COLLATERAL RULE:
EMPIRICAL EVIDENCE FROM THE CDS MARKETS

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GLOBAL RISK INSTITUTE

SYSTEMIC RISK

EXECUTIVE SUMMARY
Collateral Rule: Empirical evidence from the CDS Markets

Related Project: Central Clearing House Risk

OVERVIEW

Financial markets were at the center of the Great Recession. The interaction between volatility, leverage, and collateralization served to amplify fundamental shocks, and contributed to the creation of self-reinforcing death spirals experienced by major financial institutions. Largely in response to the crisis, a vast literature has analyzed the role of the collateral channel in amplifying financial shocks. The typical mechanism works as follows: after a fundamental shock, an increase in required collateral (due to the increased risks or to the losses incurred by market participants) may force liquidation and deleveraging, and generate additional downward price pressure, thus producing a margin spiral widely studied in the theoretical literature. At the core of models for the collateral feedback channel is the collateral rule that determines how margins are set and how they respond to changes in individual portfolio risks as well as aggregate risks. The literature has employed a variety of models of margining, from an exogenously specified function like Value at Risk to an endogenous collateral requirement.

Despite the centrality of the margining rule for theoretical models of financial crises and despite the fact that different collateral rules have different implications for the propagation and amplification of shocks through the financial system – empirical evidence on it is scarce: the collateral feedback channel has so far been studied mostly in theoretical work.

We will fill this gap by providing the first comprehensive study of the collateral rule in the market of cleared credit default swaps: we study how margins depend on portfolio risks and market conditions, and what the implications are for theoretical models of collateral equilibrium.

BACKGROUND

Clearinghouses have significant discretion over modeling assumptions and parameters used to generate and justify margin requirements. They set them taking into account market conditions, the demand for trading, and collateral quality. In practice, margining rules involve a wide range of scenarios and simulations to arrive at a portfolio loss distribution, requiring the clearinghouses to make various modeling and statistical assumptions.

Most clearinghouses, including ICC state that their margins are broadly "set to cover five days of adverse price/credit spread movements for the portfolio positions with a confidence level of 99%", which we refer to as a 5-day 99% Value-at-Risk (VaR) margining rule. Even if clearinghouses were restricted to using VaR based margining rules, the confidence level, margin period risk, and the distributional assumption of losses are inputs that give the clearinghouse significant freedom in setting the actual margin levels.

Overall, clearinghouses employ complex rules to determine the amount of required margins. These rules make it hard to understand what are the main economic determinants of collateral requirements, partly because they are complex and depend on the interactions of a myriad of variables and calibration choices, partly because they do not explicitly take into account variables that, however, may still matter indirectly: for example, aggregate volatility or default risks do not directly enter the calculations, but may still affect the collateral rule empirically because of their effect on the scenarios the clearinghouse uses to simulate portfolio losses, or through the choice of discretionary parameters.

METHOD

We use statistical hypotheses testing and multivariate linear regression techniques, executed both in the time series and in the cross-section, to estimate the portfolio-level determinants of collateral requirements. We estimate a modified version of the margining model proposed by Duffie, Scheicher and Villeumey (2016), where initial margins are determined as a mix of two specific portfolio variables: maximum shortfall and short notional (the so-called short charge). Their model parameters were calibrated to anecdotal evidence; we instead estimate them from actual data.

We incorporate both market variables and portfolio variables into our panel regressions: since collateral rules adapt to market conditions, we should expect collateral levels to respond to variables that capture the state of the economy and the markets. We also enlarge the set of predictors in the regressions by incorporating measures of aggregate risk, such as VIX and the average CDS spread of all dealers, and measures of funding opportunity costs, including the CDS spread of each member (that reflects the credit risk of the member, and in turn affects its cost of financing) and the LIBOR-OIS (Overnight Index Swap) spread.

MAIN FINDINGS

The main findings of this work include the following:

- *Provides direct empirical evidence that margins are much more conservatively set than what a Value-at-Risk (VaR) rule would imply, and are unequally implemented across participants.*
- *Shows that more extreme tail risk measures have a higher explanatory power for observed collateral requirements than VaR, consistent with endogenous collateral theories where extreme events dominate in determining collateral. The dependence of collateral requirements on extreme tail risks induces potential nonlinearities in margin spirals, dampening small shocks and amplifying large ones.*
- *We confirm empirically and quantify the main channel through which collateral- feedback effects operate in many theoretical models of equilibrium with financial frictions, highlighting the prominent role of aggregate volatility and funding costs.*

The Collateral Rule: An Empirical Analysis of the CDS Market

Preliminary Draft

Abstract

We study the empirical determinants of collateral requirements in the cleared credit default swap (CDS) market: how margins depend on portfolio risks and market conditions, and what the implications are for theoretical models of collateral equilibrium. We construct a novel data set containing CDS portfolios and margins posted by all participants to the main CDS clearinghouse, ICE Clear Credit, covering 60% of the U.S. market. We provide direct empirical evidence that margins are much more conservatively set than what a Value-at-Risk (VaR) rule would imply, and are unequally implemented across participants. We show that more extreme tail risk measures have a higher explanatory power for observed collateral requirements than VaR, consistent with endogenous collateral theories such as Fostel and Geanakoplos (2015) where extreme events dominate in determining collateral. The dependence of collateral requirements on extreme tail risks induces potential nonlinearities in margin spirals, dampening small shocks and amplifying large ones. We also confirm empirically and quantify the main channel through which collateral-feedback effects operate in many theoretical models of equilibrium with financial frictions, such as Brunnermeier and Pedersen (2009), highlighting the prominent role of aggregate volatility and funding costs.

1 Introduction

Financial markets were at the center of the Great Recession. The interaction between volatility, leverage, and collateralization served to amplify fundamental shocks, and contributed to the creation of self-reinforcing death spirals experienced by major financial institutions.

Largely in response to the crisis, a vast literature has analyzed the role of the collateral channel in amplifying financial shocks. The typical mechanism works as follows: after a fundamental shock, an increase in required collateral (due to the increased risks or to the losses incurred by market participants) may force liquidation and deleveraging, and generate additional downward price pressure, thus producing a *margin spiral* widely studied in the theoretical literature (as an example, see Geanakoplos (2010)).

At the core of models for the collateral feedback channel is the collateral rule, that determines how margins are set and how they respond to changes in individual portfolio risks as well as aggregate risks. The literature has employed a variety of models of margining, from an exogenously specified function like VaR (e.g., Brunnermeier and Pedersen (2009)) to an endogenous collateral requirement as in Geanakoplos (1997) and Fostel and Geanakoplos (2015), who study how equilibrium collateral levels depend on market conditions and portfolio risks.

Despite the centrality of the margining rule for theoretical models of financial crises – and despite the fact that different collateral rules have different implications for the propagation and amplification of shocks through the financial system – empirical evidence on it is scarce: the collateral feedback channel has so far been studied mostly in theoretical work.

In this paper, we aim to fill this gap by providing direct empirical evidence on the determinants of collateral in a large market where counterparty risk plays a particularly important role: the cleared credit default swap (CDS) market. In particular, we explore a novel data set collected and maintained by the U.S. Commodity Futures Trading Commission (CFTC) on all CDS trades cleared by Ice Clear Credit (ICC), the largest clearinghouse for these contracts; using data that covers the members' portfolios and corresponding collateral posted to the clearinghouse, we explore what portfolio and market variables determine the collateral levels and panel variation (over time and across market participants). That is, we study the empirical determinants of the collateral requirements in this market, and explore the implications for theoretical models of collateral equilibrium as well as for financial stability.[1]

The data – covering more than 18,000 contracts and all major dealers operating in the cleared CDS market for the period 2014 to 2016 – provides us not only with detailed CDS positions of the

[1] As we describe in greater detail in the next sections, ICC follows a complex set of procedures that include calibration of different scenarios and simulations – together with a discretionary component – to determine the amount of collateral each member needs to post. The goal of this paper is not to reverse-engineer this procedure, but rather to identify and quantify the main economic determinants of the variation in collateral over time and across members. We will therefore study both the direct effect of variables that the clearinghouse takes explicitly into account (like portfolio risks) as well as the effect of market variables (like aggregate volatility) that only affect margins indirectly. Despite the complexity of the procedure followed by ICC, we show that a few main portfolio risks and market variables explain the vast majority of the panel variation in collateral requirements, yielding an accurate approximation of the collateral rule that highlights and quantifies its main economic determinants.

clearinghouse members, but also with individual margins the clearinghouse requires to collateralize the members' CDS portfolios. While we do not observe this data before 2014 (as CDS clearing and systematic data reporting only started in recent years), we do observe CDS prices going back to 2004: we can therefore construct the counterfactual empirical distribution of returns to each portfolio over the 12 years – between 2004 and 2016 – and compare the observed collateral levels with measures of risk built using data that includes the financial crisis.

We start by documenting several new stylized facts of centrally cleared CDS markets. We show that collateralization, defined as the posted collateral over aggregate net notional, can vary drastically across clearing member accounts, indicating that large clearing members trade portfolios with very different characteristics. Returns on margins, defined as the profit and loss (P&L) over posted initial margins, cluster tightly around their mean and exhibit fat tails; these are due to the asymmetric distribution of CDS payoffs which make collateralization particularly important in this market.

Guided by the theoretical literature, we then explore which factors determine margins empirically. We categorize potential explanatory variables into *portfolio variables*, that are specific to the collateralized portfolio; and *market variables*, which depend on the state of the macroeconomy or the market participants holding the portfolios. Examples of portfolio variables include portfolio size measures (e.g., gross notional) and portfolio risk variables (e.g., standard deviation and VaR). Examples of market variables include aggregate and individual default risk and funding costs.

In view of the prominent influence that VaR has on modern perceptions and modeling of margin setting (Figlewski (1984), Brunnermeier and Pedersen (2009), Hull (2012), Glasserman et al. (2016)), we explore the ability of standard VaR margining rules to explain observed margins in this market. VaR plays a central role not only because of its widespread use in theoretical models, but also because most clearinghouses (including ICC itself) explicitly use it as an approximate description of their collateral-setting rule (Ivanov and Underwood (2011)). The benchmark VaR typically discussed is a 5-day 99%-level VaR, according to which collateral should be sufficient to cover 99% of the 5-day loss distribution of each member's portfolio.

In the data, we find that collateral levels far exceed those implied by the benchmark VaR. In fact, for our entire sample period where we observe portfolios and collateral levels (2014-2016), the largest realized drawdowns are only around 30% of the posted collateral: exceptions (losses above the posted collateral) did not occur at all, despite significant market events such as the Chinese stock market panic of early 2015. More strikingly, this conservativeness in collateral setting holds true even when we incorporate CDS price movements since 2004, therefore including the large shocks occurred during the financial crisis. To perform this analysis, we collect historical CDS prices since 2004, and for each portfolio observed in our sample we build the time series of returns that the portfolio would have realized over the entire period 2004-2016. We therefore obtain for each portfolio held by a member at any point in time an empirical distribution of returns estimated over the last 12 years. We can then verify whether on average the collateral posted corresponds to the 1st percentile of that distribution, as implied by the 99% VaR. We show that, even when we include large shocks that reproduce those experienced during the financial crisis, the implied VaR confidence needed to explain the average level of collateral is between 99.98% and 100%. This is a first indication that, empirically, collateral levels focus on more extreme loss realizations than typically assumed, consistent with theoretical models such as Fostel and Geanakoplos (2015) and with much of the empirical evidence in the remainder of our paper.[2]

Having established that standard Value-at-Risk does not explain observed collateral we turn to study a wider class of portfolio-specific variables. Specifically, we estimate a panel model relating margins to VaR as well as other portfolio risk measures such as expected shortfall, maximum shortfall, aggregate short notional, and aggregate net notional. That is, different measures of the riskiness of a portfolio that have been used in the theoretical literature. We perform the estimation with and without time and clearing member account fixed effects, controlling for aggregate variation in market conditions as well as dealer-specific portfolio characteristics not well captured by standard risk measures (e.g. idiosyncratic risks). We show that *portfolio variables* explain a significant portion of the panel variation with an overall R^2 of 70% without fixed effects, and 86% with fixed effects. Compared to just using VaR as the explanatory variable, there is a significant increase in explanatory power when alternative portfolio variables are incorporated: this shows that not only does the benchmark VaR rule not adequately explain average collateral levels, but VaR also misses significant panel variation. This shows that collateral rules depend on dimensions of risk not limited to standard loss quantiles, but still related to different forms of risk exposure in the member portfolios.

To better understand the portfolio-level determinants of collateral requirements, we estimate a modified version of the margining model proposed by Duffie et al. (2015), where initial margins are determined as a mix of two specific portfolio variables: maximum shortfall and short notional (the so-called *short charge*). Their model puts greater emphasis on the maximum loss that the counterparty could incur, compared to a VaR rule; it adds a short charge because short CDS positions have large downside risks, and "jump-to-default risk" is especially relevant for large net short positions. Their model parameters were calibrated to anecdotal evidence; we instead estimate them from actual data. We show that this model outperforms VaR in explaining collateral panel variation, and captures well its dependence on short positions. It therefore represents a significant improvement over simple VaR. Remarkably, the loadings proposed by Duffie et al. (2015) for the short charge are basically identical to the ones we estimate from the data. Using our margins data, we optimize the loadings in Duffie et al. (2015) to best explain collateral panel variation. This modified model achieves an R^2 of 67% without fixed effects, and 86% with time and account fixed effects, which is particularly notable for a collateral model with just one variable.

Next, we incorporate *market variables* into our panel: since collateral rules adapt to market

[2] These results are consistent with Berkowitz and O'Brien (2002), who analyze the performance of bank VaR models for six large U.S. commercial banks. They test the null hypothesis of a 99 percent coverage rate, and find that the VaR estimates tend to be conservative relative to the 99th percentile of the historical distribution of profit and losses. Differently from us, however, they find that losses can substantially exceed the VaR, possibly reflecting different collateral practices in the 1990s.

conditions, we should expect collateral levels to respond to variables that capture the state of the economy and the markets. We incorporate measures of aggregate risk, such as VIX and the average CDS spread of all dealers, and measures of funding opportunity costs, including the CDS spread of each member (that reflects the credit risk of the member, and in turn affects its cost of financing) and the LIBOR-OIS (Overnight Index Swap) spread.

We show that, consistent with standard economic intuition, increases in aggregate risk raise the required collateral levels, and we use our empirical estimates to quantify the strength of this channel. We find that a one-point increase in the CBOE's Volatility Index (VIX) leads to a 0.7% increase in initial margins. The loading on the VIX coefficient in the regressions reported in Table 8 is 0.046, indicating that a one-point increase in the VIX leads to an increase of 4.6 million in margins from each member (or 0.7% of the average margin posted). Considering that the VIX went up by nearly 50 points during the global financial crisis, this suggests a potential 35% increase in initial margins, or $230 million, for all members *just through the VIX channel*. Another way to think about this magnitude is that to maintain the same initial margins in response to a 50-point increase in the VIX, banks would need to liquidate nearly one quarter of their entire CDS positions. These estimates are conservative, because they are ignoring all feedback effects triggered by the liquidation of existing positions, which lead to rising volatility and thus increased margins. Moreover, they focus only on the VIX channel, and do not account for all the other channels that operate during a financial crisis to increase margins, like increases in individual portfolios' tail risks.

High default risk (average CDS spread or LIBOR-OIS spread) also induces a large increase in collateral requirements, with approximately a twelve million increase in initial margins per basis point, again suggesting very large magnitudes for the effect of aggregate risks on collateral requirements.

During distress periods, posting margins becomes costly as funding liquidity tightens. During the financial crisis, for example, the LIBOR-OIS spread skyrocketed from 10 basis points to approximately 364 basis points. Increases in the spread of this magnitude would induce large increases in required margins and potentially induce sell-offs of CDS positions with significant systemic implications.

Interestingly, high funding costs for the counterparties, captured by high individual CDS spreads after controlling for the average CDS spread of members, are associated with lower margin levels (though with weak statistical significance). This result is consistent with the model of Capponi and Cheng (2017), which predicts that as funding costs increase, demand for positions that require high collateralization will decrease, so that the equilibrium collateral level becomes lower. Together, these market variables help explain as much of the total variation in margins as the time fixed effects, thus capturing the entire time-series variation in average margins. These results provide direct evidence that margining rules indeed depend also on dimensions of risk not specific to the cleared portfolio.

