds actively traded on the New York Stock Exchange and New York Curb Exchange. Based on these ratings 30-35% of these bonds would not have been eligible for investment following the 1936 ruling by the Office of the Comptroller of the Currency (OCC) ruling restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade).

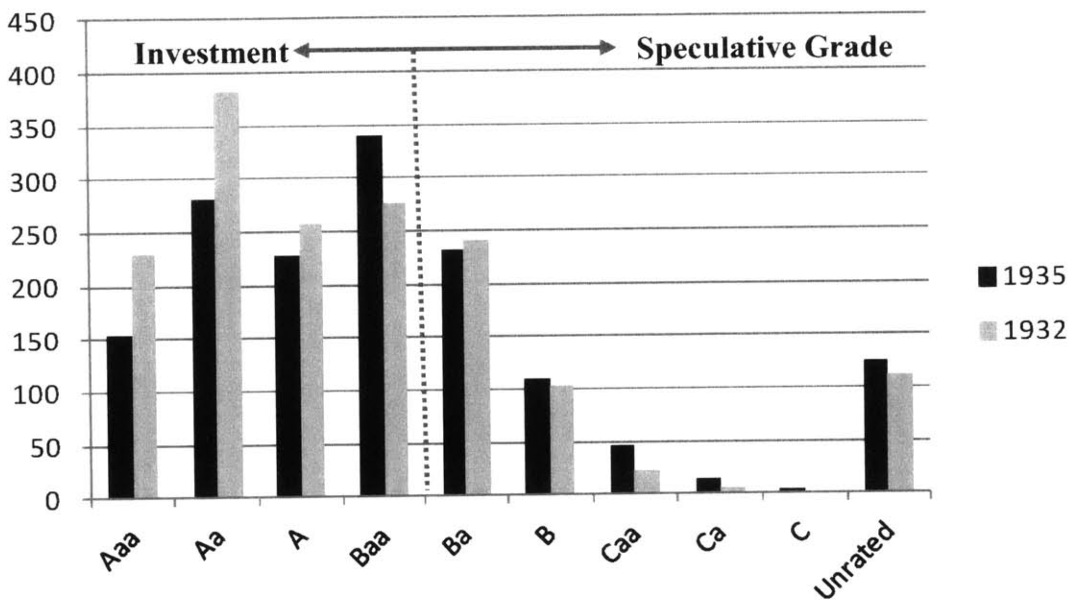
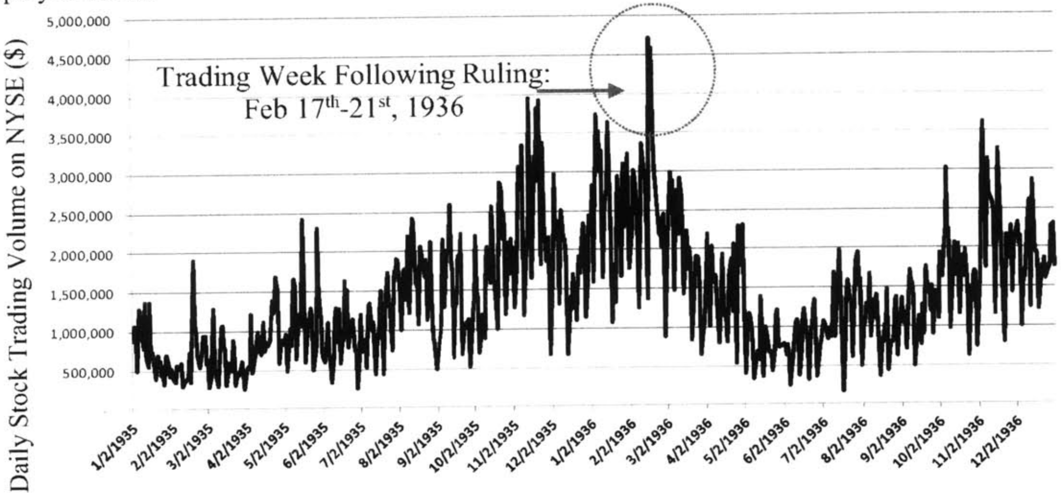


Figure 2. NYSE Daily Stock Volume ($) 1935 and 1936

The sum of all daily dollar trading volume of U.S. stocks on the New York Stock Exchange is plotted for all trading days in 1935 and 1936. The first trading week following the February 15th, 1936 comptroller restriction on speculative investment is highlighted. All data on stock trading volume is taken from the Center for Research in Security Prices (CRSP). Summary statistics covering the period 1935-1936 are displayed below.



Summary Statistics for NYSE Daily Stock Volume ($) 1935-1936

		Date
Mean	1,455,619	
Median	1,310,660	
Stdev	793,323	
Max	4,718,448	2/17/1936
2nd Highest	4,578,280	2/19/1936

Figure 3. Cumulative Abnormal Stock Returns: Investment (Aaa-Baa) vs Speculative (Ba-C) Grade

These figures display the mean cumulative abnormal returns and t-statistic from the residual of the 3-Fama French factor regression shown in equation (1), but run at a firm level in the time period surrounding the ruling by the Office of the Comptroller of the Currency (OCC) restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). Standard errors are clustered at the day level and within each investment grade group. The estimation period runs from Jan 1st, 1935 - March 17th, 1936 and results are displayed for a 1-month window before and after the comptroller ruling on Feb 15th, 1936. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

Figure 3a. Mean of Cumulative Abnormal Stock Returns by Bond Grade

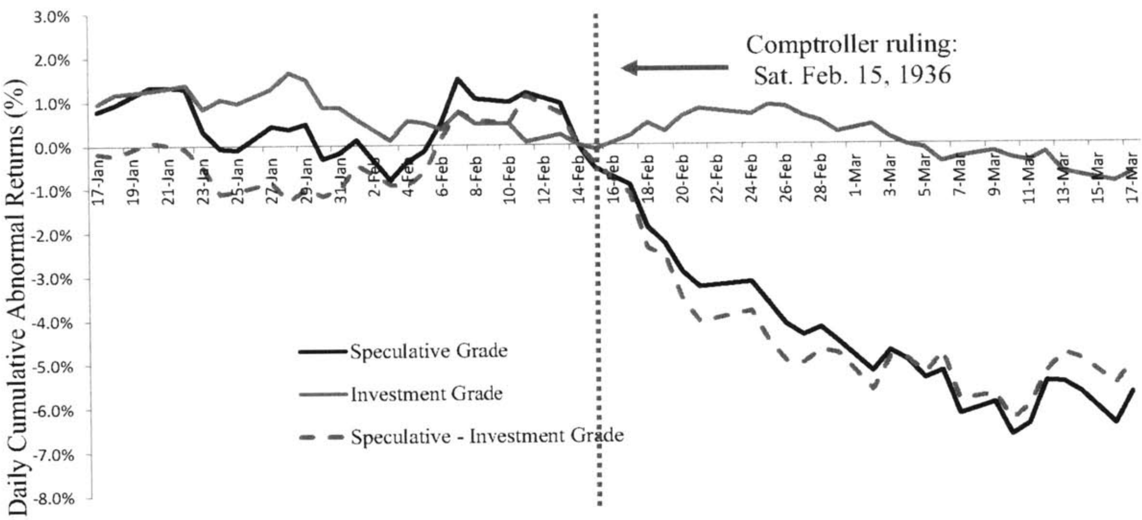


Figure 3b. T-Statistic of Cumulative Abnormal Stock Returns by Bond Grade

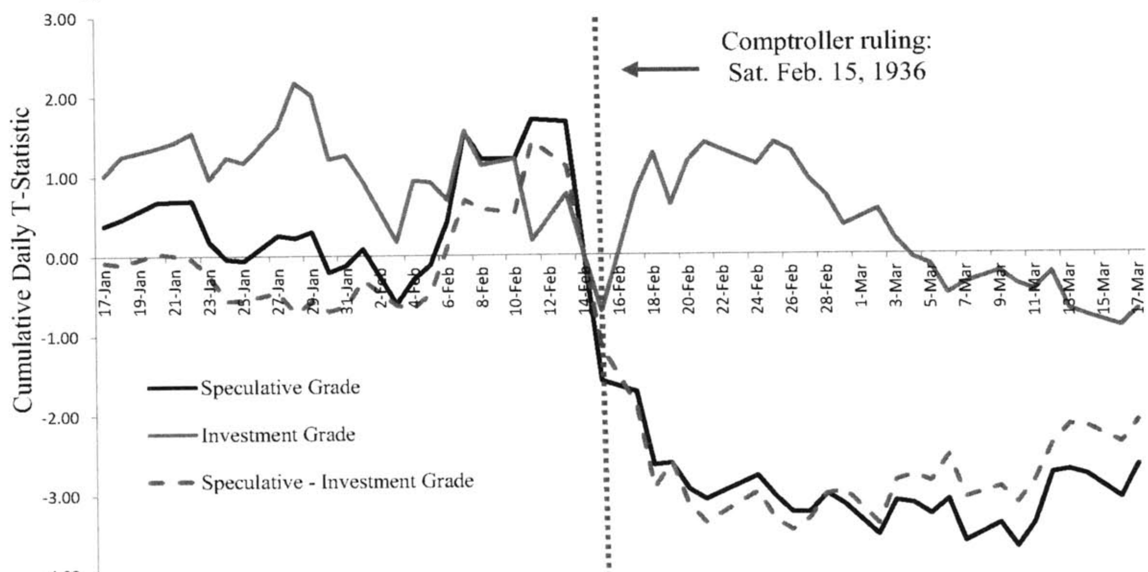


Figure 4. Cumulative Abnormal Stock Returns: Investment (Baa) vs Speculative (Ba) Grade

These figures display the mean cumulative abnormal returns and t-statistic from the residual of the 3-Fama French factor regression shown in equation (1), but run at a firm level in the time period surrounding the ruling by the Office of the Comptroller of the Currency (OCC) restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). Standard errors are clustered at the day level and within each investment grade group. The estimation period runs from Jan 1st, 1935 - March 17th, 1936 and results are displayed for a 1-month window before and after the comptroller ruling on Feb 15th, 1936. All bonds rated either Baa or Ba by Moody's Investor Services in 1935 are included in the analysis. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

Figure 4a. Mean of Cumulative Abnormal Stock Returns by Bond Grade

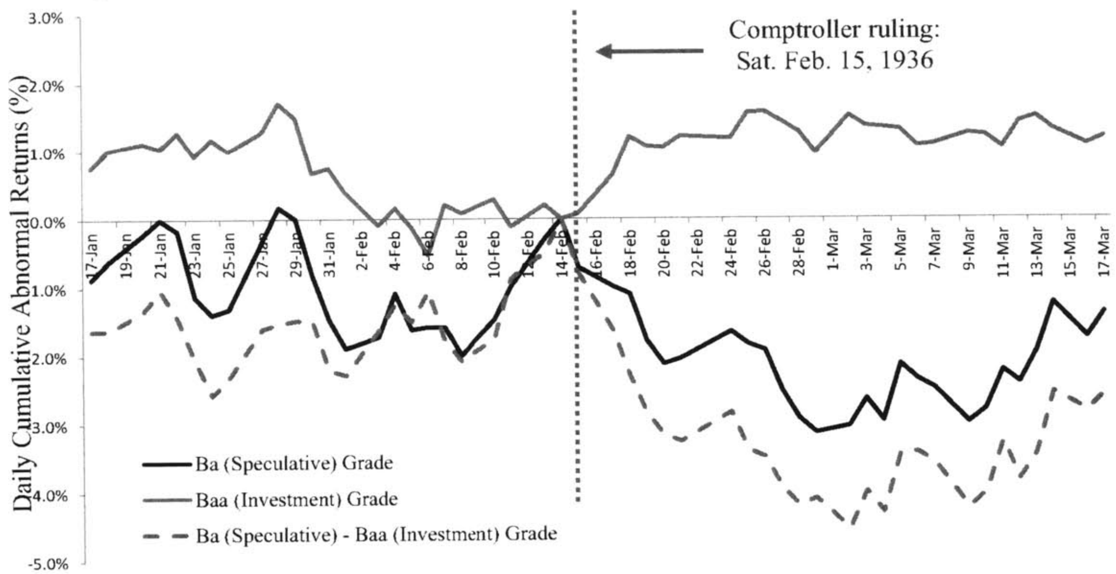


Figure 4b. T-Statistic of Cumulative Abnormal Stock Returns by Bond Grade

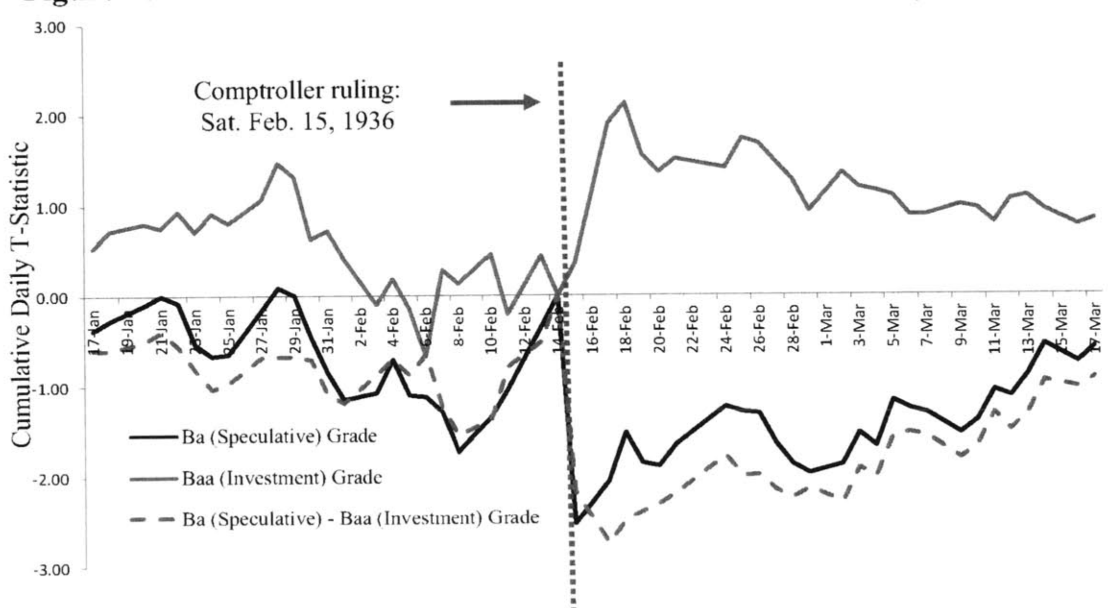


Figure 5. Cumulative Abnormal Stock Returns: No Debt Firms as Control

These figures display the mean cumulative abnormal returns and t-statistic from the residual of the 3-Fama French factor regression shown in equation (1), but run at a firm level in the time period surrounding the ruling by the Office of the Comptroller of the Currency (OCC) restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). Firms without debt and with high volatility are plotted separately as a placebo test to show that stock price movement is not driven by the release of macroeconomic news that differentially affects high risk stocks. Standard errors are clustered at the day level and within each investment grade group. The estimation period runs from Jan 1st, 1935 - March 17th, 1936 and results are displayed for a 1-month window before and after the comptroller ruling on Feb 15th, 1936. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

Figure 5a. Mean of Cumulative Abnormal Stock Returns by Bond Grade

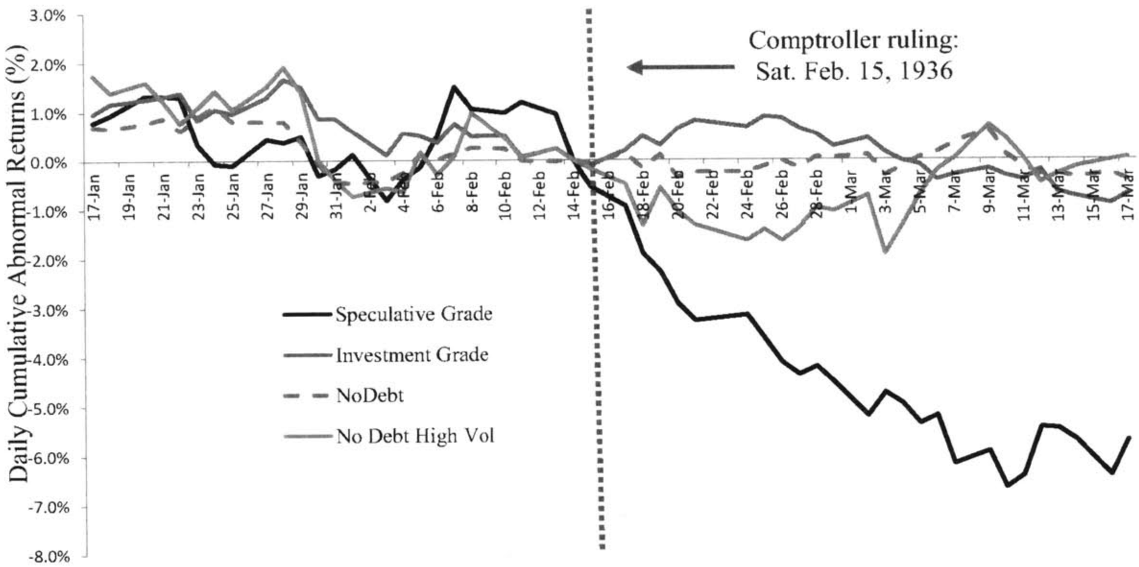


Figure 5b. T-Statistic of Cumulative Abnormal Stock Returns by Bond Grade

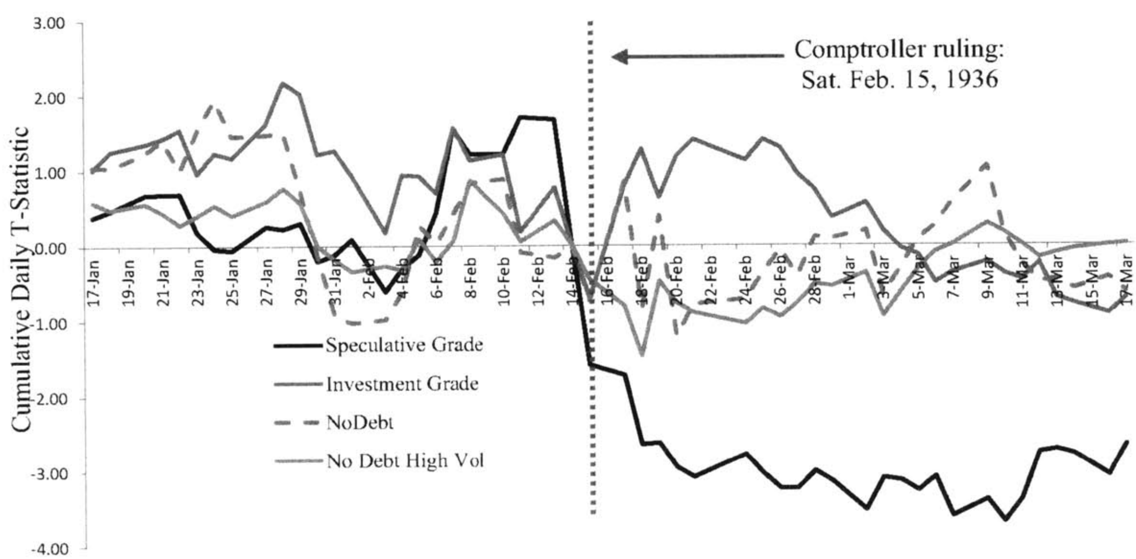


Figure 6. NYSE Daily Domestic Corporate Bond Volume ($) February 1936

This figure shows that despite the increased trading among NYSE stocks surrounding the ruling by the Office of the Comptroller of the Currency (OCC) restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade), there was no substantive change in NYSE bond trading volume. The sum of all daily domestic corporate bond trading ($) on the New York Stock Exchange taken from the *New York Times* for February 1936 bond price data described in detail in section 3.1.1 and displayed in appendix a1 for every non-Saturday market business day. Saturdays are excluded to prevent noise since trading was only half-day. All bond ratings from Moody's Investors Services and are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

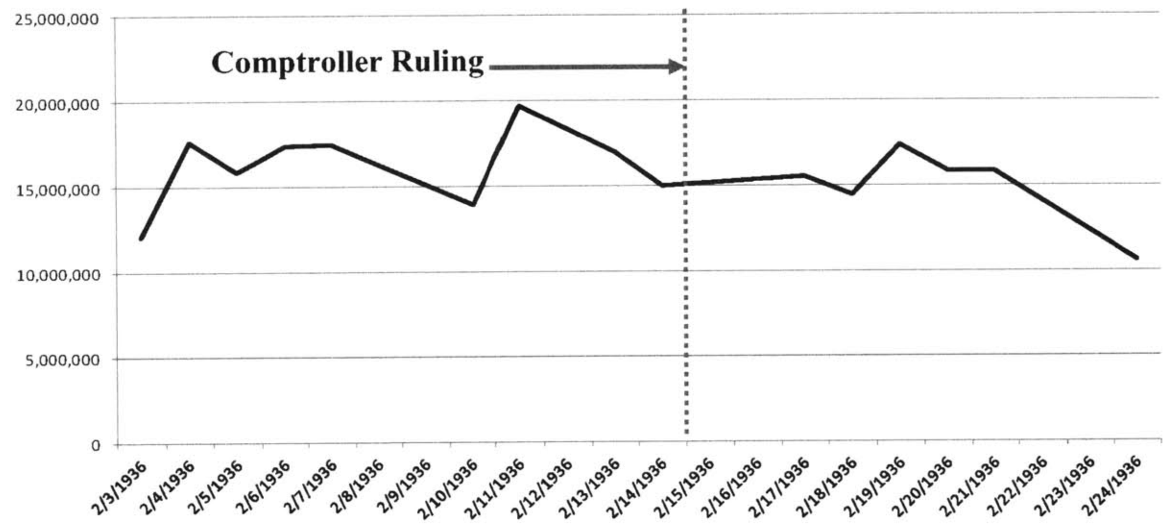


Figure 7. Cumulative Bond Returns by Rating around Announcement

The median cumulative bond return for all speculative and investment grade bonds listed on the *New York Stock Exchange* and *New York Curb Exchange* in the days surrounding the February 15th, 1936 ruling by the Office of the Comptroller of the Currency (OCC) restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). All bond ratings from Moody's Investors Services and are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

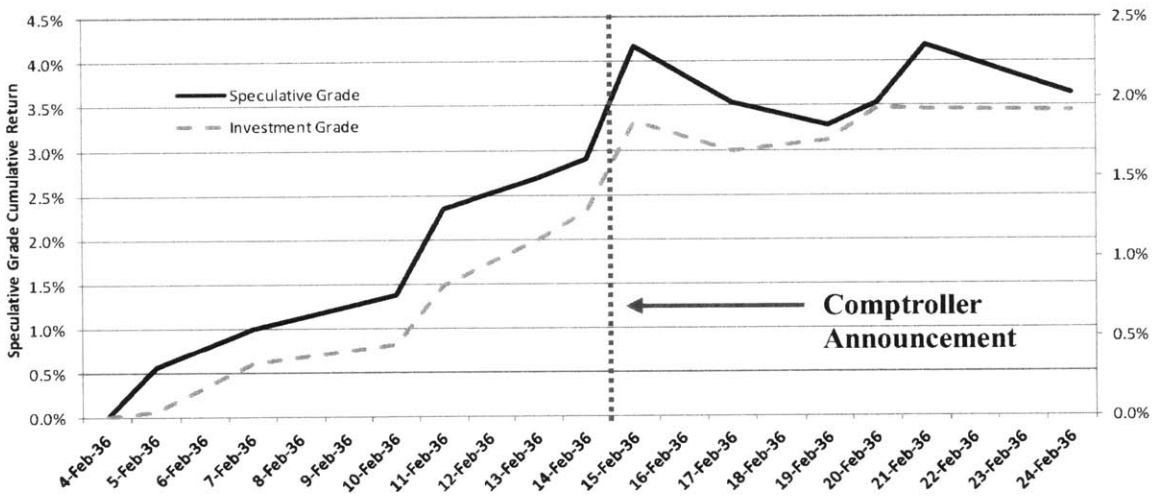


Figure 8. Bond Yields by Rating 1932-1940

The mean bond yield for all speculative and investment grade bonds listed on the *New York Stock Exchange* and *New York Curb Exchange* in the year surrounding the 1936 comptroller announcement of investment restrictions for commercial banks. All bond ratings from Moody's Investors Services and are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

