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Václav Brož, Evžen Kočenda

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Abstract

We analyze link between mortgage-related regulatory penalties levied on banks and the level of systemic risk in the U.S. banking industry. We employ a frequency decomposition of volatility spillovers (connectedness) to assess system-wide risk transmission with short-, medium-, and long-term dynamics. We find that after the possibility of a penalty is first announced to the public, long-term systemic risk among banks tends to increase. From the dynamic perspective, bank penalties represent an overlooked risk as they do not increase systemic risk immediately, but the risk accumulates and propagates over the long-term. In this respect, bank penalties resemble still waters that run deep. In contrast, a settlement with regulatory authorities leads to a decrease in the long-term systemic risk. Our analysis is robust with respect to a number of relevant criteria.

JEL-Codes: C140, C580, G140, G210, G280, K410.

Keywords: bank, global financial crisis, mortgage penalty, systemic risk, financial stability.

Václav Brož
Faculty of Humanities
Charles University
Pátkova 2137/5
Czech Republic – 18200, Prague
51682260@o365.cuni.cz

*Evžen Kočenda**
Institute of Economic Studies
Faculty of Social Sciences
Charles University
Opletalova 26
Czech Republic – 11000, Prague
evzen.kocenda@fsv.cuni.cz

*corresponding author

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1. Introduction and motivation

We analyze the link between mortgage-related regulatory penalties levied on banks in the United States and the level of systemic risk in the U.S. banking industry. In connection to the (mis)conduct during the pre-crisis years, oversight and enforcement bodies in the U.S. have levied substantial penalties on banks (Garret, 2016). Penalties linked to mortgage and foreclosure misconduct levied on banks during the post-crisis period (2010-2016) amount to almost one percent of the U.S. GDP (in 2016).[1] Consequently, a high level of the incidence of financial sector misconduct (Altunbaş et al., 2018), pronounced level of contagion in the U.S. banking sector (Straetmans and Chaudhry, 2015; Pino and Sharma, 2019), and considerable extent of imposed penalties generated serious warnings that penalties might augment the systemic risk in the industry (European Systemic Risk Board, 2015). Behavior misconduct in banks is still present and nothing signals the opposite for the future. Thus, bank penalties represent an overlooked risk, and the issue is not sufficiently covered in the literature. We focus on the type of mortgage-related penalties levied over 2010-2016 on the U.S. banks and show a link between penalties and heightened systemic risk. Penalties do not increase the risk immediately, but the risk accumulates over the long-term and propagates across the industry. In this respect, bank penalties resemble still waters that run deep.

Our main outcome that penalties seem to increase systemic risk may not be that surprising after all because initial purpose of penalties seems to be more of a micro-prudential nature focusing on the risk-taking behavior of individual banks, not the system. This reflection can be supported by the fact that the extent of penalties and associated risk pertains especially to global banks and their managements that are perceived by many as prime suspects responsible for the global financial crisis (Kalemli-Ozcan et al., 2013; McConnell and Blacker, 2013) since their weakening of mortgage standards and a general break-up of a credit market discipline fueled the U.S. mortgage and housing bubble (Duca et al., 2010; Ranciere and Tornell, 2011).[2] Consequently, commencement of the crisis was marked by significant write

[1] Typically, banks received penalties for the handling of subprime mortgages, misleading investors over mortgage backed securities, unlawful mortgage securitization, improper foreclosure processing allegations, securities law violations in connection with mortgage-backed securities sales to Fannie Mae and Freddie Mac, or misleading investors about collateralized debt obligations tied to mortgage securities. A special case was the so-called National Mortgage Settlement in February 2012, when several banks agreed to pay more than 25 billion USD to address their "mortgage servicing, foreclosure, and bankruptcy abuses" (National Mortgage Settlement, 2017).

[2] One can also consider the role of CEOs and management of large financial companies in the build-up of the global financial crisis. In this regard, Boyallian and Ruiz-Verdú (2017) show that the risk-taking behavior of CEOs of large U.S. financial companies was influenced in the period preceding the crisis by their exposure to stock returns of their firms. However, DeYoung and Huang (2016) establish that setting rules that should limit risk-taking incentives of bank management – and potentially also banks' contribution to systemic risk – can paradoxically lead to lower liquidity creation in the banking system. Finally, Altunbaş et al. (2018) test for a link

downs and losses of mortgage-backed holdings resulting from increased mortgage delinquencies (Schelkle, 2018) amounting to about 500 billion USD according to He et al. (2010). In this regard, it is not surprising that more than two thirds of penalties levied by the U.S. authorities on banks after the crisis have been linked to how banks behaved with respect to mortgages and foreclosures. We focus on this type of mortgage-related penalties and show whether and how they contribute to the propagation of risk in the U.S. banking industry.