To summarize, we show that the levels of collateral we observe in the cleared CDS market are *extremely conservative* compared to what standard VaR rules would imply, even after accounting for potential losses as large as those observed during the financial crisis. Moreover, loss quantiles and portfolio volatility do not adequately capture collateral, neither in the average level nor in the panel variation. Instead, short charge and maximum shortfall – measures focusing on extreme risks of the portfolio, to which a short CDS contract is particularly exposed – describe much better actual margin settings. Margins also depend on measures of aggregate risks and on funding costs.

These results provide supporting evidence to specific theoretical models of the collateral-feedback channel. They are consistent with the equilibrium theories of Fostel and Geanakoplos (2015), who predict that equilibrium collateral rules are set to cover the worst-case loss scenario, rather than covering only a certain quantile loss as in the VaR case, so that no defaults are observed in equilibrium. Their theory is derived in a binomial economy; our results are interesting in this regard because they provide direct evidence that their theory extends to more general economies (Fostel and Geanakoplos (2015)).

The dependence of the margining rule on the maximum of the loss distribution as opposed to other less extreme risk characteristics like lower quantiles or standard deviation also suggests the existence of an important nonlinearity: changes in risk that do not affect the extreme tail may have little effect on the total amount of collateral required – all the effects are concentrated in the risk of very large losses. This could amplify and concentrate the margin spirals at the very worst times (after large negative shocks that induce a change in the perceived risk of tail events), while at the same time dampening the margin spiral for moderate-sized shocks. While most of the theoretical models have not incorporated this nonlinearity, it could play an important role in explaining the radically different dynamics of the economy during times of crisis.

Of course, there are several differences between the purely theoretical model of Fostel and Geanakoplos (2015) and our empirical setting. For example, we consider a clearinghouse that determines collateral rules in an oligopolistic setting (given that ICC is the largest clearinghouse with a certain degree of market power), whereas the theoretical model of Fostel and Geanakoplos (2015) assumes a competitive market. Nonetheless, most real life situations of margin setting involve some degree of market power, so our analysis of the cleared CDS market should be informative about other environments and markets in which collateral is required for trading. We therefore believe that our empirical results can be quite informative about the collateral channel, especially considering the scarcity of empirical evidence available.

Our empirical findings on the relation between margins and market variables have important implications for funding liquidity and systemic risk: they show that margining rules do indeed depend strongly on market conditions, and they may dampen or reinforce margin spirals depending on the nature of market shocks, as predicted for example by Brunnermeier and Pedersen (2009). When a common market shock increases aggregate default risk in the market or aggregate volatility, this significantly increases the required margins for members, potentially triggering a margin spiral.

Our paper relates to a large theoretical literature on the relation between margin requirements and asset prices, collateral equilibrium, and demand of collateral, and to a small empirical literature studying margin setting in practice. Brunnermeier and Pedersen (2009) explore the link between margin requirements and asset prices, highlighting the self-reinforcing feedback loop between market liquidity and margin requirements set according to VaR. Other important contributions include Hardouvelis and Peristiani (1992) who study the influence of margin requirements on the stability of stock prices in the Tokyo stock exchange, and Coen-Pirani (2005) who show that margin requirements only affect stock prices when they are non-binding. Chabakauri (2013) uses a dynamic equilibrium model to show that tighter margin constraints increase prices of low-margin assets more than prices of high-margin assets.[3] A separate branch of the literature has explored the theory of endogenous collateral requirements and their general equilibrium implications. These models typically show that collateral requirements are determined not only by features of the financial contract, but also by market conditions and specific characteristics of market participants (e.g., Geanakoplos (1997), Holmström and Tirole (1997), Brunnermeier and Sannikov (2014), and Fostel and Geanakoplos (2015)).[4] While mostly stylized, these endogenous collateral rules typically do not resemble VaRs but are, in fact, more complex functions of the state of the economy and the values of the assets.

While the literature surveyed above has focused on bilaterally traded contracts, few other papers have explored the determinants of collateral requirements in the context of cleared derivatives markets, where a clearinghouse becomes the counterparty to every trade. Koeppl et al. (2012) study the economic incentives behind the design of margins rule leading to efficient clearing arrangements, while Biais et al. (2016) analyze to which extent margin requirements can be designed to mitigate counterparty risk. The impact of central clearing reforms on the collateral demand for derivatives transactions is investigated in Heller and Vause (2012), Sidanius and Zikes (2012), and Duffie et al. (2015), assuming exogenously specified margin requirements based on VaR, expected shortfall, or a mix of the two.[5]

Empirical work on the determinants of collateral is scarce, mostly for the difficulty of obtaining data on positions and collateral.[6] A small literature has studied margining in the futures market.

[3] A related literature has explored how financially constrained intermediaries can impact the macroeconomy and asset prices, see Adrian and Boyarchenko (2012), He and Krishnamurthy (2013), and Brunnermeier and Sannikov (2014). A stream of studies (Booth et al. (1997), Longin et al. (1999), Broussard (2001)) utilizes the techniques of extreme value theory to account for leptokurtic returns in financial markets, since empirically observed returns often deviate significantly from normality, which gives rise to higher "adequate" margins (assuming adequate margins are those given by VaR).

[4] Other relevant works include Fostel and Geanakoplos (2014) who study how leverage can be endogenously determined in a collateral equilibrium model along with its dependence on volatility; Geanakoplos (2003) who argues how bad shocks tighten margins requirements and lead to more likely defaults. The impact of endogenous margin requirements on asset prices has been studied in Rytchkov (2014), and in Geanakoplos and Zame (2014) for durable goods.

[5] Some other works have studied the determination of collateral requirements accounting for systemic interdependencies. Cruz Lopez et al. (2017) account for the variability and interdependence of profit and losses of clearing members when determining the clearinghouse margin requirements. Menkveld (2017) argues that crowded positions present hidden risks to the clearinghouse, and that margin requirements should take into account portfolio concentration.

[6] Collateral data for non-cleared OTC markets is often scattered among a variety of participants, with no centralized datasets available. Clearinghouse data contain proprietary information of large market participants and are often disclosed only under strict confidentiality and anonymity arrangements. Due to such data limitations, there is little empirical work focusing on portfolio-level margins (as opposed to individual-security collateral requirements), and on how well conventional risk measures relate to the required collateral levels.

Figlewski (1984) and Gay et al. (1986) test the extent to which margins in futures markets protect against counterparty losses, but do not explore the determinants of the margining rule. Fenn and Kupiec (1993) investigate empirically whether futures clearinghouses set margins in an efficient (cost-minimizing) way using two simple models of optimal margin setting, finding that neither model captures margins well. More recently, Hedegaard (2014) documents the relation between average margins and price volatility in the cross-section of futures contracts. He finds contract-specific volatility to be the main determinant of the average margin levels by contract, with a non-negligible role played by tail risks; he also shows that clearinghouses do not treat contracts uniformly when varying margin requirements. His results echo those of Fishe and Goldberg (1986) and Goldberg and Hachey (1992) who also document price volatility being a primary concern for margins.

To the best of our knowledge, our study is the first to investigate empirically the relation between actual margins, portfolio exposures, and market variables. We focus on the OTC market of credit default swaps (CDS), a major market for default risk transfer which has experienced a remarkable growth in the years before the global financial crisis, and has been at the center of many policy debates during the financial crisis. Over the past decade, the CDS market has transitioned towards mandatory clearing: after two parties (referred to as *clearing members*) enter a CDS contract, all counterparty obligations are transferred to a clearinghouse. Operating as a *central counterparty* (CCP), the clearinghouse insulates members from default risk, but requires them to post daily-settled collateral (margin).

Our study of collateral in the cleared CDS space builds upon the above-mentioned studies, but enriches and complements them along several dimensions. First, while prior studies have looked at headline margin requirements for individual securities (Figlewski (1984), Gay et al. (1986), Hedegaard (2014)), their approaches are less applicable in the modern setting of portfolio margining, where margins are set at the portfolio level rather than for individual contracts (as in the case of CDS clearinghouses). Our disaggregated, granular CDS data provide a valuable source of information for analyzing portfolio-level collateral requirements and the associated systemic risk implications (Huang and Menkveld (2016)). Second, we consider a market where payoffs are highly skewed (default probabilities can jump upward suddenly, and defaults can occur instantaneously), which implies that collateral plays a crucial role in allowing this market to function properly. Third, while existing studies focus mostly on the *cross-sectional* dimension of margins, we focus on both the cross-sectional and time-series variation. Fourth, we consider not only portfolio-specific risk measures, but also aggregate risk and funding measures as potential determinants of collateral – all factors that can play an important role in the amplification of aggregate shocks via the collateral-feedback channel. Fifth, we document that margins are best captured by using not only a tail risk measure (maximum shortfall), but also a short charge (a fixed percentage of aggregate short notional). As the short charge does not depend on historical probabilities nor on the state of the market, this shows that clearinghouses use a rule that is robust to the exact specification of the model for tail events. Lastly, we show that despite its use in many theoretical models, Value-at-Risk contributes very little to the explanation of margins after controlling for maximum shortfall and aggregate short notional.

The rest of the paper is organized as follows. Section 2 describes our data. We test the VaR rule in Section 3, and present our full analysis of the determinants of collateral rules in Section 4. Section 5 discusses the interpretation of our results and concludes.

2 Collateral requirements in the Cleared CDS Market: Institutional Details and Data

We describe the institutional setting of the cleared CDS market in Section 2.1, and the dataset in Section 2.2.

2.1 Clearinghouse margining in practice

Clearinghouses have significant discretion over modeling assumptions and parameters used to generate and justify margin requirements. They set them taking into account market conditions, the demand for trading, and collateral quality. In practice, margining rules involve a wide range of scenarios and simulations to arrive at a portfolio loss distribution, requiring the clearinghouses to make various modeling and statistical assumptions.

Most clearinghouses, *including ICC* (e.g., Ivanov and Underwood (2011)), state that their margins are broadly "set to cover five days of adverse price/credit spread movements for the portfolio positions with a confidence level of 99%", which we refer to as a 5-day 99% Value-at-Risk (VaR) margining rule. However, this is only a simplified description of their actual margining rules, for two main reasons. First, scenario-specific add-ons are often applied to produce the final margin requirement (CME Group (2010), ICE Clear US (2015)).[7] In particular, the margin requirement set by ICC is the sum of seven components. In addition to considering (i) losses due to credit quality (changing credit spreads), the methodology also considers losses due to (ii) changing recovery rates and (iii) interest rates. There are additional charges capturing (iv) bid-offer spreads, (v) large, concentrated positions, (vi) basis risk arising from different trading behavior of indices and their constituents. Finally, there is (vii) an additional jump-to-default requirement due to the potential large payouts associated with selling credit protection on single name contracts. Similar to the Basel capital requirements, the ICC margin framework is a bucket approach, where there is an individual methodology for calculating each of the seven components ("buckets"), and the final collateral requirement is the simple sum of these components. Second, even if clearinghouses were

[7] As the primary regulatory authority of U.S. Derivative Clearing Organizations, the CFTC has access not only to portfolio level margins data, but also collects documentation of the margin framework for oversight purposes. This framework contains a detailed breakdown of margin components, model assumptions, and formulas used in ICE Clear Credit's margin methodology, *though not the calibrated parameters and the calibration process*. This document is proprietary and only accessible by CFTC officials. However, ICE Clear Credit discloses publicly a high level break down of margin components, for which we provide a review in this section. For ICC's public disclosure, see https://www.theice.com/publicdocs/clear_credit/ICE_CDS_Margin_Calculator_Presentation.pdf.

restricted to using VaR based margining rules, the confidence level, margin period risk, and the distributional assumption of losses are inputs that give the clearinghouse significant freedom in setting the actual margin levels.

Overall, clearinghouses employ complex rules to determine the amount of required margins. These rules make it hard to understand what are the main economic determinants of collateral requirements, partly because they are complex and depend on the interactions of a myriad of variables and calibration choices, partly because they do not explicitly take into account variables that, however, may still matter indirectly: for example, aggregate volatility or default risks do not directly enter the calculations, but may still affect the collateral rule empirically because of their effect on the scenarios the clearinghouse uses to simulate portfolio losses, or through the choice of discretionary parameters.

The goal of this paper is not to reverse-engineer this complicated procedure, but rather to identify and quantify the main economic determinants of the variation in collateral over time and across members. As we show later, despite the complexity of the procedure followed by ICC, empirically a few main portfolio risks and market variables explain the vast majority of the panel variation in collateral requirements. The ability to extract and quantify the main economic determinants of the collateral rule allows us to speak to economic models of the collateral equilibrium, that cannot possibly include the entire margin-setting procedure, but can instead incorporate the main channels that we highlight to be most important in our empirical analysis.

2.2 Data and Summary Statistics

We provide an overview of our data and present descriptive statistics of the key variables. We construct a database of the entire universe of CDS positions cleared by the dominant CDS clearinghouse, ICE Clear Credit (ICC), for the two years between May 2014 and April 2016. ICC managed 60% of the U.S. cleared CDS market in 2015.

2.2.1 Clearinghouse collateral data: the Part 39 data set

The Dodd–Frank Wall Street Reform and Consumer Protection Act grants the U.S. Commodity Futures Trading Commission (CFTC) authority over Derivative Clearing Organizations (DCOs). As a result, major clearinghouses recognized as DCOs are required to report confidential swap trade data to CFTC on a daily basis. The data are collectively referred to as "Part 39 data," as the relevant rules and regulations are codified in Title 17, Chapter I, Part 39 of the Code of Federal Regulations. Part 39 data provides a complete overview of the centrally cleared swaps in the U.S..[8]

We obtain clearing member data from the CFTC Part 39 database.[9] Our data set consists of both positions data and account summary data for CDS trades cleared by ICE Clear Credit

[8]While some swaps are under the jurisdiction of the Securities and Exchange Commission (SEC) so that bilateral trades need not be reported to CFTC, they are captured by Part 39 once cleared.

[9]Because the data set contains proprietary and confidential trade positions and margins, they can only be accessed by CFTC officials and are not distributed for legal reasons.

(ICC), which is the main clearinghouse for the CDS market (combined with ICEU, the European arm of ICE's CDS clearing, they account for over 90% of the cleared CDS market registered in the database). Our sample period covers two years, from 2014/05/01 to 2016/05/01, for a total of 517 business days.

2.2.2 CDS positions data

Credit default swaps (CDS) are credit derivatives used to trade the credit risk of a reference entity. With a fixed reference obligation, the protection buyer (the long position) is obligated to pay a quarterly premium (the coupon payment) to the protection seller (the short position) up until contract maturity or the arrival of a credit event, whichever occurs earlier. Upon arrival of the credit event, the seller pays to the buyer the difference between the face value and the market value of the reference obligation. When the reference entity is a sovereign or corporate entity, the CDS is referred to as a single name CDS, and is uniquely identified by its coupon rate, maturity, reference bond seniority, and doc clause (that defines what constitutes a credit event), typically rolled out quarterly. When the reference entity is a weighted basket of bonds from various sovereign or corporate entities, it is referred to as an index CDS, typically rolled out semiannually. When components of the reference basket default, the protection seller pays a pro rata cash flow depending on the weights on the components. The index contract is then reversioned (i.e., the basket is updated), and coupon payments and the contract notional are reduced accordingly. A CDS index contract is identified by its notional, coupon rate, maturity, reference basket, version, and doc clause. Our data set includes both single name and index contracts.

The CDS position component of the Part 39 data set contains daily reports of each account's end-of-day (EOD) position in each cleared CDS contract. For each day/account/contract combination, we observe long/short gross notional, EOD prices for the contract,[10] the currency denomination and exchange rates, and the mark-to-market (MtM) value of the position.

In the considered sample period (2014–2016), the most liquid CDS index contracts (but not all single name CDSs) were mandatorily required to be cleared through a clearinghouse. As a result, while our data set captures 100% of the mandatorily cleared contracts, it presents only a partial view of the entire CDS market. Our data set includes 465 distinct contract names, 455 of which reference single name CDS contracts and 10 of which correspond to indices. A total of 18,179 distinct contracts referencing these names were cleared during our sample period.