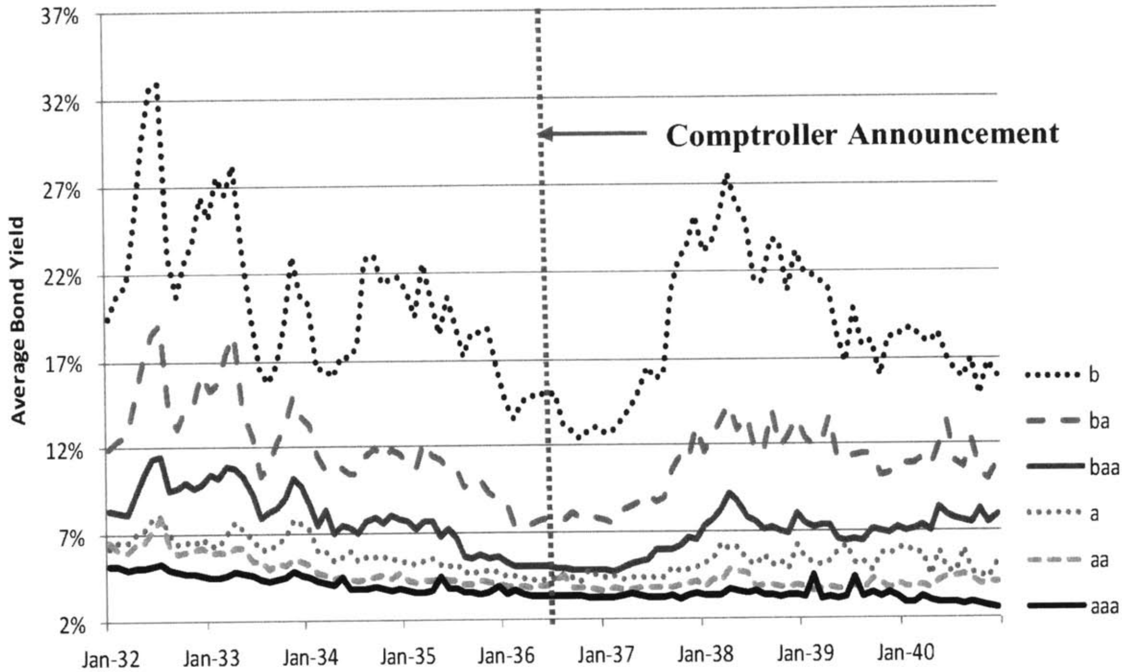


Figure 9. Assets in Below Investment Grade Corporate Securities (Non-member Insured Commercial Banks)

This figure shows the % of the total book value of assets for non-Federal Reserve Member banks in speculative grade corporate securities. The data comes from the *Annual FDIC Reports* from 1935-1940. All bond ratings from Moody's Investors Services and are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

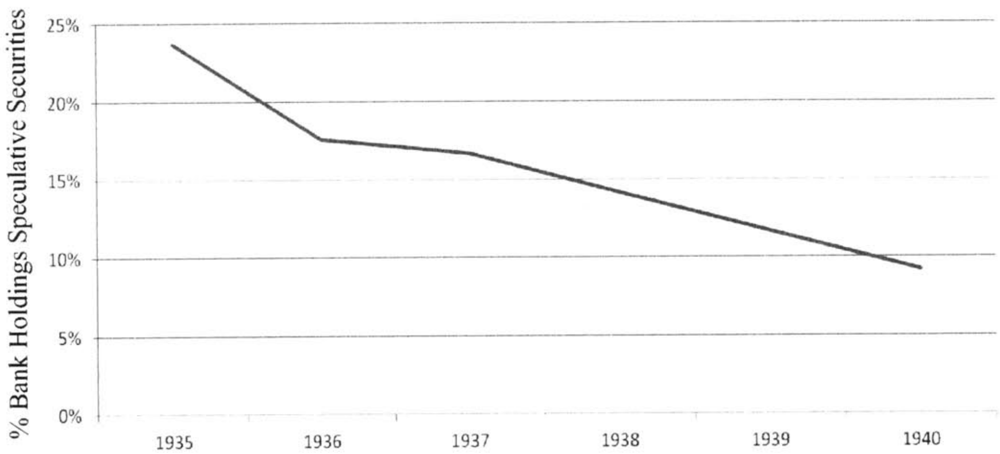


Figure 10. %Δ Met and New York Life Holdings 1935-1937 by Rating Type

This plots for domestic corporate railroad bonds with the same rating in 1935 and 1937 the % change in holdings in those bonds by Metropolitan Life Insurance Company and New York Life Insurance Company (the two largest insurance company in the early 20th century) between Dec 31st, 1935 and Dec 31st, 1937 as reported in the *Annual Report of the Superintendent of Insurance for the State of New York*. All bond ratings from Moody's Investors Services and are collected from the 1935 *Moody's Industrial Manual*, *Moody's Transportation Manual*, and *Moody's Utilities Manual*. These two companies accounted for 1/3rd of all insurance company holdings nationwide at the time.

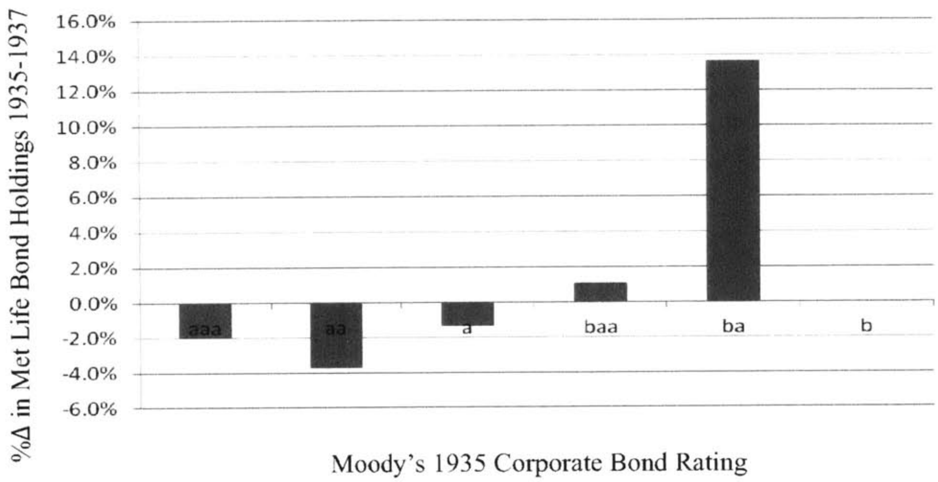


Figure 11. Cumulative New Bond Offerings by Initial Rating 1930-1940

This plots the cumulative (millions) of new offerings by initial rating as taken from the tables in Hickman (1957) with speculative grades denoted by dashed lines. Figure 11a shows the results in dollars while 11b shows the raw number of new corporate bond issues by rating grade.

Figure 11a. Cumulative New Bond Offerings ($)

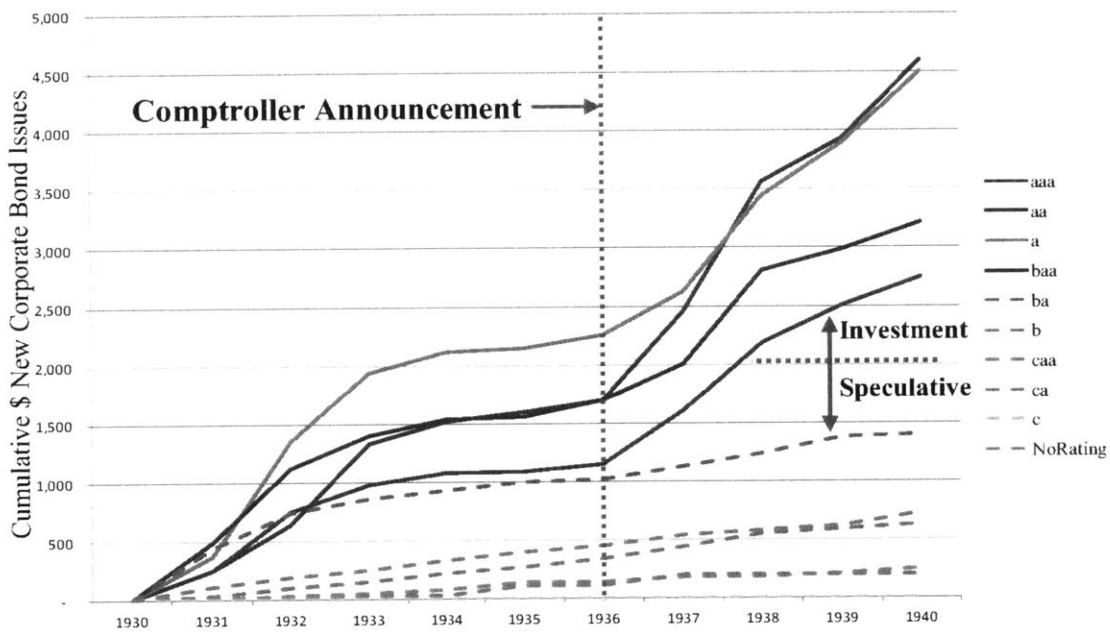


Figure 11b. Cumulative New Bond Offerings (#)

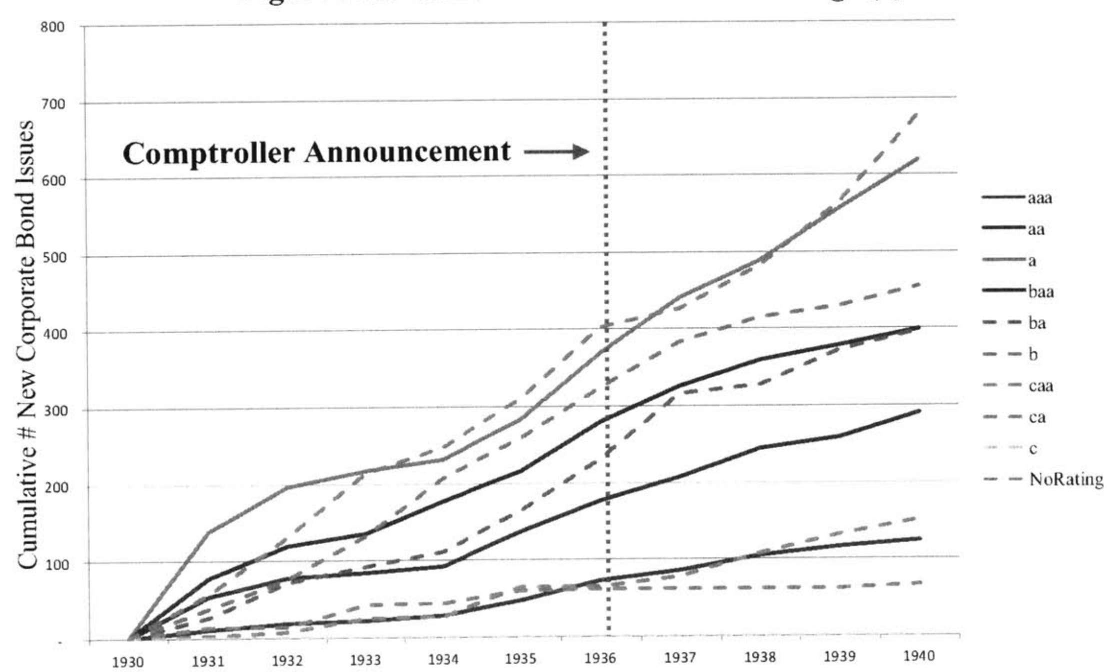
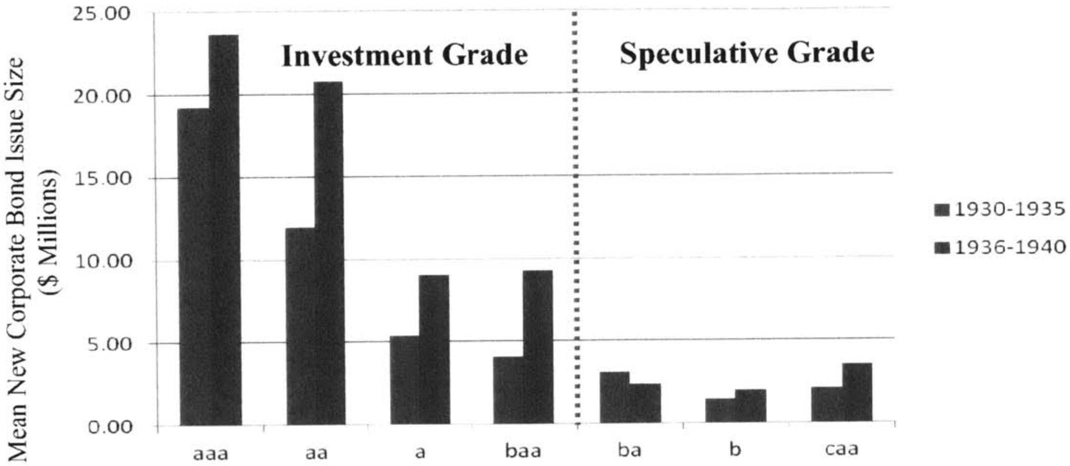


Figure 12. Average Issuance Size by Initial Rating 1930-1940

This plots the mean size ($ millions) of new offerings by initial rating as taken from the tables in Hickman (1957) with speculative grades denoted by dashed lines for 1930-1935 and 1936-1940. The table directly below shows the numeric values used in the table.



	aaa	aa	a	baa	ba	b	caa
1930-1935	19.26	11.98	5.37	4.05	3.09	1.42	2.11
1936-1940	23.61	20.78	9.00	9.26	2.38	1.96	3.55
Change	4.35	8.79	3.63	5.20	-0.71	0.54	1.43

Table 1. Matched CRSP-Moody's Sample Statistics

Summary statistics for a sample of 721 firms from the Center for Research in Securities Prices (CRSP) matched with ratings from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* broken down by rating. Each of the ratings refers to minimum bond rating for each firm. For firms without debt they have also been split into the highest quartile by volatility, *No Debt High Vol*, and the lowest quartile by volatility, *No Debt Low Vol*. Financial firms were not rated by Moody's at the time so they have been listed separately.

	Aaa	Aa	A	Baa	Ba	B	Caa	Ca	C
Mean β_{Mkt}	0.57	0.66	0.90	1.25	1.28	0.97	1.03	0.47	-1.84
Mean β_{smb}	0.21	-0.15	0.07	0.22	0.55	0.94	1.34	2.24	3.35
Mean β_{hml}	0.11	0.24	0.43	0.42	0.54	0.91	0.50	1.86	2.08
Mean Log(Market Cap)	5.22	5.02	4.64	4.29	3.78	3.35	2.99	2.97	2.61
Mean Ann. Volatility	25%	33%	36%	50%	67%	103%	122%	141%	279%
# Observations	10	13	19	56	43	43	19	6	2

	Investment Grade	Speculative Grade	No Debt All	No Debt High Vol	No Debt Low Vol	Unrated	Missing	Financial
Mean β_{Mkt}	1.04	1.02	0.99	1.11	0.61	0.94	0.31	0.91
Mean β_{smb}	0.14	0.97	0.46	1.04	0.11	0.81	0.19	0.55
Mean β_{hml}	0.37	0.79	0.08	0.33	-0.04	0.35	0.46	0.43
Mean Log(Market Cap)	4.55	3.42	4.06	3.40	4.73	3.64	3.70	3.80
Mean Ann. Volatility	42%	98%	50%	92%	23%	86%	82%	72%
# Observations	98	113	422	106	105	61	2	25

Table 2. Financial Statement Summary Statistics for 1935

Summary statistics for a sample of 422 firms from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* that have detailed financial information, including total assets, long-term debt, and property, plant, & equipment (PP&E) from 1932-1940 matched to those that also have stock prices in the Center for Research in Securities Prices (CRSP).

	Mean	Median	Stdev	#Firms
Total Assets ($Mil)	125.2	30.1	233.0	422
Long-term Debt ($Mil)	28.9	2.5	70.4	422
Long-Term Debt/Assets	0.55	0.50	0.30	422
Net PP&E/Assets	0.51	0.51	0.24	422

Table 3. Pooled Stock Return Regressions for Firms with Investment and Speculative Grade Bonds

This table shows the regression results from the 3-factor Fama-French regression specification (1) pooled for firms based on the investment grade status (Baa or higher) in 1936 of the lowest rated bond by Moody's Investor Services. Regressions are run over the window Jan 1st, 1935-Jan 17th, 1936. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.*

	(1)	(2)
Mean Stock Returns	Investment	Speculative
Market-Rf	1.03***	1.01***
	(0.03)	(0.03)
Small Minus Big	0.15***	0.99***
	(0.03)	(0.03)
High Minus Low	0.38***	0.84***
	(0.03)	(0.03)
Constant	0.002***	0.002***
	(0.0004)	(0.0004)
Observations	315	315
R-squared	0.93	0.84

Table 4. Baseline Event Study Results: Excess Stock Returns for Firms with Speculative Grade Bond Rating

In this table regression specification (2) is run on daily excess stock returns around the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). This table focuses on the baseline results where regressions are run over the period from Jan 1st, 1935 – February 21st, 1936 with the event window defined as 5 days from February 17, 1936- February 21, 1936. Specification (2) is the panel regression specified in equation 2 which allows for different factors loadings on the 3 Fama-French factors for every firm. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.* Column (1) is the baseline results comparing stock returns of investment grade vs speculative grade firms following the comptroller ruling with standard errors clustered at the firm level. Column (2) clusters standard errors at the day level. Column (3) has no clustering of errors. Column (4) drops all factor controls. Column (5) uses the market excess return as the only factor. Standard errors reported in parentheses. Column (6) reruns the baseline regression in Table 3 column (1) but also includes 2-digit SIC code interacted with event fixed effects. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable: Excess Stock Returns	Baseline (1)	Day Cluster (2)	No Cluster (3)	No Factors (4)	1 Factor (5)	Industry Controls (6)
Event x Speculative Dummy	-0.0069***	-0.0069***	-0.0069***	-0.0059***	-0.0043**	-0.0103***
	(0.0019)	(0.0015)	(0.0020)	(0.0019)	(0.0021)	(0.0020)
Event	0.0013	0.0013	0.0013	0.0018*	0.0002	0.0071***
	(0.0011)	(0.0016)	(0.0009)	(0.0011)	(0.0012)	(0.0010)
Constant	0.0011***	0.0011***	0.0011***	0.003***	0.0011***	0.0011***
	(0.00001)	(0.00023)	(0.00021)	(0.00001)	(0.00002)	(0.00021)
"Investment" Grade	Aaa-Baa	Aaa-Baa	Aaa-Baa	Aaa-Baa	Aaa-Baa	Aaa-Baa
"Speculative" Grade	Ba-C	Ba-C	Ba-C	Ba-C	Ba-C	Ba-C
Estimation Window	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36
Event Window ('36)	2/15-2/21	2/15-2/21	2/15-2/21	2/15-2/21	2/15-2/21	2/15-2/21
Event Days	[0,5]	[0,5]	[0,5]	[0,5]	[0,5]	[0,5]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Mkt-Rf	Yes	Yes	Yes	No	Yes	Yes
SMB	Yes	Yes	Yes	No	No	Yes
HML	Yes	Yes	Yes	No	No	Yes
Event x Industry FEs	No	No	No	No	No	Yes
Firm Bond Rating	Minimum	Minimum	Minimum	Minimum	Minimum	Minimum
Clustered Errors	Firm	Day	None	Firm	Firm	Firm
Observations	70,867	70,867	70,867	70,867	33,136	70,867
Adj. R-squared	0.09	0.09	0.09	0.000	0.07	0.09

Table 5. Event Study Fuzzy Discontinuity at Investment Grade Cut-off

In this table regression specification (2) is run on daily excess stock returns around the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). This table focuses on testing the discontinuity at the investment grade (Baa vs Ba) border. Specification (2) is the panel regression specified in equation 2 which allows for different factors loadings on the 3 Fama-French factors for every firm. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis (unless otherwise specified). All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.* Column (1) computes the difference between Baa and Ba firms. Column (2) compares Aaa-A vs Baa firms. Column (3) compares Ba vs B-C firms. Standard errors reported in parentheses. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable:	Cut-off 1	Cut-off 2	Cut-off 3	Cut-off 4
Excess Stock Returns	(1)	(2)	(3)	(4)
Event x Speculative Dummy	-0.0055***	0.0019	-0.0036	-0.0098***
	(0.0026)	(0.0022)	(0.0031)	(0.0027)
Event	0.0021	0.00015	-0.0035	0.0013
	(0.0015)	(0.0016)	(0.0022)	(0.0011)
Constant	0.0002***	0.00011***	0.0019***	0.0006***
	(0.00002)	(0.00001)	(0.00003)	(0.00001)
"Investment" Grade	Baa	Aaa-A	Ba	Aaa-Baa
"Speculative" Grade	Ba	Baa	B-C	B
Estimation Window	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36
Event Window ('36)	2/15-2/21	2/15-2/21	2/15-2/21	2/15-2/21
Event Days	[0,5]	[0,5]	[0,5]	[0,5]
Firm FE	Yes	Yes	Yes	Yes
Mkt-Rf	Yes	Yes	Yes	Yes
SMB	Yes	Yes	Yes	Yes
HML	Yes	Yes	Yes	Yes
Event x Industry FEs	No	No	No	No
Firm Bond Rating	Minimum	Minimum	Minimum	Minimum
Clustered Errors	Firm	Firm	Firm	Firm
Observations	33,136	33,080	37,787	47,521
Adj. R-squared	0.18	0.23	0.07	0.12

Table 6. Event Study Robustness Tests

In this table regression specification (2) is run on daily excess stock returns around the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). Robustness checks of results in Table 3. Specification (2) is the panel regression specified in equation 2 which allows for different factors loadings on the 3 Fama-French factors for every firm. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.* Column (1) shortens the estimation period to include only as far back as 3 months prior to the announcement. All interactions are included and are available upon request. Column (2) uses the maximum rating of any bond as the firm rating instead of the minimum. Column (3) compares firms with no debt vs those with Aaa-Baa rating. Column (4) compares firms with no debt but the highest quartile by volatility vs Aaa-Baa rated firms. Column (5) alters the event window to include 10 days straddling the comptrollers ruling. Standard errors reported in parentheses. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable:	Est. Window	Max Rating	No Debt All	No Debt Hi Vol	10-Day Window
Excess Stock Returns	(1)	(2)	(3)	(4)	(5)
Event x Speculative Dummy	-0.0082***	-0.0046**	0.0017	-0.0034	-0.0048***
	(0.0021)	(0.0021)	(0.0012)	(0.0022)	(0.0015)
Event	0.0013	-0.0006	-0.0004	0.0013	0.00022
	(0.0011)	(0.0012)	(0.0006)	(0.0011)	(0.0007)
Constant	0.0018***	0.0011***	0.0004***	0.0010***	0.0011***
	(0.00007)	(0.0002)	(0.000001)	(0.00002)	(0.0002)
"Investment" Grade	Aaa-Baa	Aaa-Baa	Aaa-Baa	Aaa-Baa	Aaa-Baa
"Speculative" Grade	Ba-C	Ba-C	No Debt All	No Debt Hi Vol	Ba-C
Estimation Window	11/21/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36	1/1/35-2/21/36
Event Window ('36)	2/15-2/21	2/15-2/21	2/15-2/21	2/15-2/21	2/10-2/21
Event Days	[0,5]	[0,5]	[0,5]	[0,5]	[-4,5]
Firm FE	Yes	Yes	Yes	Yes	Yes
Mkt-Rf	Yes	Yes	Yes	Yes	Yes
SMB	Yes	Yes	Yes	Yes	Yes
HML	Yes	Yes	Yes	Yes	Yes
Event x Industry FEs	No	No	No	No	No
Firm Bond Rating	Minimum	Maximum	Minimum	Minimum	Minimum
Clustered Errors	Firm	Firm	Firm	Firm	Firm
Observations	19,065	70,867	172,429	69,214	70,867
Adj. R-squared	0.10	0.09	0.11	0.08	0.09