Why are bank penalties important for the risk at first place? While bank penalties aim to establish a corrective to the inflicted social harm and to serve as a deterrent for other banks, European Systemic Risk Board (2015) has warned that penalties might create systemic risk in the banking sector - related concerns were echoed in the literature assessing the issue of trust in banks during crisis (Knell and Stix, 2015), the ways how banks have transferred credit risk (Nijskens and Wagner, 2011) and systemic risk (Acharya et al., 2017; Altunbaş et al., 2018) in the financial system. Why should penalties generate an impact on banks' exposure or contribute to the systemic risk? Besides of a direct financial impact in terms of substantial profit losses (Köster and Pelster, 2017) there are potentially even more important effects that might transform into financial impacts in less than direct way. First, negative publicity surrounding the policy actions can destabilize the offender's business operations, jeopardizing its stock price as well as trust of investors and clients (Murphy et al., 2009). Penalties are likely to damage reputation that is a strategic asset for banks whose business is based on trust (Fiordelisi et al. 2014). Negative reputational damage was shown to reduce equity, substantially exceeding the financial penalty costs (Karpoff and Lott, 1993; Karpoff et al., 2008; Armour et al., 2017). In addition, size of the penalty might impact investment decisions and strategies in relation to severity of the financial misconduct (Choi and Kahan 2007) and affects bank noninterest income that was found to be positively correlated with the total systemic risk for U.S. banks (Brunnermeier et al., 2020). Second, the troubles of one bank are not isolated but they may spill over to the operations of its competitors as the banking sector is highly interconnected (Morgan, 2002; Chen at al., 2013; Anginer et al., 2014) and "systemic risk is often triggered by financial institutions that are too big to fail or too interconnected to fail" (Chen at al., 2013; p. 623). Specifically, a penalty can be understood as an idiosyncratic shock that "could have a dramatic impact on other banks, and the domino impact could potentially transmit failures from the initially affected bank to a broad group of banks and potentially to the overall banking system"

between CEO tenure and misconduct by the U.S. banks and show that banks are more likely to commit misconduct when CEOs have a relatively long tenure.

(Allen et al., 2018; p. 148). In the end, the above negative impacts may lead to individual bank failures resulting in contagion effects and systemic risk (Acharya et al., 2017).

Our analysis is linked to the literature of the *systemic risk contribution* that aims to assess the impact of a negative shock (a penalty) in a single institution (a bank) on systemic risk, or how a shock to one bank affects a group of other banks; other strand of the *systemic risk sensitivity* aims to assess the extent to which institutions are affected by a systemic macroeconomic event (for an informative exposition see Kleinow and Moreira, 2016). Despite of importance of the systemic risk propagation among banks, research on the link between penalties and systemic risk is negligible.[3] In terms of the existing evidence, Koester and Pelster (2018) do not find that a bank's contribution to a build-up of systemic risk is higher after a penalty is imposed, and Flore et al. (2021) conclude that the settlement has a rather calming effect on markets.[4] We differ from both studies in two ways that constitute a novelty that we bring to the literature. First, and from a truly network perspective, we provide assessment of the link between mortgage-related regulatory penalties levied on banks and the level of systemic risk in the U.S. banking industry simultaneously in two ways: from a bank to its peers within industry and vice versa. This way we are able to quantify extent of connectedness in the banking industry and spillovers of risk within, similarly as Singh et al. (2015); spillover risk approach was earlier adopted by Straetmans and Chaudhry (2015), albeit from a different methodological perspective. We also assess penalty's impact for two distinctive dates: when a penalty is first publicized and when a settlement is announced. Second, by employing a frequency decomposition of volatility spillovers among banks based on Baruník and Křehlík (2018), we deliver evidence about system-wide risk transmission with short-, medium-, and long-term

[3] The focus of the research in the area of bank penalties is on its impact on stock prices and/or profitability of banks and, its recent applications include Koester and Pelster (2017), Tilley et al. (2017), and De Batz (2020a, 2020b). However, to the best of our knowledge, we are not aware of studies examining the link between penalties and extreme downside risk ("tail risk") of banks. Hence, the investigation of the link between penalties and banks' tail risk can be seen as a viable avenue for future research.

[4] Koester and Pelster (2018) focus on the link between penalties to internationally listed banks and two measures of systemic risk: dynamic marginal expected shortfall (MES) and daily conditional value at risk (CoVaR). They show that there is a positive statistical association between financial penalties and the level of systemic risk exposure of banks (captured by the MES measure) but not between financial penalties and the level of systemic risk contribution of banks (proxied by the daily ΔCoVaR). In other words, financial penalties make banks more vulnerable to market downturns but there is no evidence of the transmission of shocks between banks. Flore et al. (2021) focus on market reactions (stock, bond, credit default spreads) to both the announcements of penalties and settlements of banks and interpret their results in terms of systemic risk. They cover large global banks and find that uncertainty decreases following the settlement as the settlement is perceived by the market as good news. This is also reflected in a positive market reaction (valuation effect) for banks under investigation with the same regulatory authority. Thus, the authors conclude that settlements do not contribute to a build-up of systemic risk in the economy.

dynamics. This specific distinction enables to illustrate how the risk due to penalties propagates over the time.

To deliver the novelty contributions, we first proceed with data gathering. In our analysis, we focus on publicly-traded banks operating in the United States that have been subject to financial penalties regarding their (mis)conduct related to mortgages and foreclosures from U.S. authorities. Based on the publicly available data from the Financial Times and the Wall Street Journal, we construct a unique hand-crafted dataset on bank penalties that covers the period from 2010 to 2016. Most notably, our dataset includes information on two types of dates related to a penalty: the announcement date, when the possibility of a penalty is first publicly released, and the settlement date, when an agreement about the penalty is reached between the bank and the relevant U.S. authority. Further, our interest in mortgage-related penalties is grounded also in the fact that they constitute an overwhelming majority of penalties levied on banks operating in the U.S. during the post-crisis period (see Section 3.1 for details). Then, we use a network methodology enabling analyzing the systemic risk in a comprehensive way. We adopt approach of Diebold and Yilmaz (2014) and model systemic risk as system-wide connectedness (Diebold and Yilmaz, 2015). This approach integrates extent of risk that a bank discharges into the industry with extent of risk that banks receive or are exposed to. In order to analyze those effects from different time horizons, we adopt the frequency decomposition of volatility spillovers designed by Baruník and Křehlík (2018); details on the methodology are provided in Section 2.