We adjust for changes in reference names due to spin-offs, split-offs, or combined firms from mergers and acquisitions. After accounting for this, we are left with a total of 443 distinct names.

2.2.3 Part 39 CDS prices

End-of-day (EOD) prices within the Part 39 data set are provided by ICC in terms of points upfront. CDS prices historically have been quoted in terms of conventional or "break-even" spreads, defined

[10] See Section 3.1 for a brief overview of standardized CDS price quote conventions.

as the annualized quarterly spread payment per unit of purchased protection that makes the market value of the position zero at initiation. Contracts thus were negotiated bilaterally over the counter and, depending on when they were traded, carried different spreads. The push for standardized CDS contracts, however, has drastically changed the landscape of CDS price quotes and traded contracts. In particular, the 2011 "CDS Big Bang" resulted in standardized CDSs having fixed coupons (usually 100 or 500 basis points). Thus, contract market values are often non-zero at outset. When trading standardized CDSs, the protection buyer makes an upfront payment to the protection seller at initiation (or vice versa). Price quotes are then in "points upfront" instead of break-even spreads. For instance, if a CDS contract were quoted at 0.97, the protection buyer would pay $1 - 0.97 = 3\%$ of the notional to the seller at contract initiation. Notice that this quote convention is analogous to bond quotes, where a higher price quote represents a lower payment for the buyer. Some data providers, such as Bloomberg, convert the quoted prices using a standardized model provided by the International Swaps and Derivatives Association (ISDA) and, by convention, record break-even spreads.

We note that quoted prices are model prices. Since CDSs trade relatively thinly, EOD transaction prices are not always available. ICC and Markit have a specific price discovery process tailored to the CDS market. Participants submit price quotes at the end of every business day and the clearinghouse creates periodic trade executions among participants via an auction process. The resulting prices are used for daily mark-to-market purposes.

2.2.4 External CDS spreads and interest rates data

We complement our Part 39 data with several additional data sets. First, we collect 5-year on-the run CDS spreads from Markit and Bloomberg, going back before the beginning of our sample period.[11] Second, we obtain from Bloomberg the time series of the Overnight Index Swap (OIS), the overnight swap rate, and the London Interbank Offered Rate (LIBOR), the overnight rate for unsecured lending between banks, both of which we use to measure funding costs. The LIBOR-OIS spread is typically viewed as a measure of financial sector stress. Third, we obtain from Bloomberg the time series of the VIX. For all three data sets, we obtain time series ranging between 2004/01/01 and 2016/09/13.

2.2.5 Account and margin data

The account summary portion of the Part 39 data set contains daily reports of EOD account-level information for each clearing member account. For each day/account combination, we observe the so-called *initial margin* requirement, the actual amount of initial margin posted, the currency denomination and exchange rates, and the MtM value of the portfolio. The initial margin requirement is the level of collateral the clearinghouse demands from the account holders, whereas the margin

[11] Markit had complete data for all of our 443 names except for the 10 credit indices and 3 single name series, STCENT, ATSL and COMMSAL; for these 13 names, data was unavailable or incomplete, so we instead obtained the CDS spreads data from Bloomberg.

posted is the actual level that account holders supply (after haircuts), which is usually the same as the required amount or very close.[12]

It is useful to emphasize that what is referred to as the initial margin in this market – the collateral requirement we study in this paper – is the collateral kept by the clearinghouse with the purpose of buffering against potential *future* losses in case clearing members default on their obligations. Note that despite the name "initial" margin, this margin is not just posted at initiation of a CDS positon: instead, it is updated every day and it covers the entire portfolio of a clearinghouse member. It therefore corresponds directly to what is typically referred as collateral requirement in standard models. We will use the terms initial margin, margin requirements, and collateral requirements interchangeably.

All cleared contracts are marked to market daily, so that the change in *current* value of the portfolio is transferred to the clearinghouse by the next day. This transfer is referred to as *variation margin*, which is distinct from the initial margin as it does not represent a stock of collateral meant to cover for future changes in the value of the portfolio, but rather a cash flow reflecting the mark-to-market process.

To clarify, suppose that two clearing members A and B have a position with the clearinghouse. Each member posts a certain amount of collateral to the clearinghouse to buffer against future changes in the value of the cleared position when he defaults on his obligations: this is the initial margin. Now suppose that after a price movement, the value of A's cleared portfolio decreased by \$1, and the value of B has increased by \$2. A then has to transfer \$1 to the clearinghouse, which is then passed through to the counterparty holding the offsetting position; similarly, B will receive \$2 from the clearinghouse, that in turn the clearinghouse has obtained from members holding the opposite position: these payments, that correspond to the marking to market of the positions, constitute the variation margin. After the price move, or in fact even independently of that, it is possible but not necessary that the two members might have to *also* adjust the initial margin, if the clearinghouse determines that the riskiness of their positions has changed (directly because of the price changes, or indirectly because of changes in market conditions). But in general, the variation margin and the initial margin are not in a mechanical relation to each other. To sum up, the posting of initial margins is to cover for future potential price changes, whereas the variation margin payment is to settle current price changes. In this paper, we focus on the initial margin, as it maps directly into the collateral requirements of economic models.

The cleared CDS market is dominated by a handful of *clearing members* who act as dealers to the outside market. Smaller clearing participants access the cleared market by becoming customers to clearing members. Each clearing member may have several accounts with ICC. The account is designated as a "customer account" if the account positions are taken on behalf of a customer, and designated as a "house" account if the positions are proprietary. Customer accounts are

[12] Margin requirements are reported separately in USD and Euro; we combine them using the appropriate exchange rate to arrive at the total initial margin requirement for the entire portfolio in USD. The actual collateral posted is often reported entirely in USDs and covers both the USD and Euro requirements, so the distinction between currencies is immaterial.

commingled; that is, they consist of multiple sub-accounts for many customers, and segregated customer specific data are not reported. We observe 44 accounts in total, each identified by a distinct clearing firm identification number. Of these accounts, 13 are designated as customer accounts and 31 are house accounts.

Many house accounts are set up to help with the processing of client trades, but have little open interest, as clearing members usually use one house account to hold the majority of their proprietary positions. We thus define a house account to be "auxiliary" if there are little to no positions associated with them. To be precise, a house account is auxiliary if (i) the average gross notional is less than $20 billion USD, (ii) the average number of distinct CDS contracts traded is less than 200, and (iii) the number of distinct CDS reference entities traded is less than 40. We refer to the remaining house accounts as "active" house accounts, of which there are 13.

We provide descriptive statistics for each of the three account categories (active house, customer, and auxiliary house) in Table 1. Table 1 reports, for each account, the pooled averages of the key variables over our sample period. Pooled averages are computed by averaging point observations within the account categories and across the sample time period.

Table 1

Active house accounts trade on average 4,042 different contracts, measured by the number of distinct contracts in which the account has an open interest, distributed over 240 names, measured by the number of distinct reference entities in which the account has an open interest. In contrast, the customer accounts trade around 73 contracts distributed over 33 names. Customer positions tend to concentrate in the most liquid index contracts, whereas house positions contain many more single name trades. Auxiliary house accounts, which have open positions in about 99 contracts over 24 names on average, resemble characteristics of customer accounts as they are set up primarily to facilitate client needs.

The larger number of names traded by house accounts contribute to a higher gross notional compared to customer accounts, $169 billion versus $46 billion. The levels of collateral posted (initial margins), however, are similar in magnitude for both, at $658 million versus $614 million. Measured by margins to gross notional, the clearinghouse requires a lower collateralization rate for active house accounts compared to customer accounts. This is because dealers usually have better-hedged portfolios in their active house accounts, which results in a lower margin per unit of gross notional, compared to customers who often have large directional exposures. The gross notional and initial margins of auxiliary house accounts, at $8.3 billion and $58 million, respectively, indicate little trading activity. In fact, six auxiliary house accounts had zero margins throughout, indicating no trading activity at all. We excluded these accounts when calculating descriptive statistics of auxiliary house accounts.

We next analyze the time variation and average level of aggregate required and posted collateral. More specifically, Table 2 reports statistics of the time series of aggregate initial margin requirement and actual posted collateral in USD, aggregated over all accounts, over all customer accounts, and over all active house accounts respectively. We note that the summary statistics of both initial margin requirements and posted collateral trace each other closely, and support our approach of focusing only on the initial margin requirement.[13]

In the aggregate, about $17 billion worth of assets are immobilized as collateral (initial margin). There is significant variation over time, ranging from 15 to 22 billion USD. Customer margins are on the same order as that of active house margins, consistent with numbers reported in Table 1. The time variations in margins for active house accounts and customer accounts are highly correlated, as evidenced by the sum of their standard deviations (0.6+1.2 billion USD) being close to the standard deviation of the aggregate (1.7 billion USD) – the two would be exactly the same if they were perfectly correlated.[14]

Table 2

Our analysis of portfolio specific margins in the remainder of the paper focuses only on the active house accounts. This is because customer accounts are commingled and margins information aggregated. Therefore, the observed margins are not associated with a specific institution's portfolio in our dataset, so that we cannot study the relationship between collateral posted and portfolio characteristics. We note, however, that our empirical analysis does not require observing the collateral posted by *all* clearing participants, at least to the extent that enough information can be extracted from the clearing members whose portfolios we fully observe. We also exclude auxiliary house accounts because there are little to no positions associated with them.

To gain further insight into active house account margins, we compute the level of collateralization for cleared portfolios. We measure this with the *margin to net notional* ratio, which accounts for varying sizes of cleared portfolios. We compute a portfolio's net notional by computing the net notional amount for each reference name, and then summing the absolute net notionals across all names.

The results are reported as a histogram in Figure 1. For each active house account/day combination we compute a margin to notional ratio by taking the ratio of the initial margins requirement and aggregate net notional. The resulting 6,721 observations cluster into two distinct sub-populations. 92.3% of the observations cluster around a sub-population mean margin to notional ratio of 2.4%. The remaining 7.7% of the observations cluster around a sub-population mean margin to notional ratio of 14.3%. All observations of the latter sub-population are associated with one single clearing member account. This analysis demonstrates that house account portfolios can have very different risk characteristics, leading to very different collateralization levels.

[13] The initial margin requirement is arguably economically more important, as it represents the level of collateral that *has* to be immobilized, whereas posted collateral levels may fluctuate due to clearing member operational reasons.

[14] We note that there are few instances where the posted collateral is actually *lower* than the margin requirement, representing under-collateralization. We attribute this to member operational issues that are permitted by the clearinghouse. As no member was declared to be in default during this period, the clearinghouse may have considered such deficits of secondary importance.

Figure 1

2.2.6 Market events during our sample period

Mandatory clearing of standardized CDS contracts was imposed only after the financial crisis. Thus, our data set does not include the years of the crisis in which the financial system (and the CDS market) underwent significant stress, which are particularly interesting times for understanding collateral requirements and their interaction with the broader economy.

There are, however, two mitigating elements that allow us to understand the behavior of collateral even in the limited span of time covered. First, even within our two-year sample, many significant events resulted in volatility spikes, shocking the global economy and, in particular, the CDS market. Examples of these events include (i) the plummet of oil prices in November 2014, when Saudi Arabia blocked OPEC from cutting oil production; (ii) the plunge in the Euro when the ECB chief Mario Draghi expressed unexpectedly dovish outlooks on monetary policy in January 2015, (iii) the 2015–2016 stock market sell-off, starting with the Chinese stock market burst ("Black Monday"), and followed by an unexpected devaluation in the Renminbi, which was further fueled by Greek Debt default; (iv) the unexpected negative interest rate policy announced by the Bank of Japan in January 2016, and (v) the volatility spike when the Brexit referendum was announced in February 2016. Our sample period also covers the (widely expected) interest rate hike by the Federal Reserve in December 2015, the first increase in nearly a decade.

Second, while we do not have positions data going back to the financial crisis, we do observe CDS spreads going back to 2004. This means that we are able to do *counterfactual simulations* of portfolio returns, that include market movements observed during the financial crisis. This will provide additional information about how well collateral buffers can absorb shocks of magnitudes as large as those observed in 2008–2009.

3 Collateral requirements and the *Value-at-Risk* rule

Most of the theoretical literature on the collateral channel (e.g., Brunnermeier and Pedersen (2009)) makes the assumption that collateral constraints are specified by an exogenous VaR rule. This is consistent with claims of major clearinghouses that collateral requirements are set targeting a VaR rule (e.g. Ivanov and Underwood (2011) for ICC). A small theoretical literature (e.g. Geanakoplos (1997) and Fostel and Geanakoplos (2014)) has explored the endogenous determination of collateral requirements, and has highlighted that rather than a VaR rule, more extreme measures of portfolio risk should be expected to arise in equilibrium.

In this section, we directly test whether a VaR margining rule (or a close approximation of it) is observed in the cleared CDS market. We only use data associated with active house accounts. This is because, as mentioned above, ICC practices portfolio margining: margins are set at a portfolio level and based on the portfolio rather than individual position characteristics, while customer accounts' portfolio margins are commingled (aggregated) in our data, so we do not observe them separately. Since VaR is not summable, using aggregated customer account data would distort our analysis.

We find that collateral requirements in the CDS market are not consistent with those prescribed by a VaR constraint, regardless of the confidence level and margin period of risk used for the returns. One of our main findings is that the required collateral levels appear to be orders of magnitude higher than what is required to cover 1%-probability losses, even if we include in our sample returns that would have been observed during the financial crisis.

Our results thus suggest that the observed collateral requirements differ significantly from those typically assumed exogenously in existing theoretical work, and that theoretical models should account for additional determinants of collateral levels (explored in more detail in Section 4).

3.1 Notation

Consider a set of dates $\mathcal{T} := \{1, \ldots, T\}$, a set of contracts $\mathcal{I} := \{1, \ldots, I\}$, and a set of market participants (clearing members) $\mathcal{N} := \{1, \ldots, N\}$. The portfolio held by participant n at the end of date t is a vector $\mathbf{X}_t^n \in \mathbb{R}^I$. The i-th component of $\mathbf{X}_t^n$, $X_{i,t}^n$, is the portfolio's *notional position* in contract i. $X_{i,t}^n$ can be positive or negative, depending on whether n has a long or a short position in the contract i.

We denote the end-of-day (EOD) prices of cleared contracts at time t by $\mathbf{P}_t$, whose i-th component, $P_{i,t}$, is the EOD price of contract i. As discussed in Section 2, the mark-to-market value of the portfolio $\mathbf{X}$ at time t can be computed as[15]

$$\begin{aligned} MtM_t(\mathbf{X}) :&= \text{Mark-to-market value} \\ &= \sum_i \text{Position net notional} \times (1 - \text{price}) \\ &= \mathbf{X} \cdot (1 - \mathbf{P}_t) \end{aligned}$$

The profit and loss ($P\&L$) between day t and day $t+M$ (for a given time-t portfolio $\mathbf{X}$) is computed as

$$\begin{aligned} \Psi_{M,t}(\mathbf{X}) &:= MtM_{t+M}(\mathbf{X}) - MtM_t(\mathbf{X}) \\ &= \mathbf{X} \cdot (\mathbf{P}_t - \mathbf{P}_{t+M}) \,. \end{aligned} \tag{1}$$

We use $VaR_t^{M,\alpha}(\cdot)$ to denote the $\alpha-$th quantile of the profit-and-loss (P&L) distribution over an $M-$day period starting at t. Hence, Value-at-Risk (VaR) is defined by

$$\mathbb{P}(\Psi_{M,t}(\mathbf{X}) < -VaR_t^{M,\alpha}(\mathbf{X}) | \mathcal{F}_t) = \alpha,$$

[15] There is an additional adjustment factor for CDS indices that have been reversioned after the default of a component, which we omit here for ease of exposition but account for in our empirical analysis. The adjustment factor is smaller than one and accounts for a proportional decrease in effective notional due to the contract payout.

where $\mathcal{F}_t$ represents the information set available at time t. M is commonly referred to as the margin period of risk (or liquidation period), and $1-\alpha$ is the confidence level.