Table 7. Heterogeneity in Effects for Firms Reliant on External Financing

In this table regression specification (2) is run on daily excess stock returns around the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). Specification (2) is the panel regression specified in equation 2 which allows for different factors loadings on the 3 Fama-French factors for every firm. This table focuses on how firm's equity value responded heterogeneously to the announcement based on how dependent the firm is on external financing. All data on stock returns are taken from the Center for Research in Security Prices (CRSP) and bond ratings are collected from the 1935 *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual.* Column (1) interacts the event and dummy for having the lowest rated corporate bond be speculative grade (Ba or lower) with a dummy variable, *External Finance Dependent*, that equals one if firm is not in the manufacturing sector, as a proxy for firms that are more reliant on external financing. All interactions are included in the specification and are available upon request. Column (2) is the same as (1) but *External Finance Dependent* equals one if the firm is in the Railroad or Transit sectors. Column (3) is the same as (2) but only for the Railroad sector. Column (4) is the same as (2) but *External Finance Dependent* equals one if the firm is in the Transportation or Utilities sectors. Standard errors reported in parentheses and clustered at the firm level. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable: Excess Stock Returns (%)	Ext Fin 1 (1)	Ext Fin 2 (2)	Ext Fin 3 (3)	Ext Fin 4 (4)
Event x Speculative Dummy	0.24	-0.36*	-0.40*	-0.32
	(0.30)	(0.21)	(0.22)	(0.22)
Event x Speculative Dummy x External Finance Dependent	-1.21***	-1.23***	-1.18**	-0.86**
	(0.39)	(0.50)	(0.54)	(0.44)
Event	-0.23	-0.12	-0.12	0.00
	(0.16)	(0.11)	(0.11)	(0.12)
Constant	0.06**	0.01	0.01	0.00
	(0.03)	(0.02)	(0.02)	(0.02)
External Finance Measure	Not Mfg.	RR&Transit	RR	Trans/Utils
Firm FE	Yes	Yes	Yes	Yes
Mkt-Rf	Yes	Yes	Yes	Yes
SMB	Yes	Yes	Yes	Yes
HML	Yes	Yes	Yes	Yes
Observations	71,192	71,192	71,192	71,192
Adj. R-squared	0.066	0.066	0.066	0.066

Table 8. Event Study of Excess Bond Returns for Investment vs. Speculative Grade Bond Rating

In this table regression specification (2) is run on daily excess bond returns around the comptroller announcement on February 17, 1936. This table focuses on the baseline results where regressions are run over the period from Jan 1st, 1935 – February 21st, 1936 with the event window defined as 5 days from February 17, 1936- February 21, 1936. Specification (2) is the panel regression specified in equation 2 which allows for different factors loadings on the 3 Fama-French factors for every firm. All bonds rated C or higher by Moody's Investor Services in 1935 are included in the analysis. Data includes all bonds listed on the *New York Stock Exchange* and *New York Curb Exchange* and are collected from the *New York Times*. For all dates Jan 1st, 1935 – Jan 31st ,1936 returns are daily but only sampled at a monthly frequency. Event window is defined as 5 days from February 17, 1936- February 21, 1936. Regression is the panel regression specified in equation (3) which allows for different factors loadings on the Fama-French factor DEF, which is just the average return of all bonds in excess of the short-term treasury bill rate. All interactions are included in the specification and are available upon request. Standard errors clustered at the issuance level are reported in parentheses. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable: Excess Bond Returns	Baseline (1)
Event x Speculative Dummy	0.0008
	(0.0015)
Event	0.000004
	(0.0006)
Constant	-0.0005***
	(0.0001)
"Investment" Grade	Aaa-Baa
"Speculative" Grade	Ba-C
Estimation Window	1/1/35-2/21/36
Event Window ('36)	2/15-2/21
Event Days	[0,5]
Firm FE	Yes
DEF	Yes
Event x Industry FEs	No
Clustered Errors	Firm
Observations	8,490
Adj. R-squared	0.23

Table 9. Sub-sample Average Issuance Size ($ million par) by Initial Rating 1936-1940

This table shows summary statistics issuance size ($ million) for 60 corporate bond issuances from 1936-1940 taken from *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* which had initial ratings of Baa or Ba.

	Baa	Ba
Mean Issuance Size	14.2	5.8
Standard Deviation	13.1	4.4
Median	10.0	4.6
# Observations	37	23
Standard Error	2.15	0.91

Table 10. Long-Run Real Effects of Investment Restrictions

This table looks at the long-run real effects on debt issuance, asset growth, and investment from the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade). All data come from the *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* which have detailed financial information, including total assets, long-term debt, and property, plant, & equipment (PP&E) from 1932-1940. These are matched to corporate bond ratings from the same manuals, but only for 1935 and SIC code industry classifications from the Center for Research in Security Prices (CRSP). All data is at the annual frequency. Column (1) regresses the logarithm of the book value of long-term debt on a dummy variable, *Event*, equal to one if the year is 1936 or later interacted with a dummy variable, *Speculative Dummy*, equal to one if the lowest rated corporate bond of the firm is Ba or lower. It also includes firm fixed effects and industry interacted with event dummy fixed effects, where industry grouping is based on four digit SIC codes. All interactions are included in the specification and are available upon request. Column (2) is the same as column (1) but looks at the logarithm of total book assets. Column (3) is the same as column (1) but looks at the logarithm of the book value of net property, plant, and equipment. Column (4) is the same as column (1) but looks at the ratio of the book value of long-term debt to total book asset value. Standard errors clustered at the issuance level are reported in parentheses. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable:	ln(Long Term Debt)	ln(Assets)	ln(PP&E)	Debt/Asssets
	(1)	(2)	(3)	(4)
Event x Speculative Dummy	-0.212***	-0.064***	-0.077**	-0.0173
	(0.075)	(0.025)	(0.033)	(0.0167)
Event	-0.9007**	0.0130	-0.0764	-0.3004***
	(0.3796)	(0.1260)	(0.1689)	(0.0849)
Constant	3.01***	4.46***	3.86***	0.798***
	(0.025)	(0.01)	(0.011)	(0.006)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Industry x Event Fixed Effects	Yes	Yes	Yes	Yes
Observations	1,186	1,186	1,186	1,186
Adj. R-squared	0.941	0.990	0.986	0.795

Table 11. Long-Run Real Effects of Investment Restrictions: Fuzzy Regression Discontinuity

This table looks at the long-run real effects on debt issuance, asset growth, and investment from the Office of the Comptroller of Currency announcement on February 17, 1936, restricting bank investment to bonds rated at least Baa or higher (aka "investment" grade), but focuses on only those firms whose lowest rated bond were Baa or Ba in 1935. All data come from the *Moody's Industrial Manual, Moody's Transportation Manual,* and *Moody's Utilities Manual* which have detailed financial information, including total assets, long-term debt, and property, plant, & equipment (PP&E) from 1932-1940. These are matched to corporate bond ratings from the same manuals, but only for 1935 and SIC code industry classifications from the Center for Research in Security Prices (CRSP). All data is at the annual frequency. Column (1) regresses the logarithm of the book value of long-term debt on a dummy variable, *Event*, equal to one if the year is 1936 or later interacted with a dummy variable, *Speculative Dummy*, equal to one if the lowest rated corporate bond of the firm is Ba or lower. It also includes firm fixed effects. All interactions are included in the specification and are available upon request. Column (2) is the same as column (1) but looks at the logarithm of total book assets. Column (3) is the same as column (1) but looks at the logarithm of the book value of net property, plant, and equipment. Column (4) is the same as column (1) but looks at the ratio of the book value of long-term debt to total book asset value. Standard errors clustered at the issuance level are reported in parentheses. P-Values: * 10%; ** 5%; ***1%.

Dependent Variable:	ln(Long Term Debt) (1)	ln(Assets) (2)	ln(PP&E) (3)	Debt/Asssets (4)
Event x Speculative Dummy	-0.157**	-0.051**	-0.029	-0.007
	(0.069)	(0.022)	(0.037)	(0.020)
Event	-0.059	0.078***	0.028	-0.067***
	(0.044)	(0.014)	(0.024)	(0.013)
Constant	3.13***	4.53***	3.86***	0.819***
	(0.03)	(0.01)	(0.02)	(0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Rating Grades Included	Baa-Ba	Baa-Ba	Baa-Ba	Baa-Ba
Observations	552	552	552	552
Adj. R-squared	0.958	0.994	0.985	0.738

Appendix A: Data Collection Examples

A1. Bond Price Data Collection Example

Company Name	Coupon	Maturity	Class	Date	Sales	Last	Change
GOODRICH (B.F.) CO.	6	1945		2/15/36	241	104.75	0
GOODRICH (B.F.) CO.	6.5	1947		2/15/36	20	108.25	0

Range '36. High.	Low.	Sales in 1000s.		High.	Low.	Last.	Net Chge.
98	89⅞	157	Gen Stl C 5½s,'49....	98	97	97⅛	— ⅜
26⅝	19	130	Gen Thea Eq 6s,'40‡§	24¼	23½	24	+ ½
26¼	19	524	Do 6s, 1940, ctfs.*§	24¼	23½	23⅞	..
20¾	18⅛	10	Ga & Ala 5s, 1945.*§	20¾	20¾	20¾	+ ⅛
32	20	1	Ga, C&N 1st 6s,'34.*§	32	32	32	+ 2
105	104	241	Goodrich 6s, 1945....	105	104½	104¾	..
108½	107⅞	20	Do 6½s, 1947	108½	108¼	108¼	..

A2. Bond Ratings Collection Example

Company Name	Coupon	Maturity	Class	Date	Rating
GOODRICH (B.F.) CO.	6	1945		6/22/36	Ba
GOODRICH (B.F.) CO.	6.5	1947		6/22/36	Baa

Goodman Manufacturing Company (Ill.)
Goodrich (B. F.) Company (N. Y.)................ 1
First 6½s, July 1, 1947, J&J 1 (1) [107]..Baa
Conv. deb. 6s, June 1, 1945, J&D 1 (2) [‡]..Ba
7% cum. preferred ($100) (1)................
Common stock (2)..........................
Goodrich (William O.) Company (Wis.)..........
See Archer-Daniels-Midland Company.........

Company Name	Coupon	Maturity	Class	Date	Old Rating	New Rating
GOODRICH (B.F.) CO.	6	1945		3/19/34	B	Ba
GOODRICH (B.F.) CO.	6.5	1947		3/19/34	Ba	Baa

RATINGS RAISED

Brookline, Mass.:
General obligations ....Aa to Aaa
Goodrich, B. F. Co.:
1st mtg. 6½s, 1947..................Ba to Baa
Deb. 6s, 1945........................B to Ba

A3. Balance Sheet Information

Company Name	B.F. Goodrich Co.	B.F. Goodrich Co.
Year	1936	1936
Funded Debt	Funded debt	Subsidiary bonded debt
Funded Debt	36,956,300	332,600
Total Assets	total	
Total Assets	124,020,982	
Fixed Assets	depreciated value	
Fixed Assets	49,765,611	

Company Name	Coupon	Maturity	Class	Date	Outstanding
GOODRICH (B.F.) CO.	6.5	1947		6/22/36	17,156,500

THE B. F. GOODRICH COMPANY

Company was incorporated May 2, 1912 in New Yo
o corporation with the same name incorporated in 18
rich. The main plant is in Akron, O., and occupies

Comparative Consoli

	(b)1935
Assets:	
Real estate, plants, etc.	$92,899,099
Less depreciation	43,133,488
Depreciated value	49,765,611
Investments, advances, etc.	§4,135,598
Treasury stock	....
**Purchase fund	600,000
Inventory	138,325,208
Trade notes & accts. rec. (net)	20,033,693
Other accts. & bills receivable	1,189,255
Cash	***8,711,406
‡‡*Government securities*	....
Deposits in closed banks (net)	....
Deferred charges	1,260,212
Total	$124,020,982
Liabilities:	
Preferred stock	$29,430,800
*Common stock	39,316,910
Funded debt	36,956,300
Subsidiary bonded debt	332,600
Mortgages, etc., payable	....
Minority interest	2,151
Bills payable and bank loans	†††2,664,578
Accounts payable	6,112,689
Sundry accrued liabilities	1,193,979
Mortgages payable	47,642
Subsidiary notes payable	‡‡‡2,778,000
Federal tax reserve	812,772
Reserve for commitments, etc.	....
Reserve for pensions	600,000
Miscellaneous reserves	§§§1,220,298
Employees' stock subscriptions	....
Surplus	(a)2,344,268
Total	$124,020,982
Current assets	$68,259,562
Current liabilities	13,609,655
Working Capital	$54,649,907

Funded Debt: 1. The B. F. Goodrich Co. first gold 6½s, due 1947:

Authorized—$25,000,000; outstanding, Dec. 31, 1935, $17,156,500; redeemed and cancelled, $7,843,500.
Dated—July 1, 1922; due July 1, 1947.

as to after-acquired
chase money mortgag
Further secured by

A4. Insurance Company Holdings Data

Company Name	Crucible Steel Co of America
Coupon	5
Maturity	1940
Class	deb
Date	12/31/38
Insurance Company	Met Life
Par Held	113,000

Crucible Steel of America deb 1940 5s	112,844 63	113,000
Dow Chemical deb 1951 3s	250,000 00	250,000
General American Tank Car Corp equip tr ser 26 4s	[illegible]	960,000
27 4s	2,400,000 00	2,400,000
General American Trans Corp equip tr ser 28 2½s	1,869,209 42	1,875,000
29 3s	2,297,862 20	2,307,000
General American Transportation Corp notes 3s	1,837,477 52	1,827,000
Sys Inc notes 1941 5s	5,088,732 16	4,840,000

Chapter 3
Counterparty Risk and the Establishment of the New York Stock Exchange Clearinghouse*

Joint with Eric Hughson[a] and Marc Weidenmier[b]

"For more than a century, financial stability has depended on the resilience under stress of clearinghouses and other parts of the financial infrastructure. As we rely even more heavily on these institutions in the United States and around the world, we must do all that we can to ensure their resilience, even as our financial system continues to evolve rapidly and in ways that we cannot fully predict."

– Federal Reserve Chairman Ben Bernanke April 4, 2011

1 Introduction

On September 14th, 2008 dealers from every major Wall Street firm involved in the $600 trillion over-the-counter (OTC) derivatives market came into work on a Sunday for an unprecedented emergency trading session. The goal? A frantic effort the day before Lehman Brothers declared bankruptcy to try and net counterparty risk in their bilateral over-the-counter contracts with Lehman and limit the knock-on losses of its collapse on other financial institutions. Lehman's global OTC derivatives position at the time was estimated at $35 trillion in notional, which included being a counterparty in 930,000 derivatives

* The authors would like to thank Jinlin Ye for excellent research assistance and seminar participants at the Massachusetts Institute of Technology, Harvard Business School, Harvard Economic History, 2014 London Business School Summer Finance Symposium, University of Colorado – Boulder, and HEC Montreal. We would also like to thank Michael Greenstone, John Campbell, Randall Kroszner, Nittai Bergman, Neil Shephard, Eric Hilt, William Roberds, Vania Stavrakeva, Jun Pan, Adrien Verdelhan, Rajkamal Iyer, Haoxiang Zhu, Jean-Noel Barrot, Erik Loualiche, Daan Struyven, Stephen Murphy, Nils Wernerfelt, Daniel Green, and Michael Abrahams for helpful comments. We greatly appreciate Janet Linde and New York Stock Exchange Archives for their assistance in accessing historical archives. We also wish to thank the Lowe Institute of Political Economy and the Financial Economics Institute at Claremont McKenna College for financial support. Any errors are our own.

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transactions representing $24 billion in counterparty liabilities[61]. This ad-hoc attempt at clearing was described by market participants as "a bust", with very little successful netting prior to Lehman's bankruptcy filing[62]. The result was an unprecedented rise in counterparty risk, contagion, and financial instability among global financial market participants exemplified by a dramatic increase in indicators of counterparty risk including the credit default swap-bond basis and deviations from covered interest rate parity[63].

The collapse of Lehman Brothers and the subsequent spillovers raised concerns about the role counterparty risk plays in the stability of the financial system and the importance of clearinghouses in mitigating that risk. In particular, policymakers in the United States and European Union have tried to address counterparty risk concerns not only by substantially increasing counterparty risk-based capital requirements for banks with Basel III, but also by mandating centralized clearing of the majority of OTC derivatives via the Dodd-Frank and European Markets Infrastructure Regulation Acts. Prior to Lehman's collapse, OTC derivatives were not required to engage in multilateral net settlement through a centralized clearinghouse and often relied on bilateral netting and ad-hoc margin requirements between counterparties. Under bilateral netting, traders can be exposed to additional counterparty risk through contagion, since if one trader defaults he can set off a cascade of additional defaults. All else being equal, when OTC derivatives contracts instead engage in multilateral netting, Cecchetti et al. (2009) estimate that gross notional exposures can be reduced by as much as 90 percent. Policy makers point to these potential ex-post netting benefits and the rise in counterparty risk concerns after Lehman's bankruptcy as evidence that mandated OTC derivative clearing would reduce the probability of an initial default as well as counterparty risk arising from contagion.

Despite the response of policymakers, academic evidence of the effects of clearing on financial stability and asset values are still unclear. From a theoretical standpoint Duffie and Zhu (2011) demonstrate that a single party clearing all assets should reduce counterparty risk, *ceteris paribus*, leading to lower volatility and higher asset value, but this result does not generalize to multiple clearinghouses or a single clearinghouse that does nott clear all transactions. Acharya and Bisin (2014) establish that in the absence of a clearinghouse there can be a counterparty externality which encourages excess risk taking, but Pirrong (2009) shows that a clearinghouse itself can reduce monitoring incentives which subsequently increases moral hazard and counterparty risk. Biais et al. (2012) also note that a reduction in idiosyncratic risk from clearing may endogenously increase systematic risk taking and Menkveld, et al. (2013) point

[61] Lehman Brothers Holdings Inc. First Creditors Section 341 Meeting, January 29, 2009, Summe (2012), and their most recent 10Q filing on July 10th, 2008.
[62] Financial Times, "Dealers hold emergency trading session", September 15, 2008.
[63] Levich (2011) and Giglio (2013)

out that if the introduction of clearinghouse causes increases in collateral and margin requirements, then the effect of funding and market liquidity on asset prices makes the response of prices theoretically ambiguous (see also Garleanu and Pedersen 2011). Therefore, the effect of the introduction of a clearinghouse on asset prices remains inevitably an empirical question.

Unfortunately, empirical evidence on the role of clearing is still limited and the effects on counterparty risk are mixed. Examining the introduction of a clearinghouse for Nordic equities in 2009, Menkveld et al. (2013) find that clearing reduces asset values, but Loon and Zhong (2013) show that the clearing of credit derivative contracts in 2009 actually increased their values. Interpretation of these opposing empirical results can be challenging because in both cases clearing was driven by the collapse of Lehman Brothers in the fall of 2008 and the resulting financial crisis. It is hard to know if the introduction of a clearinghouse in those markets was co-incident with the subsequent deterioration or improvement in fundamental value and risk of those securities chosen to be cleared. It is precisely because the introduction of the clearinghouse was a response to a crisis that makes it problematic to attribute any changes in liquidity or counterparty risk to the clearinghouse and why it is important to control for economic conditions.

Fortunately, history provides an experiment to study the effects of a clearinghouse on counterparty risk where we can directly control for fundamental value. During the late 19th and early 20th centuries, the Consolidated Stock Exchange (CSE) was a major exchange that competed head-to-head with the Big Board, traded many NYSE-listed securities, and as noted by Brown et al. (2008) averaged more than a 50 market percent share during the 1890s. Located across the street from the NYSE, the CSE netted stock transactions through a clearinghouse starting in 1886, while the NYSE did not until May of 1892[64]. Using identical securities on the two exchanges, we compare relative prices on the NYSE with those on the CSE both before and after the introduction of the NYSE clearinghouse which allow us to control for changes in fundamental security value and volatility. This allows for clean identification of the causal effect on asset prices of the introduction of the clearinghouse by controlling for economic conditions in a way that is difficult to replicate with modern data[65]. We also examine the relative prices for more than 30 years following the introduction of clearing allowing us to observe the behavior during

[64] The CSE began competing head-to-head with the NYSE in 1885 when the rival exchange began trading securities on the Big Board using their ticker. This action set off a lengthy legal battle between the two exchanges with the NYSE ultimately establishing ownership of its price quotes (Mulherin et al. 1991).

[65] The beginning of multilateral net settlement through a clearinghouse on the NYSE in 1892 was driven by a variety of factors, most notably financial panics in the early 1890s (McSherry and Wilson 2013). This again highlights the need to use the CSE as a control to cleanly identify the effect of counterparty risk.

periods of relative calm and crisis, as well as allowing time for endogenous general equilibrium effects by market participants.

We find that the introduction of netting on the NYSE increased the value of stocks relative to the CSE by 24bps. Consistent with the findings in McSherry et al. (2013), who document a decline in broker defaults on the NYSE after the introduction of clearing, the empirical results suggest that clearing increases rather than reduces equity values. Because brokers had to fund positions overnight, daily borrowing rates were a major determinant of counterparty risk. Prior to the introduction of clearing, a one standard deviation (3.7 percentage point) increase in the overnight collateralized borrowing rate for brokers, also known as the call loan rate, is associated with an 8bp decline in the value of a stock on the NYSE relative to the identical security on the CSE. After the introduction of clearing, shocks to the call loan rate no longer affect prices on the NYSE relative to the CSE, suggesting a decline in the volatility of NYSE prices. Consistent with this prediction, we find that relative to the CSE, annualized NYSE return volatility is reduced by 90-173bps immediately following the introduction of clearing and remains low, even during subsequent financial crises, in the subsequent 34 years.

Clearing on the NYSE was also introduced in stages, so we also examine the staggered introduction and find that at least half of the average reduction in counterparty risk is driven by a reduction in contagion risk through spillovers in the trader network. We run a series of robustness tests to demonstrate that our results are driven by changes in counterparty risk coming from the introduction of clearing, rather than changes in asynchronous trading, market liquidity improvements on the NYSE, a decrease in market liquidity on the CSE, or financial crises. Our results do not hold without the CSE control, demonstrating again the importance of controlling for macro-economic changes in fundamental value and volatility co-incident with the introduction of a clearinghouse. We also find that the introduction of mutualization of risk and a formal centralized counter party (CCP) by the NYSE clearinghouse in April of 1920 does not alter the benefits found from the introduction of centralized clearing with multi-lateral netting in 1892, providing additional evidence consistent with a role for CCPs in improving financial stability in asset markets.

Section 2 begins with a brief historical background on the introduction of clearing on the NYSE. We describe the data used in Section 3. In Section 4, we present the empirical methodology and predictions. We discuss the empirical results in Section 5. Section 6 concludes the paper.

2 Historical and Institutional Background

2.1 Trading on the NYSE Prior to Clearing

Like OTC derivatives today, NYSE equities settled on a bilateral rather than a multilateral basis prior to the introduction of a clearinghouse in 1892. In the absence of multilateral netting, brokers are required to write and receive checks/securities for every transaction. To illustrate, consider the hypothetical set of transactions in Example 1.