Combination of the detailed data and efficient methodology enables to examine the extent of risk that banks discharge and receive, in the form of high volatility spillovers, in response to an announcement of potential penalty or settlement. Further, we hypothesize that the interaction between bank penalties and systemic risk might differ with respect to the short-, medium- and long-term. The potential differences in the interaction stem from the fact that agents operate on different investment horizons—these are associated with various types of investors, trading tools, and strategies that correspond to different trading frequencies (Gençay et al., 2010; Conlon et al., 2016). Shorter or longer frequencies are the result of the frequency-dependent formation of investors' preferences, as shown in the modeling strategies of Bandi et al. (2021), Cogley (2001), or Ortu et al. (2013) that represent a theoretical framework behind interpretation of our results. In order to assess how the risk due to penalties propagates with respect to a time-frame, we employ a convenient frequency decomposition methodology introduced by Baruník and Křehlík (2018) that extends the Diebold and Yilmaz (2009, 2012) index to analyzing volatility spillovers at various frequencies. Since we frequency-decompose the systemic risk from the stock prices of banks, the short-, medium, and long-term investment horizons are actually reflected in volatility spillovers at short-, medium- and long-term frequencies as shown in Baruník and Křehlík (2018) who themselves document the rich time-frequency dynamics of volatility connectedness in U.S. financial institutions. Their versatile approach allows us to distinguish system-wide risk transmission with short-, medium-, and long-term persistence. In other words, we are also able to assess whether the effect of bank penalties propagates in long- or short-term, whether it is persistent or short-lived.

Our key result is robust evidence on the differences between the penalty announcement and penalty settlement effects. We show that after the possibility of a penalty is first announced to the public, long-term systemic risk in the U.S. banking sector tends to increase. In contrast, a settlement with regulatory authorities leads to a decrease of the long-term risk. Finally, our analysis is relevant to authorities imposing the penalties as well as those in charge of financial stability. While penalties affect both the performance and valuation of the penalized banks, they might also influence other (innocent) banks. We can also conjecture that heightened risk among the U.S. banks due to imposed penalties can transfer elsewhere because Elyasiani et al. (2015) document the existence of an asymmetric volatility transmission mechanism among financial institution after the crisis, where the U.S. banking industry assumes the leadership role of a global exchange center of information. Our results also indicate that intended corrective effect of the penalties is in contrast with the heightened risk in the industry.

The paper is structured as follows. In Section 2, we describe the methodological approach based on volatility spillovers. Section 3 presents the data, variables, and testable hypotheses. We display our results and inferences in Section 4. The last section concludes.

2. Methodology

2.1 Non-technical exposition

We use a methodology based on the concept of volatility spillovers introduced in Diebold and Yilmaz (2009, 2012, 2014) and adopt the frequency decomposition of volatility spillovers designed by Baruník and Křehlík (2018). In the end, we work with time series (based on stock prices) of bank-specific spillovers at various frequencies that mimic investment horizons and allow to capture an investor's perspective to what extent a bank contributes to the system-wide connectedness/systemic risk (*to*-spillovers) and to what extent a bank receives shocks from the banking industry (*from*-spillovers). For the clarity, we first introduce a non-technical description of our approach.

What is the basis of the methodology? In general, volatility connectedness quantifies the dynamic and directional characterization of volatility spillovers among various assets, units, or across markets (Diebold and Yilmaz, 2015). The connectedness measure in a form of the spillover index is computed with a volatility/variance as its key input and it is able to quantify various degrees of spillovers (total, directional, net) as shown in Diebold and Yilmaz (2009, 2012). The computation of the spillover index is based on a simple variance decomposition associated with a vector autoregressive (VAR) model from which an *H*-step-ahead forecast of error variance and corresponding forecast error vector are computed. Via the off-diagonal values, the spillover index quantifies contribution of shocks to a specific variable coming from other variable(s). Further decomposition of the spillovers allows to distinguish spillovers coming *from* or *to* a particular source; these are termed directional spillovers. Finally, frequency decomposition of the original time series (stock prices) allows to analyze connectedness at different frequencies.

How does the methodology allow to analyze systemic risk? Diebold and Yilmaz (2014) show that systemic risk can be modelled as a system-wide connectedness that captures volatility spillovers originating at specific sources and travelling in observable directions (*to*- and *from*-spillovers).[5] With respect to measuring systemic risk, Diebold and Yilmaz (2014) argue that connectedness combines two existing approaches: (i) spillovers capturing the contribution of an individual network element (a bank) to the system-wide connectedness (*to*-spillovers) is an analogy to the conditional value at risk (CoVaR) approach of Adrian and Brunnermeier (2016), (ii) the extent to which individual network elements (banks) are exposed to system-wide shocks (*from*-spillovers) can be related to the marginal expected shortfall (MES) approach of Acharya et al. (2010). The connectedness-based approach enables to assess the extent of risk that banks discharge and receive (in the form of high volatility spillovers) in response to an announcement of penalty or a settlement.

In addition, frequency decomposition of the connectedness enables to investigate whether and how the interactions between bank penalties and systemic risk differ in short-, medium- and long-term. These differences might originate in varying perceptions of risk with respect to different investment horizons on which specific investors operate; they are associated with various types of investors, trading tools, and strategies that correspond to different trading

[5] Diebold and Yilmaz (2014) approach to quantify systemic risk was further adopted by Singh et al. (2015) who provide evidence of the systemic risk (connectedness) among the eurozone banks, albeit at somewhat lower level than that documented by Diebold and Yilmaz (2014) for U.S. financial institutions. In addition, Singh et al. (2020) analyze connectedness between the eurozone banking sector and sovereign risk, and document existence of directional risk clusters in the center and periphery.

frequencies (Gençay et al., 2010; Conlon et al., 2016) based on which investors' preferences are formed (Bandi et al., 2021; Cogley, 2001; Ortu et al., 2013). Frequency decomposition of connectedness, introduced by Baruník and Křehlík (2018), allows to frequency-decompose the systemic risk (from the stock prices of banks) with respect to the short-, medium, and long-term investment horizons.[6] Consequently, it also enables to assess whether the effect of bank penalties is persistent or short-lived.