3.2 Testing the *Value-at-Risk* margining rule: two approaches

The standard VaR collateral rule assumed in the literature stipulates that collateral requirements (initial margins) at time t are set equal to $VaR_t^{M,\alpha}(\cdot)$, for a certain confidence level α and margin period of risk M. That is, under the VaR rule, it states that initial margins are set as

$$H_0 : IM_t(\mathbf{X}_t^n) = VaR_t^{M,\alpha}(\mathbf{X}_t^n),$$

where $IM_t(\mathbf{X})$ are the actual margins required by the clearinghouse at time t for holding the portfolio $\mathbf{X}$. This hypothesis can be directly tested using our data by comparing the actual clearinghouse collateral requirements with the empirical quantiles of the loss distribution.

We test the VaR hypothesis H_0 using two different approaches. The first approach is applicable in the circumstances where α and M are known. For instance, CDS clearinghouses typically claim that initial margins are set to cover 5-day losses with 99% confidence (Ivanov and Underwood (2011)). Under this assumption, H_0 should hold with $\alpha = 1\%$ and $M = 5$. If initial margins are set to be a certain conditional quantile of the returns distribution (say the 1% quantile), the fraction of times the portfolio loss exceeds the posted collateral is expected to be on average equal to that quantile (1% of the time). We refer to this approach as the *time-series* test of the VaR hypothesis; this is in fact equivalent to the "backtesting" procedure advocated by the Basel Accords (Hull (2012)).

A second approach can be considered in the cases that α and M are unknown. Rather than testing the rule jointly across all counterparties, this test looks at whether the *same* VaR rule is applied to all counterparties, similar to the approach implemented by Gay et al. (1986). That is, no matter what α and M are, under VaR margining we would expect the *same* margining rule to be applied to all counterparties. This is a conceptually important test because it reveals whether the proposed rule (VaR) is able to capture all portfolio- and counterparty-specific factors that are relevant for determining margin requirements. We refer to this as the *cross-sectional* test of the VaR hypothesis.

3.3 Time-series test of the VaR hypothesis

We start with the time-series test of the VaR hypothesis, using the null H_0 described by the clearinghouse (Ivanov and Underwood (2011)): $M = 5$ days and $\alpha = 1\%$. Since $IM_t(\mathbf{X}_t^n)$ is observable in our data set, the critical point of our analysis is to estimate the empirical distribution of the P&L of the portfolio over M days ($\Psi_{M,t}$) .

We consider two different approaches. First, we perform a factual analysis and compute realized 5-day ahead P&L for each actual portfolio held by active house accounts, during our sample period. That is, we ask the following question: have actual exceptions, i.e exceedance of losses beyond the posted collateral, been as frequent as the margining rule would predict (1% of the time) within our sample?

Second, we perform a counterfactual analysis and estimate what the 5-day ahead P&L would have been for each portfolio, using historical 5-year CDS spreads going back to 2004. This counterfactual analysis compares the collateral requirement with the distribution of losses on the portfolios held at each point in time, but using the distribution of returns for a much longer time period, that includes the financial crisis. That is, we ask the following question: would exceptions be as frequent as the margining rule would predict (i.e. 1% of the time), given the portfolios we observe in our sample period and returns observed during the entire period 2004-2015? Such a counterfactual analysis remedies to the lack of crisis-level downturns data in our sample.

3.3.1 Using realized returns

Recall that Part 39 data contains not only positions data, but also EOD price data in points upfront for each cleared contract. We can then easily compute the 5-day ahead P&L using Eq. (1). In particular, for each account/business day pair (n, t), we compute

$$\Psi_{5,t}(\mathbf{X}_t^n) = \mathbf{X}_t^n \cdot (\mathbf{P}_t - \mathbf{P}_{t+5}).$$

We define the ratio of P&L to initial margins as the *return on margins* for cleared portfolios. Since both the size of losses and margins are expected to increase with portfolio volatility, this normalized measure that controls for size is better suited for comparison across portfolios. Notice that a margin exception corresponds to a negative return on margin that drops below -100%. So the empirical distribution of exceptions is simply the fraction of times we observe a return on margin below -100%.

Figure 2 reports the distribution of realized 5-day ahead returns on margins. We compute returns on margins for each account/day in our sample and obtain $(517 - 5) \times 13 = 6,656$ observations. A few interesting patterns emerge from the figure. First, the dispersion of returns in our sample period is quite small relative to the amount of collateral posted. Second, the distribution of returns on margins does not exhibit distinctly heavy tails, despite the fact that several important events occurred during our sample period as highlighted in Section 2.2.6. Third, and most relevant for the analysis of margining and losses, the *most negative* return observed during our sample period is only 30% of the collateral requirement. That is, while under a 5-day 99% VaR rule we would expect to see exceptions in 1% of our sample (or about 66 account/days), no exceptions were actually observed. This serves as a preliminary indication that the VaR rule with 99% confidence does not describe well the actual collateral requirements in the CDS market.

Figure 2, Table 3

In order to formally test the hypothesis H_0, we perform a statistical test comparing the observed empirical frequency (0) to the one predicted by the model (α). The distribution of the test statistic (the difference between the empirical frequency and α) is derived in Appendix A.2.

Table 3 reports both the descriptive statistics for these returns on margins and the results of our time-series test. We observe that the mean return on margin is close to zero, and much smaller than the standard deviation of 5.22%. The distribution tightly clusters around the mean: about half of the mass of the empirical distribution of returns lies between $\pm 2\%$. Aggregating over account/time observations, we find that the empirical 99% Value-at-Risk is approximately 14% of posted margins; that is, around 7 times lower than the one implied by the VaR rule. Approximately 98% of the observations lie within 3 standard deviations from the mean.

The test result reported in the table shows strong evidence against the 5-day 99% VaR rule: the frequencies of exceptions are statistically significantly smaller than 1%. Overall, the analysis of exceptions during our sample period indicates that the amount of collateral posted was far larger than what would be expected by a 99% confidence VaR rule.

3.3.2 Counterfactual return estimates (historical simulation)

In this section, we use historical simulation methods to estimate P&Ls over a longer time period than that considered in the previous section. Such a time period covers the financial crisis, the most significant market stress since the Great Depression. By doing so, we remedy to the potential lack of power of the hypothesis test conducted in the previous section caused by the limited sample size, and by the fact that the sample period (2014–2016) considered in the previous section was relatively tranquil except for a few notable events (like those described in Section 2.2.6).

More specifically, the idea of our counterfactual simulation is as follows. For each period, we observe the portfolio held by each counterparty, $\mathbf{X}_t^n$. By looking at the history of (joint) price movements for all the constituents of those portfolios, we can ask what the historical distribution of returns of that specific portfolio would have been *since 2004* (the first time for which CDS data are available in our data set). The resulting distribution of P&L therefore includes the large price changes that occurred during the financial crisis, and incorporates the dramatic increase in correlations observed in those years.

Since there are new contracts issued and old contracts expiring every quarter, historical prices for a currently traded contract are not always available. To deal with this practical obstacle, we consider a historical simulation approach that closely follows that proposed in Duffie et al. (2015). We first aggregate net exposures by name (reference entity), and then use the historical 5-year CDS spread on those names (for which we have accurate spreads data) to compute counterfactual returns for all days for which CDS spreads are available. We review the details of the methodology in Appendix A.1. As discussed in Section 2.2, the time series of credit spreads data span dates between 2004/01/01 and 2016/09/13; this gives us a total of 3,303 days with 5 days ahead observations. We will refer to these days as *evaluation days*.[16]

[16] A day is included in our data analysis only if prices are observed for at least 250 out of the 443 reference entities; this filter excludes few days in the early part of the sample for which price information was not uniformly available across contracts.

Using this approach, for each portfolio held by account n on each day t, we compute all possible returns on margins that could have occurred to that portfolio if the spread movements for all the CDSs in the portfolio in the following 5 days mimicked the realized spread movements that occurred on each of the 5-day windows since 2004. We compute this by evaluating the P&L for each account/day/evaluation day combination. We note that the approach of Duffie et al. (2015) we follow uses data on 5-year CDS spreads as opposed to the actual maturities, and thus requires an approximation that depends on the average duration of the CDS portfolio, d. We choose $d = 3$ years as in Duffie et al. (2015).[17] This procedure yields a number of counterfactual return observations equal to $517 \times 13 \times 3,303 = 22,199,463$.

We report the distribution of counterfactual returns on margins in Figure 3. Due to the large number of observations clustering around zero, we only display the histogram in the range $[-50\%, +50\%]$ in Figure 3a, and zoom in on the left tail of the histogram in Figure 3b. We see that the distribution is sharply peaked, and that most returns on margins lie between $\pm 20\%$. The frequency of returns decreases rapidly as we move away from the mean.

Figure 3a, Figure 3b, Table 4

When we consider counterfactual returns, we observe a small number of margin exceptions: the portfolios held during 2014-2016 would have sometimes experienced losses larger than the posted collateral if prices moved as they did during the financial crisis.

There are 3,456 observations (out of about 22 million) of returns on margins which are smaller than -100%, amounting to 0.016% of our sample. The most negative return amounts to 268% of the posted initial margins.

Table 4 reports both descriptive statistics for the counterfactual returns and results of the associated time-series test. Compared to the realized returns, the counterfactual returns have a mean closer to zero, a higher standard deviation equal to 8.22%, and fatter tails (the excess kurtosis is 31.26). It should not be surprising that counterfactual returns exhibit higher standard deviation and kurtosis than those in the period 2014–2016, because they reflect the large spread movements experienced during the financial crisis. The distribution tightly clusters around the mean: about half of the empirical distribution of returns lies between $\pm 2\%$. Aggregating over all counterfactual returns, we conclude that the empirical 99% Value-at-Risk is approximately 27% of posted margins.

To test the VaR margining rule, we compare realized losses to initial margins for each observation. We extend the test of the previous section to include all the historical counterfactual returns for each portfolio held in each day by each account. As before, the test compares the empirical frequency of margin exceptions with that predicted by the VaR rule. To fully account for potential time-series and cross-sectional correlation of the residuals, we double-cluster the standard errors of the test statistic at both the day and the account levels (as described in Petersen (2009)). Appendix

[17] In Section 4.4 we explore robustness to the choice of d, and also report how closely the counterfactual losses mimic the actual losses for the time period when both are observed. The two loss series are positively (though less than perfectly) correlated. As expected, the absolute magnitude of the counterfactual returns is higher because it includes the financial crisis.

A.3 reports the detail of the test statistic and its distribution.

Strikingly, these exceptions are extremely rare, as shown in Table 4: even including losses as large as those observed during the financial crisis, exceptions of the initial margins only occur about 0.016% of the time, that is, they are about one hundred times less frequent than a 99% VaR rule would imply. The results are reported in Table 4, which shows that the hypothesis of a 1% VaR rule is strongly rejected.

We also report the ratio of exceptions separately for each day $t \in \mathcal{T}$. For each t, we estimate the probability of an exception during the next 5 days using the historical distribution of price changes observed since 2004, given the portfolios held by the accounts at t. For each of the 13 active house accounts, we count the number of counterfactual returns that exceed the amount of initial margins required for their portfolio. The number of exceptions are then averaged over the $13 \times 3303 = 42,939$ observations for that day.

Figure 4 plots the frequency of simulated exceptions over our sample period. The ratio is much lower than 1% even at its maximum (around 0.6%). Interestingly, there is significant time variation in the ratio: there are periods in which the effective exception confidence levels are high (05/2014–01/2015 and 06/2015–09/2015) and periods when they are much lower (02/2015–05/2015 and 10/2015–05/2016). This indicates the existence of additional factors affecting the fitted VaR confidence level, which in turn suggests that there are other elements affecting the collateral requirements beyond simple a VaR rule.

Figure 4

3.4 Cross-sectional test of the VaR hypothesis

In this section we perform the cross-sectional test of the VaR hypothesis outlined in Section 3.2, that requires no assumption about the confidence level (α). Recall that the margining rule requires that

$$\mathbb{P}(\Psi_{M,t}(\mathbf{X}_t^n) < -IM_t(\mathbf{X}_t^n)) = \alpha \quad \text{for all}\, n$$

that is, the exceedance ratios should be the same across clearing members. We analyze the validity of this rule by testing that the empirical frequencies of exceptions across accounts (a $G-$test) are the same, as described in Appendix A.4.

Table 5 reports the results of the cross-sectional test of the hypothesis that a VaR margining rule (at any confidence level α) can explain observed margin levels. For robustness, we perform the test for various combinations of CDS duration parameter (following the duration approximation formula in Duffie et al. (2015), choosing $d = 3$ and 5 years) and margin periods of risk ($M \in \{1, 3, 5, 7, 10\}$ days). The null hypothesis is that if a VaR rule is in place, then it is fairly implemented so that the frequencies of exceptions are independent of clearing member identities.

The p-values reported in Table 5 are essentially zero. There is therefore extremely strong evidence against equality of exception probabilities, and thus against the null hypothesis that there exists a VaR rule which can explain observed initial margins for all counterparties.

Table 5

We remark that since we pool exception observations across evaluation dates (and the respective portfolios) for each account, there is a possibility that autocorrelation of exceptions could reduce the power of our test. To adjust for possible autocorrelation, we also perform an autocorrelation-robust extension of the G-test, described in detail in Appendix A.4. As before, we test whether a VaR margining rule of any confidence level can explain observed margin levels for various combinations of CDS duration (d) and margin period of risk (M) assumptions. We report the p-values for the various cases in Table A.1. In all but one case the p-values are essentially zero. This indicates that, even after taking into consideration potential correlation issues, there is still extremely strong evidence against the VaR hypothesis.

Table A.1

4 The Determinants of the Collateral Rule: Portfolio Risk and Market Characteristics

The previous section has shown that a simple VaR rule fails to capture observed collateral requirements in the time series and in the cross-section. This suggests that other variables (both at the level of the individual member's portfolio and marketwide) might enter the collateral rule. In this section we perform an extensive investigation of the empirical determinants of collateral requirements.

We consider two groups of potential explanatory variables. *Portfolio variables* are those that are specific to the portfolio that an account holds with the clearinghouse, and are conventionally used to measure the risk of positions. These include the Value-at-Risk of the portfolio (studied above), expected shortfall, maximum shortfall, aggregate net notional, aggregate short notional, and the volatility of the portfolio (see the definitions in Table 6 and in the next section). *Market variables* are those that are determined by market forces, including the clearing members' CDS spreads, the LIBOR-OIS spread, the average clearing member CDS spread, and aggregate volatility as measured by VIX. Table 6 summarizes the full list of variables and the notation used in this section.

Table 6

This section proceeds as follows. We describe the computations of various portfolios and market variables in Section 4.1. In Section 4.2, we perform a panel analysis and relate the observed margin requirements for each account n and each day t to various portfolio-specific measures. We also use our results to evaluate the initial margins model proposed by Duffie et al. (2015) (henceforth, DSV), a reduced-form alternative to VaR with emphasis on the maximum potential loss and the short position of the clearing member. We find that a slight modification of it yields a good approximation of the portfolio-level determinant of the collateral rule observed in the CDS market, and has a better explanatory power than VaR.

Section 4.3 incorporates market variables into the panel analysis. The collateral requirements imposed by the clearinghouse are not fixed in time but respond to aggregate events. Those events include changes in the aggregate conditions of the economy, and more specifically changes in the demand for intermediation and clearing. It is thus plausible to expect that market conditions such as market volatility or members' funding costs would affect the equilibrium collateral levels. This analysis highlights and quantifies potential channels for general equilibrium effects of margining.

4.1 Description of portfolio and market variables

Portfolio variables

Let Ω_k denote the set of CDS contracts with reference name k (that differ by maturity, doc clauses, etc...). For each reference k, net notionals are defined by

$$Y^n_{k,t} := \sum_{i \in \Omega_k} X^n_{t,i}.$$

The aggregate net notional AN^n_t, is then defined as

$$AN^n_t := \sum_{k \in K} |Y^n_{k,t}|,$$

which is the absolute sum of net notionals across reference names. The aggregate *short* notional, AS^n_t, is instead defined as

$$AS^n_t := \sum_{Y^n_{k,t} < 0} |Y^n_{k,t}|.$$

The aggregate short notional plays an important role because of the highly asymmetric nature of CDS payoffs. While the premium leg makes fixed payments, the protection leg (i.e. the short side of the CDS position) is exposed to jump-to-default risk. Such an asymmetry induces strong left skewness in the payoff function of a short position, which may prompt larger collateral requirements.