Example 1. Visual representation of bilateral trades between 3 brokers

Next day deliverables:
Shares: 200
Cash: $20,100

Broker A sells 100 shares of stock for $10,000 ($100/share) to broker B and later in the day B sells 100 shares to C for $10,100. In the absence of multilateral netting, broker C owes a check to broker B for $10,100 and broker B would owe a check to broker A for $10,000 resulting in $20,100 of checks and 200 shares of stock being transferred. There are direct counterparty risks since, for example, if broker B defaults (and has no wealth) broker A loses $10,000, but there is also a possibility of large spillovers causing contagion counterparty risk throughout the trading network. For example, if broker C defaults (and has no wealth) broker B loses $10,100. If in turn this pushes broker B into default (and again has no wealth) then A loses $10,000. As we add more brokers into the network, the chain of defaults can multiply. Depending on how interconnected the trading network is, the spillover from contagion could be a substantial component of total counterparty risk. Eliminating counterparty risk for security A should also reduce the counterparty risk of security B even if it is unrelated because there is less chance of a broker, or brokers he is trading with, defaulting on positions. For clarity we refer to the counterparty risk caused by network spillovers as *contagion risk* and the remaining as *direct counterparty risk.*

At the time the NYSE clearinghouse was introduced, securities traded on the NYSE settled at time T+1, which meant all brokers were required to deliver gross checks/securities from trades by the next day at 2:15pm. Brokers engaged in transactions with numerous other brokers throughout the day, so they rarely had enough assets on hand to pay every single transaction. Customers also bought securities on margin so brokers would often have to borrow the additional funds necessary. Therefore, banks were forced to extend significant uncollateralized credit and day loans to brokers to allow them to fulfill their daily contracts. This practice was called overcertification since banks endorsed checks which certified an amount greater than the balance in the broker's account[66], effectively providing short-term leverage to brokers to finance their daily positions. This bears similarities to modern broker-dealers who use the repo market and asset-backed commercial paper to provide short-term financing for trades in the OTC markets[67]. McSherry and Wilson (2013) find that leverage, measured as the value of certified checks divided by total capital, for 9 "broker banks" increased from 1.4 to 9.0 from 1875 to 1882. Anecdotal evidence suggests even higher leverage ratios in the 1890s.

Just as short-term collateralized financing rates in the modern period are set by repo rates, brokers would also finance positions via overnight collateralized borrowing organized on the floor of the NYSE. The rate to buy and sell securities on margin via these overnight collateralized loans was known as the call loan rate. The call loan rate could fluctuate wildly depending on the market environment. Short-term interest rates were prone to seasonal increases during the harvest months and tended to increase dramatically during late nineteenth and early twentieth century banking panics (Miron 1986, Bernstein et al. 2010). For example, the call loan rate reached a daily annualized value of 125 percent during the Panic of 1907 (Moen and Tallman 2003).

The volatility of funding costs to finance overnight positions led to a significant number of broker defaults and increased counterparty risk. McSherry et al. (2013) find evidence of a statistically significant relationship between spikes in call loan rates and broker insolvencies during this period. Contemporaneous researchers, such as Sprague (1903), also blamed the immediacy of the liquidity requirements inherent in the NYSE system of daily settlement for broker failures; which tended to spike during periods of financial stress. During periods of panic, buyers might walk away from buy orders, leaving brokers with losses and potential defaults on overcertified checks. Anticipating this outcome, Wall Street banks and trust companies that normally participated in overcertification might withdraw the privilege extended to brokers. This is exactly what happened in the Panic of 1873 when banks suspended overcertification to NYSE brokers. This action led to a suspension of trading for nine days and 57 broker failures (Eames, 1894). By early 1892, R. L. Edwards, the President of the Bank of the State of New York, threatened that certification for brokers would be cut unless decisive action was taken to lessen the strain on bank lending and clerks[68]. NYSE President Francis L. Eames subsequently pushed for the

[66] While technically illegal, overcertification was endemic during the period and used by most brokers and banks to finance their overnight positions.

[67] It is worth noting though while that lending in modern repo markets also extends massive credit on an intraday basis, this lending is done on a fully collateralized basis. We thank an anonymous referee for raising this point.

[68] Meeker (1922) also documents that without the introduction of multilateral netting, it would have been physically impossible to maintain daily settlement. If, however, physical constraints rather than counterparty risk were the

creation of the New York Stock Exchange Clearinghouse in May of 1892 which engaged in multi-lateral netting across all NYSE members (Pratt 1909).

The NYSE clearinghouse function would then be extended in April of 1920 to include mutualization of risk by acting as a centralized counterparty on trades between all members. The staggered timing of the introduction of centralized clearing and then mutualization of risk provide a novel setting to try and distinguish the effects of the two major functions of modern clearinghouses[69]. The analysis in 1920 is made more challenging though since accusations of fraud on the Consolidated Stock Exchange in February of 1922, which led to its eventual downfall, limit our identification strategy in the post-mutualization period. We therefore focus our primary analysis on the introduction of clearing in 1892, but also briefly examine the introduction of mutualization in 1920.

2.2 Timing of Introduction of Clearing on the NYSE

On May 17th, 1892 the New York Stock Exchange introduced multilateral netting for four firms. The decision to introduce clearing was driven by the financial panics of the early 1890s, concerns that banks would restrict overcertification again, as well as evidence on the effectiveness of multilateral netting used on the Consolidated Stock Exchange[70]. Because many NYSE stocks were already clearing on the Consolidated Exchange, we can disentangle the effects of economic events from the effects of clearing on counterparty risk. As indicated in the clearinghouse meeting minutes, the NYSE had pre-scheduled meeting dates and decided "the list of stocks to be cleared will be enlarged as members become familiar with the clearing system." Since having some NYSE stocks clearing had spillover benefits through a reduction in contagion risk for the remainder, the staged and independent timing of the introduction multilateral netting for different securities allows separate identification of contagion and

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main reason for the introduction of multilateral netting, a perhaps more plausible response would have been to increase the settlement period.

[69] Securities market clearinghouses serve two primary and distinct functions; multilateral netting and mutualization of risk. Since clearinghouses observe all trades on a given exchange they can net transactions across traders in an attempt to reduce the size outstanding liabilities and subsequent counterparty risk. The NYSE clearinghouse in 1892 engaged in exactly this sort of netting function and is the primary function of clearing analyzed in this paper. In today's regulatory environment clearinghouses are also typically mandated to provide mutualization of risk by including themselves as counterparties in all transactions. In order to more clearly assess the modern implications of our analysis we also explore the introduction of mutualization of risk by the NYSE clearinghouse in 1920, but are limited by the length of time available for our empirical methodology in the post-mutualization period.

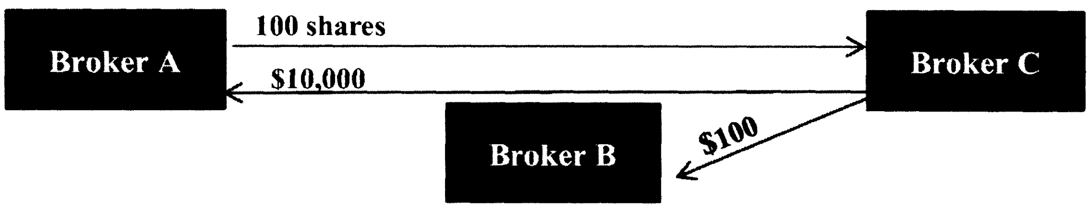
[70] In fact, by 1892 there were numerous examples of effective clearing systems in the United States, including the establishment of a clearinghouse for New York City bank deposits in 1853 (Gorton 1985) and for commodity trading on the Chicago Board of Trade in 1883 (Kroszner 1999).

direct counterparty risk. The NYSE continued to have meetings and clear additional stocks throughout the 1890s and by the end of 1893, most of the major securities were clearing[71].

2.3 Trading on the NYSE after Clearing

To understand the benefits of the introduction of clearing on the NYSE, we examine multilateral netting between three brokers. A hypothetical set of transactions is shown in Example 2.

Example 2. Visual representation of trades between 3 brokers w/ clearing



Next day deliverables:
Shares: 100
Cash: $10,100

Each transaction a broker made was recorded on the broker's clearance sheet for a given day. In our example, A's clearance sheet had a single sale, C's clearing sheet had a single purchase, and B's clearance sheet had a purchase and a sale. It is at this stage that netting occurred – and here, netting occurred only for B. B bought 100 shares for $10,000 and then immediately sold them for $10,100[72]. The purchase and sale were netted out and B received the difference of $100. Broker A had a balance to deliver 100 shares valued at $10,000 and C had a balance to deliver of $10,100. Therefore, A wrote a draft on the Clearinghouse of $10,000; B wrote a draft for $100, and C wrote a check to the Clearinghouse of $10,100. By 10:00 a.m. the next day, the Clearinghouse returned a complete statement to each firm, specifying to whom a delivery must be made by 2:15 p.m. that day (here A delivered to C). Creditors to the Clearinghouse received checks for their remaining balances by noon, which were then deposited in the bank (American Bankers Association 1910)[73].

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[71] For example, by the end of 1893 more than 80% of NYSE volume in Dow Jones stocks was clearing.

[72] This simple example overlooks one complication. In reality for ease of netting, delivery prices were not simply what one paid or sold his or her shares for, but were instead determined by the Clearinghouse. At the end of each day, representatives set a price based on the quotation of the last day's sales, which was then announced over the ticker. Small additional checks were then written between parties to account for the differences between the delivery prices and the actual executed prices (Pratt 1909).

[73] These exact times may have varied throughout the years, but they provide a rough picture of the daily operations of the Clearinghouse.

Under gross bilateral clearing, there were $20,100 worth of checks and 200 shares which could be defaulted on, but after multilateral netting there were only $10,100 worth of checks and 100 shares to be transferred. In this case there is a reduction in direct ex-post counterparty risks since with multilateral netting, if broker B defaulted (and had no wealth) broker A lost nothing. There was also a reduction in spillovers causing contagion counterparty risk throughout the trading network. For example, if broker C defaulted (and had no wealth) broker A lost $10,000 and if broker A defaulted (and had no wealth) broker B lost only $100. With multilateral netting, typically the chain of defaults does not grow as we quickly as it would with bilateral netting when we add more brokers into the network.

Anecdotal evidence suggests that the NYSE clearinghouse may have been successful in reducing counterparty risk on the NYSE in the years immediately following its establishment. In the post clearinghouse period (i.e. between 1892 and 1920), Pratt (1909) estimated that the demand for day loans from certifying banks decreased by nearly 65 percent, and 90 percent of all checks were eliminated. On average, transactions in securities valued at $25 million necessitated only $5 million to change hands. In one case, 204,000 shares, valued at $12.5 million were settled by a payment of only $10,000 (Meeker 1922).

That being said, anecdotal evidence of the effect of multi-lateral netting on counterparty risk through contagion is mixed. The Chicago Board of Trade introduced a "ring" settlement system in 1883 similar to the one introduced on the NYSE and in 1902 the bankruptcy of member George Phillips led to losses for more than 42 percent of members of the Board (Kroszner 1999, Moser 1998). Direct measures of broker insolvencies also may not necessarily provide the full picture, since changes in counterparty risk caused by a clearinghouse could lead to differences in margin requirements, borrowing rates, and commissions between customers, brokers, and/or banks. The aggregate effect of all these channels should show up in prices, either through expected losses from counterparties or changes in the discount rate coming from volatility in counterparty risk and/or margin-driven asset pricing changes (Garleanu and Pedersen 2011). Another challenge in interpreting effects is controlling for the counterfactual changes in broker defaults and security value and volatility in the absence of a clearinghouse.

2.4 Consolidated Stock Exchange: An Ideal Control

As illustrated in the timeline in figure 1, the Consolidated Stock Exchange, also known as the "Little Board," was established in New York City in 1885 with 2,403 members[74] and provides an

[74] Based on annual reports of the *Consolidated Stock and Petroleum Exchange of New York.*

excellent control for our difference-in-difference analysis of the effect of the introduction of clearing. The Little Board competed head-to-head with the NYSE (Michie 1986). The rival exchange averaged a respectable 23 percent market share (Brown et al. 2008) over its 40-year history although CSE stocks generally had less trading volume and market liquidity than the same security on the Big Board. While the NYSE waited until 1892 to introduce clearing, the CSE began multilateral net settlement in 1886. As noted by McSherry and Wilson (2013), one reason that the NYSE introduced clearing was that the CSE had "reduced financing needs and also lowered counterparty risk and broker defaults" by netting through a clearinghouse.

We provide some suggestive evidence of the impact of the clearinghouse on the CSE by hand-collecting information on broker defaults from the annual reports of the *Consolidated Stock and Petroleum Exchange of New York*. Consistent with the contemporaneous accounts, the CSE clearinghouse was successful in minimizing counterparty risk. We find that losses from broker defaults were less than 0.03% of total trading volume in 1893, a year that included one of the most severe financial panics in American history.

Therefore, prices on the CSE for NYSE-CSE dual-listed stocks provide an almost ideal control for the price response on the NYSE to the introduction of clearing[75]. This is why the introduction of clearing on the NYSE can be used to identify the causal effects of multilateral netting. In addition to having cross-listed securities, we also benefit from the close proximity of the two exchanges. Since the two exchanges were across the street from each other, arbitrageurs could effectively prevent price discrepancies between the two exchanges not caused by "real differences" such as market illiquidity or counterparty risk premia. Nelson (1907) dedicates an entire chapter to the "expertise" of arbitrageurs on the Consolidated who were, he felt, only exceeded in their expertise by the arbitrageurs on the NYSE. In

[75] The CSE and NYSE also had similar governance structures and internal regulations. Both exchanges were cooperatively owned and governed by their members, with a board of governors, including a president, elected by members of the exchange, and committees with members appointed by the president overseeing various functions of the exchange. The constitution of both exchanges also allowed either party in the transaction for the sale or purchase of stocks, bonds, or any outstanding contracts, to call, at any time, a mutual deposit of cash for margin, with as little as thirty minutes notice. The NYSE and CSE allowed any party to demand maintenance margins of 5 percent, while the NYSE and CSE constitutions provided for initial margin requirements of 10 and 5 percent respectively. In practice though it is unclear if these minimal margin constraints were actually binding. As noted in a report by the CSE's Governor's *Committee on Securities and Commodities* in 1909, "the amount of margin which a broker requires from a speculative buyer of stocks depends, in each case, on the credit of the buyer". Based on minutes from the NYSE's *Insolvency Committee* from 1876-1925 brokers were occasionally removed from the exchange for "reckless dealing" because they required insufficient margins from customers. Even among this subset of potentially reckless brokers the majority reported margins of 5%-8% and sometimes as high as 25%, depending on the reported trustworthiness of customers. All additional information on governance structure come from the *Constitution of the New York Stock Exchange* and *Constitution of the Consolidated Stock Exchange* from 1892.

fact, in table A4 of the online appendix we show that more than 92% of all variation of individual NYSE stock returns can be explained by the returns of identical securities listed at the same time on the CSE[76]. Another benefit of their close proximity is that both exchanges paid in the same currency. Cross-listed securities in markets quoted in different currencies are confounded by the need to convert currencies using OTC foreign exchange (FX) markets. Normally this is not problematic, but since these markets are OTC, during times of financial distress, FX swaps may also include potentially significant counterparty risk. For example, Levich (2011) shows that immediately following the Lehman bankruptcy covered interest rate parity in the highly liquid FX swap GBP/USD deviated from no arbitrage conditions (in the absence of counterparty risk) by hundreds of basis points[77].

3 Data Description

3.1 Security Market Data

We focus our empirical analysis on common stocks in the Dow Jones Indices using monthly data from September 1886 – December 1925 because these securities tended to be very liquid and traded on both the NYSE and CSE (Brown et al. 2008). We use the original Dow Jones Index from September 1886 until October 1896, when the index is then split into the Dow Jones Railroad Index and the Industrial Index. We use hand-collected data from the *New York Times* and *Commercial and Financial Chronicle* for each security in the index at a given point in time and rely on Farrell (1972) for changes in the composition of the indices. Data are sampled from the last trading day of each month. We collected firm-specific information on NYSE high, low, open, and closing transaction prices, bid and ask closing prices, and trading volume. For NYSE stocks listed on the CSE, we use data on CSE closing prices as well as CSE trading volume. We also use hand-collected monthly data on seat prices for the NYSE and CSE for the period 1888-1925 from the *Commercial and Financial Chronicle*. In addition, we collect daily closing

[76] By comparison, Lewellen (2014) regresses monthly stock returns on lagged individual firm stock returns, size, and book-to-market ratios and on average only explains 3.3% of cross-sectional variation in NYSE stock returns from 1964-2013. Even when including 15 lagged stock-specific individual factors expected to explain stock returns, he finds that less than 8% of cross-sectional variation in returns are explainable.

[77] Another benefit of proximity, besides the ones previously emphasized, is that since both exchanges were in the same time zone, daily data on opening and closing prices are easily comparable. This is not only because it reduces timing mismatches in the quotes, but also because they are comparable periods of the trading day. Oftentimes opening and closing price behavior can behave differently and while high frequency quotes allow for quotations across time zones at the same time of day this can't be done while also preserving the period of the trading day considered.

bid and ask quotes on the NYSE starting in 1893[78]. We also use end-of-month broker call loan rates from the NBER macro-history database for the entire sample period.

For robustness checks, we hand-collected daily data on high, low, close, and open transaction prices as well as trading volumes from January 1892- December 1901 for all stocks on the NYSE, CSE, and the Boston Stock Exchange (BSE). Closing prices for the BSE are collected from the *Boston Globe* from 1892-1901 at a weekly frequency. We construct an absolute difference estimator using daily high, low, open, and closing transaction prices to estimate CSE bid-ask spreads and NYSE bid-ask spreads prior to 1893. Our estimated NYSE bid-ask spreads have an 88 percent correlation with actual bid-ask spreads on the Big Board from 1892-1925. Our estimator performs slightly better in-sample than one used by Corwin and Schultz (2012), which has an 81 percent correlation with actual NYSE spreads over the same period. In addition, our estimator has the desirable property, since unlike that used by Corwin and Schultz, it is always positive, which was not the case for our Corwin-Schultz bid-ask estimates in our sample period. For more details on the methodology and a comparison of the bid-ask spreads see online appendix B.

3.2 Clearinghouse Data

The NYSE started clearing securities in stages, beginning with four stocks in May 17th, 1892, followed by four additional stocks each week. By 1894, more than 90 percent of volume was cleared on the exchange and only a handful of stocks were subsequently added to the clearinghouse each year[79]. The dates stocks were added and dropped from clearing on the NYSE were reported in the minutes of the *Committee on the Clearinghouse of the New York Stock Exchange* at the New York Stock Exchange archives. The minutes of the clearinghouse were useful for understanding the function and implementation of netting trades on the exchange. Data on broker defaults on the NYSE were collected from the NYSE archives *Committee on Admissions* and *List of Suspended Members.* Information on CSE broker defaults were collected from the *Annual Reports of the Consolidated Stock and Petroleum Exchange of New York.*

[78] Beginning on May 24, 1882, the *New York Times* reports NYSE bid-ask spreads on a daily basis. The data on daily bid-ask spreads continue through April 14, 1886. Between April 15, 1886, and May 12, 1893, the *New York Times* does not report bid-ask spreads for the NYSE. In this time interval, we gather monthly bid-ask spread data from the *Commercial and Financial Chronicle*. The bid-ask spread data are reported for Thursday trading and are matched with the appropriate trading volume data from the *New York Times.*

[79] Authors' calculations.

4 Empirical Predictions and Methodology

4.1 Theoretical Predictions

In the presence of counterparty risk and market liquidity costs, we can decompose the price of any traded asset into its fundamental value minus market liquidity costs and counterparty risk, plus any additional market microstructure noise:

$$P_{i,t,E} = P_{i,t}^{Fun} - P_{i,t,E}^{MktLq} - P_{i,t,E}^{CP} + \epsilon_{i,t,E} \quad (2)$$

where $P_{i,t,E}$ is the price on exchange E (ex. NYSE) for stock i at time t, $P_{i,t}^{Fun}$ is the firm's exchange invariant fundamental value, $P_{i,t,E}^{MktLq}$ is the discount caused by the market illiquidity premia which include both the explicit and implicit costs of trading[80] and how they co-vary with the pricing kernel (Acharya and Pedersen 2005, Brunnermeier and Pedersen 2009, Garleanu and Pedersen 2011), $P_{i,t,E}^{CP}$ is the discount caused by the counterparty risk premium, and $\epsilon_{i,t,E}$ is market microstructure noise with mean zero, such as bid-ask bounce. This decomposition arises naturally from the original framework of Amihud and Mendelson (1986) where investors who buy securities anticipate paying transactions costs when selling them, as do the next buyers. Consequently, when valuing the asset the investor rationally discounts the fundamental value by the present value of the expected future transaction costs. If we consider the same asset trading on two exchanges, E and E', then even in the presence of active arbitrageurs the price should differ whenever there are differential trading costs, liquidity, and counterparty risk by the following spread:

$$P_{i,t,E} - P_{i,t,E'} = P_{i,t,E'}^{MktLq} - P_{i,t,E}^{MktLq} + P_{i,t,E'}^{CP} - P_{i,t,E}^{CP} + \epsilon_{i,t,E'} - \epsilon_{i,t,E} \quad (2)$$

A substantial literature has documented these kind of price spreads among securities paying the same cash flows. A few examples of such deviations include on-the-run Treasuries that trade at lower yields than off-the-run Treasuries (Amihud and Mendelson, 1991), restricted resale stocks that trade at a substantial discount to publicly traded stock (Silber 1992), corporate bond vs. identical name CDS spreads (Duffie 2010), and corporate bond variations in spreads among identical CDS contracts (Arora, Gandhi, and Longstaff 2012). The sign of these deviations depend on the relative trading costs in both markets and whether costs are born more by buyers or sellers. A number of empirical papers including work by Jones (2001), Amihud (2002), and Acharya and Pedersen (2005), have documented that in modern markets stocks that are more illiquid trade at discounted prices and have higher expected returns. These results are

[80] The explicit costs includes commissions and the bid-ask spread, while implicit costs include price movement from larger orders (market depth) and borrowing costs to finance the trading position (margin).

also consistent with research on fire sales in asset prices (Coval and Stafford 2006, Benmelech and Bergman 2011) where sellers of assets are those in need of liquidity and thus willing to sell the security at a discount, which means that market illiquidity cost asymmetrically affect market participants and subsequently alter traded asset prices. So holding counterparty risk constant, if market liquidity were better (ex. lower bid-ask spreads) on exchange E than on E' we would expect the prices for identical securities on E' to trade at a discount. If on the other hand, market liquidity is lower on exchange E, but counterparty risk is higher on E than E' then the direction of the price spread is ambiguous. Since traders that face a liquidity shock are more likely to be asset sellers and a high counterparty risk in transactions, securities that trade on exchanges with higher counterparty risk are likely to trade at a relative discount.

To illustrate this point consider a simplified model with N risk neutral traders in a competitive market where each trader, n, has a random endowment of assets, i, each asset trades at a price P_i, and the total trader's portfolio value, A_n, is the aggregated value of all assets so that:

$$A_n = \sum_i P_{i,n}$$

Let each trader also owe a fixed value of notional debt, D_n, such that if, $A_n < D_n$, the trader is forced to liquidate all assets. If this forced liquidation occurs then all trading counterparties and debt holders recover a fixed percent, R_n, of the total liabilities owed which is just:

$$R_n = \frac{A_n}{D_n + S_n}$$

where S_n is the total amount owed by trader n for outstanding trades after settlement. Buyers of securities do not know the value of the trading portfolio of their counterparties, but do know the distributional properties of the endowment shock. Since markets are competitive and agents are risk neutral the value of any security for buyers is equal to

$$P_i = P_{i,fund}(1 - E_n[\,(1 - R_i)\,])$$

where $P_{i,fund}$ is the fundamental value of the security, in the absence of counterparty risk[81], and $E_n[\,(1 - R_i)\,]$ is the expected losses due to counterparty risk across all traders in asset i. As long as some positive number of traders are forced to sell assets then buyers will rationally discount the value of these securities. Since markets are competitive and subject to market clearing condition (i.e. assets are in short-run fixed net supply) traders only sell if forced to liquidate and all buyers of these securities are unconstrained (no counterparty risk) traders. Only those traders forced to liquidate have recoveries less than 100%, so counterparty risk induces a discounted price in equilibrium. As the expected recovery falls,

[81] Or other market trading costs, which for simplicity are excluded from this model.

this premium rises. So in times of distress, when endowment dispersion is large, this premium should be large, while in less turbulent times it could be close to zero. Just as in the case of other trading costs (ex. bid-ask spreads, trading fees) considered in models of market illiquidity by a number of previous authors including Amihud and Mendelson (1991), Acharya and Pedersen (2005), and Garleanu and Pedersen (2011), counterparty risk costs are asymmetrically born by buyers and sellers leading to predicable price changes in equilibrium.