2.2 Formal description

A starting point of the analysis are time series of daily total volatility measures derived from banks' stock prices. Because we do not work with high-frequency data, we compute the daily volatility of stock prices by following the approach introduced by Parkinson (1980) and used by Diebold and Yilmaz (2012).[7] We compute daily variance based on the deviation between high and low stock prices as:

$$\widehat{PV}^2 = \frac{1}{4ln2}(h-l)^2, \tag{1}$$

where h and l stand for high and low prices, respectively, and $\widehat{PV}^2$ is the estimator of daily variance. To obtain the annualized daily percentage volatility, we further compute:

$$PV = 100 \times \sqrt{252 \times \widehat{PV}^2}, \tag{2}$$

where 252 represents the number of trading days in a year as in Shu and Zhang (2003) and Taylor et al. (2010).

The spillover measures by Diebold and Yilmaz (2009) rely on variance decomposition from vector autoregressions (VARs) that captures how much of the future error variance of a variable j is due to innovations in another variable k. For N assets, we consider an N-dimensional vector of daily volatilities, $\mathrm{PV_t} = (\mathrm{PV_{1t}}, \dots, \mathrm{PV_{Nt}})'$, to measure total volatility spillovers.

Let us model the N-dimensional vector PV_t by a weakly stationary VAR(p) as $PV_t = \sum_{\mathrm{l}=1}^{p} \boldsymbol{\Phi}_\mathbf{l} \boldsymbol{PV}_{\boldsymbol{t}-\mathbf{l}} + \epsilon_t$, where $\boldsymbol{\epsilon_t} \sim N(0, \boldsymbol{\Sigma_\epsilon})$ is a vector of iid disturbances and $\boldsymbol{\Phi}_\mathbf{l}$ denotes p coefficient matrices. For the invertible VAR process, the moving average representation has the following form:

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6 The frequency decomposition proved to be a useful tool in economic and financial analyses brought recently by Trabelsi (2018), Tiwari at el. (2018; 2019), Baruník and Kočenda (2019), Wang and Zong (2019), Fan et al. (2020), Su (2020), Arreola Hernandez et al. (2020), or Qarni and Gulzar (2020).

7 The other possibility, suitable primarily for very high-frequency data, is to quantify volatility in terms of the realized variance (RV) introduced by Andersen et al. (2001) and Barndorff-Nielsen (2002) and used in Diebold and Yilmaz (2014).

$$PV_t = \sum_{\text{l}=0}^{\infty} \mathbf{\Psi}_\text{l} \boldsymbol{\epsilon}_{t-\text{l}}. \tag{3}$$

The $N \times N$ matrices holding coefficients $\mathbf{\Psi}_\text{l}$ are obtained from the recursion $\mathbf{\Psi}_\text{l} = \sum_{j=1}^{p} \mathbf{\Phi}_j \mathbf{\Psi}_{\text{l}-j}$, where $\mathbf{\Psi}_0 = I_N$ and $\mathbf{\Psi}_\text{l} = 0$ for $\text{l} < 0$. The moving average representation is useful for describing the dynamics of the VAR system as it allows isolating the forecast errors that can be used for the computation of the connectedness of the system. Diebold and Yilmaz (2012) further assume the generalized VAR of Koop et al. (1996) and Pesaran and Shin (1998) to obtain forecast error variance decompositions that are invariant to variable ordering in the VAR model, and it also explicitly accommodates the possibility of measuring directional volatility spillovers.[8]

In order to define the total spillovers index of Diebold and Yilmaz (2012), we consider the H-step-ahead generalized forecast error variance decomposition matrix having the following elements for $\text{H} = 1,2, \ldots$:

$$\theta_{jk}^{H} = \frac{\sigma_{kk}^{-1} \sum_{h=0}^{H-1} \left(e_j' \mathbf{\Psi}_h \mathbf{\Sigma}_\epsilon e_k\right)^2}{\sum_{h=0}^{H-1} \left(e_j' \mathbf{\Psi}_h \mathbf{\Sigma}_\epsilon \mathbf{\Psi}_h' e_k\right)}, \qquad j,k = 1, \ldots, N, \tag{4}$$

where $\mathbf{\Psi}_h$ are moving average coefficients from the forecast at time t, $\mathbf{\Sigma}_\epsilon$ denotes the variance matrix for the error vector $\boldsymbol{\epsilon}_t$, σ_{kk} is the kth diagonal element of $\mathbf{\Sigma}_\epsilon$, and e_j and e_k are the selection vectors, with one as the jth or kth element and zero otherwise. Normalizing elements by the row sum as $\tilde{\theta}_{jk}^{H} = \theta_{jk}^{H} / \sum_{k=1}^{N} \theta_{jk}^{H}$, Diebold and Yilmaz (2012) then define the total connectedness as the contribution of connectedness from volatility shocks across variables in the system to the total forecast error variance:

$$S^H = 100 \times \frac{1}{N} \sum_{\substack{j,k=1 \\ j \neq k}}^{N} \tilde{\theta}_{jk}^{H}. \tag{5}$$

Note that $\sum_{k=1}^{N} \widetilde{\theta_{jk}^{H}} = 1$ and $\sum_{j,k=1}^{N} \widetilde{\theta_{jk}^{H}} = N$, hence, the contributions of connectedness from volatility shocks are normalized by the total forecast error variance. To capture the spillover dynamics, we use a 300-day rolling window running from point $t - 299$ to point t. Further, we assume a forecast horizon $H = 10$ and a VAR lag length of 2 based on the AIC.