Duffie et al. (2015) propose an initial margin model alternative to VaR that uses only portfolio variables, and follows the rule:

$$DSV^n_t = MS_5(\mathbf{X}^n_t) + 0.02 \times AS(\mathbf{X}^n_t), \tag{2}$$

where $MS_M(\cdot)$ represents the maximum shortfall of the cleared portfolio for a $M-$day margin period of risk, and $AS(\cdot)$ is the aggregate net notional.[18] The margin model incorporates both the

[18]While Duffie et al. (2015) compute maximum shortfall for a fixed look-back period of 1000 days, we use a longer price series starting from the year 2004. As both ours and their time series data cover the years of the crisis, when

maximum historical loss, and a 2% "short charge". It is noteworthy that the DSV model was not estimated from data or derived theoretically, but calibrated to anecdotal evidence (which makes its empirical success even more remarkable, as we discuss later).

We also consider a modified version of the DSV model:

$$MDSV_t^n = 0.5 \times MS_5(\mathbf{X}_t^n) + 0.02 \times AS(\mathbf{X}_t^n), \tag{3}$$

which places a lower weight on maximum shortfall; the weights in this modified version of the DSV model are estimated from our sample, as described later.

We estimate the empirical distribution of the simulated series $\psi := \left\{\hat{\Psi}_{5,\mathrm{t}}(\mathbf{X}_t^n)\right\}_{t=1}^{T}$ of 5-day ahead P&L via the historical simulation approach discussed in Section 3.3.2. Using the empirical distribution, we form estimates of volatility (standard deviation), Value-at-Risk, expected shortfall, and maximum shortfall of the portfolio. All portfolio variables are in millions of USD to conform with the level of initial margins.

Market variables

We collect from Bloomberg time series data of the 3-month Overnight Index Swap (OIS) spread, the 3-month USD LIBOR rates, clearing member 5-year CDS spreads, and aggregate volatility as measured by VIX.

CDS spreads serve as a measure of funding cost for a clearing member, because higher spreads (and, by no-arbitrage relations, bond yields) make it more costly for a member to borrow funds, and thus disincentivize the member from executing collateralized trades that tie up funds as collateral. The relation between CDS spreads and margins, however, is not straightforward because large CDS spreads also imply high default risk, which may contribute to higher margins.

To disentangle the two effects of funding costs and default risk, we study separately the effects of the cross-sectional average clearing member CDS spread, $ACDS_t$, and the deviation of the individual member CDS spread from the average, CDS_{it}. The intuition behind this decomposition is the following. An increase in average credit risk is naturally expected to induce the clearing house to increase collateral requirements from all clearing members, as the clearinghouse is more exposed to aggregate default risk. The deviation of the CDS spread of an individual from the average could either raise the amount of collateral posted by that member or decrease it. It would raise it if the clearinghouse, worried that the increase in default risk of that member is not captured by its portfolio characteristics, were to decide to increase the requirement just for that member, even though that would violate the rule that collateral requirements should be implemented uniformly across accounts. It would decrease it if an increase in funding costs were to induce a decrease in demand for collateralized trades. Which force prevails in practice is an empirical question. As we discuss below, our results suggest that the funding cost effects dominate once average default risk is controlled for.

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the largest losses occurred, the difference between the initial margins computed by the two approaches is negligible. We also explore robustness with respect to this choice in Section 4.4.

As an alternative to the average credit spread $ACDS_t$, we also consider the LIBOR-OIS spread to control for market stress. The LIBOR-OIS spread

$$LOIS_t := LIBOR_t - OIS_t.$$

is typically viewed as a measure of financial sector stress, capturing mainly the interest rate differential between uncollateralized and collateralized loans.

All market variables are recorded in basis points (bps) to conform with market convention (the only exception is VIX, which is typically reported in percentage points).

Summary statistics

Table 7 displays summary statistics of our key portfolio variables and initial margins, in millions of USD. Note that each portfolio variable (like SD, VaR, etc) is computed separately for each time t and each member n; in the table, in addition to the pooled mean and standard deviations (which we also refer to as dispersions) across all n and t, we also describe other measures of portfolio dispersion in the time series and in the cross-section.

We observe that all measures of dispersion increase in the order of extreme tail risk captured. That is, as more weight is put into the tail of the distribution, there is more variability in the computed measures both across time and across accounts. The measure with the smallest value is the standard deviation (SD), followed in order by Value-at-Risk (VaR), expected shortfall (ES), and maximum shortfall (MS).

Consistent with the results of the previous section, Table 7 shows that VaR is much lower than initial margins on average. Interestingly, this is true also for SD, ES, and MS, suggesting that these variables, used individually, would not be able to explain the observed level of margins. The table also shows that the DSV model matches well not only the level of margins, but also all the dispersion measures, except for the dispersion in time averages which is better approximated by the modified DSV model ($MDSV$).

Table A.2 in the Appendix reports summary statistics of our key market variables and initial margins, in basis points and millions of USD.

4.2 Margins and Portfolio-specific Risks

In this section, we perform a panel analysis relating observed margins to portfolio variables. In particular, we estimate the following panel regression model with time and account fixed effects:

$$IM_t^n = \alpha^n + \eta_t + \sum_{v \in PV} \beta_v v_t^n + u_t^n, \tag{4}$$

where PV is the set of portfolio variables included in the panel regression. We include fixed effects to capture other determinants of initial margins that we do not control for explicitly, but we also run our analysis without fixed effects. Notice that in the model specification of Eq. (4), the regression coefficients do not depend on the specific clearing member n, a necessary condition if margining rules are implemented uniformly across accounts.

We start examining the set of portfolio variables to include in the regression. First, we note that aggregate net notional (AN) serves primarily as a measure of portfolio size. As portfolio size is already accounted for by risk measures such as VaR, MS and AS, all expressed in dollar units, we drop this variable from our regression.[19] Second, we perform a check for multicollinearity, reported in Appendix Table A.3. The table shows that VaR explains more than 96% of the variation of both expected shortfall and standard deviation. This strongly points to multicollinearity issues, and thus we leave out standard deviation and expected shortfall in our panel model specification.

Our final set of portfolio variables includes Value-at-Risk, maximum shortfall, aggregate short notional, and margins produced by the DSV model given in Eqs. (2) and (3). That is, we estimate Eq. (4) using $PV = \{VaR, MS, AS, DSV, MDSV\}$, considering both the case with and without (time and account) fixed effects, and report the results in Table 8.[20] We use double-clustered standard errors (by time and account) as in Petersen (2009), thus accounting for potential correlation in the residuals, both within each account over time, and across accounts within each day. The signs of all the coefficients are in line with intuition: because larger values for each of the explanatory variables point to a riskier portfolio, all coefficients are expected to be positive.

Table 8

Columns (1) and (2) of Table 8 show that Value-at-Risk alone can explain 56% of the variation in initial margins, and 83% of the variation if fixed effects are added to the regression. The estimated slope coefficient, however, is much higher than unity in either case. In particular, a multiplier of at least 250% is needed for the regression fit, again showing that collateral requirements are set much more conservatively than what would be implied by the conventional 5-day 99% VaR rule. Columns (3) and (4) introduce maximum shortfall (MS) and aggregate short notional (AS) as explanatory variables in conjunction with Value-at-Risk. Compared to columns (1) and (2), we see that introducing these measures enhances explanatory power by 12%. Moreover, the magnitude of the VaR slope coefficients are much closer to unity (and statistically insignificant) once these variables are included. Our results therefore show that initial margins depend on risk characteristics which cannot be captured only by VaR.

We drop Value-at-Risk as an explanatory variable in columns (5) and (6), and find that there is little loss in explanatory power compared to columns (3) and (4). In particular, maximum shortfall is positively correlated with Value-at-Risk, and dropping Value-at-Risk increases the statistical significance of the maximum shortfall loading. Interestingly, the aggregate short notional coefficient estimate remains very stable and highly significant (in the 1–2% range) for all the models estimated.

Columns (7)–(12) investigate the usefulness of the Duffie et al. (2015) initial margin model

[19] We have conducted a regression analysis including AN as an explanatory variable, and found that, qualitatively, our results are largely unaffected.

[20] The margins produced by the DSV model are included only when DSV and AS are not.

(DSV) in explaining empirically observed margins, computed as

$$DSV = MS + 0.02 \times AS.$$

Columns (7) and (8) show that the DSV model captures a significant portion of variation in initial margins. The DSV model also outperforms Value-at-Risk in terms of explanatory power (columns (1) and (2)). The DSV model seems to overestimate the level of initial margins by 27–60%, most likely due to the higher loading on maximum shortfall compared to the optimal mix (estimated in columns (5) and (6)). The significance of the DSV slope coefficient persists when we introduce Value-at-Risk, and the explanatory power remains roughly the same, showing that Value-at-Risk has little explanatory power beyond that already captured by DSV. The overall explanatory power improves when we consider a modified version of DSV (columns (11) and (12))

$$MDSV = 0.5 \times MS + 0.02 \times AS,$$

whose coefficients are based on the estimates reported in columns (5) and (6) (so we should expect a similar explanatory power). Again, Value-at-Risk still has little explanatory power beyond that already captured by our modified DSV model.

Our empirical results provides support to the margins model of Duffie et al. (2015), where they again assume short charge parameters in the 1–2% range. As the main objective of Duffie et al. (2015) was to assess relative changes in margins, it was arguably more important for the DSV model to capture initial margin variation than the precise level of margins. However, we find that their model specification matches empirically observed CDS margins quite well. Compared to the optimal mix of maximum shortfall and aggregate short notional (columns (5) and (7)), we see that the 2% short charge parameter of DSV is remarkably accurate, and that the loss in explanatory power when maximum shortfall is overweighted is small (about 4%). DSV outperforms the simple VaR rule alone by 7% in terms of explanatory power (columns (1) and (7)).

The magnitude of the effects of portfolio variables differs greatly. For example, combining the information in Table 7 and Table 8 (column (3)), we see than an increase of one standard deviation for Value-at-Risk corresponds to an increase in 0.23 standard deviations in initial margins. All else equal, the corresponding increases from maximum shortfall and aggregate short notional are 0.16 and 0.52, respectively.[21]

When we consider the DSV model augmented by VaR (column (9)), a one standard deviation increase in the DSV variable corresponds to a 0.72 standard deviation increase in initial margins. Hence, DSV better captures the variation in margins compared to Value-at-Risk, whose variation associates with only a 0.08 standard deviation increase in initial margins. Comparing the baseline DSV model and the modified DSV model (columns (9) and (11)), we see that the modified DSV slightly outperforms the baseline version. Overall, while DSV performs well in capturing most of the dispersion measures in Table 7, the modified DSV captures better the dispersion in accounts

[21]This model is computed using Column 3 in Table 8, i.e. it is the full model without fixed effects.

average margin. Our results thus imply that this dispersion has significant time variation, and by capturing this variation well the modified DSV explains margins better than the baseline DSV.

While our proposed measures capture significant variations in initial margins, the explanatory power is far from perfect. This implies that there are other contributing factors to clearinghouse decisions, which are not captured by standard portfolio risk measures.[22]

4.3 Funding Cost, Collateral Rates and other Market Variables

In this section we incorporate market variables into our panel analysis and assess their ability to explain margin requirements. The theoretical model proposed by Capponi and Cheng (2017) shows that, regardless of the risk characteristics of the cleared portfolio, the clearinghouse has the incentive to increase margins when (i) the average default risk is high, to protect itself from larger expected default losses, and (ii) funding costs are low, as the deterrent effect of margin costs is weak. The included market variables are chosen to capture variations in margins that are due to changes in default risk and funding cost.

We consider the following panel regression model:

$$IM_t^n = \alpha^n + \eta_t \sum_{v \in PV} \beta_v v_t^n + \sum_{v \in MV} \beta_v v_t^n + u_t^n, \tag{5}$$

where $PV = \{VaR, MS, AS, DSV, MDSV\}$ and $MV = \{LOIS, CDS, ACDS, VIX\}$ are, respectively, the portfolio and market variables included in the panel regression. Because market variables are often not account specific (e.g. LIBOR-OIS spreads), time fixed effects cannot be included in the regression. Thus, in this section we only consider time fixed effects when non-account-specific variables are excluded.

Table 9

We estimate the model in Eq. (5) using least squares regressions, choosing initial margins as the dependent variable and portfolio and market variables as explanatory variables. The results with double-clustered (by account and time) standard errors are reported in Table 9.

First, we observe that the inclusion of market variables does not change our previous results, obtained by including only portfolio variables. The modified DSV model (columns (1)–(4)) and aggregate short notional (columns (5)–(6)) appear to be strong drivers of initial margins, and Value-at-Risk shows little statistical significance when these two measures are controlled for.

Columns (1) and (2) report the results when the CDS spreads, average CDS spreads, and the VIX index are included, and Value-at-Risk and modified DSV margins are controlled for. There is a non-negligible increase in explanatory power compared to models including only portfolio variables (Table 8), showing again the usefulness of market variables in explaining initial margins. Comparing

[22] We have performed the same exercise for different VaR confidence levels. We find that additional multicollinearity problems are introduced as the confidence level approaches one, because VaR then converges to maximum expected shortfall. Our results, however, remain broadly the same.

Tables 8 and 9, however, we notice that a significant fraction of explanatory power is attributed to portfolio variables alone. Thus, while market variables do seem to influence margin levels, their effects seem to be smaller than that of portfolio variables. Interestingly, although statistically insignificant, our measure of funding cost (individual CDS spreads) is negatively related to initial margins, once the average CDS spread is controlled for.

Columns (3) and (4) report our results when we replace the average CDS spread with the LIBOR-OIS spread. There is almost no change in explanatory power and the loadings on the portfolio variables. The change in loadings for CDS spreads and the VIX is small, and the loadings on CDS spreads remain negative but insignificant. Columns (5) and (6) report the results when maximum shortfall and aggregate short notional are used to replace the modified DSV margins. The increase in explanatory power is small when compared to the results in columns (1) and (2). However, the comparison demonstrates the usefulness of the individual components of the DSV model in explaining observed margins. The estimated coefficient for aggregate short notional is significant and in the range of 1-2%, again showing the robustness of our previous results in Table 8. Maximum shortfall also plays a significant role when account fixed effects are not controlled for.

We find that measures of market stress or default risk, which include the average CDS spread, the VIX, and the LOIS spread, all play a significant role in explaining margins (columns (1), (2), (5), and (6)). As increases in each of these variables indicate a high probability of default for the members, and especially joint default, the clearinghouse asks for more margins even after controlling for portfolio risk.

The magnitude of these effects is substantial. In our estimates, a one-point increase in the VIX increases required margins by \$4.6 million; a 1-basis-point increase in the average CDS spread increases margins by \$4.5 million, and a 1-basis-point increase in the LIBOR-OIS spread by \$12 million. Given that during crisis episodes movements of the VIX of 50 points or changes in LIBOR and CDS spreads of hundreds of basis points are possible, these estimates imply very large potential effects on prices and systemic stability through the collateral channel.[23]

The effect of funding costs (individual CDS spreads) is smaller and insignificant, but nonetheless is consistently found to be negative in all our regressions, in line with the predictions of Capponi and Cheng (2017). The low statistical significance is likely due to the fact that variation in individual clearing members CDS spreads (orthogonal to the average CDS spread) in our sample period is small.