4.2 Baseline Empirical Methodology

The expected change in the NYSE price after the introduction of clearing equals the change in the stock price caused by changes in the fundamental value minus changes in the expected market illiquidity and counterparty risk premia, or equivalently:

$$\mathrm{E}[\Delta P_{i,NYSE}] = E[\Delta P_i^{Fun}] - E[\Delta P_{i,t,NYSE}^{MktLq}] - E[\Delta P_{i,t,NYSE}^{CP}] \tag{3}$$

If we assume that the expected market illiquidity premium is unaffected by the introduction of multilateral netting, an assumption which we will examine later, we can rewrite (3) as:

$$\mathrm{E}[\Delta P_{i,NYSE}] = \mathrm{E}[\Delta P_i^{Fun}] - E[\Delta P_{i,t,NYSE}^{CP}] \tag{4}$$

where expected changes in price are driven by changes in expected fundamental value and the counterparty risk premium.

We are interested in estimating $E[\Delta P_{i,t,NYSE}^{CP}]$, the change in the counterparty risk premium caused by the introduction of multilateral netting. If the introduction of the clearinghouse were exogenous, we could simply estimate a panel regression

$$P_{i,t,NYSE} = \alpha_i + \mathrm{D}1_{\{clear,i,t\}} + \epsilon_{i,t}, \tag{5}$$

where $1_{\{clear,i,t\}}$ is a dummy variable indicating when a stock starts clearing and D is the average treatment effect of clearing on the stock price. The problem, as shown in equation (4), is that if the introduction of clearing coincides with changes in the fundamental value of the firm, omitted variables rather than counterparty risk changes could be driving results. Here, for example, the introduction of clearing on the NYSE was driven, in part, by financial panics in the early 1890s (McSherry and Wilson 2013). Without an alternative identification strategy, it would be impossible to identify the effect of the introduction of the NYSE clearinghouse. Fortunately, our historical experiment provides a unique opportunity to do exactly this.

Ideally, to determine the effect of clearing on counterparty risk, we would have prices for identical securities which do not experience any change in counterparty risk to control for changes in asset value not related to clearing. Fortunately, such securities exist. During the late 19th and early 20th centuries, stocks were dual-listed on the NYSE and CSE. Further, there was no change in the trading environment at the CSE when the NYSE introduced its clearinghouse. For the CSE price we have

$$P_{i,t,CSE} = P_{i,t}^{Fun} - P_{i,t,CSE}^{MktLq} - P_{i,t,CSE}^{CP} + \epsilon_{i,t,CSE} \tag{6}$$

Using the CSE prices as a control, the difference in prices between the dual-listed securities is:

$$P_{i,t,NYSE} - P_{i,t,CSE} = P_{i,CSE}^{MktLq} - P_{i,NYSE}^{MktLq} + P_{i,t,CSE}^{CP} - P_{i,t,NYSE}^{CP} + \epsilon_{i,t,NYSE} - \epsilon_{i,t,CSE} \tag{7}$$

where the fundamental value drops out of the equation. Then looking at the difference after the introduction of clearing we have

$$\mathrm{E}[\Delta P_{i,NYSE}] - \mathrm{E}[\Delta P_{i,CSE}] = E[\Delta P_{i,t,CSE}^{MktLq}] - E[\Delta P_{i,t,NYSE}^{MktLq}] - E[\Delta P_{i,t,NYSE}^{CP}] \tag{8}$$

so that the difference-in-differences between the expected prices on the two exchanges is caused by changes in the relative market illiquidity premium and changes in the counterparty risk premium on the NYSE. If there is no change in clearing on the CSE, then the expected change in the CSE counterparty risk premium, $E[\Delta P_{i,t,CSE}^{CP}]$, is zero and drops out of equation (8).

If the difference in expected market liquidity between the two exchanges is the same before and after the introduction of clearing on the NYSE[82], then the difference-in-difference in prices can be written as

$$\Delta\mathrm{E}[P_{i,t,NYSE}] - \Delta\mathrm{E}[P_{i,t,CSE}] = -E[\Delta P_{i,t,NYSE}^{CP}] \tag{9}$$

which is a causal estimate of the effect of clearing on the counterparty risk premium. Formally, our baseline empirical specification is

$$\hat{P}_{i,t,NYSE} - \hat{P}_{i,t,CSE} = \alpha_i + D1_{\{clear,i,t\}} + X_{i,t}{}'\beta + \epsilon_{i,t} \tag{10}$$

where $\hat{P}_{i,t,CSE}$ and $\hat{P}_{i,t,NYSE}$ are the normalized closing prices on the NYSE and CSE.

Throughout our analysis, we consider two normalizations for price: (1) dividing by the average closing prices on both exchanges and (2) dividing by the NYSE bid-ask spread. The former is natural since it is the percentage premium or discount an investor would require for buying the same stock on the NYSE relative to the CSE. The latter is also intuitive since it adjusts for the relative cost of trading the security and indicates how many bid-ask spreads the price on the NYSE deviates from the same security on the CSE. As discussed above, $1_{\{clear,i,t\}}$ is a dummy variable indicating when a stock starts clearing

[82] Of course, trading might migrate to the NYSE since the clearinghouse improved the NYSE trading environment which might also result in a degradation of trading conditions on the CSE. We examine this however, and as we show trading volumes and spreads on both exchanges remained relatively stable after the NYSE clearinghouse was introduced.

and D is the average treatment effect of clearing on the relative normalized stock prices. In addition, we include stock-specific time varying controls, $X_{i,t}$, including bid-ask spreads and volumes.

It is important to note that in this core specification, we are implicitly assuming that there are no spillovers in counterparty risk reduction when only a fraction of NYSE stocks join the clearinghouse. That is, it is likely that counterparty risk for stocks not yet cleared is likely to fall once a sufficient fraction of NYSE stock volume is cleared. We investigate such spillover effects in section 4.4.

4.3 Price Volatility Induced by Counterparty Risk

Because counterparty risk was driven by the costs of financing overnight positions, we expect the counterparty risk premium to be small during periods of calm, but increase dramatically during times of financial market distress. Because the cost of financing overnight positions was likely much less after the onset of multilateral netting, its introduction may have significantly reduced or eliminated the impact of short-term financing shocks on NYSE stocks. Hence, interest rate shocks should not reduce stock prices on the NYSE relative to the CSE after the establishment of a clearinghouse[83]. We formalize this test by interacting call loan rates with the clearinghouse dummy to yield the following specification

$$\hat{P}_{i,t,NYSE} - \hat{P}_{i,t,CSE} = \alpha_i + D1_{\{clear,i,t\}} + D_2 C_t \times 1_{\{clear,i,t\}} + \phi C_t + X_{i,t}{}'\beta + \epsilon_{i,t} \qquad (11)$$

where C_t is the call loan rate, ϕ is the estimated effect of call loan rate spikes on NYSE relative prices pre-clearing, and D_2 is the estimated effect of the introduction of clearing on call loan rate sensitivity.

Before the introduction of the NYSE clearinghouse, interest rate volatility and the volatility of the NYSE-CSE price spread will move in response to fluctuations in counterparty risk. If we consider the change in volatility of the price difference, instead of the expectation, and make slightly stronger assumptions (relative to those needed to arrive at equation 9)[84] then we can rewrite equation (9) as:

$$\Delta\sigma[P_{i,t,NYSE} - P_{i,t,CSE}] = \Delta\sigma[P^{CP}_{i,t,NYSE}] \qquad (12)$$

Equation (12) indicates that the change in the volatility of the price premium provides an estimate of the change in counterparty risk volatility caused by clearing. We estimate the volatility of price spreads by

[83] One might wonder whether the onset of multilateral netting might also affect the magnitude of interest rate shocks. Although this is theoretically a possibility, anecdotal evidence (see Meeker, 1922) suggests that the main driver of shocks to the call loan rate was the commercial paper market. Indeed Bernstein *et al.* (2010) find the correlation between the commercial paper rate and the call loan rate were over 90% during our sample period.

[84] Previously we assumed no changes in the relative market illiquidity premium. In this case we need to assume no changes in the volatility of the market illiquidity premium, but in addition we have to assume no change in the volatility of relative market microstructure noise or in the covariance between the counterparty risk premia, market illiquidity premia, and/or market microstructure premia.

taking the absolute value of the price differences between the exchanges on each date normalized by the average closing price on the exchanges and then scaling by a constant to generate an estimate for the volatility[85]. In our robustness analysis, we also consider the volatility estimator using the ratio of the high and low prices on each exchange presented in Parkinson (1980).

4.4 Counterparty Risk and Contagion

Counterparty risk can be divided into two parts: contagion risk and direct counterparty risk. Contagion risk is higher for an asset when a broker is more likely to default on other positions, starting a cascade which results in default on a trade for that asset. When other stocks start to clear, contagion risk is smaller, even if the asset is traded through a clearinghouse. We define the reduction in direct counterparty risk as the direct effect of a stock clearing after accounting for any contagion risk reduction. One of the benefits of analyzing the introduction of clearing on the NYSE is that clearing was introduced in stages. Using prices on the CSE as a control again, we can decompose the volatility induced by counterparty risk by estimating the following model

$$|\hat{P}_{i,t,NYSE} - \hat{P}_{i,t,CSE}| = \alpha_i + D1_{\{clear,i,t\}} + \gamma PercClear_{i,t} + X_{i,t}{}'\beta + \epsilon_{i,t} \tag{13}$$

$PercClear_{i,t}$ is the percentage of stocks already clearing.[86] We also include a dummy for the stock that is clearing which allows a natural interpretation for D as the change in counterparty risk caused by direct counterparty risk, while γ is the percent caused by a change in contagion risk. Since the breakdown of these two types of risk depends on how connected traders of those securities are to the network of traders, we would expect these to vary across securities. In particular we might expect securities with traders who are more exposed to traders in the rest of the network, such as large firms with high volume securities, to be more exposed to contagion risk[87].

[85] If $X \sim N(\mu, \sigma)$ then the absolute value of X is distributed folded-normally. Then if the expected normalized price difference is sufficiently small relative to the volatility then the volatility is proportional to the absolute value of X. In particular, $\sigma \approx \sqrt{\frac{\pi}{2}} E[|X|]$. In our analysis the expected normalized price difference is significantly smaller than the volatility so our estimated volatility using this approximation are within ~1bp of the change in volatility accounting for any changes in the mean normalized price difference. For a complete discussion of the estimator and its properties see appendix B.

[86] We consider weights by both \$ sales and equally weighted, but focus on \$ sales for our primary analysis since it is more representative of the actual volume of trading of the security.

[87] While this seems intuitive since high trading volumes would seem to suggest more interconnected traders, without specifics on the exact nature of the network it is inevitably impossible to know with certainty which security types are most exposed to contagion. Inevitable it becomes an empirical question based on how D in specification 13 varies with security trading volume.

5 Results

We first compare the sign and volatility of the counterparty risk premium before and after the introduction of clearing on the NYSE. To do so, we reconsider equation (7):

$$P_{i,t,NYSE} - P_{i,t,CSE} = P_{i,CSE}^{MktLq} - P_{i,NYSE}^{MktLq} + P_{i,t,CSE}^{CP} - P_{i,t,NYSE}^{CP} + \epsilon_{i,t,NYSE} - \epsilon_{i,t,CSE}$$

Because the NYSE is more liquid than the CSE (Brown et al. 2008 and Table 1 summary statistics), the price discount due to illiquidity should be smaller on the NYSE, $E\left[P_{i,CSE}^{MktLq} - P_{i,NYSE}^{MktLq}\right] > 0$. Therefore, when counterparty risk is small, stocks should trade at a premium on the NYSE relative to the CSE. In times of financial market crisis *before* stocks are cleared on the NYSE, stocks on the NYSE might well trade at a discount instead because during crises, counterparty risk might be much larger on the NYSE than on the CSE. Before the introduction of clearing on the NYSE then, stocks trade at a discount on the NYSE when the counterparty risk premium is high and at a slight premium otherwise. If the introduction of clearing on the NYSE eliminates (or substantially reduces) counterparty risk there, equation (7) implies that that after the onset of clearing, prices on the Big Board should be consistently higher than those on the CSE.

In figure 2 we plot the average for all Dow stocks of the 12-month moving average of the price on the NYSE minus the price on the CSE normalized by the NYSE bid-ask spread. Prior to the introduction of clearing this price difference is highly volatile, but after the introduction of clearing, stocks on the NYSE consistently trade at a premium. In Table 2, we estimate equation (10) to show that the introduction of clearing on the NYSE reduces the average counterparty risk premium by 24bps or 0.73 NYSE bid-ask spreads[88]. NYSE prices are 9bp lower on average than CSE prices prior to clearing, but 15bp *higher* afterward. This result is robust to including stock-specific time-varying market liquidity controls on the NYSE and CSE, including the bid-ask spread on the NYSE, the dollar trading volume on the NYSE, and the dollar trading volume on the CSE. The result is not robust, however, to not using the CSE as control[89]. This highlights the importance of using identical securities traded on the CSE to control for the changing macroeconomic environment.

The 24bps reduction is a substantial decline in the counterparty risk premium. This estimate for the reduction in the counterparty risk premium is on the high end of those obtained in analyses of modern counterparty risk in the credit derivative markets. Arora et al. (2012) note that estimates of the size of the

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[88] The specification includes firm fixed effects, clustering standard errors at the stock level, and using identical securities on the CSE as a control.

[89] These results are available from authors upon request.

counterparty risk premium for credit default swaps in the modern period range from 7-20bps. If we scaled the effects to size of the modern NYSE this would equate to approximately a $40 billion increase in value caused by the reduction in counterparty risk from the introduction of a clearinghouse[90].

We next investigate the drivers of the counterparty risk premium on the NYSE. Because brokers had to fund substantial levered positions overnight, shocks to overnight borrowing rates were an important determinant of counterparty risk prior to clearing on the NYSE. In figure 2, we also plot the 12-month moving average of the broker's call loan rate. As expected, prior to the introduction of clearing NYSE stocks tend to trade at a discount relative to identical securities on the CSE during periods when the call loan rate is high and at a premium when call loan rates are low. In table 2, we formally investigate whether high call loan rates are associated with price discounts on the NYSE. We find that call loan rates appear unrelated to changes in the NYSE-CSE relative prices after the introduction of clearing. Column 4 shows that there is not a statistically significant relationship between the normalized difference in NYSE and CSE prices and the call loan rate for the full sample period. This is because the relationship is masked by the change in the relationship between call loan rates and counterparty risk after the introduction of clearing. In Column 5, we estimate equation (10). We find that before the introduction of clearing, a one standard deviation increase in the call loan rate in the pre-clearinghouse period is associated with approximately an 8bp reduction in the price on the NYSE relative to the CSE[91]. The effect is not statistically significant, however, after the introduction of clearing. As expected, we do not find evidence of a relationship between call loan rates and our normalized measure of relative NYSE-CSE prices after a stock joins the clearinghouse (see column 6). The result is consistent with the introduction of clearing mitigating the impact of funding shocks on counterparty risk for NYSE stocks. In table A5 of the online appendix we rerun the analysis, but instead look at the effect of commercial paper rates on the premium before and after the introduction of clearing. Again we find that a rise in funding costs reduces the value of the NYSE stocks, but that this is no longer true after the introduction of the NYSE clearinghouse. These results hold for both rates, though are stronger for call loan rates, when both measures of funding costs are included[92].

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[90] Market cap of $16.6 trillion for NYSE taken from NYSE website as of August 2014.

[91] We find that a one percentage point increase in the call loan rate is associated with more than a 2bps reduction in the relative price of NYSE stocks that also trade on the CSE and the standard deviation of the call loan rate was 3.7 percent before the introduction of the NYSE clearinghouse.

[92] These results are consistent with a relationship between counterparty risk and costs of borrowing. Since even call loan rates were overnight borrowing rates, but the NYSE clearinghouse in 1892 only engaged in night clearing, the relationship between counterparty risk and call loan rates likely arises from the high correlation between intraday borrowing rates and overnight borrowing rates rather than call loan rates directly. This is also consistent with clearinghouses not causing or serving as a panacea for macroeconomic financial crises, but rather that the absence of a clearinghouse can exacerbate a crisis, by increasing market turbulence and contagion risk.

After the introduction of clearing on the NYSE, shocks to the call loan rate no longer affect prices on the NYSE relative to those on the CSE. Call loan rates continue to be volatile, however (see figure 2). Therefore, we would expect a decline in the volatility of NYSE returns given the reduction in the volatility of the counterparty risk premium. In figures 3 and 4, we observe a dramatic decline in the volatility of the counterparty risk premium after the introduction of clearing. In table 3, we show that the monthly average absolute price difference of the NYSE relative price falls 20bps or 0.93 NYSE bid-ask spreads after the introduction of clearing. These results are robust to including stock-specific time-varying market liquidity controls such as bid-ask spreads on the NYSE and CSE and the broker call loan rate interacted with a post-clearinghouse dummy. As we discussed previously, the results represent a lower bound on the effects of clearing since other stocks clearing reduce the counterparty risk for non-clearing stocks, reducing the estimated effect of clearing on counterparty risk. Since most stocks were already clearing by the end of 1893, we include a post-1893 dummy variable instead of the post-clearinghouse dummy. Post-1893, the average absolute price deviation fell by 40bps. Scaling the absolute values by $\sqrt{\frac{\pi}{2}}$ to obtain estimates of the change in standard deviation and then annualizing these monthly estimates suggests that the introduction of the clearinghouse reduced the annualized volatility of the returns on the NYSE by 90-173bps[93]. Since, the average annualized volatility for stocks on the Dow Jones was 29.6% this represents a 3.0%-4.8% reduction in annualized volatility. Now if we assume further that approximately one-tenth of this is systematic risk and the slope of the security market line is approximately 0.3 then this would imply a decline in the counterparty risk premium of 9-14bps coming from the increased volatility or approximately one-third to one-half of the total decline in the counterparty risk premium we estimated[94].

In table 4, we attempt to distinguish the effects of contagion risk through network spillovers from the effects of direct counterparty risk. We first include monthly date fixed effects and find that the point estimate for the effect of clearing on the counterparty risk premium volatility falls from -0.93 (column 2 of table 3) to -0.37 (column 1 of Table 4) when normalizing by bid-ask spread, but only moves from -0.20 (column 3 of table 3) to -0.16 (column 2 of table 4) when normalizing by stock price. This suggests that

[93] If instead of assuming normality we bootstrap from the original residual distribution we obtain similar estimates of 111-217bps decline in annualized return volatility. Since these results are similar to those obtained under normality and those under normality are slightly more conservative we focus primarily on that interpretation. We thank Neil Shephard for the suggestion.

[94] Based on statistics in McSherry et al (2014) it appears that as a percent of total NYSE trading volume initial reported losses from broker insolvencies fall approximately 42bps in the period after the introduction of a clearinghouse. If we account for subsequent partial recovery of those losses, this appears consistent in magnitude with the estimates we obtain for the fall in counterparty risk premium coming from the decline in expected losses with our formal difference-in-difference analysis of prices.

the netting of other stocks increases the prices of stocks that have not yet cleared and that effect is picked up by the date fixed effects. The clearing dummy remains marginally significant only when we normalize by the bid-ask spread. If large firms have a high price, a low bid-ask spread, and large trading volume this is what we would expect because traders in those securities would be more exposed to traders in the rest of the broker network. To test this explicitly, in column 3 we remove the date fixed effects and replace them with a dummy for clearing for the percentage of all stocks clearing. We find the post-clearinghouse dummy is now a statistically significant -0.56 bid-ask spreads and the coefficient on the percentage of all stocks clearing is a marginally significant 0.51[95]. Hence, spillover effects are likely to be important for the reduction of counterparty risk.

Since contagion risk depends on how connected traders of a given stock are to the rest of the trader network, we expect stocks trading higher volumes (relative to their average) on a particular day to be more affected by others stocks clearing because they are more connected to the network. In columns 5 and 6 of table 4 we consider the effect of the percentage of stocks clearing on the relative prices of stocks that have not yet cleared and. include a dummy for high trading volume[96]. Prior to clearing, on high volume days counterparty risk premium volatility is higher on the NYSE, but that effect disappears as more and more Dow stocks clear. In particular, the reduction in the counterparty risk (relative stocks on low-volume days) is 0.77 bid-ask spreads times the percentage of Dow stocks clearing (column 4) or 30bp times the percentage of Dow stocks clearing (column 5). If we combine the results of the high trading volume dummy and the interaction term, we can see that prior to clearing stocks with a high trading volume on a given day are associated with large volatility in the price difference, but after clearing the difference is no longer statistically significant. On low volume days, the volatility of the counterparty risk premium does not change in a significant way after the onset of clearing.

We run a number of robustness checks to test whether our results are driven by changes in counterparty risk coming from the introduction of clearing, or changes in asynchronous trading, market liquidity, or financial crises. If asynchronous trading declines after the introduction of clearing, this might confound interpretation of our results. Despite the sudden decline in counterparty risk depicted by figures 3 and 4, there is not a sudden increase in trading volume[97] that would be consistent with a story about a decline or change in asynchronous trading for the two rival exchanges[98]. The lack of any sudden change

[95] The co-efficient on the *percent of Dow Clearing* has a natural interpretation since it is the expected reduction in counterparty risk if 100 percent of all other stocks clear.
[96] The *High Volume* dummy is 1 for stocks with a trading volume higher than the median trading volume.
[97] For more details see figure A1 of the online appendix.
[98] In addition to practical frictions which could slow any transition of volume from one exchange to another it has been shown that in the presence of limited competition, as existed during this period, market makers can earn

in volume is also inconsistent with results being driven by changes in relative market liquidity. In columns 1 and 2 of table 5 we do not find a significant change in relative trading volumes after the introduction of clearing. We also show in column 3 that there is little evidence of increased relative price impact since the Amihud illiquidity measure sees no statistically significant change. We also show in columns 4-6 of table 5 that all baseline results are robust to restricting our analysis to only days with at least 500 shares (5 standard contracts) traded on both exchanges and including non-linear relative measures of market liquidity on both exchanges for each stock as a control. In columns 1-3 of table A3 of the online appendix, we show that the basic tenor of the results remain unchanged when we use daily data for all stocks on the NYSE or CSE. The results hold if we consider only stocks with at least 20 observations before and after the introduction of clearing, including estimated bid-ask spreads on the CSE as a control, and using open instead of closing prices. Again, the findings are not consistent with changes in asynchronous trading or market liquidity as drivers of the change in the relative NYSE-CSE price volatility after the introduction of clearing. We examine NYSE-CSE relative bid-ask spreads for the same securities in figure A2 and Column 6 of table A3 of the online appendix. We again do not find evidence of a sudden change in the relative market liquidity between the NYSE and CSE. Even though there is not a statistically significant change in any of our market liquidity proxies it is still theoretically possible for them to affect pricing, so in column 6 of table 5 we include controls for *$ Volume (NYSE-CSE)*, *Volume (% CSE)*, the Amihud illiquidity measures on both exchanges and their ratio, seat prices on the NYSE and CSE and their ratio[99], and natural logs of dollar volume on both the NYSE and CSE. The post-1983 dummy remains statistically significant with the market liquidity controls suggesting that changes in the relative prices are not driven by any changes in the market liquidity on either exchange[100]. As a further robustness check we also rerun our analysis using identical securities simultaneously listed on the Boston Stock Exchange as a control in column 8 of table A3 and find that results are consistent with our specifications using the CSE[101]. We again find a decline in price dispersion after the clearinghouse is introduced on the NYSE relative to identical securities' closing prices on the BSE.

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positive profits and relationship dealers could prevent trading on either exchange from disappearing (Bernhardt et al. (2005) and Desgranges et al. (2005)).

[99] Since the number of seats on the NYSE were fixed the primary driver of seat prices were changes in trading volume. Thus changes in seat prices provide a good estimate of changes in expectations about future exchange trading volumes.

[100] We also found no substantive changes in corporate governance on either exchange around this time period, besides those related to the introduction of a clearinghouse on the NYSE.