The total connectedness indicates how shocks to volatility spill over throughout the system. Further, directional spillovers allow us to decompose the total spillovers to those coming from, or to, a particular asset in the network. Diebold and Yilmaz (2012) propose to

[8] The generalized VAR allows for correlated shocks; hence, the shocks to each variable are not orthogonalized.

measure the directional spillovers received by asset j from all other assets k (*from*-spillovers) as:

$$S_{N,j\leftarrow\bullet}^{H} = 100 \times \frac{1}{N}\sum_{\substack{k=1\\ j\neq k}}^{N} \tilde{\theta}_{jk}^{H}, \tag{6}$$

i.e., we sum all numbers in rows j, except the terms on the diagonal that corresponds to the impact of asset j on itself. The N in the subscript denotes the use of an N-dimensional VAR.

In a similar fashion, the directional spillovers transmitted by asset j to all other assets k (*to*-spillovers) can be measured as:

$$S_{N,j\rightarrow\bullet}^{H} = 100 \times \frac{1}{N}\sum_{\substack{k=1\\ j\neq k}}^{N} \tilde{\theta}_{kj}^{H}. \tag{7}$$

Having introduced the directional spillovers that constitute a crucial dimension of our analysis, we further assume frequency decompositions of *to*- and *from*-volatility spillovers into those that reflect short-term (up to 5 days), medium-term (up to 20 days), and long-term (up to 300 days) dynamics. Importantly, these intervals correspond to connectedness within a business week, a business month, and a business year, respectively. They may be also understood as investment horizons of different lengths.

A natural way to describe the frequency dynamics (whether long, medium, or short term) of connectedness is to consider the spectral representation of variance decompositions based on frequency responses to shocks instead of impulse responses to shocks. As a building block, Baruník and Křehlík (2018) consider a frequency response function, $\left(\mathrm{e}^{-\mathrm{i}\,\omega}\right) = \sum_{h} \mathrm{e}^{-\mathrm{i}\omega \mathrm{h}}\, \boldsymbol{\Psi}_{h}$, which can be obtained as a Fourier transform of coefficients $\boldsymbol{\Psi}_{h}$ with $i = \sqrt{-1}$. The spectral density of the annualized daily percentage volatility $\mathrm{PV_t}$ defined in (2) and (3) at frequency ω can then be conveniently defined as a Fourier transform of the $MA(\infty)$ filtered series:

$$\mathrm{S_{PV}}(\omega) = \sum_{\mathrm{h}=-\infty}^{\infty} \mathrm{E}(\mathbf{PV_t PV_{t-h}'})\mathrm{e}^{-\mathrm{i}\omega \mathrm{h}} = \boldsymbol{\Psi}\left(\mathrm{e}^{-\mathrm{i}\omega}\right)\boldsymbol{\Sigma}\boldsymbol{\Psi}'\left(\mathrm{e}^{+\mathrm{i}\omega}\right). \tag{8}$$

The power spectrum $\boldsymbol{S_{PV}}(\omega)$ is a key quantity for understanding frequency dynamics since it describes how the variance of $\mathbf{PV_t}$ is distributed over frequency components ω. Using the spectral representation for covariance, i.e., $E(\mathbf{PV_t PV_{t-h}'}) = \int_{-\pi}^{\pi} \boldsymbol{S}_{x}\,(\omega) e^{i\omega h} d\omega$, Baruník and Křehlík (2018) naturally define the frequency domain counterparts of variance decomposition.

The spectral quantities are estimated using standard discrete Fourier transforms. The cross-spectral density on the interval $d = (a, b): a, b \in (-\pi, \pi), a < b$ is estimated as $\sum_{\omega} \widehat{\boldsymbol{\Psi}}(\omega)\widehat{\boldsymbol{\Sigma}}\,\widehat{\boldsymbol{\Psi}}'(\omega)$ for $\omega \in \left\{\left\lfloor\frac{aH}{2\pi}\right\rfloor, \dots, \left\lfloor\frac{bH}{2\pi}\right\rfloor\right\}$, where $\widehat{\boldsymbol{\Psi}}(\omega) = \sum_{h=0}^{H-1} \widehat{\boldsymbol{\Psi}}_h\, e^{-2i\pi\omega/H}$, and $\widehat{\boldsymbol{\Sigma}} = \hat{\boldsymbol{\epsilon}}'\hat{\boldsymbol{\epsilon}}/(T - z)$, where z is a correction for a loss of degrees of freedom and depends on the VAR specification.

The decomposition of the impulse response function at the given frequency band can be estimated as $\widehat{\boldsymbol{\Psi}}(d) = \sum_{\omega} \widehat{\boldsymbol{\Psi}}(\omega)$. Finally, the generalized variance decompositions at a desired frequency band are estimated as:

$$\widehat{\boldsymbol{\theta}}_{j,k}(d) = \sum_{\omega} \widehat{\Gamma}_j(\omega) \frac{\widehat{\sigma}_{kk}^{-1}\left(e_j'\widehat{\boldsymbol{\Psi}}(\omega)\widehat{\Sigma}e_k\right)^2}{e_j'\widehat{\boldsymbol{\Psi}}(\omega)\widehat{\boldsymbol{\Sigma}}\widehat{\boldsymbol{\Psi}'}(\omega)e_j}, \tag{9}$$

where $\widehat{\Gamma}_j(\omega) = \frac{e_j'\widehat{\Psi}(\omega)\widehat{\boldsymbol{\Sigma}}\widehat{\boldsymbol{\Psi}'}(\omega)e_j}{e_j'\Omega e_j}$ is an estimate of the weighting function, where $\Omega = \sum_{\omega} \widehat{\boldsymbol{\Psi}}(\omega)\widehat{\boldsymbol{\Sigma}}\widehat{\boldsymbol{\Psi}'}(\omega)$.