Because of the granularity of our data set, we are able to separate the two counteracting effects of increased CDS spreads, namely, the increased default risk and increased funding costs. These results have important implications for funding liquidity: margin spirals (Brunnermeier and Pedersen (2009)) may be dampened or reinforced by collateral rules. We find direct evidence for the reinforcement channel through the average CDS spread, which has a positive and significant coefficient (columns (1), (2), (5), and (6) in Table 9). Our results show that if a market shock

[23] Of course, these estimates are obtained in a relatively calm period, so it is hard to extrapolate the estimates to times of crisis; however, they give a sense of the magnitude of these effects.

increases the aggregate default risk in the market ($ACDS$ increases), the required margins for members increase, holding everything else constant. Brunnermeier and Pedersen (2009) argue that this can potentially lead to destabilizing margins spirals if the margin-setting financier, the clearinghouse in our setting, is unsure whether these market shocks are due to fundamental news or to liquidity shocks. In the language of Brunnermeier and Pedersen (2009), if the clearinghouse behaves like an uninformed financier, possibly over-estimating fundamental volatility through the average CDS spread and over-tightening margin constraints, then it can set off margin spirals. However, different from them, our results imply that such a market shock needs to affect all members' spreads (i.e. a general increase in credit risk) and not only an individual member's CDS spread, since we control for individual member CDS spreads. This is consistent with the fact that the clearinghouse is most worried about the joint default risk of several members as opposed to the individual default risk of a single member.

4.4 Robustness

We discuss several robustness checks, and report additional details in Appendix A.5. The historical simulation method presented in Section 3.3.2 uses the so-called DV01 approximation as in Duffie et al. (2015). This allows us to compute counterfactual portfolio returns even for time periods not covered by our main sample period 2014-2016. In the Appendix, we compare the approximation method with the actual portfolio realizations for the two years of our sample, where we observe both. We find a relatively high (though not perfect) correlation between the two – just above 50%, indicating that the counterfactual losses do serve as a reasonable approximation of the realized losses.

In addition, the DV01 approximation used in Section 3.3.2 depends on the calibration of the average duration of the portfolio d. Duffie et al. (2015) choose $d = 3$ years; we show in the Appendix that all our results continue to hold under the more conservative assumption of $d = 5$ years.

Finally, two of the portfolio variables (Value-at-Risk and maximum shortfall) used in our analysis were based on P&L generated from our entire sample of credit spreads. Because our dataset covered the financial crisis, the risk measures captured extreme movements and may thus be viewed as overly conservative for estimating portfolio losses. In the Appendix, we consider using only the last 1000 days (approximately 4 years) of credit spreads data to generate P&L, as in Duffie et al. (2015). Our analysis in the appendix shows that the results remain qualitatively similar to those reported in Tables 8 and 9.

5 Discussion and Concluding Remarks

We study the empirical determinants of collateral requirements in a large market in which counterparty risk plays an important role – the cleared CDS market. Our analysis exploits the availability of a unique dataset on clearing members' portfolio exposures and associated margin levels. Margins in this market are set at the portfolio level rather than at the individual security level; this allows us to study how risk measures like VaR and other portfolio characteristics affect margins. We also study how market variables – in addition to portfolio variables – affect collateral requirements, highlighting the implications of our findings for models of the collateral feedback channel.

A large theoretical literature has explored how collateral rules can amplify fundamental shocks, by requiring margin payments – or forcing deleveraging – after bad shocks occur. Papers in this literature typically assume that margins are set according to a VaR rule. In the first part of our paper, we directly test this hypothesis and show that it is violated in several ways. First, it appears that collateral is set much more conservatively than what would be implied by a standard (99%) VaR rule – a rule that clearinghouses themselves state they use. Therefore, VaR cannot explain the average levels of collateral. Note that we calibrate our VaR taking into account the entire distribution of historical CDS returns since 2004 – therefore including the financial crisis in our sample. Yet, margins are set much more conservatively than what VaR predicts. Second, if collateral requirements were determined according to VaR, they should be implemented equally across counterparties: we show that this is not the case empirically, as there is large heterogeneity in the amounts of collateral posted across members, in excess of what VaR would predict.

We find that other market and portfolio variables dominate VaR in explaining the time-series and cross-sectional variation in margins. In particular, maximum shortfall (the largest potential portfolio loss) and aggregate short notional (the notional amount held by a member in short CDS positions, those most exposed to jump-to-default risk) dominate VaR in explaining the panel variation of required collateral. These two variables, previously employed in a reduced-form model of collateral proposed by Duffie et al. (2015), account alone for almost 70% of the entire variation in margins. They are able to match the average observed levels of collateral, considering that maximum shortfall is a much more extreme loss than even the 1st percentile on which VaR is based. These variables also capture sudden changes in risk exposures, because the aggregate short notional does not depend on a conditional distribution which needs to be estimated from historical data, but is instead directly observed. Interestingly, both variables (but especially the short notional) are independent or at least less sensitive to the exact specification of the loss distribution, compared to other risk-based measures. Hence, they represent a more robust choice for a collateral requirement in the face of model uncertainty for the distribution of losses.

Our analysis finds that shocks to market variables such as aggregate default risk (captured by the average CDS spreads of members) and aggregate market risk (captured by the VIX) increase the total amount of required collateral, even after controlling for portfolio-level risks. In contrast, an increase in the individual member's funding costs induces a reduction of the posted collateral, consistent with the predictions of Capponi and Cheng (2017).

Our findings have several implications for theoretical models of the collateral feedback channel. First, the fact that maximum shortfall and short charge explain margins better than VaR indicates that the clearinghouse is worried about more extreme losses than what the VaR captures. This induces a nonlinearity in the collateral rule: collateral levels will respond little to small changes in risks (like an increase in the variance of the portfolio), but will spike if the probability of an extreme event increases or the worst-case-scenario worsens. This nonlinearity can potentially play an important role in general equilibrium models, amplifying the largest shocks but dampening moderate-sized shocks.

Second, our results are consistent with some (but not all) of the theoretical literature on general collateral equilibria (Geanakoplos (1997), Geanakoplos (2010), Fostel and Geanakoplos (2014), Fostel and Geanakoplos (2015)). These works have shown that VaR rules *do not* arise in equilibrium: instead, the amount of collateral that emerges endogenously should cover the losses in the worst possible state, precisely like a maximum shortfall rule. Our findings therefore provide direct empirical evidence in favor of the main implications of those theoretical models of endogenous collateral.[24]

Third, collateral requirements are directly affected by market conditions: increases in aggregate risks directly induce an increase in collateral requirements, holding the portfolios fixed. Our empirical analysis therefore documents the existence of two channels for the amplification of fundamental shocks (studied, for example, in Brunnermeier and Pedersen (2009)): at the portfolio level, where an increase in perceived tail risk following a shock may affect the member's margin requirement; and at the macro level, where an increase in aggregate risk can increase the collateral requirements of all members.

Taken together, these empirical findings both validate specific theories of collateral equilibria (like the focus on maximal loss and resulting nonlinearity of the margin function) and provide guidance for building empirically grounded models of the collateral feedback channel.

[24]The analysis in these works was originally developed in a binomial economy, and the most recent papers have explored the conditions under which it extends to richer economies. In this paper, we provide empirical evidence that indeed, in a rich real environment, maximum shortfall appears to be an important driver of collateral rules.

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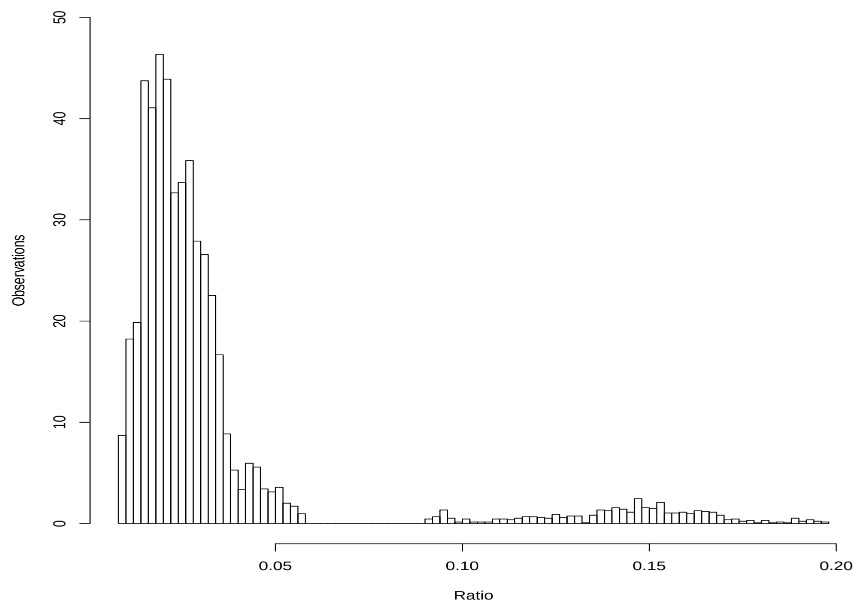
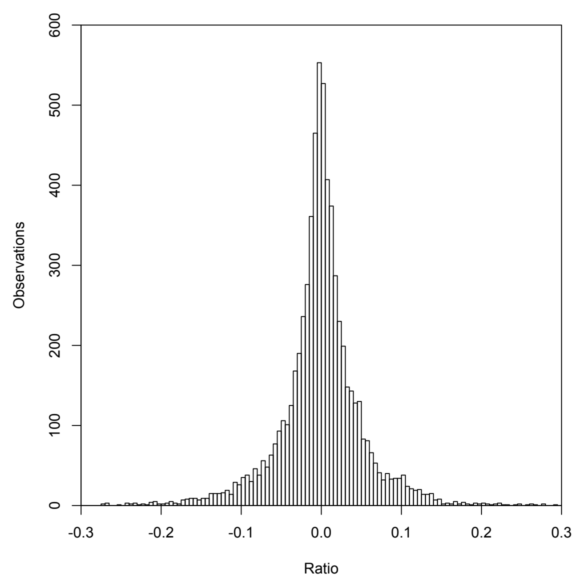
Figure 1: Histogram of margin/notional ratio observations.



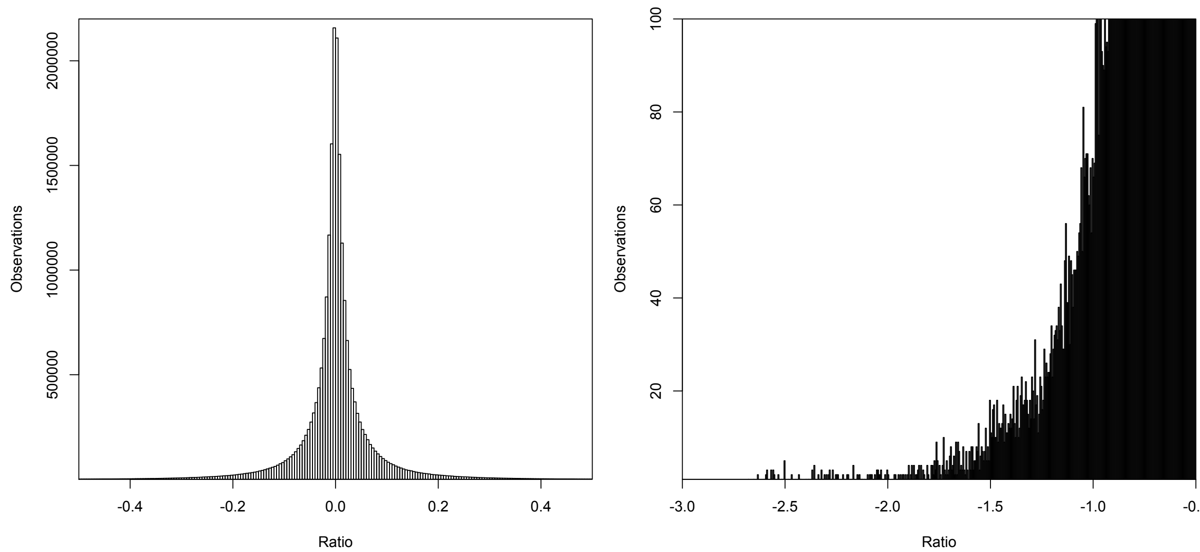
Note: For each active house account/day combination, we compute a margin to notional ratio by dividing recorded initial margins with aggregate net notional. We arrive at the aggregate net notional by computing the net notional for each reference name and then summing the absolute net notionals across names. The figure reports the histogram of margin/notional ratio across all 6,721 account/day observations.

Figure 2: Histogram of realized return on margins for cleared portfolios.



Note: We compute the realized 5-day ahead returns on margins as the 5-day ahead $P\&L$ divided by posted margins. We compute this for each account/day and obtain 6,656 observations. The figure plots the histogram of the return on margins.

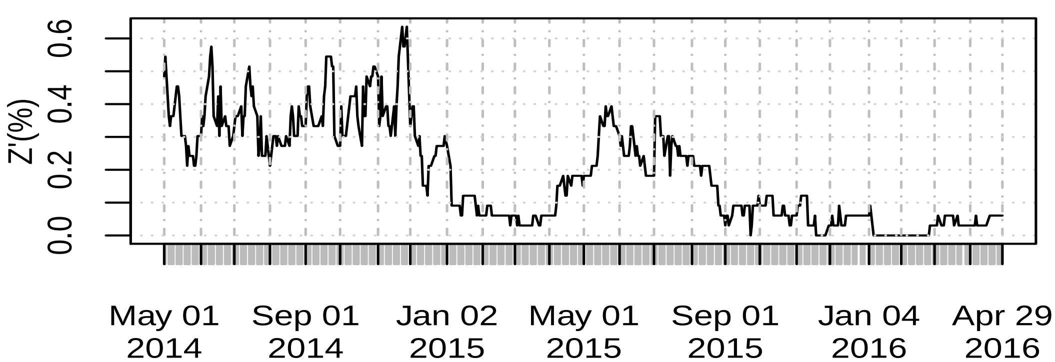
Figure 3: Histogram for historically simulated return on margins (left), with zoom in on left tail (right).



(a) (b)

Note: The figure shows the histogram of simulated returns on margins. Due to the large number of observations clustering around zero, we only display observations between ±50% in Figure 3a, and report the left tail of the histogram in Figure 3b. We use the DV01 formula to approximate the 5-day ahead $P\&L$ with the product of net exposures to a reference name and the change in 5-year credit spreads for rolling 5-day windows from 2004/01/01 and 2016/09/13, adjusted for an average duration of $d = 3$. We compute this for each account/day/historical 5-day window and obtain 22,199,463 observations.

Figure 4: Ratio of exceptions over time.



Note: The figure reports the average ratio of exceptions actual portfolios held in each day, using the historical distribution of returns since 2004. For any fixed day within our sample period, we count for each of the 13 active house accounts the number of counterfactual returns that exceed the amount of initial margins required for their portfolio, using historical CDS spreads to simulate the counterfactual return of that portfolio. The number of exceptions are then averaged over the $13 \times 3303 = 42,939$ observations for that day.

Table 1: Descriptive statistics for different account categories.

	Active House	Customer	Auxiliary House
Number of accounts	13	13	12(18)†
Number of contracts	4,042.2	73.4	99.1
Number of names	239.3	32.6	23.8
Gross notional (billions $)	168.6	45.5	8.3
Initial margins (millions $)	657.9	614.1	57.5

Note: The table reports the pooled averages of key variables within our data set depending on account type over our sample period. The number of contracts/names for each account is the number of contracts/names in which the account has a non-zero position. Gross notional is computed by summing the absolute notional exposure for all contracts for each account. Margins are computed by summing together the USD requirement with the Euro requirement after accounting for the historical exchange rates.

†Six auxiliary house had zero margins throughout, indicating no trading activity at all. We excluded these accounts when calculating auxiliary house account descriptive statistics.

Table 2: Time series summary statistics of initial margins over time.

Initial margins (billions $)	Aggregate	Active House	Customer
Time average	17.1	8.6	7.9
Median	16.4	8.7	7.6
Range	[14.8, 22.4]	[7.1, 10.4]	[6.2, 11.1]
Standard deviation	1.7	0.6	1.2
Posted Collateral (billions $)			
Time average	17.6	8.7	8.1
Median	17.1	8.9	7.7
Range	[9.4, 23.0]	[4.8, 10.4]	[4.0, 11.5]
Standard deviation	1.7	0.7	1.3

Note: The table reports the aggregate initial margin requirement and posted collateral in USD for each point in time in our sample for all accounts, all customer accounts, and all active house accounts.

Table 3: Descriptive statistics for (realized) return on margins and test results for the 5-day 99% VaR rule.