[101] The BSE introduced a clearinghouse in January of 1892. Several securities were dual listed on the BSE and the NYSE, although not as many as the CSE. The fact that we find similar results using the BSE as a control should alleviate concerns that a CSE-specific change could be driving results.

As a robustness check for our volatility estimator, in column 7 of table A3 of the online appendix, we use the volatility estimator based on the high and low values on each exchange as the dependent variable. According to Parkinson (1980), the difference between the high and low values is proportional to volatility. The results in column 7 suggest that stocks on the CSE that also traded on the NYSE had 4% lower volatility when including market liquidity controls before the introduction of the clearinghouse. The difference in volatility between NYSE and CSE dual listed securities disappeared after the onset of clearing on the Big Board. The 4% reduction in the volatility of NYSE securities is statistically significant and consistent with the 3.0%-4.8% estimate obtained using the primary volatility estimator in this paper.

Another possibility is that the reduction in counterparty risk is driven by reduced macro-economic risk, independent of the introduction of clearing. First, we find, that relative prices were no longer sensitive to call loan rate shocks after the introduction of clearing which suggests that changes the volatility of call loan shocks, even if they did occur, do not drive our results. The possibility also seems unlikely because in the period after clearing there were numerous major panics, including the Panic of 1907, where call loan rates increased precipitously. Indeed, the incidence of financial crises did not fall until the introduction of the Federal Reserve (see Bernstein et al. 2010 and Figure A3 of the online appendix). In 1911, Shea noted that "the clearing system of the exchange was severely tested during the Panic of 1907, and its efficiency was fully demonstrated." The results are also robust to restricting our analysis to the period prior to the passage of the Aldrich-Vreeland Act in 1908 and the subsequent introduction of the Federal Reserve (Column 7 of table 5). This leaves a 17 year period after the introduction of clearing on the NYSE where conditions were as ripe for financial crises as the period prior to 1892.

Examining the period prior to 1907 also shows the results are not driven by the introduction of the mutualization of risk on the NYSE clearinghouse in April of 1920, accusations of fraud on the Consolidated Stock Exchange beginning in February of 1922, or the subsequent decline in volume on the CSE. In table 6 we explicitly examine the introduction of mutualization of risk in April of 1920 prior to the accusations of fraud on the CSE in February of 1922. We do not find statistically significant evidence of changes in counterparty risk driven by mutualization of risk. These results should be interpreted with caution given the limited post-mutualization period, but we do not find any evidence that the reduction in counterparty risk caused by introduction of clearing in 1892 were negated, or significantly improved, by the separate introduction of mutualization of risk.

6 Conclusion

The dramatic rise in counterparty risk in the OTC derivatives markets during the recent financial crisis has brought the role clearinghouses play in reducing market turbulence to the forefront of public policy debate. In this paper, we show that a clearinghouse can improve financial stability in asset markets by reducing counterparty risk. We use a novel historical experiment to cleanly identify the change in counterparty risk of NYSE stocks after the introduction of a clearinghouse in 1892. We can identify the effect of introducing clearing for NYSE stocks because the same securities were trading concurrently on the Consolidated Stock Exchange, a rival exchange that already had centralized clearing. This is important, because the introduction of clearing is usually driven by macro-economic turbulence, so that before vs. after comparisons can be contaminated by changes in fundamental security value and risk. In our setting, however, changes in counterparty and illiquidity risk can be more easily attributed to the introduction of a clearinghouse. Our results suggest that prior to the introduction of net settlement on the NYSE, identical stocks on the NYSE traded at a discount of 9bp relative to the Consolidated Stock Exchange, the NYSE's principal competitor. After the establishment of a clearinghouse, NSYE stocks traded at a premium of 15bp. The difference of 24bp is statistically significant. Furthermore, the change can be attributed almost entirely to the reduction in counterparty risk.

Before the establishment of the NYSE clearinghouse, the NYSE traded at a premium relative to the same stocks on the CSE the majority of the time. However, when overnight collateralized borrowing rates rose sharply, prices on the NYSE fell precipitously relative to those on the CSE. A one standard deviation increase in interest rates (3.7 percentage points) reduced the value of stocks on the NYSE by 8bp, relative to identical stocks on the CSE. After the introduction of clearing, the difference between prices on the NYSE and the CSE were no longer affected by changes in these overnight funding rates. Call loan rates remained volatile, but annualized NYSE stock return volatility fell dramatically after clearing by 90-173bps. We also use the staggered introduction of clearing on the NYSE to show that at least half of this reduction in counterparty risk is driven by a reduction in contagion risk through spillovers in the trader network.

Overall, our results indicate that clearinghouses can play a significant role in improving market stability and increase asset values by reducing network contagion and counterparty risk. Two of the primary functions of clearinghouses are netting without novation and mutualization of risk. We demonstrate that even in the absence of a centralized counterparty, policies aimed at introducing centralized clearing through a clearinghouse can substantially increase netting and subsequently improve global financial stability.

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Table 1. NYSE and CSE Summary Statistics

This table reports the sample statistics for the trading data for stocks on the New York Stock Exchange (NYSE) and Consolidated Stock Exchange (CSE). Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. All data is winsorized at the 1st and 99th percentile. To be included in the first level of summary analysis a security must trade at least 1 share on both exchanges on a given date, while for the second, which is the one used in our primary econometric specifications, we require at least 200 shares (2 standard contracts).

	NYSE Closing Price	NYSE Trading Volume (# Shares)	CSE Trading Volume (#Shares)	NYSE Trading Volume ($000s)	CSE Trading Volume ($000s)	NYSE Bid-Ask Spread (bps)
			With Minimum 1 Shares *Traded (n = 9,373)*			
Mean	84.4	13,726	3,241	1,352	322	52
Median	81.4	4,400	410	324	29	32
Std Dev.	42.4	29,340	8,710	3,304	965	66
Minimum	4	5	5	0.2	0.04	7
Maximum	323	489,444	291,870	52,300	24,100	1,818
			With Minimum 200 Shares *Traded (n = 6,065)*			
Mean	85.5	19,911	4,958	1,972	493	41
Median	81.0	8,425	1,150	644	88	26
Std Dev.	41.1	34,912	10,432	3,966	1,164	52
Minimum	4.7	200	200	3.6	1.4	7
Maximum	319.5	489,444	291,870	52,300	24,100	1,481

Table 2. Price Deviations and Establishment of NYSE Clearinghouse

Following econometric specifications (9) and (10), in this table we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In column 1, *NYSE-CSE/Close* is the price on the NYSE minus the CSE normalized by the average closing price on both exchanges. *Clearinghouse* is a stock-specific dummy variable which equals 1 if a stock is cleared on the NYSE. In column 2, *NYSE-Con/NYSE Bid-Ask* is the LHS variable and is the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE. Column 3 shows the results including stock-specific time-varying market liquidity controls on the NYSE and CSE. These include the bid-ask spread on the NYSE, the dollar trading volume on the NYSE, and the dollar trading volume on the CSE. Column 4 shows the results after including *Call Loan Rate (%)*, the overnight collateralized borrowing rate. Column 5 includes an interaction term between the *Clearinghouse* dummy variable and the *Call Loan Rate (%)* as described in specification (10). Column 6 repeats the analysis in column 4, but restricting the sample to only stocks already clearing. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	NYSE-CSE /Close (%)	NYSE-CSE / NYSE Bid-Ask	NYSE-CSE /Close (%)	NYSE-CSE /Close (%)	NYSE-CSE /Close (%)	NYSE-CSE /Close (%)
Clearinghouse	0.237***	0.733***	0.234***	0.230***	0.122*	
	(0.062)	(0.126)	(0.061)	(0.061)	(0.068)	
Call Loan Rate				-0.0029	-0.0217***	0.0022
				(0.0019)	(0.0055)	(0.0017)
Call Loan Rate x Clearinghouse					0.0247***	
					(0.0058)	
Constant	-0.094**	-0.295***	-0.107**	-0.093*	-0.0083	-0.0062
	(0.040)	(0.082)	(0.045)	(0.048)	(0.0055)	(0.023)
Security Fixed Effects	Y	Y	Y	Y	Y	Y
Liquidity Controls	N	N	Y	Y	Y	Y
Only Clearinghouse	N	N	N	N	N	Y
# Clusters	90	90	90	90	90	51
# Observations	5,997	5,984	5,994	5,994	5,994	3,904
Adjusted R-squared	0.0086	0.0056	0.0104	0.0105	0.0138	0.0326

Table 3. Counterparty Risk Premium and Establishment of NYSE Clearinghouse

Following econometric specifications (9), (10) and (11), in this table we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the *volatility* of the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day by looking at the absolute value of the relative price differences. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from Sept 1886 – Dec 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In column 1, *|NYSE-CSE|/Close* absolute value of the price on the NYSE relative to the CSE, normalized by the average closing price on both exchanges in percent. *Clearinghouse* is a stock-specific dummy variable which equals 1 if a stock is cleared on the NYSE. In column 2, *|NYSE-CSE|/NYSE Bid-Ask* is the volatility of the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE. Column 3 shows the results with stock-specific time-varying market liquidity controls on the NYSE and CSE including the stock's bid-ask spread on the NYSE and dollar trading volume on NYSE, and CSE. Column 4 shows results after including *Call Loan Rate (%)*, the overnight collateralized borrowing rate, and an interaction term between the *Clearinghouse* dummy variable and the *Call Loan Rate (%)* as described in specification (10). Column 5 repeats the analysis in column 4, but restricting the sample to only stocks already clearing. Column 6 includes a dummy, *Post 1893,* which is equal to 1 for all securities clearing after 1893 and zero prior to May 1892. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: *10%; **5%; ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	|NYSE-CSE| /Close (%)	|NYSE-CSE| / NYSE Bid-Ask	|NYSE-CSE| /Close (%)	|NYSE-CSE| /Close (%)	|NYSE-CSE| /Close (%)	|NYSE-CSE| /Close (%)
Clearinghouse	-0.204**	-0.929***	-0.207***	-0.174**		
	(0.105)	(0.323)	(0.082)	(0.089)		
Post 1893						-0.399***
						(0.077)
Call Loan Rate				0.0081*	0.0009	
				(0.0046)	(0.0015)	
Call Loan Rate x Clearinghouse				-0.0067		
				(0.0049)		
Constant	0.544***	1.840***	0.397***	0.360***	0.174***	0.557***
	(0.069)	(0.210)	(0.055)	(0.064)	(0.016)	(0.0068)
Security Fixed Effects	Y	Y	Y	Y	Y	Y
Liquidity Controls	N	N	Y	Y	Y	Y
Only Clearinghouse	N	N	N	N	Y	N
# Clusters	90	90	90	90	51	54
# Observations	5,997	5,984	5,994	5,994	3,904	4,314
Adjusted R-squared	0.223	0.165	0.293	0.293	0.157	0.171

Table 4. Contagion (Indirect Counterparty) Risk

Following econometric specifications (12), in this table we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day broken out by contagion risk and direct counterparty risk. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from Sept 1886 – Dec 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In Column 1, *|NYSE-CSE|/NYSE Bid-Ask* is an estimate of monthly volatility of the price on the NYSE relative to the CSE, normalized by the bid-ask spread on the NYSE. *Clearinghouse* is a stock-specific dummy variable which equals 1 if a stock is cleared on the NYSE. This column includes date fixed effects. Column 2 is the same as Column 1, but *|NYSE-Con|/NYSE Close* is the volatility of the price on the NYSE minus the CSE normalized by the average closing price on both exchanges in percent. Column 3 includes the effects of spillovers by including, *% of Dow Clearing,* which is the percent of NYSE stocks in a Dow Jones Index currently clearing in addition to the *Clearinghouse* dummy. Column 4 restricts the sample to only stocks not clearing to show spillover effects and contagion risk. This regression includes variable, *High Trading Volume,* which is 1 if the $ trading volume is higher than the median for all stocks over the period. This variable is then interacted with *% of Dow Clearing*. Column 5 is the same as Column 4 but looks at *|NYSE-CSE|/NYSE Close.* All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: *10%; **5%; ***1%.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	|NYSE-CSE| / NYSE Bid-Ask	|NYSE-CSE| /Close (%)	|NYSE-CSE| /NYSE Bid-Ask	|NYSE-CSE| /NYSE Bid-Ask	|NYSE-CSE| /Close (%)
% of Dow Clearing			-0.508*	0.328	0.020
			(0.295)	(0.278)	(0.16)
Clearinghouse	-0.370	-0.156*	-0.558***		
	(0.269)	(0.088)	(0.108)		
High Volume Dummy x % of Dow Clearing				-0.772**	-0.300**
				(0.308)	(0.137)
High Volume Dummy				0.538**	0.201**
				(0.203)	(0.096)
Constant	1.830**	0.582***	2.091***	1.199***	0.329***
	(0.119)	(0.038)	(0.210)	(0.193)	(0.098)
Security Fixed Effects	Y	Y	Y	Y	Y
Date Fixed Effects	Y	Y	N	N	N
Stock Liquidity Controls	Y	Y	Y	Y	Y
Only Pre-Clearing Stocks	N	N	N	Y	Y
# Clusters	90	90	90	50	50
# Observations	5,983	5,994	5,983	2,086	2,090
Adjusted R-squared	0.236	0.326	0.186	0.332	0.398

Table 5. Microstructure Noise and Market Liquidity Robustness Tests

In this table, we show that the introduction of clearing on the NYSE is not associated with a change in the relative trading on the NYSE vs. the CSE and that the introduction of multilateral net settlement through a centralized clearing party reduced the premium and volatility of the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from Sept 1886 – Dec 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In column 1, *$ Volume (NYSE-CSE)*, is the difference in the dollar volume of trading for a stock on the NYSE minus the volume on the CSE on the same day. *Clearinghouse* is a stock-specific dummy variable which equals 1 if a stock is cleared on the NYSE. Column 2 shows the same as column 1, but now the looking at *Volume (% CSE)*, which is the dollar trading volume on the CSE divided by the sum of the trading volume on the NYSE and CSE for a given security on a given day. Column 3 is the same specification as Column 2, but the left-hand side variable is the Amihud illiquidity measure, $(|NYSE\ Ret_t|/NYSE\ Sales_t)/(|CSE\ Ret_t|/CSE\ Sales_t)$. In specifications in columns 4-5 securities are restricted to those with at least 500 contracts trading on the NYSE and CSE on a given day. In column 4, *|NYSE-Con|/Close* is an estimate of monthly volatility of the price on the NYSE relative to the CSE by looking at the absolute price deviation, normalized by the average closing price on both exchanges in percent. In Column 5, *NYSE-CSE/Close* is the price on the NYSE minus the CSE normalized by the average closing price on both exchanges. Column 6 is the same as column 5 but only restricts to at least 200 shares traded on both exchanges and includes relative stock-specific time-varying market liquidity controls. These include *$ Volume (NYSE-CSE)*, *Volume (% CSE)*, the Amihud illiquidity measures on both exchanges and their ratio, seat prices on the NYSE and CSE and their ratio, and natural logs of $ volume on both the NYSE and CSE. Column 7 repeats the baseline results in Table 2 column 1, but only for the period prior to passage of the Aldrich-Vreeland Act in 1909. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: *10%; **5%; ***1%.

Dependent Variable:	(1) $000s Volume (NYSE-CSE)	(2) Volume (% CSE)	(3) Amihud Illiquidity (NYSE/Con)	(4) |NYSE-Con| /Close (%)	(5) NYSE-CSE /Close (%)	(6) NYSE-CSE /Close (%)	(7) NYSE-CSE /Close (%)
Clearinghouse	251	-3.45	-0.103	-0.226**	0.289***	0.224***	0.238***
	(354)	(2.49)	(0.067)	(0.101)	(0.077)	(0.079)	(0.059)
Constant	1,311***	20.96***	0.363***	0.386***	-0.147***	-0.245	-0.152***
	(231)	(1.62)	(0.044)	(0.0697)	(0.059)	(0.203)	(0.055)
Security Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Stock Liquidity Controls	N	N	N	Y	Y	Y	Y
Relative Liquidity Controls	N	N	N	N	N	Y	N
Period	1886-1925	1886-1925	1886-1925	1886-1925	1886-1925	1888-1925	1886-1908
Min Traded Shares	200	200	200	500	500	200	200
Data Frequency	Monthly	Monthly	Monthly	Monthly	Monthly	Monthly	Monthly
Price Used	Close	Close	Close	Close	Close	Close	Close
# Clusters	90	90	89	85	85	88	62
# Observations	5,996	5,996	5,623	4,272	4,272	5,504	2,983
Adjusted R-squared	0.213	0.280	0.185	0.306	0.019	0.020	0.010

Table 6. Counterparty Risk and the Introduction of NYSE Novation

In this table we show that the introduction of novation (mutualization of risk though a centralized counterparty) on the NYSE in April 1920 does not appear to significantly affect the counterparty risk premium between the NYSE and CSE. Security market data were hand collected and are analyzed at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from Sept 1886 – January 1922 for all stocks in the Dow Jones Indices. The period February 1922-December 1925 is excluded from this analysis because of accusations of fraud on the CSE, which eventually led to its downfall, beginning with the failure of MacMasters& Co. in February of 1922. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In column 1, *NYSE-CSE/Close* is the price on the NYSE minus the CSE normalized by the average closing price on both exchanges and *Novation* is a dummy variable equal to 1 if the date is after April 1920. Column 2 is the same as column 1 but includes relative stock-specific time-varying market liquidity controls. These include *$ Volume (NYSE-CSE), Volume (% CSE),* the Amihud illiquidity measures on both exchanges and their ratio, seat prices on the NYSE and CSE and their ratio, and natural logs of $ volume on both the NYSE and CSE. In column 3, *NYSE-Con/NYSE Bid-Ask* is the LHS variable and is the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE. In column 4, *|NYSE-Con|/Close* is an estimate of monthly volatility of the price on the NYSE relative to the CSE by looking at the absolute price deviation, normalized by the average closing price on both exchanges in percent. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: *10%; **5%; ***1%.

Dependent Variable:	(1) NYSE-CSE / Close (%)	(2) NYSE-CSE / Close (%)	(3) NYSE-CSE /NYSE Bid-Ask	(4) |NYSE-CSE| /NYSE Bid-Ask
Novation	-0.0462	-0.1198	-0.1391	0.0912
	(0.0434)	(0.0762)	(0.1240)	(0.1011)
Constant	0.0548***	-0.326	0.1828***	1.064***
	(0.0028)	(0.212)	(0.0079)	(0.0065)
Security Fixed Effects	Y	Y	Y	Y
Stock Liquidity Controls	N	Y	N	N
NYSE/CSE Stock Liquidity Controls	N	Y	N	N
Period	Sep 1886 – Jan 1922	Sep 1886 – Jan 1922	Sep 1886 – Jan 1922	Sep 1886 – Jan 1922
Only Post-clearing Stocks	Y	Y	Y	Y
# Clusters	51	50	51	51
# Observations	3,487	3,313	3,479	3,479
Adjusted R-squared	0.019	0.046	0.012	0.017

Figure 1. Timeline of Introduction of Clearing on New York and Consolidated Stock Exchanges

This timeline shows the introduction of a clearinghouse on the Consolidated Stock Exchange in June 1886 and the introduction in stages on the New York Stock Exchange beginning in May 1892. Data on trading volumes are taken from Sobel (2000).

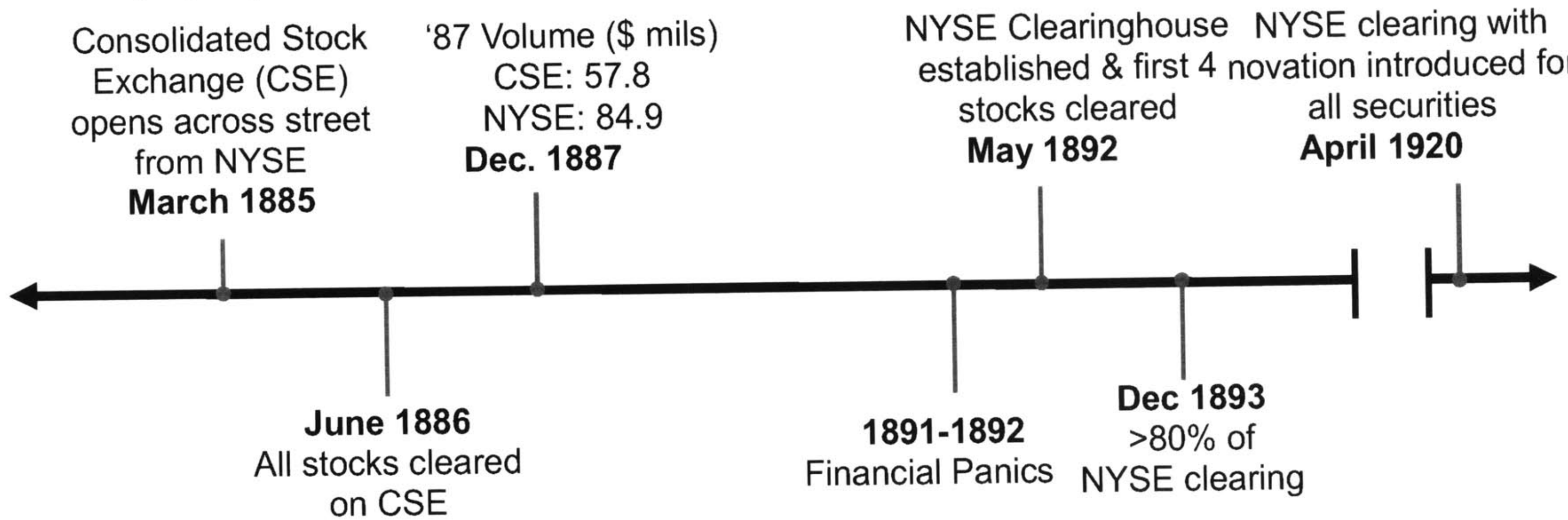


Figure 2. Counterparty Risk Premium and Introduction of Clearing on NYSE (1887-1925)

In this figure we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. *NYSE-Con/NYSE Bid-Ask* is the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE and *Call Loan Rate* is the overnight collateralized broker borrowing rate. The period prior to the establishment of the NYSE clearinghouse May 17th, 1892 is highlighted in red.

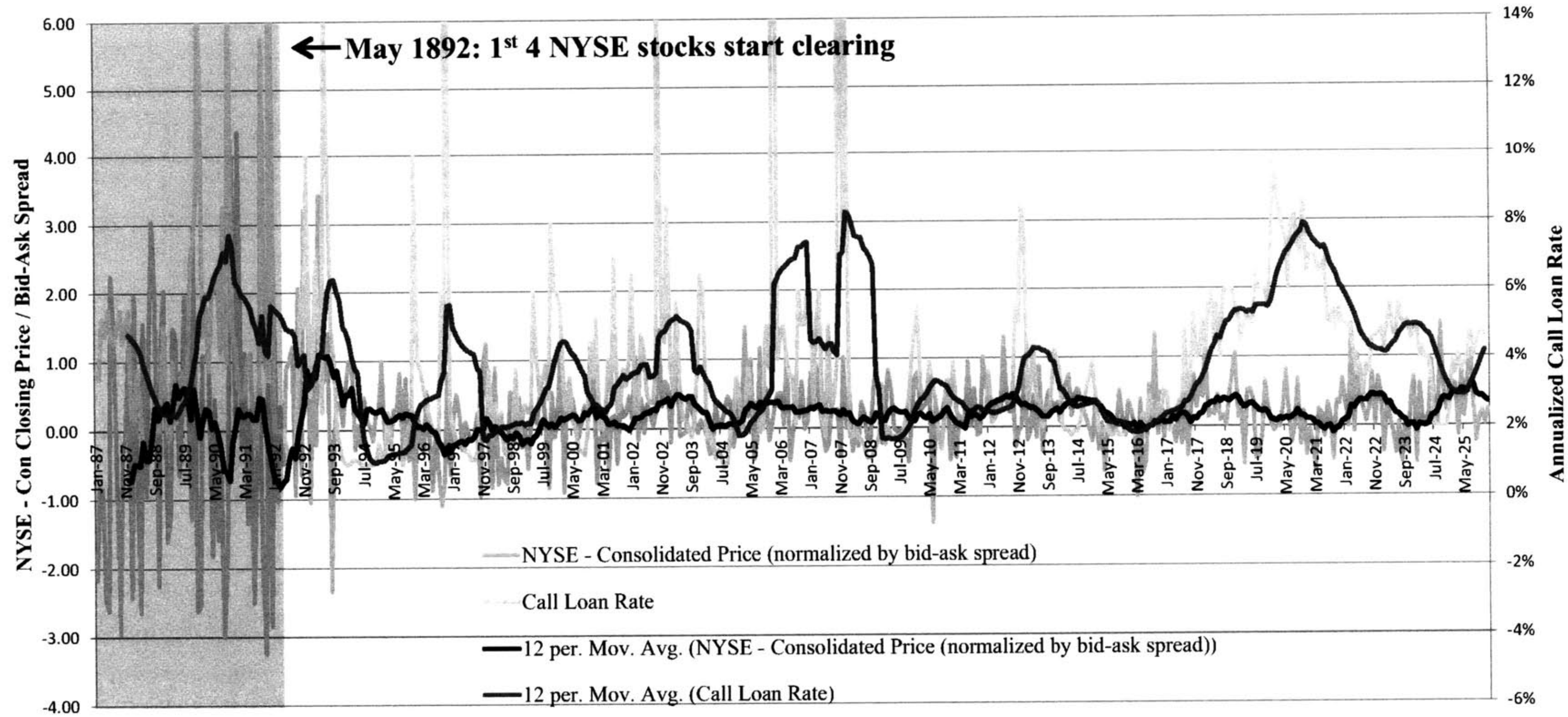


Figure 3. Absolute Value of NYSE-CSE Price Deviations and Introduction of Clearing on NYSE (1887-1925)

In this figure we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on average absolute value of the price difference of the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis, a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. The blue plot is the *|NYSE-CSE|/NYSE Bid-Ask* which is the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE and the change is driven by a reduction in volatility on the NYSE . The red dash lines indicate the average before and after the end of 1893.