Then, the connectedness measure at a given frequency band of interest can be readily derived by substituting the $\widehat{\boldsymbol{\theta}}_{j,k}(d)$ estimate into the traditional measures outlined above.[9]

3. Data, variables, and hypotheses

3.1 Sample of banks and bank penalties

In our analysis, we cover 17 key banks operating in the United States. The analyzed network is comprised of publicly-traded banks that were given a penalty for their (mis)conduct related to mortgages and foreclosures by various U.S. oversight and enforcement authorities.[10] The sample of banks includes the largest U.S. banks operating nationwide (Bank of America, JPMorgan Chase, Citigroup, Wells Fargo, Goldman Sachs, and Morgan Stanley), U.S.-domiciled banks with a more regional focus (SunTrust, PNC, U.S. Bancorp, Flagstar Bank, and

[9] The entire estimation is done using the package *frequencyConnectedness* in *R* software. The package is available on CRAN or at https://github.com/tomaskrehlik/frequencyConnectedness.

[10] The authorities that reached a settlement with banks include the Department of Housing and Urban Development, the Department of Justice, the Federal Deposit Insurance Corporation, the Federal Housing Finance Agency, the Federal Reserve, the National Credit Union Administration, the Office of the Comptroller of the Currency, the Securities and Exchange Commission, several state attorneys, and the Attorney General. For an overview of major U.S. law enforcers and regulators, see Flore et al. (2021) whose methodology related to misconduct results we follow and correspondingly, we do not distinguish between settlement or verdict as means of a case closure as the vast majority of cases is resolved through settlements. However, we do not assess potentially different impact of penalties on systemic risk with respect to the type of enforcement authority as we would be forced to work with number of fragmented subsamples; with a single exception (Office of the Comptroller of the Currency), Flore et al. (2021) report statistically insignificant results linked to the type of enforcement authority. This option might be explored in the future should the sample sizes become of statistical relevance.

Fifth Third Bancorp), and several major non-U.S. banks operating in the United States (Deutsche Bank, Credit Suisse, Royal Bank of Scotland, HSBC, UBS, and Barclays). Many of the non-U.S. banks received very large (volumes of) penalties when compared to some U.S. banks with a more regional focus, as we later present in Figure 1.[11] For the above banks, we compute volatility spillovers based on the stock prices. Daily stock price data were downloaded from Yahoo Finance and stock price volatility was estimated using the ranged-based estimator introduced by Parkinson (1980) as shown in (1) and (2). Descriptive statistics of the volatility data are shown in Table A1.

Composition of our sample is driven by several criteria. With respect to individual banks, (i) we need available evidence on imposed penalties with clear indication of amount of penalty and exact timing of its announcement/settlement. We need this information (ii) for banks whose stocks are publicly traded in order to compute their stock price volatility. Since our aim is (iii) to analyze a systemic risk in the banking sector, we chose 17 banks for which criteria (i) and (ii) are met and that are classified as systemically important banks according to eight systemic risk measures, no matter of their domicile, as documented by Varotto and Zhao (2018; Appendix Table 3, panels A and B). Moreover, (iv) 11 banks in our sample are also included in the Financial Stability Board (FSB) list of "systemically important financial institutions" (SIFI) and our sample thus exceeds a smaller group of the officially categorized SIFIs. We strived to enlarge the sample but did not identify other important banks with sufficient penalty data whose stocks would be publicly traded and for which we could compute stock price volatility. Hence, 17 banks we analyze constitute a representative sample to proxy for the population of systemically important banks affected by mortgage-related penalties for which we have required data support.

Our analysis covers the years from 2010 to 2016 as we examine regulatory action taken *after* the global financial crisis based on the banks' behavior before the crisis. For our analysis, we construct a unique handcrafted dataset of the mortgage-related penalties imposed on banks operating in the United States that are listed in Table A2. Importance of the mortgage-related penalties is accentuated by the fact that they represent about 76% of all penalties imposed on the banks in our sample operating in the U.S. during the post-crisis period. The core of the

[11] An example of a non-U.S. bank with a large penalty is given in Altunbaş et al. (2018; p.1) who state that a "case in point is Deutsche Bank with widespread press reports in September 2016 that the US Department of Justice was seeking a $14 billion civil settlement for the bank allegedly having sold toxic mortgage-backed securities; the fine was equivalent to about four-fifths of the bank's market capitalization and raised doubts about its future viability and the systemic consequences should it fail".

dataset was collected by Financial Times reporters.[12] However, the core of the dataset does not contain any data after July 2015 and, more importantly, it does not provide any information about when the possibility of a penalty was first publicly announced. Therefore, we broaden the dataset substantially. First, we use the Factiva database to cross-check the accuracy of the dataset and we further extend it until the end of 2016. Second, and most importantly, for each penalty we further add a date when the possibility of a penalty (that eventually materialized) was first publicly announced in the Wall Street Journal.[13] It needs to be stressed that the announcement date is, in fact, the very first public announcement related to the penalty as during our news search we did not find any previous indication about a penalty. Thus, the first announcement of a possibility of a penalty should be indeed unanticipated by the general public. As for the settlement, there might be available (but not necessarily) some news about the development in the case before the settlement itself. However, as we have identified only handful of unresolved cases, the settlement is not a question of "whether it happens" but rather "when it happens". This makes it quite distinct from the first announcement of the possibility of a penalty.