5-day-ahead return on margins (r_m)	
Observations	6,656
Mean(%)	-0.06
S.D.(%)	5.22
Excess kurtosis	4.19
Range(%)	[-27.5, 30.04]
Interquartile Range (Q3-Q1)(%)	[-2.06, 2.08]
Empirical $VaR^{5,0.05}$(%)	-8.86
Empirical $VaR^{5,0.01}$(%)	-16.06
Empirical $VaR^{5,0.001}$(%)	-24.37
$\|r_m\| \leq 0.15$ (%)	97.99
Test results for $H_0 : \mathbb{P}(r_m < -100\%) = 1\%$	
Initial Margin Exceptions	0
Ratio (Z')	0
S.E.†	3.7×10^{-3}
t-stat	-2.73

Note: We report descriptive statistics for the realized 5-day ahead returns on margins, defined as the 5-day ahead realized $P\&L$ divided by margin requirements. We observe no exceptions within our sample period. We compute standard errors using binomial probabilities, and further assume that exception events are perfectly correlated when underlying losses overlap for robustness against autocorrelation. We also assume exception events are uncorrelated across accounts.

Table 4: Descriptive statistics for historically simulated return on margins and test of the 5-day 99% VaR rule

5-day-ahead return on margins (r_m)	$d = 3$
Observations	22,199,463
Mean(%)	0.01
S.D.(%)	8.64
Kurtosis	31.26
Range(%)	[-268.00, 162.40]
Interquartile Range (Q3-Q1)(%)	[-1.88, 1.90]
Empirical $VaR^{5,0.05}$(%)	-11.08
Empirical $VaR^{5,0.01}$(%)	-26.96
Empirical $VaR^{5,0.001}$(%)	-60.89
$\lvert r_m\rvert \leq 0.26$ (%)	97.80
Test results for $H_0 : \mathbb{P}(r_m < -100\%) = 1\%$	
Initial Margin Exceptions	3,456
Ratio (Z')	1.56×10^{-4}
S.E.†	8.90×10^{-5}
t-stat	-110.63

Note: We report descriptive statistics for the historically simulated 5-day ahead returns on margins. We use the DV01 formula to approximate the 5-day ahead $P\&L$ with the product of net exposures to a reference name and the change in 5-year credit spreads for rolling 5-day windows from 2004/01/01 and 2016/09/13, adjusted for an average duration of $d = 3$. The estimated return on margins are then defined as the simulated 5-day ahead $P\&L$ divided by margin requirements. To test the VaR margining rule, we compare counterfactual returns to initial margins for each observation. We compute two-way clustered standard errors, clustering both at the account and time level.

Table 5: Test results for M-day VaR rules.

p-values	*Effective Duration (Years):*	
M : MPoR in days	3	5
1	$< 10^{-4}$	$< 10^{-4}$
3	$< 10^{-4}$	$< 10^{-4}$
5	$< 10^{-4}$	$< 10^{-4}$
7	$< 10^{-4}$	$< 10^{-4}$
10	$< 10^{-4}$	$< 10^{-4}$

$df = 12$

Note: The table reports the results of the test for whether a VaR margining rule of any confidence level can explain observed margin levels for various combinations of CDS duration (d) and margin period of risk (M) assumptions. The null hypothesis is that if a VaR rule is implemented, it is fairly implemented so that the frequencies of exception are independent of clearing member identities. We report the p-values for various combinations of CDS duration (d) and margin period of risk (M).

Table 6: Portfolio and market variables.

Notation	Units	Definition
ψ	millions \$	Empirical 5-day distribution of profit and losses for a portfolio
IM	millions \$	Observed initial margins posted for a portfolio
Y	millions \$	Net notional aggregated over reference names for a portfolio
Portfolio Variables		
SD	millions \$	Sample standard deviation of ψ
VaR	millions \$	1 percent quantile of ψ
ES	millions \$	Average of profit and losses less than or equal to VaR
MS	millions \$	Minimum of ψ
AN	millions \$	Aggregate net notional (by reference entity) of portfolio
AS	millions \$	Aggregate short notional (by reference entity) of portfolio
DSV	millions \$	Initial margin estimate used by Duffie et al. (2015), equal to $MS + 0.02 \times AS$
$MDSV$	millions \$	Adjusted initial margin from DSV, equal to $0.5 \times MS + 0.02 \times AS$
Market Variables		
OIS	bps	End of day 3-month Overnight Index Swap spreads
$LOIS$	bps	End of day 3 month USD LIBOR-OIS spreads
CDS	bps	End of day market quote for clearing member specific 5-year CDS spread
$ACDS$	bps	Average end of day clearing member 5-year CDS spread
VIX	bps	End of day CBOE Volatility Index

Note: This table displays the key variables and notation we use in our regression analyses. Portfolio variables are those that are specific to the portfolio that an account holds with the clearinghouse, and are conventionally used to measure the risk of positions. Market variables are those that are determined by market forces. Portfolio variables estimated from the empirical distribution via the historical simulation method outlined in Section 3.3.2 include Value-at-Risk, expected shortfall, maximum shortfall, and standard deviation. Portfolio variables estimated directly from positions include aggregate net notional and aggregate short notional. We record portfolio variables in millions USD. Market variables include the Overnight Index Swap (OIS) spread, the LIBOR-OIS spread, clearing member CDS spreads, the average clearing member CDS spread, and the aggregate volatility as measured by VIX. We record market variables in basis points.

Table 7: Initial margins and portfolio variables summary statistics

	Portfolio variables, in millions \$				
Summary Statistic	In. Margins ($IM_{n,t}$)	Portfolio SD ($SD_{n,t}$)	VaR ($VaR_{n,t}$)	Exp. Shortfall ($ES_{n,t}$)	Max Shortfall ($MS_{n,t}$)
Pooled mean (over all n and t): $\mu(x_{n,t})$	654.5	43.1	128.6	183.2	363.3
Std. deviation (over all n and t): $\sigma(x_{n,t})$	367.0	28.4	83.6	114.0	243.7
Time-series variation of cross-sectional averages: $\sigma(\bar{x}_t)$	48.7	4.3	11.2	18.9	57.7
Mean cross-sectional dispersion: $\mu(\sigma_t(x_{n,t}))$	370.7	28.2	83.5	113.5	236.0
Cross-sectional dispersion of time-series averages: $\sigma(\bar{x}_n)$	325.1	24.1	71.6	95.8	187.9
Mean time-series dispersion: $\mu(\sigma_n(x_{n,t}))$	162.4	13.4	39.5	55.7	134.5

	Portfolio variables, in millions \$			
Summary Statistic	Aggr. Notional ($AN_{n,t}$)	Aggr. Short Notional ($AS_{n,t}$)	Duffie et al. ($DSV_{n,t}$)	Modified DSV ($MDSV_{n,t}$)
Pooled mean (over all n and t): $\mu(x_{n,t})$	25,906.7	12,392.5	611.1	429.5
Std. deviation (over all n and t): $\sigma(x_{n,t})$	14,215.1	9,092.9	370.4	265.1
Time-series variation of cross-sectional averages: $\sigma(\bar{x}_t)$	1,684.2	1,041.2	76.1	47.7
Mean cross-sectional dispersion: $\mu(\sigma_t(x_{n,t}))$	14,656.9	9,186.6	364.7	263.4
Cross-sectional dispersion of time-series averages: $\sigma(\bar{x}_n)$	13,690.0	8,262.7	315.0	233.4
Mean time-series dispersion: $\mu(\sigma_n(x_{n,t}))$	4,683.9	3,735.7	170.7	110.8

Note: Table 7 displays summary statistics of our key portfolio variables and initial margins, in millions of USD. Definitions of portfolio variables are reported in Table 6. In addition to the pooled mean and standard deviations (dispersions), we report panel statistics that describe properties of panel variables both across accounts and time. In particular, for panel data $x_{n,t}$, we define

$$\bar{x}_t := \frac{1}{N}\sum_{n=1}^{N} x_{n,t}, \quad \bar{x}_n := \frac{1}{T}\sum_{t=1}^{T} x_{n,t}, \quad \sigma_t^2(x) := \frac{1}{N-1}\sum_{n=1}^{N}(x_{n,t}-\bar{x}_t)^2, \quad \sigma_n^2(x) := \frac{1}{T-1}\sum_{t=1}^{T}(x_{n,t}-\bar{x}_n)^2.$$

Above, we refer to $\sigma(\bar{x}_t)$ as the *time-series variation of cross-sectional averages*, $\bar{\sigma}_t(x_{n,t})$ as the *mean cross-sectional dispersion*, $\sigma(\bar{x}_n)$ as the *cross-sectional dispersion of time-series averages*, and $\bar{\sigma}_n(x_{n,t})$ as the *mean time-series dispersion*.

Table 8: Regression results for explaining initial margins with portfolio variables

	Dependent variable: Initial margins (IM)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Value-at-Risk (VaR)	3.292***	2.537***	1.002	1.361					0.363	0.756	0.370	0.709
	(0.593)	(0.588)	(0.986)	(0.932)					(1.158)	(1.056)	(1.000)	(1.154)
Maximum shortfall (MS)			0.254	0.125	0.493***	0.443**						
			(0.191)	(0.240)	(0.167)	(0.213)						
Aggregate short notional (AS)			0.021***	0.017**	0.024***	0.021***						
			(0.006)	(0.008)	(0.004)	(0.007)						
Duffie et al. model (DSV)							0.787***	0.622***	0.711***	0.468***		
							(0.132)	(0.109)	(0.259)	(0.170)		
Modified DSV model ($MDSV$)											1.029***	0.769**
											(0.296)	(0.327)
Observations	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721
Adjusted R^2	0.562	0.830	0.680	0.862	0.673	0.855	0.631	0.844	0.632	0.846	0.672	0.857
Account FE	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*
Time FE	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*

Note: *p<0.1; **p<0.05; ***p<0.01

Note: We perform least squares regressions using initial margins as the dependent variable and portfolio variables as explanatory variables. Two-way clustered (by time and account) standard errors are reported in parentheses and used for the significance tests. We consider both the case with and without (time and account) fixed effects.

Table 9: Regression results for explaining initial margins with portfolio and market variables

	Dependent variable: Initial margins (IM)					
	(1)	(2)	(3)	(4)	(5)	(6)
Value-at-Risk (VaR)	0.340	0.781	0.336	0.836	0.970	1.444
	(0.911)	(1.262)	(0.914)	(1.262)	(0.831)	(1.007)
Modified DSV Model ($MDSV$)	1.075^{***}	0.641^{*}	1.063^{***}	0.655^{*}		
	(0.269)	(0.334)	(0.270)	(0.335)		
Maximum Shortfall (MS)					0.279^{*}	0.069
					(0.154)	(0.232)
Aggregate Short Notional (AS)					0.022^{***}	0.015^{*}
					(0.005)	(0.008)
CBOE volatility index (VIX)	0.046^{**}	0.046^{***}	0.068^{***}	0.063^{***}	0.044^{**}	0.044^{**}
	(0.019)	(0.017)	(0.017)	(0.020)	(0.019)	(0.018)
Member CDS spread (CDS)	−1.643	−1.660	−0.770	−0.480	−2.042	−1.284
	(1.566)	(1.246)	(1.166)	(0.717)	(1.659)	(1.228)
Average CDS spread ($ACDS$)	4.591^{***}	4.024^{**}			4.764^{***}	3.520^{**}
	(1.489)	(1.827)			(1.449)	(1.669)
LIBOR-OIS spread ($LOIS$)			12.011^{***}	9.758^{**}		
			(3.301)	(3.930)		
Observations	6,721	6,721	6,721	6,721	6,721	6,721
Adjusted R^2	0.700	0.856	0.700	0.854	0.707	0.860
Account FE	*N*	*Y*	*N*	*Y*	*N*	*Y*
Time FE	*N*	*N*	*N*	*N*	*N*	*N*
Note:				*p<0.1; **p<0.05; ***p<0.01		

Note: We perform least squares regressions using initial margins as the dependent variable and portfolio and market variables as explanatory variables. Two-way clustered (by time and account) standard errors in parentheses are reported and used for the significance tests. We consider both the case of with and without fixed effects. Because market variables are often dependent only on time, we consider only account fixed effects when such variables are introduced.

A Appendix

A.1 Procedure to compute counterfactual Returns

Following Duffie et al. (2015), we group together all I contracts written on the K underlying reference entities, and denote the net position in that reference entity by Y_k. Precisely, let Ω_k denote the collection of contracts referencing name k, then

$$Y_{t,k}^n := \sum_{i \in \Omega_k} X_{t,i}^n.$$

For each reference entity, therefore, Y_k indicates the net exposure to reference entity k, aggregating together the CDS contracts on that reference entity across maturities, seniority level, and doc clause. We then collect historical on-the-run 5-year credit spread series for each reference entity, $\mathbf{S}_t \in \mathbb{R}^K$, and estimate historical losses via the DV01 formula:

$$\Psi_{5,u}(\mathbf{X}_t^n) \approx d \times \mathbf{Y}_t^n \cdot (\mathbf{S}_u - \mathbf{S}_{u+5}),$$

where d is the effective duration of the positions. We use $d = 3$ as in Duffie et al. (2015), meaning that the average duration of CDS positions is 3 years (corresponding to the median maturity of the CDS market). Section 4.4 performs a robustness check with respect to the choice of d.

A.2 Time-series test of the VaR rule using realized returns

We consider the Z statistic:

$$Z := \frac{1}{NT} \sum_{t=1}^{T} \sum_{n=1}^{N} \mathbb{I}\{\Psi_{M,t}(\mathbf{X}_t^n) < -IM_t(\mathbf{X}_t^n)\},$$

where $\mathbb{I}\{\cdot\}$ is the indicator function. The indicator takes value 1 when realized $M-$day losses exceed the initial margin requirement; this is typically referred to as an *exception* (or *exceedance*). The statistic Z is the empirical frequency at which exceptions occur, averaged over time and across market participants. We have, for quite general correlation structures:

$$Z \xrightarrow{\mathbb{P}} \alpha,$$

by the law of large numbers. For M and α specified in the null hypothesis, we can test H_0 using Z as the test statistic.

We compute standard errors for the test using binomial probabilities. While we would ideally compute cluster-robust standard errors for our test, having observed no exceptions means residuals are all zero. To proceed, we assume that exceptions are perfectly correlated when underlying losses overlap (for robustness against autocorrelation), and also assume that exceptions are uncorrelated across accounts.

In particular, standard errors are computed as

$$S.E. = \sqrt{\frac{\alpha(1-\alpha)\zeta_M}{NT}}.$$

In the above equation, the term $\zeta_M := 2M - 1$ adjusts for our assumption that exceptions are perfectly correlated when underlying losses overlap. For $\alpha = 1\%$, $NT = 6,656$ and $M = 5$, we obtain a standard error of 0.37%.

We remark that our standard errors are likely to be overly conservative. As a robustness check, we also compute one-day returns and autocorrelation estimates. For each account, we find autocorrelation estimates on the orders of 10^{-4} for the first five lags. Thus, autocorrelation would likely have a smaller impact on actual standard errors compared to our assumption of perfect correlation.

Finally, to explicitly account for potential cross-sectional correlation in the returns on margins, we perform the test separately account by account, finding that the null hypothesis is rejected in every case.

A.3 Time-series test of the VaR rule using counterfactual returns

We compute our test statistic using an extended version of Eq. (A.2):

$$Z' = \frac{1}{NTU} \sum_{t=1}^{T} \sum_{u=1}^{U} \sum_{n=1}^{N} \mathbb{I}\{\Psi_{5,u} MtM(\mathbf{X}_t^n) < -IM_t(\mathbf{X}_t^n)\},$$

where $\Psi_{5,u} MtM(\mathbf{X}_t^n)$ is constructed as in Duffie et al. (2015) (see Appendix A.1 for additional details), and U is the number of evaluation dates. For each portfolio $\mathbf{X}_t^n$, we estimate the frequency at which losses exceed portfolio margins. Under the null hypothesis of a 5-day 99% VaR margining rule, Z' should converge to 1% in probability.

The test can be simply implemented as a regression of observed exceedances onto a constant, with double-clustering as in Petersen (2009) by time and by account (there is no need to use the binomial model as exceedances are observed in the data, so the variance of the residuals is nonzero).