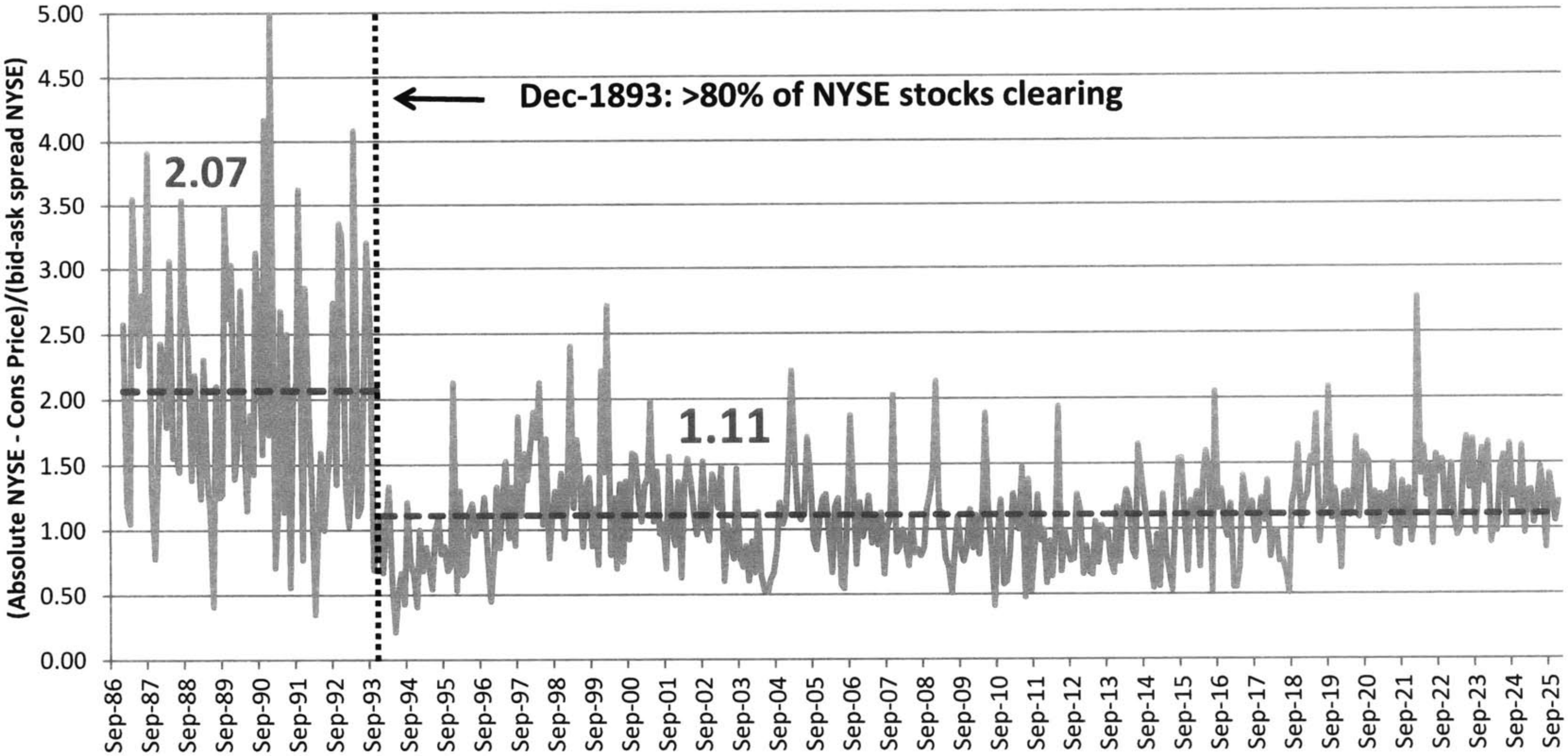
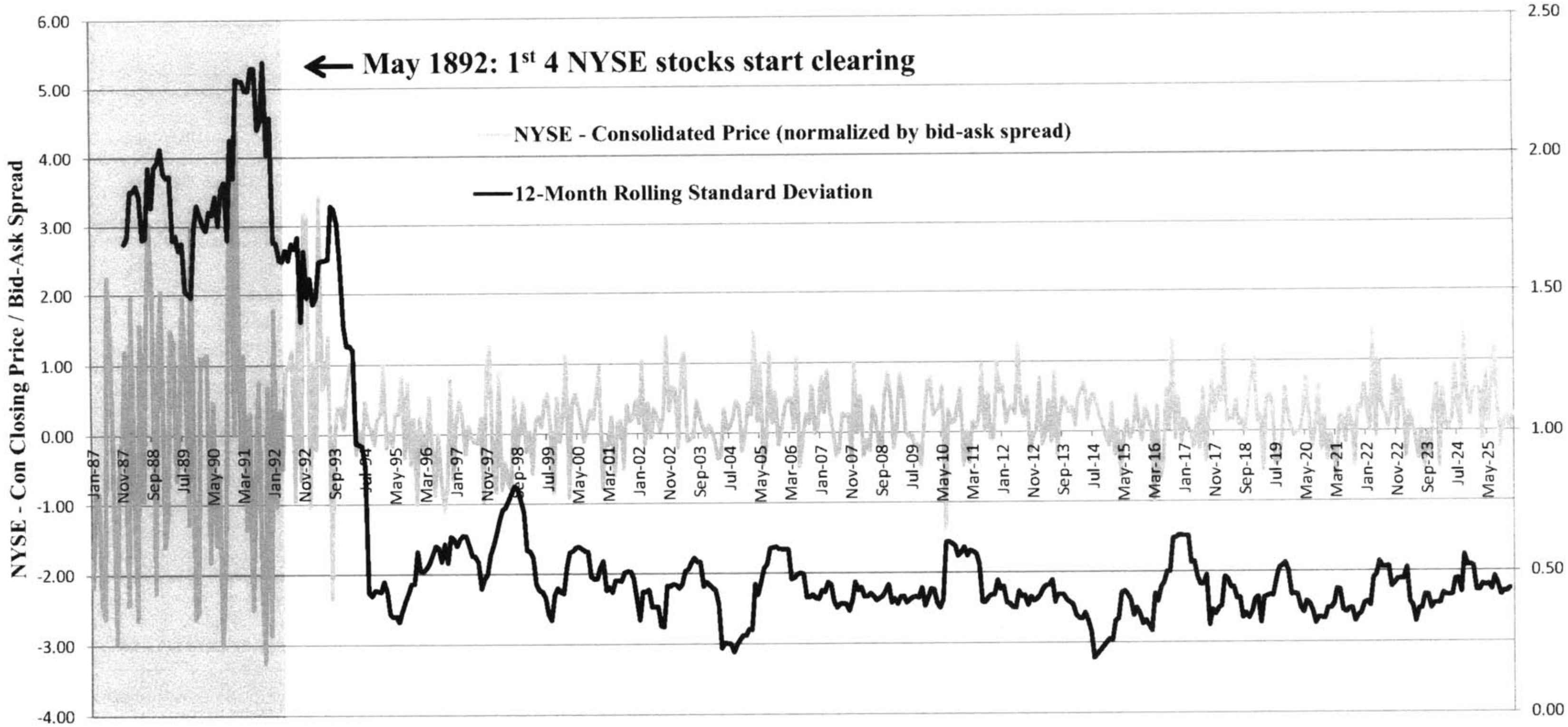


Figure 4. Volatility of NYSE-CSE Premium and Introduction of Clearing on NYSE (1887-1925)

In this figure we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the rolling 12-month standard deviation of closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. *NYSE-Con/NYSE Bid-Ask* is the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE. The period prior to the establishment of the NYSE clearinghouse May 17th, 1892 is highlighted in red.



Appendix A: Supplementary Tables & Figures

Table A1. Summary Statistics: All NYSE/CSE Stocks Daily Data

This table reports sample statistics for the trading data for stocks on the NYSE or CSE. Security market data were hand collected at a daily frequency from the *New York Times* from January 1892 – Dec 1901 for all stocks on the NYSE or CSE. All data is winsorized at the 1st and 99th percentile. To be included in the first level of summary analysis a security must trade at least 1 share on both exchanges on a given date, while for the second, which is the one used in our primary econometric specifications, we require at least 200 shares (2 standard contracts) and 20 observations before and after the introduction of clearing.

	NYSE Closing Price	NYSE Trading Volume (#Shares)	CSE Trading Volume (#Shares)	NYSE Trading Volume ($000s)	CSE Trading Volume ($000s)	NYSE Bid-Ask Spread (bps)	CSE Bid-Ask Spread (bps)
				With Minimum 1 Shares Traded (n = 62,959)			
Mean	56.3	10,122	3,055	743	252	68	120
Median	47.4	3,750	320	167	14	37	41
Standard Dev.	38.5	19,930	7,613	1,873	773	92	241
Minimum	0.8	1	2	0.004	0.009	9	9
Maximum	259	957,955	262,250	75,200	30,800	625	1,667
				With Minimum 200 Shares Traded and 20 Observations before and after Clearing (n = 28,161)			
Mean	64.6	15,820	5,789	1,255	496	44	49
Median	58.4	8,310	1,880	456	96	28	28
Standard Dev.	37.4	24,633	10,022	2,509	1,070	53	87
Minimum	0.8	200	200	0.6	0.4	9	9
Maximum	230	957,955	262,250	75,200	30,800	625	1,667

Table A2. Summary Statistics for Call Loan Rates and Exchange Seat Prices

This table reports the sample statistics for the average overnight collateralized borrowing rate, the *Call Loan Rate*, and seat prices for membership on the New York and Consolidated Stock Exchanges over four periods from September 1886- December 1925. Seat price data were hand collected at a monthly frequency from the *Commercial and Financial Chronicle*.

	Full Sample 1886-1925	Pre-Clearinghouse 1886-1893	Clearinghouse 1894-1925	Clearinghouse Subsample 1894-1908
		Call Loan Rate (%)		
Mean	4.0	4.7	3.8	3.6
Median	3.2	4.0	3.0	2.5
Standard Dev.	3.6	3.7	3.6	4.8
Minimum	0.9	1.1	0.9	0.9
Maximum	40.0	22.0	40.0	40.0
		New York Stock Exchange Seat Price ($000s)		
Mean	56.9	20.0	63.9	50.6
Median	63.0	20.0	68.5	54.5
Standard Dev.	29.1	1.8	26.5	26.3
Minimum	14.3	16.5	14.3	14.3
Maximum	150.0	24.0	150.0	95.0
		Consolidated Stock Exchange Seat Price ($000s)		
Mean	1.1	0.4	1.3	0.8
Median	0.7	0.3	0.8	0.7
Standard Dev.	1.3	0.3	1.5	0.7
Minimum	0.1	0.1	0.1	0.1
Maximum	6.0	1.0	6.0	2.5

Table A3. Robustness Tests for Changes in Microstructure Noise or Market Liquidity

In this table we show that the introduction of clearing on the NYSE is not associated with a change in the relative trading on the NYSE vs. the CSE and that the introduction of multilateral net settlement through a centralized clearing party reduced the premium and volatility of the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day. Security market data were hand collected at a daily frequency from the *New York Times* from January 1892 – Dec 1901 for all stocks on the NYSE or CSE. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. In Column 1, *|NYSE-Con|/Close* is an estimate of monthly volatility of the price on the NYSE relative to the CSE, normalized by the average closing price on both exchanges in percent. *Post 1893,* which is equal to 1 for all securities clearing after 1893 and zero prior. In Column 2, *|NYSE-CSE|/NYSE Bid-Ask* is the volatility of the price on the NYSE minus the CSE normalized by the bid-ask spread on the NYSE. Column 3 shows results if we only include stocks with at least 20 daily observations before and after the introduction of clearing. Column 4 shows results with stock-specific time-varying market liquidity controls. In addition to the estimated bid-ask spread on the NYSE, the dollar trading volume on the NYSE, and the dollar trading volume on the CSE. *CSE Bid-Ask Control,* indicates that it also includes the estimated bid-ask spread on the CSE. Column 5 shows results using opening instead of closing transaction prices. In Column 6, *Bid-Ask (%) NYSE-CSE,* is the NYSE minus CSE percent bid-ask spreads (normalized by price) on each exchange. In Column 7 *(Hi-Lo)/Open CSE/NYSE* is the high minus low value normalized by the opening price on the CSE divided by the same on the NYSE. In Column 8 we rerun the same specification as column 2, but use closing prices on the Boston Stock Exchange (BSE) as a control. Closing prices for the BSE are collected from the *Boston Globe* from 1892-1901 at a weekly frequency. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: *10%; **5%; ***1%.

Dependent Variable:	(1) |NYSE-CSE| /Close (%)	(2) |NYSE-CSE| /NYSE Bid-Ask	(3) |NYSE-CSE| /Close (%)	(4) |NYSE-CSE| /Close (%)	(5) |NYSE-CSE| /Open (%)	(6) Bid-Ask (%) NYSE-CSE	(7) Hi-Lo/Open CSE/NYSE	(8) |NYSE-BSE| /NYSE Bid-Ask
Post-1893	-0.141***	-0.393***	-0.194***	-0.193***	-0.225***	-0.056	0.038**	-0.264*
	(0.051)	(0.062)	(0.055)	(0.034)	(0.055)	(0.074)	(0.016)	(0.136)
Constant	0.745***	1.742***	0.692***	0.386***	0.600***	-0.314***	0.957***	1.695***
	(0.038)	(0.046)	(0.040)	(0.036)	(0.063)	(0.055)	(0.012)	(0.080)
Security Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Liquidity Controls	N	N	N	Y	Y	N	Y	N
CSE Bid-Ask Control	N	N	N	Y	Y	N	Y	N
Period	1892-1901	1892-1901	1892-1901	1892-1901	1892-1901	1892-1901	1892-1901	1892-1898
Min Pre/Post Obs	N/A	N/A	20	20	20	20	20	N/A
Price Used	Close	Close	Close	Close	Open	Close	Open	Close
# Clusters	188	188	48	48	48	48	48	11
# Observations	37,682	37,666	28,165	28,100	28,097	43,271	27,183	818
Adjusted R^2	0.164	0.078	0.049	0.101	0.052	0.097	0.061	0.016

Table A4. CSE as Control for NYSE

In this table we show that most of the variation stock returns on the New York Stock Exchange can be explained by the monthly returns for identical securities listed across the street at the Consolidated Stock Exchange. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. All data is winsorized at the 1st and 99th percentile. In column 1 we regress the monthly percent change in closing prices of NYSE-listed securities on identical stock returns on the CSE. Column 2 is the same but looks at changes in NYSE prices, not normalized by closing prices. All standard errors are clustered at the security-level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)
Dependent Variable:	%ΔNYSE Closing Prices	ΔNYSE Closing Prices
%ΔCSE Closing Prices	0.9528***	
	(0.008)	
ΔCSE Closing Prices		0.9497***
		(0.0088)
Constant	0.0005***	0.0358***
	(0.0002)	(0.0125)
# Observations	8,205	8,205
Adjusted R-squared	0.9291	0.9264

Table A5. Funding Costs and the Counterparty Risk Premium

In this table we show the estimated effect of the introduction of multilateral net settlement through a centralized clearing party on the closing price of a stock on the New York Stock Exchange relative to the closing price on the Consolidated Stock Exchange for the same security on the same day and show how it varies with changes in the short term interest rates. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. Commercial paper and call loan rates are taken from the NBER macrohistory database. In column 1, *NYSE-CSE/Close* is the price on the NYSE minus the CSE normalized by the average closing price on both exchanges regressed on the commercial paper rate. Column 2 is the same as column 1 but interacts the commercial paper rate with, *Clearinghouse,* which is a stock-specific dummy variable that equals 1 if a stock is cleared on the NYSE. Column 3 is the same as column 2 but also interacts call loan rates with the clearinghouse dummy variable. All specifications are run with security-level fixed effects and errors are clustered at the security-level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)
Dependent Variable:	NYSE-CSE /Close (%)	NYSE-CSE /Close (%)	NYSE-CSE /Close (%)
Clearinghouse		-0.010	0.049
		(0.107)	(0.108)
Call Loan Rate			-0.0163***
			(0.0064)
Call Loan Rate x Clearinghouse			0.0211***
			(0.0066)
Commercial Paper Rate	-0.0096	-0.0525***	-0.0301*
	(0.1090)	(0.0142)	(0.0167)
Commercial Paper Rate x Clearinghouse		0.0473***	0.0167
		(0.0179)	(0.0201)
Constant	0.047	0.157*	0.157*
	(0.058)	(0.085)	(0.085)
Security Fixed Effects	Y	Y	Y
Liquidity Controls	Y	Y	Y
Only Clearinghouse	Y	N	N
# Clusters	51	90	90
# Observations	3,904	5,994	5,994
Adjusted R-squared	0.0328	0.0128	0.0144

Figure A1. Daily Volumes for Dow Jones Stocks on NYSE and CSE (1887-1900)

In this figure we show that the introduction of clearing on the NYSE is not associated with a change in the relative trading on the NYSE vs. the CSE. Security market data were hand collected at a monthly frequency from the *New York Times* and *Commercial and Financial Chronicle* from September 1886 – December 1925 for all stocks in the Dow Jones Indices. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. The green dashed line indicates the establishment of the NYSE clearinghouse May 17th, 1892. All data is winsorized at the 1st and 99th percentile.

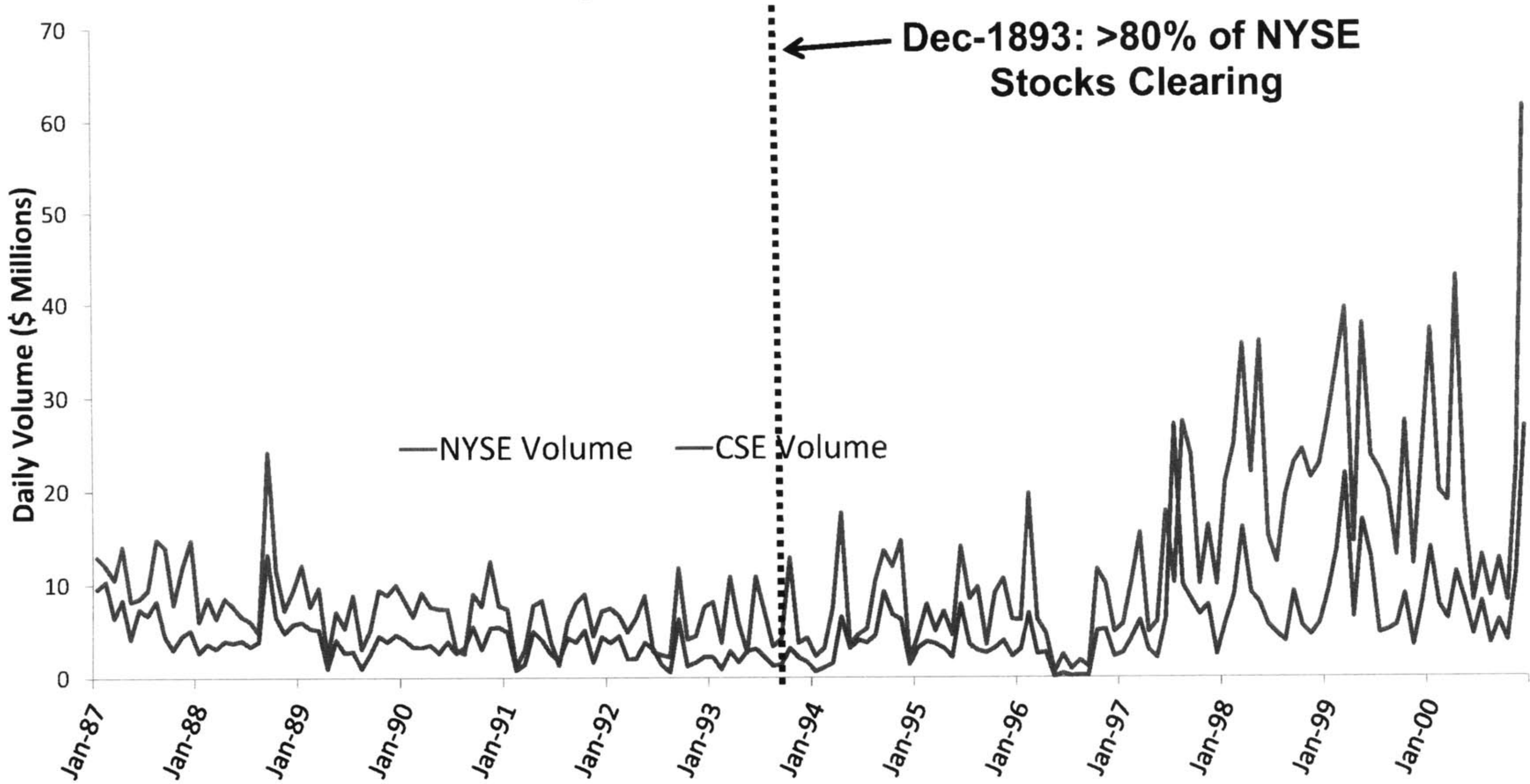


Figure A2. CSE/NYSE Bid-Ask Spreads 1892-1901

In this figure we show that the introduction of clearing on the NYSE is not associated with a change in the relative bid-ask spread on the NYSE vs. the CSE. Security market data were hand collected at a daily frequency from the *New York Times* from January 1892 – Dec 1901 for all stocks on the NYSE or CSE. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. For a description of the estimation of the bid-ask spreads see appendix A.