Figure 1 shows the gross volumes of penalties related to mortgage and foreclosure misconduct that several banks in the United States had to pay in the period from 2010 to 2016. The total amount stands at almost 140 billion USD.[14] Among the mortgage-related penalties, those related to mortgage-backed securities were the largest category (85 billion USD), followed by penalties related to foreclosures (36 billion USD) and penalties levied in connection to mortgage repurchases (19 billion USD). The outlay of the single largest receiver – Bank of America – constitutes around 40% of the total volume; the results are robust with respect to this large penalty receiver as we show via a robustness check in Section 4.4. In

[12] The data can be downloaded at http://ig-legacy.ft.com/content/e7fe9f25-542b-369f-83b2-5e67c8fa3dbf.
[13] In our analysis we consider cases of penalties that eventually materialized. We do not consider cases when banks were acquitted after an announcement of an investigation related to mortgages or foreclosures. We admit that such an analysis could yield insights on market's ability to foresee whether a case is relevant (i.e. leads to a penalty). However, our search in the Wall Street Journal shows that the number of such cases is negligible and immaterial with respect to the analysis.
[14] The penalties we analyze are 'monetary' fines in their nature and their sum of 140 billion USD amounts to almost 1% of the 2016 U.S. GDP. We acknowledge that there might be additional rules and regulations, or more scrutiny in supervision after misconduct, that can indirectly also lead to higher costs. In the post-crisis period banks have had to adapt to new rules and regulations that might potentially restrict certain business activities of banks and thus impact their financial performance; in this sense new rules and regulations can be, to a certain extent, considered somewhat similar to penalties (Wilmarth Jr., 2012; Pridgen, 2013). However, assessment of such a hypothetical impact is beyond the scope of our analysis. We also do not consider potential effect of positive news in a form of various awards acknowledging the best banks etc. The reason is that (i) this type of news is not comparable to our data as it originates from different sources than from official oversight and enforcement authorities, and (ii) it is well established that volatility tends to react disproportionally more to bad news (Koutmos and Booth, 1995; Braun et al., 1995). This avenue is left for further research.

general, the U.S. banks paid in penalties significantly more than their European counterparts. In terms of the yearly dispersion of penalties, Figure 2 illustrates that a decisive share of the penalties was levied between 2012 and 2014 (around 110 billion USD). After a quiet 2015, U.S. authorities collected almost 24 billion USD in 2016.[15]

Our set of bank penalties contains various types of banks. In general, one would expect that retail banking (e.g. relationship lending model) generally leads to less misconduct and lower fines than wholesale banking (e.g. transactions- based lending). Based on the data we analyze, the misconduct of the retail banks leading to penalties was related to their mortgage origination activity: penalties linked to foreclosures and mortgage repurchases account for about 25 and 14 percent, respectively. That is about 39 percent of total mortgage-related penalties, and the median of penalty amount was 330 million USD. On the other hand, behavior of the wholesale banks was related to mortgage securitizations and their trading: penalties linked to mortgage bank securities represent about 61 percent of all mortgage-related penalties and the median of penalty amount was 285 million USD.[16] When comparing the two bank types, the above data indicate that the wholesale banks were the biggest villains in terms of the overall volume of penalties, but retail banking recorded higher individual (median) penalties. A detailed overview of the penalties is presented in Appendix Table A2, which contains precise information on the announcement date, the settlement date, the name of the bank that received a penalty, the name of the regulator who imposed the penalty, and the value of the penalty (in million USD).[17] Interestingly, the same announcement date applies for several cases that were, however, settled at various dates. The size of the penalties typically ranges between 0.1 and 0.5 billion USD, as Figure 3 shows; still, there are several cases of very large penalties over 5 billion USD. Further, Figure 4 reveals that the enforcement process (i.e. the time span from the announcement date to the settlement date) takes in most cases more than 2 years.

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[15] The heat wave of penalties has not receded after that, as the Trump administration levied penalties on Barclays and the Royal Bank of Scotland in 2017 and 2018. From our dataset concerning both announcement and settlement of penalties, we infer that most of the penalties were settled as cash transfers to federal and state governments. Some proportion of the penalties was settled by banks via loan forgiveness and supporting debt restructuring. In any event, the settlement and indeed the very existence of a penalty lowered the profit of a punished bank. Thus, it can be generalized that penalties are treated as expenses that lower profitability of a bank. We did not identify cases of selling-off the bank assets due to insufficient profit. However, we admit that the issue requires a detailed treatment using non-public bank and regulatory data that are out of our reach.

[16] Other types of penalties are represented in small or marginal proportions (indicated in parentheses) and are related to Sanctions/Money Laundering/Tax Evasion (14 %), Market manipulation (10 %), Lending/Consumer Practices (4 %), and M&A (less than 1 %).

[17] There are a few cases when the announcement dates are unavailable. This means that the announcement of the settlement was also the first time when the possibility of the penalty was first announced. We classify these cases as settlement dates (and not announcement dates). A similar approach is used in Tilley et al. (2017).