A.4 Cross-sectional test of the VaR rule

The margining rule H_0 implies $\mathbb{P}(\Psi_{M,t}(\mathbf{X}_t^n) < -IM_t(\mathbf{X}_t^n)) = \alpha$ for all n, which further implies

$$H_0' : \mathbb{P}(\Psi_{M,t}(\mathbf{X}_t^n) < -IM_t(\mathbf{X}_t^n)) = \mathbb{P}(\Psi_{M,t}(\mathbf{X}_t^{n'}) < -IM_t(\mathbf{X}_t^{n'})),$$

for all $n \neq n'$. The statistics to consider are then

$$Z_n := \frac{1}{T} \sum_{t=1}^{T} \mathbb{I}\{\Psi_{M,t}(\mathbf{X}_t^n) + IM_t(\mathbf{X}_t^n) < 0\} \xrightarrow{\mathbb{P}} \alpha.$$

We describe here how to implement a test for equality is implemented for H_0'. The most straightforward test for equality of the frequencies of exceptions across accounts is the $G-$test (i.e. the two-way likelihood ratio test). Because the confidence level is expected to be large (the expected frequency of exceptions is low), the typical χ^2-test for homogeneity is not appropriate (Hoey (2012)). As exceptions are expected to occur with low probability, we instead use the $G-$test to test the null hypothesis.

The test statistic is computed as:

$$G := 2 \sum_{n=1}^{N} O_n \log \frac{O_n}{E_n},$$

where O_n is the observed number of exceptions for clearing member n, and E_n is the expected number of exceptions for account n. The probability of observing an exception, needed for calculating E_n, is estimated by pooling observations across accounts. In particular,

$$E_n := TU \times Z = \frac{1}{N} \sum_{t=1}^{T} \sum_{u=1}^{U} \sum_{n'=1}^{N} \mathbb{I}\{\Psi_{5,u} MtM(\mathbf{X}_t^{n'}) < -IM_t(\mathbf{X}_t^{n'})\},$$

and

$$O_n := \sum_{t=1}^{T} \sum_{u=1}^{U} \mathbb{I}\{\Psi_{5,u} MtM(\mathbf{X}_t^{n}) < -IM_t(\mathbf{X}_t^{n})\}.$$

Under the null that frequencies are the same for each account, $G \xrightarrow{d} \chi^2_{N-1}$.

We also derive an extension of this test that explicitly accounts for potential autocorrelation of the exceptions. For a fixed portfolio, we first count the number of exceptions, and then divide it by the number of evaluation dates. This gives an estimate for the probability of an exception occurring for that portfolio. We then sum the exception probabilities for portfolios associated with each fixed account, and use the rounded up integer as the estimate of observed exceptions for that account. We enter this estimate into the contingency table used for the $G-$test. Formally, we estimate the probability of an exception for an account/day combination (n, t) as

$$\hat{p}_{n,t} = \frac{1}{T} \sum_{u=1}^{T} \mathbb{I}\{\Psi_{5,u} MtM(\mathbf{X}_t^{n}) < -IM_t(\mathbf{X}_t^{n})\}.$$

The number of (estimated) observed exceptions is then

$$\hat{O}_n := \left\lceil \sum_{t=1}^{T} \hat{p}_{n,t} \right\rceil.$$

The estimate $\hat{O}_n$ replaces O_n in our computation of the G statistic (Eq. (A.4)).[25] The estimated

[25]The ceiling operation is performed to ensure that the contingency table only contains integer entries. We also

observations are thus more robust to autocorrelation compared to treating each observation as an individual count, which may inflate the sample size.

A.5 Robustness: Details

In this section we provide more details about the robustness tests of Section 4.4.

A.5.1 Counterfactual losses and realized returns

The historical simulation method presented in Section 3.3.2 uses the so-called DV01 approximation as in Duffie et al. (2015). We assess the robustness to this approximation by using the historical simulation method to compute losses for the 2014–2016 sample period, during which we also observe realized losses for the actual portfolios. Comparing the two sets of losses, we can assess how well the simulation method captures realized returns.

Table A.4

Table A.4 indicates that while the counterfactual losses are on average smaller than the realized losses, they are more variable. In fact, compared to either the standard deviation of realized or counterfactual losses, the two average losses are statistically indistinguishable. A higher standard deviation for the counterfactual losses is expected, because these are computed using a first order linear approximation to real price movements, and do not incorporate second order convexity effects that stabilize price movements.

The correlation between the two variables is 50%, indicating that the counterfactual losses do serve as a reasonable approximation of the realized losses, even though they are not identical.

A.5.2 CDS DV01 approximation duration

We assess the robustness of the time series test presented in Section 3.3.2 to varying assumptions on the duration used in the DV01 formula. We assess this by "stressing" our simulated losses: the entire set of counterfactual losses is recomputed with the alternative assumption $d = 5$. This adjustment directly scales up the level of losses and increases the number of exceptions recorded. Of course, this assumption is very conservative, since the most actively traded CDS contracts are five year contracts, so they have a duration slightly lower than five at the contract's inception. Their duration becomes lower as they are held in the portfolio and maturity shortens.

Table A.5

We compute the return on margins with the counterfactual losses computed using $d = 5$. We report descriptive statistics and test results in Table A.5. As the duration assumption merely scales the level of losses, the descriptive statistics display a similar loss distribution to that reported in

performed the test with unrounded data, yielding similar, if not stronger, results.

Table 4. About half of the empirical distribution of returns lies between ±3.2%. Aggregating over observations account/time/historical 5-day window combinations, we see the empirical 99% Value at Risk is approximately 45% of posted margins.

When testing the VaR margining rule, we see that there are more observed exceptions (23,268 versus 3,456 for the $d = 3$ case). This brings the empirical frequency of exceptions closer to 1%. However, the result is still statistically significantly less than 1%. Thus, even with very conservative duration assumptions and including the crisis period of 2007–2009 in the calculations, there is still strong evidence against the 5-day 99% VaR rule, showing robustness to our previous conclusions.

A.5.3 Varying risk measurement windows

Value-at-Risk and maximum shortfall used in the panel analyses (Tables 8 and 9) were based on P&L generated from our entire sample of credit spreads (that is, on the entire historical distribution of returns for each portfolio held at time t by member n). Because our dataset covered the financial crisis, the risk measures captured extreme movements and may thus be viewed as overly conservative for estimating portfolio losses. In this section we consider using only the last 1000 days (approximately 4 years) of credit spreads data to generate P&L, as in Duffie et al. (2015). Using these newly estimated counterfactual P&L, we compute Value-at-Risk and maximum shortfall. We replicate our panel analyses and report the results in Tables A.6 and A.7.

Table A.6

Table A.7

There is a general increase in standard errors when we use only recent data to estimate Value-at-Risk and maximum shortfall, and a general decrease in explanatory power. This may indicate that clearinghouse margining rules place significant weight on historical crisis and downturns, and that their estimates of portfolio losses mostly replicate extreme losses such as those observed during the financial crisis.

In particular, comparing Table A.6 to Table 8, we see there is a distinct decrease in explanatory power of Value-at-Risk (VaR) (columns (1) and (2)). This is likely due the exclusion of the financial crisis period in our simulation, resulting in both lower level and variability of Value-at-Risk. Maximum shortfall (MS) and aggregate short notional (AS) still retain strong explanatory power (columns (3) and (4)), and the aggregate short notional coefficient estimate remains around the 2% range (columns (5) and (6)). Columns (5) and (6) show that Value-at-Risk has little explanatory power beyond that already captured by maximum shortfall and aggregate short notional. Columns (7) and (8) show that the DSV model still captures a significant portion of variation in initial margins, and outperforms Value-at-Risk in terms of explanatory power (columns (1) and (2)). The significance of the DSV slope coefficient persists when we introduce Value-at-Risk, and the explanatory power remains roughly the same, showing that Value-at-Risk has little explanatory power beyond that already captured by DSV. The explanatory power increases when we consider the modified DSV model (columns (11) and (12)). However, Value-at-Risk still has little explanatory power beyond that already captured by our modified DSV model. Our conclusions remain consistent with our previous results.

Comparing Table A.7 to Table A.6, we observe again that there is a non-negligible increase in explanatory power compared to models that include only portfolio variables (Table A.6). This confirms that market variables can capture a dimension of initial margins not explained by portfolio variables. Compared to Table 9, we see that the modified DSV model retains significance and explanatory power, and that Value-at-Risk still has little explanatory power beyond that already captured by the modified DSV model (columns (1) and (2)). We find again that market volatility (measured by VIX) plays a significant role in explaining margins (columns (1), (2), (5), and (6)). The loading for member CDS spreads remains negative and is significant in column (3). Columns (5) and (6) replace the modified DSV margins using maximum shortfall and aggregate short notional. The estimated coefficient for aggregate short notional is significant around 2%, showing robustness to our previous results. Maximum shortfall becomes insignificant, likely due to the high correlation with Value-at-Risk.

Table A.1: Test results for M-day VaR rules, correcting for autocorrelation.

p-values	*Effective Duration (Years):*	
M : MPoR in days	3	5
1	$< 10^{-4}$	$< 10^{-4}$
3	$< 10^{-4}$	$< 10^{-4}$
5	$< 10^{-4}$	$< 10^{-4}$
7	$< 10^{-4}$	$< 10^{-4}$
10	$< 10^{-4}$	$< 10^{-4}$

$df = 12$

Note: The table repeats the analysis of Table 5, but using a more conservative procedure that accounts for potential serial correlation.

Table A.2: Initial margins and market variables summary statistics

Summary Statistic	In. Margins ($IM_{n,t}$)	*Market variables, in basis points (bps)* Overnight Index Swap Spread (OIS_t)	LIBOR-OIS spread ($LOIS_t$)	CBOE VIX (VIX_t)
Pooled mean (over all n and t): $\mu(x_{n,t})$	654.5	18.4	15.5	$1,613.5$
Std. deviation (over all n and t): $\sigma(x_{n,t})$	367.0	10.6	4.1	434.9

Summary Statistic	In. Margins ($IM_{n,t}$)	*Market variables, in basis points (bps)* CDS spread ($CDS_{n,t}$)	Avg. CDS spread ($ACDS_t$)
Pooled mean (over all n and t): $\mu(x_{n,t})$	654.5	76.0	76.0
Std. deviation (over all n and t): $\sigma(x_{n,t})$	367.0	22.0	15.3
Time-series variation of cross-sectional averages: $\sigma(\bar{x}_t)$	48.7	15.3	
Mean cross-sectional dispersion: $\mu(\sigma_t(x_{n,t}))$	370.7	15.0	
Cross-sectional dispersion of time-series averages: $\sigma(\bar{x}_n)$	325.1	10.7	
Mean time-series dispersion: $\mu(\sigma_n(x_{n,t}))$	162.4	17.5	

Note: The table displays summary statistics of our key market variables and initial margins, in basis points and millions of USD, respectively. Definitions of market variables are reported in Table 6. In addition to the overall mean and standard deviations (dispersions), we report panel statistics that describe properties of variables both across accounts and time, the calculations of which are reviewed in Table 7. Panel summaries are not reported for market variables that do not vary across accounts.

Table A.3: Check for multicollinearity

Estimates (R^2)	*Dependent variable:*	
	SD	ES
VaR	0.338***	1.342***
(OLS)	(98.6%)	(96.7%)
VaR	0.341***	1.371***
(Two-way Panel)	(99.3%)	(98.7%)
Observations	6,721	6,721

***p<0.01

Note: We regress both expected shortfall and standard deviation on Value-at-Risk, and report the results. The first row corresponds to estimates from (pooled) OLS regression, and the second row corresponds to estimates after accounting for time and account fixed effects. Coefficient estimates are all significant at the 1% level. R^2's are in parentheses.

Table A.4: Comparing counterfactual losses to realized losses

	Realized Losses	Counterfactual Losses
Observations	6,604	6,604
Average losses ($)	110,234	24,553
Standard deviation of losses ($)	30,617,420	44,885,349
Pearson Correlation	0.5026	

Note: This table compares the historical simulation method to compute losses for the 2014–2016 sample period (based on the methodology of Duffie et al. (2015)) with the observed realized losses for the actual portfolios.

Table A.5: Descriptive statistics for historically simulated return on margins for $d = 5$ with two-way clustered standard errors

5-day-ahead return on margins (r_m)	$d = 5$
Observations	22,199,463
Mean(%)	0.02
S.D.(%)	14.40
Kurtosis	31.25
Range(%)	[-446.60, 270.70]
Interquartile Range (Q3-Q1)(%)	[-3.14, 3.16]
Empirical $VaR^{5,0.05}$(%)	-18.47
Empirical $VaR^{5,0.01}$(%)	-44.93
Empirical $VaR^{5,0.001}$(%)	-101.48
$\|r_m\| \leq 0.43$ (%)	97.78
Test results for $H_0 : \mathbb{P}(r_m < -100\%) = 1\%$	
Initial Margin Exceptions	23,268
Ratio (Z')	1.05×10^{-3}
S.E.†	$5.29 \times 10^{-4}\%$
t-stat	-16.92

Note: Same as Table 4, but calibrating the average portfolio duration d to 5 years instead of 3.

Table A.6: Regression results for initial margins with portfolio variables using last 1000 days of P&L

	Dependent variable: Initial margins (IM)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Value-at-Risk (VaR)	4.104***	2.268**	2.440*	2.097					0.008	$-$0.419	0.939	0.289
	(1.090)	(0.891)	(1.280)	(1.694)					(0.863)	(1.113)	(0.757)	(0.816)
Maximum shortfall (MS)			$-$0.225	$-$0.399	0.884**	0.437***						
			(0.542)	(0.662)	(0.356)	(0.147)						
Aggregate short notional (AS)			0.025***	0.023***	0.026***	0.024***						
			(0.004)	(0.007)	(0.004)	(0.007)						
Duffie et al. model (DSV)							1.166***	0.882***	1.165***	0.962***		
							(0.187)	(0.196)	(0.223)	(0.349)		
Modified DSV model ($MDSV$)											1.216***	1.091***
											(0.213)	(0.345)
Observations	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721	6,721
Adjusted R^2	0.391	0.764	0.663	0.837	0.647	0.829	0.641	0.818	0.641	0.819	0.655	0.829
Account FE	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*
Time FE	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*	*N*	*Y*

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Same as Table 8, but computing risk measures only using last 1000 days of simulated return on margins

Table A.7: Regression results for initial margins using portfolio and market variables with last 1000 days of P&L

	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Value-at-Risk (VaR)	0.844	0.382	0.867	0.386	2.450**	2.064
	(0.752)	(0.789)	(0.749)	(0.809)	(1.166)	(1.564)
Modified DSV model ($MDSV$)	1.270***	0.935**	1.257***	0.984***		
	(0.193)	(0.363)	(0.199)	(0.352)		
Maximum shortfall (MS)					−0.257	−0.402
					(0.492)	(0.607)
Aggregate short notional (AS)					0.026***	0.020***
					(0.004)	(0.007)
CBOE volatility index (VIX)	0.045**	0.042**	0.062***	0.060***	0.046**	0.044**
	(0.018)	(0.017)	(0.015)	(0.021)	(0.018)	(0.018)
Member CDS spread (CDS)	−2.041	−2.818*	−1.424	−1.586	−2.208	−2.363
	(1.920)	(1.687)	(1.428)	(0.977)	(1.889)	(1.560)
Average CDS spread ($ACDS$)	3.775**	4.390**			3.882**	3.895*
	(1.681)	(2.164)			(1.622)	(2.045)
LIBOR-OIS spread ($LOIS$)			11.002**	10.993**		
			(4.333)	(5.073)		
Observations	6,721	6,721	6,721	6,721	6,721	6,721
Adjusted R^2	0.674	0.836	0.673	0.834	0.683	0.842
Account FE	*N*	*Y*	*N*	*Y*	*N*	*Y*
Time FE	*N*	*N*	*N*	*N*	*N*	*N*
Note:						*p<0.1; **p<0.05; ***p<0.01

Note: Same as Table 9, but computing risk measures only using last 1000 days of simulated return on margins