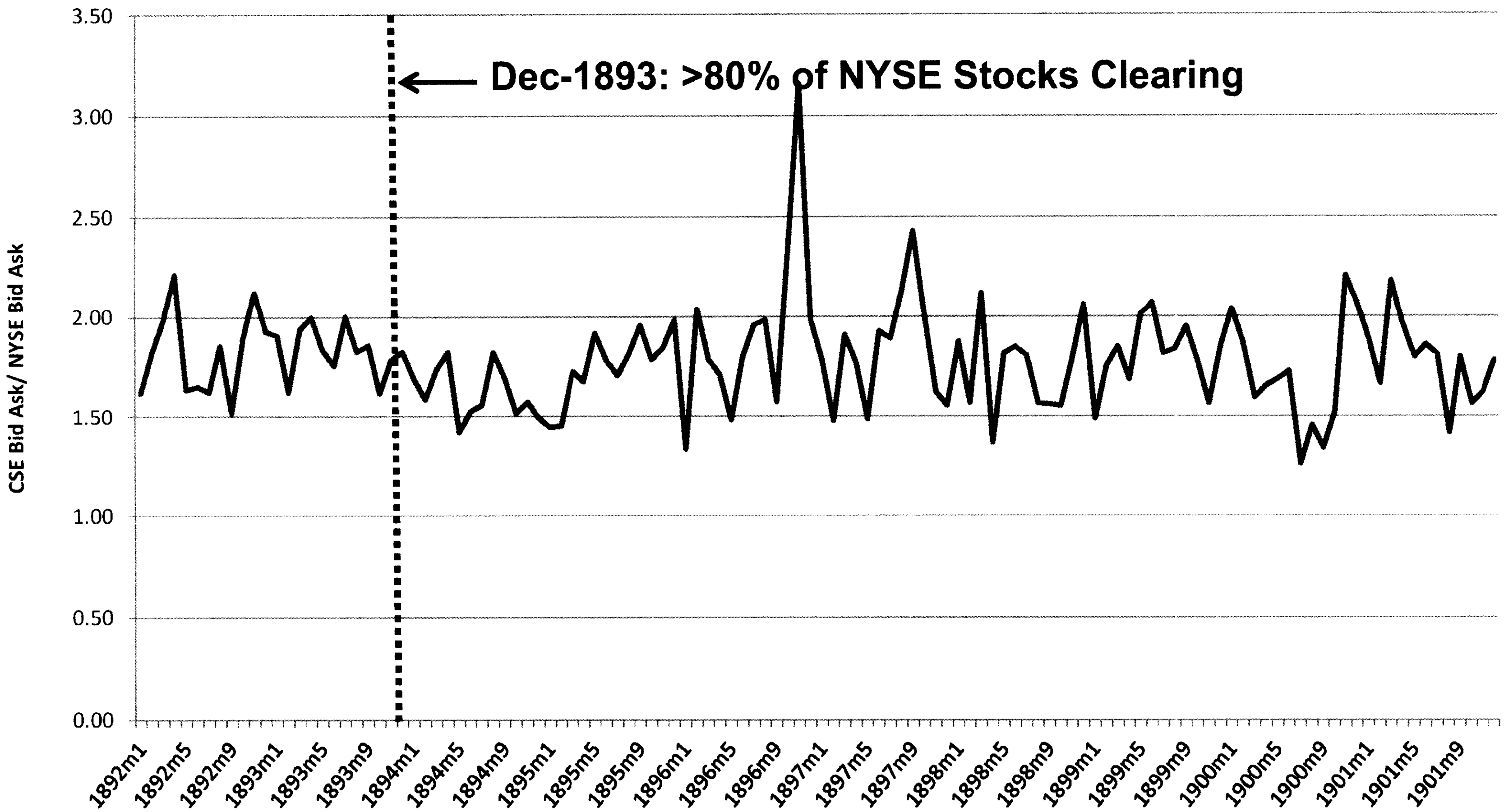
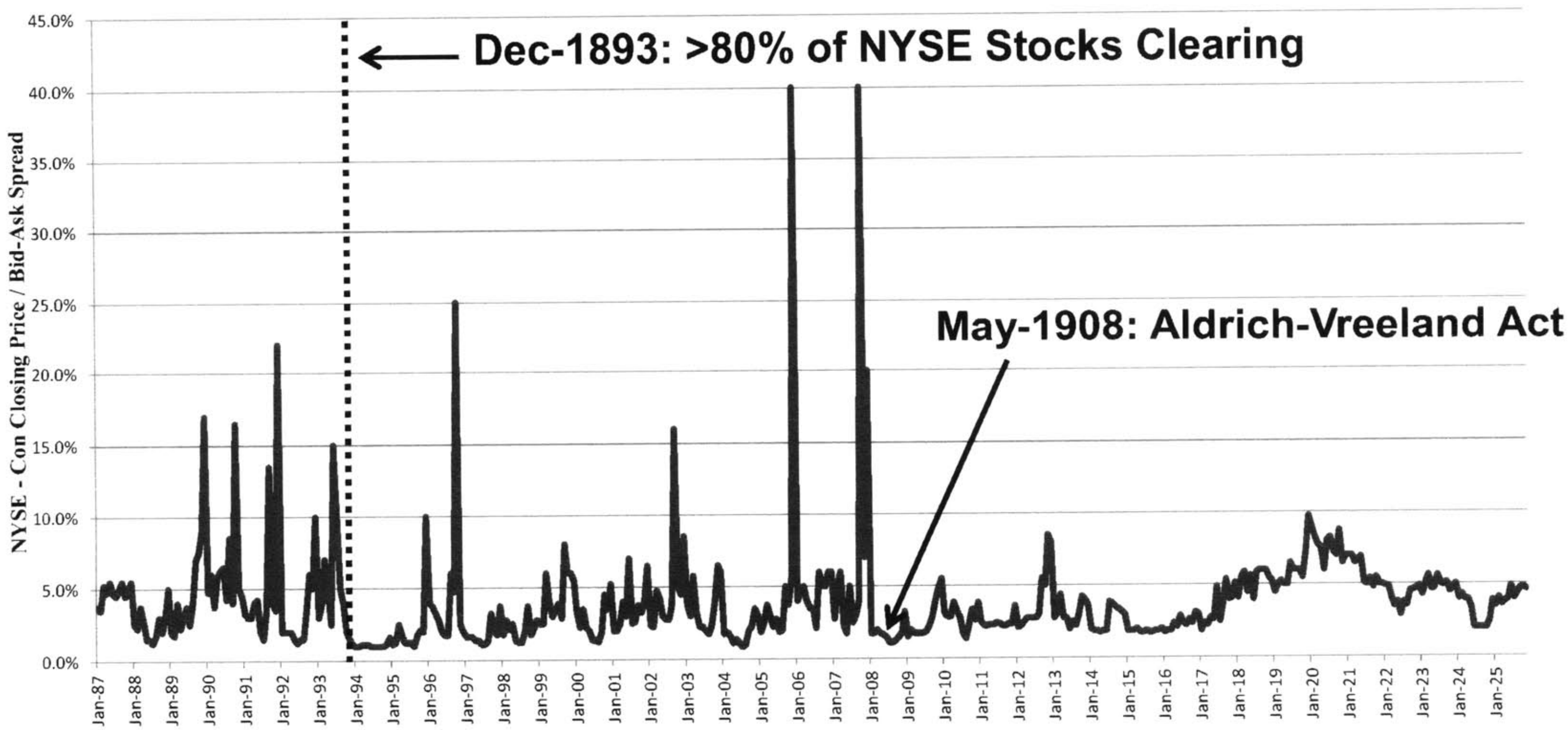


Figure A3. Call Loan Interest Rates (Overnight Collateralized Borrowing Rate) 1887-1925

In this figure we show that the introduction of clearing on the NYSE is not associated with a change in macro-economic risk. Closing monthly broker call loan rates are taken from NBER macro-history database for the entire period.



Appendix B: Estimating CSE Bid-Ask Spreads

For some of robustness checks we consider daily data from 1892-1901, which include estimated bid-ask spreads from the Consolidated Stock Exchange (CSE) for our robustness tests that are shown in Figure 4 and reported in table 6. We estimate the bid-ask spreads since historical data on CSE bid and ask prices do not exist for this period. Daily data on open, high, and low transactions prices were hand collected from the *New York Times* from 1892-1901.

For our analysis, we consider a daily estimator of the bid-ask spread based on daily high and low prices presented by Corwin and Schultz (2012), which we will refer to as the CS estimator. We also constructed our own estimator which uses absolute differences (AD) of open and closing prices in addition to high and low prices to arrive at an estimate of the bid-ask spread. This is referred to as the AD estimator. We focus on estimators that utilize high and low prices, rather than time series covariance estimators, like in Roll (1984). Corwin and Schultz (2012) find that the standard deviation of their estimates is ¼ to ½ as large as the estimator presented in Roll (1984).

The high minus low price spread on a given day combines both the fundamental variance of a stock price as well as any bid-ask spread, but while the variance grows proportionally with time, the bid-ask spread does not. This is the basic insight behind the CS estimator which gives an estimate of the bid-ask spread by comparing the high-low price ratio over two consecutive days to the high-low price ratio on each of those days. In particular let, β, be the sum of the squared difference between the log of the high, H, and low prices, L, on two consecutive days, t and $t+1$,

$$\beta = E\left[\sum_{j=0}^{1}\left(\ln\left(\frac{H_{t+j}^{0}}{L_{t+j}^{0}}\right)\right)^{2}\right] \tag{13}$$

and γ be the squared log difference of the high and low price over the two days,

$$\gamma = \left[ln\left(\frac{H_{t,t+1}^{0}}{L_{t,t+1}^{0}}\right)\right]^{2} \tag{14}$$

then the CS estimate, S, for the bid-ask spread is

$$S = \frac{2(e^{\alpha}-1)}{1+e^{\alpha}} \tag{15}$$

where α is the following function of β and γ:

$$\alpha = \frac{\sqrt{2\beta}-\sqrt{\beta}}{3-2\sqrt{2}} - \sqrt{\frac{\gamma}{3-2\sqrt{2}}} \tag{16}$$

Corwin and Schultz find that this estimator has excellent properties, including a time series correlation between high-low spread estimates and true spreads of about 0.9. We find that even in our period, 1892-1901, there is an 83% time series correlation between the monthly average actual bid-ask spread on the NYSE and the CS estimated bid-ask spreads. One of the unfortunate properties of this estimator is that estimates of the bid-ask spread can be negative. In simulations, Corwin and Schultz show that for stocks with a true bid-ask spread of 50bps, setting negative values to zero results in an average estimate of the bid-ask spread of 143bps. As the true bid-ask spreads become larger, the number of negative values diminishes and the bias becomes negligible. Unfortunately, this does not appear to be the case in our analysis. When we use the CS estimator from 1892-1901, we find that more than ½ of all bid-ask spread estimates are negative. This is especially problematic in our analysis since in one of our normalization methods we divide by the bid-ask spread, so we need the bid-ask spread to be strictly positive. To avoid this issue we set negative values to the minimum bid-ask spread on the NYSE, 1/8th.

Since in our period more than half of all observations require this ad-hoc adjustment, we considered another bid-ask estimator as a robustness check. In particular, we estimate the bid-ask spread by taking the minimum non-zero pair-wise absolute differences (AD) between the open, close, high, and low prices on two consecutive days. The insight for the estimator is that if we observe two prices and there is no change in fundamental value, or the change is small relative to the minimum tick size, then if the prices differ, the absolute difference between them is equal to the bid-ask spread. In our period the tick sizes were 1/8th which means that as long as fundamental value between two prices differ by less than 1/16th and observed prices differ we can recover the exact bid-ask spread. Unlike the CS estimator the AD estimator is never negative, by construction, since the estimate is bounded below by the minimum1/8th tick size. In addition, Figure A1 shows that the AD estimator does a good job of predicting actual bid-ask spreads during this period. For NYSE stocks from 1892-1901, we find an 88 percent time series correlation between the monthly average actual bid-ask spread on the NYSE and the AD estimated bid-ask spreads and a 75 percent correlation in changes in the averages. This compares favorably with the CS estimator which has correlations of 83 percent and 57 percent in levels and changes respectively, which is why we use the AD estimator in our primary analysis. The bid-ask spread estimates using the AD and CS estimators have over an 80 percent correlation during this period. As suggested by the high correlation between the estimates, also displayed in Figure A2, the results are robust to using either estimator[102].

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[102] Results using CS estimator are available upon request.

Figure B1. Validity of Estimated Bid-Ask Spreads for CSE and NYSE 1892-1901

This figure shows bid-ask spreads for security market data hand collected at a daily frequency from the *New York Times* from January 1892 – Dec 1901 for all stocks on the NYSE or CSE. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. The blue line is the actual bid-ask spread on the NYSE. The red and green lines are the estimated bid-ask spread on the NYSE and CSE respectively using the absolute difference (AD) method. For a description of the estimation method see the appendix.

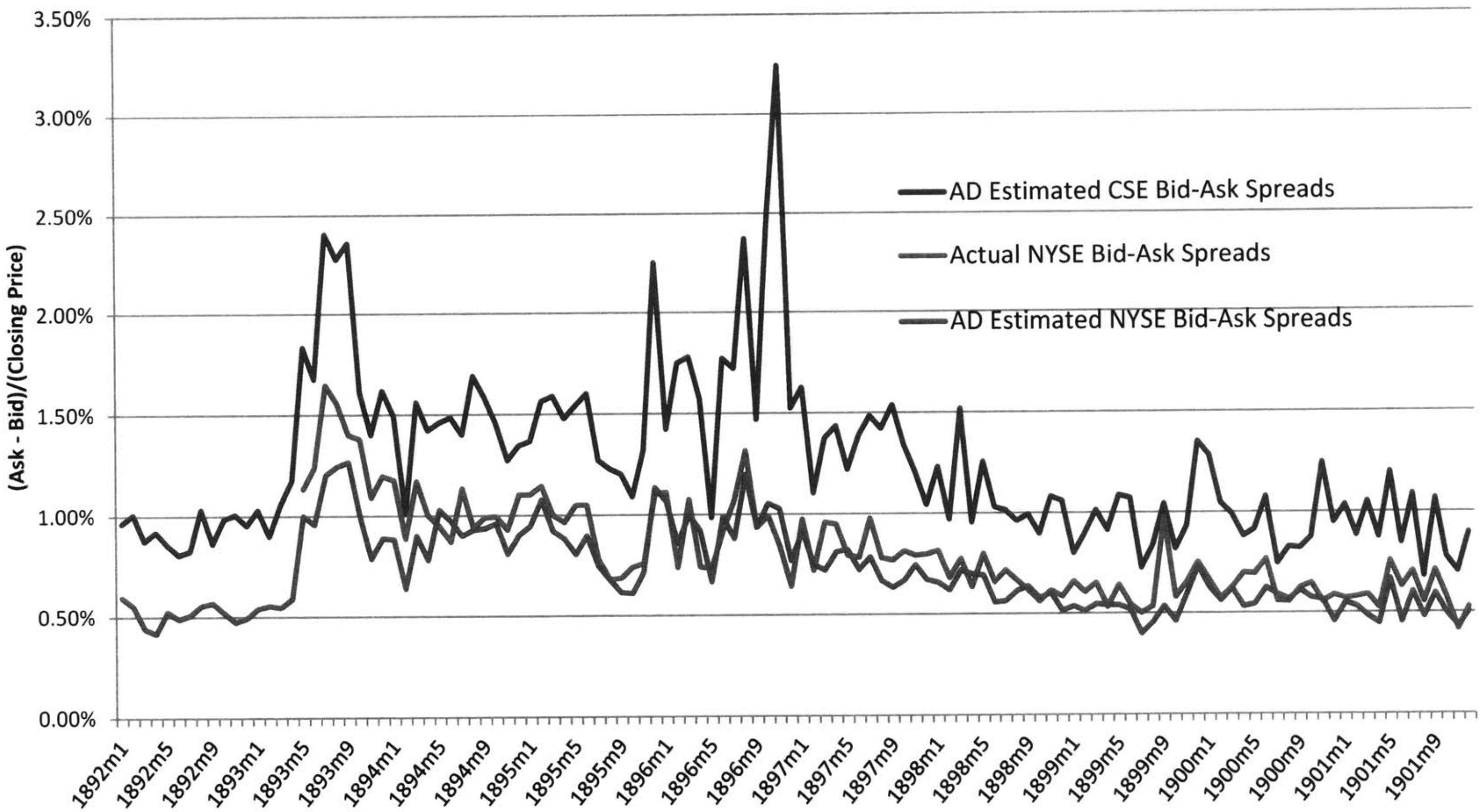
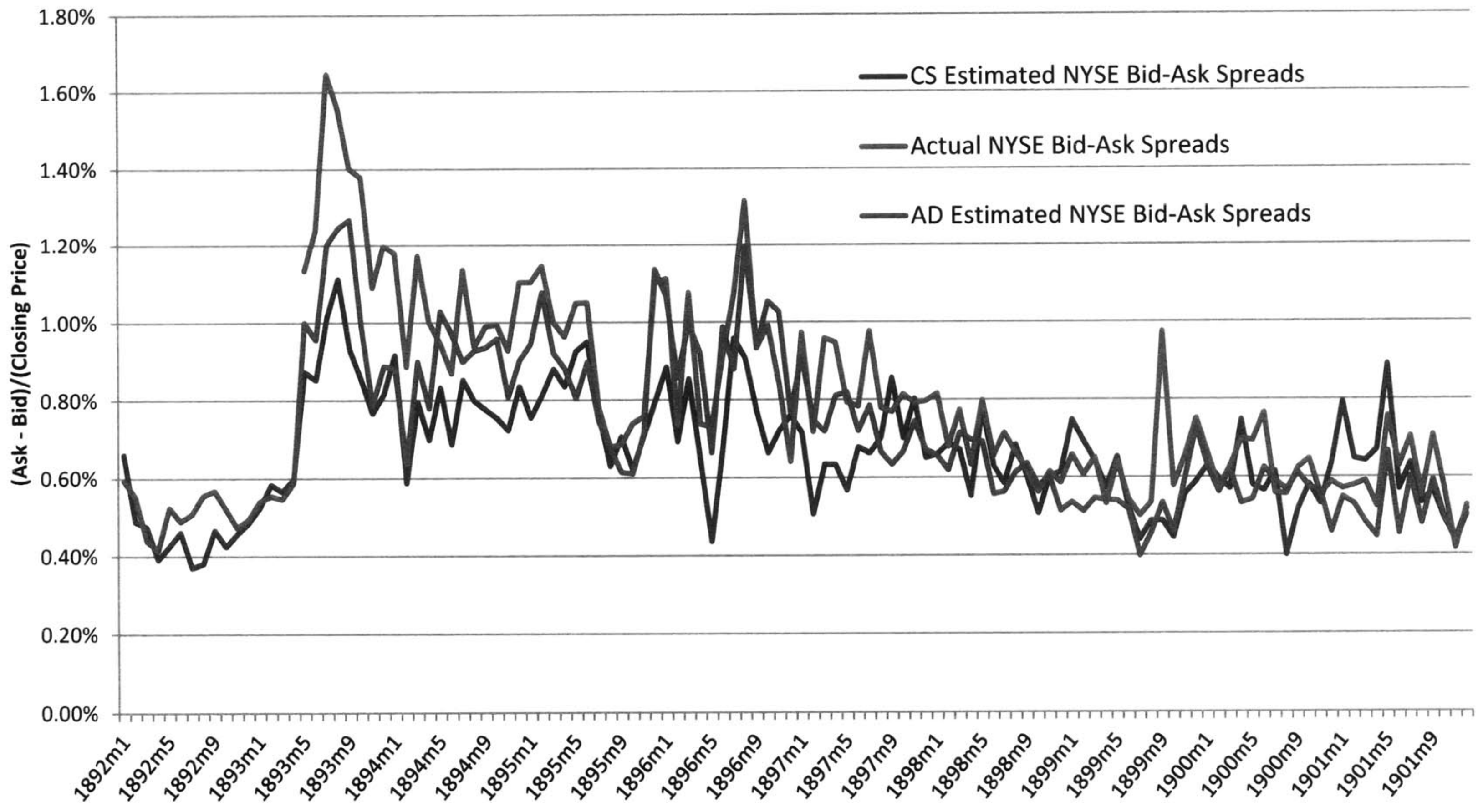


Figure B2. CS vs. AD vs. Actual Bid-Ask Spreads for NYSE 1892-1901

This figure shows bid-ask spreads for security market data hand collected at a daily frequency from the *New York Times* from January 1892 – Dec 1901 for all stocks on the NYSE or CSE. To be included in the analysis a security must trade at least 200 shares on both exchanges on a given date. All data is winsorized at the 1st and 99th percentile. The solid black line is the actual bid-ask spread on the NYSE. The solid red and dashed blue lines are the estimated bid-ask spread on the NYSE using the absolute difference (AD) and Corwin-Schultz (CS) estimators, respectively. For a description of the estimation methods see appendix A.



Appendix C: Estimating Change in Volatility

Let X be defined as the price difference between the NYSE and CSE normalized by the average price on the two exchanges so that, $X \equiv \hat{P}_{i,t,NYSE} - \hat{P}_{i,t,CSE}$, is the return required to equalize the price on both exchanges and let $Y \equiv |X|$. Now if we assume that $X \sim N(\mu, \sigma)$ then Y is distributed folded normal and

$$E[Y] = \sigma\sqrt{\frac{2}{\pi}}e^{-\frac{1}{2}\left(\frac{\mu}{\sigma}\right)^2} + \mu\left(1 - 2\Phi\left(-\frac{\mu}{\sigma}\right)\right) \tag{B1}$$

Therefore any change in the expectation of the absolute value is a function of any change in the mean and/or volatility of X. Under the additional assumption that $\mu \ll \sigma$ the expectation of a folded normal distribution becomes

$$E[Y] \approx \sigma\sqrt{\frac{2}{\pi}} \tag{B2}$$

so that the absolute value is just proportional to σ, which is the primary estimator for the change in volatility used in our paper.

From Table 2 we estimated that prior to the clearinghouse μ= -9bps and afterwards it is 15bps and from summary statistics computed separately we have that our estimate of $E[Y]$ prior to the introduction of the clearinghouse is approximately 73bps and 52bps afterwards. Using the change in μ and the change in $E[Y]$ we have that the implied σ from equation B1 is 94bps pre-clearing and 64bps afterwards, which is a reduction of 30bps. If instead we use our estimator in B2 we get that the implied σ is 94bps pre-clearing and 65bps afterwards, which is a reduction of 29bps. Thus our estimator in B2 is only approximately 1bp off without having to estimating μ before and after as we would need to in B1.

To build an intuition for the when it is reasonable to use the approximation in B2 instead of B1 we start by taking the partial derivative with respect to μ and σ in B1. Defining $\hat{\mu} \equiv \frac{\mu}{\sigma}$ we get that:

$$\frac{\partial E[Y]}{\partial \mu} = -\hat{\mu}\sqrt{\frac{2}{\pi}}e^{-\frac{1}{2}\hat{\mu}^2} + 1 + 2\hat{\mu}\phi(-\hat{\mu}) - 2\Phi(-\hat{\mu}) \tag{B3}$$

and

$$\frac{\partial E[Y]}{\partial \sigma} = \sqrt{\frac{2}{\pi}}e^{-\frac{1}{2}\hat{\mu}^2}(1 + \hat{\mu}^2) - 2\hat{\mu}^2\phi(-\hat{\mu}) \tag{B4}$$

From equations B3 and B4 we can see that the ratio of the mean of X to the standard deviation is a sufficient statistic for the partial derivative with respect to each of them. Since in our paper σ tends to be larger than μ in figure B1 we consider the value of those derivatives for a range of values for the ratio of σ divided by μ from 0 to 10. As you can see in equations B3 and B4 above and in figure

B1 as σ gets large relative to μ the partial derivative with respect σ asymptotes to $\sqrt{\frac{2}{\pi}}$ while the partial derivative with respect to μ shrinks continuously so that the ratio of the partials increases linearly and the mean has less effect on the expectation of the absolute value. The intuition behind this result is that in the limit where $\mu \ll \sigma$ the expectation of a folded normal distribution becomes $E[Y] \approx \sigma\sqrt{\frac{2}{\pi}}$ so that the absolute value is just proportional to σ.

In our sample we have a σ 5 to 10 times larger than μ so the effect of changes in μ are minimal. In particular, in the example provided previously, if σ remains unchanged at 73bps the change in μ from -9bps to 15bps results in a change in the $E[Y]$ of only 4/5ths of a basis point. The effect is so small because the symmetry of the normal distribution means the change from -9bps to 15bps is the same as effect of a change from 9bps to 15bps, or only 6bps. Also, since σ is about 7 times larger than μ the partial derivative is around 0.12. Thus, taking these together $6bps\ x\ 0.12 \approx 0.7bps$ which is approximately the result we arrived at previously.