3.2 The link between bank penalties and systemic risk

Our working hypotheses are focused on system-wide connectedness after the announcement date and the settlement date as such incidents exhibit potential to create systemic risk in the banking sector (European Systemic Risk Board, 2015). Investors' trust might evaporate quickly (Murphy et al., 2009) and the troubles of a specific bank might swiftly transfer to its competitors (Morgan, 2002; Anginer et al., 2014). However, in terms of empirical evidence, Koester and Pelster (2018) do not find that a bank's contribution to a build-up of systemic risk is higher after a penalty is imposed; they do not distinguish short- versus long-term impact, though. Also, Flore et al. (2021) conclude that the settlement has a rather calming effect on markets. Thus, in our working (null) hypotheses, we ask if a bank's contribution/exposure to systemic risk is higher after the announcement/settlement date or not. The extent of the contribution of a specific bank after it has its own penalty announced/settled (while nothing happens to its competitors) is captured in the *Hypothesis #1: A bank's contribution to systemic risk does not increase after the announcement date or settlement date.* The extent to which a specific bank is exposed to systemic risk after one of its competitors has its own penalty announced/settled is represented by the *Hypothesis #2: A bank's exposure to systemic risk does not increase after the announcement date or settlement date.*

We expect that the announcement date might lead to a build-up of systemic risk due to its unexpected nature. By construction, the announcement date is the first time when the possibility of a penalty (which was eventually imposed) was announced publicly. On the other hand, the settlement date might come as a relief for markets after a protracted period of uncertainty. Moreover, prior to the settlement, banks might disclose that they created provisions for legal matters, giving markets some indication that the penalty was already internally accounted for (Flore et al., 2021).[18] In terms of the three measures of connectedness, the long-term measure in particular might be affected by penalty-related specifics, as it represents shifts in investors' preferences and beliefs considered by Murphy et al. (2009). On the other hand, short-term and medium-term connectedness might also appear relevant if penalties were perceived by markets as one-time incidents. Finally, it might be insightful to assess Hypotheses #1 and #2 from two angles: to distinguish if there is any difference in a specific bank's contribution/exposure to systemic risk depending on whether the specific bank was the target of the penalty or one of its competitors was the target.

[18] Such behavior would be also consistent with the requirements grounded in the International Financial Reporting Standards (IFRS) that banks are obliged to follow and that are enforced by the IAS 39.

To assess both hypotheses empirically, we develop a testing strategy in the spirit of Doners and Vorst (1996), Clayton et al. (2005), and Uhde and Michalak (2010). As a tool we use the test of Wilcoxon (1945) to examine if two (paired) samples share the same distribution. The Wilcoxon test is quite effective for our purpose as it is especially suited to assess non-normal data (Gibbons and Chakraborti, 2011). As an alternative we also use a non-parametric paired sign test to check robustness of our results with respect to the choice of our testing strategy tool. Specifically, we test whether the extent of spillovers before and after the announcement or settlement differs substantially or not at all. For that we calculate the median differences in spillover values before and after, and test whether each median difference is statistically significant. In our testing, the null hypothesis of the Wilcoxon test is that the median difference between pairs of observations is zero. In our empirical part we show values and signs of the median differences along with the statistical significance of such values at conventional significance levels.

Initially, for each bank in our sample, we form two types of vectors of penalties for both the announcement and the settlement date. The first two vectors capture all the dates when a bank has its own penalty announced or settled; the two vectors are labelled as "own penalties". The other two vectors capture all the dates when all the other banks have their penalties announced or settled; these two vectors are labelled as "other banks' penalties". Note that all four vectors contain mutually exclusive information.

Second, for each bank in our sample, we collect median values of *to*- and *from*-spillovers with the short-, medium-, and long-term dynamics around the announcement and settlement dates with the intervals indicated in Figure 5.[19] Note that the length of the intervals corresponds to how all three connectedness measures are defined: the short-term measure captures spillovers of up to 5 days (one business week), the medium-term measure up to 20 days (one business month), and the long-term measure up to 300 days (one business year). The length of the intervals is same as that adopted by Baruník and Křehlík (2018) or Baruník and Kočenda (2019) and allows to conveniently asses the systemic risk from the perspective of three investment horizons.

[19] For the short-term connectedness measure, we assume the time intervals [-5 days, 0 days] and [0 days, 5 days] before and after the announcement or settlement dates. For the medium term we consider the intervals [-20 days, 0 days] and [0 days, 20 days], and for the long term we work with the intervals [-300 days, 0 days] and [0 days, 300 days]. Despite that we use intervals before and after the announcement or settlement dates, in no way we carry an event analysis. On contrary, we use the three different intervals to compute the systemic risk measures over three investment horizons.

Third, we obtain tables of median values of *to*- and *from*-spillovers across banks with the short-, medium-, and long-term dynamics before and after the announcement or settlement date. The median values are obtained for each of the type of vectors of penalties ("own penalties" or "other banks' penalties"). Then, we employ the Wilcoxon test to determine if the distribution of penalties before and after the announcement/settlement date is the same or not. Specifically, we examine if the difference between the median values of spillovers before and after the announcement/settlement is statistically different from 0. The aggregate quantification of the tests is then presented in Tables 1-5 along with a statistical significance assessment. In order to provide information on economic significance of spillovers, we present the test results on median differences in a form of percentage change of the median values of spillovers before and after the announcement/settlement. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

In sum, if we find that the results (*to*- and *from*-spillovers) for a specific bank are similar regardless of whether it received a penalty or its competitor did, we can argue that *any* penalty affects the entire banking system. Thus, rather than having a desired corrective impact on a particular financial institution, a penalty increases the systemic risk, potentially making the banking sector less stable and more vulnerable.

4. Results

4.1 Total and frequency connectedness

As a preliminary step, we briefly comment on the total and frequency connectedness of our network of 17 banks. Corresponding spillovers are shown in Figure 6. Total connectedness stands at more than 80% throughout the entire sample period (2009–2017), except for the period after mid-2012 when it temporarily recedes after the "whatever it takes" speech by ECB President Mario Draghi (2012).[20] In terms of frequency connectedness, the dynamics of short- and long-term components differs substantially. First, the long-term component prevails in the aftermath of the subprime mortgage crisis in 2009 and then briefly from mid-2011 to mid-2012. The result for our sample of banks exhibits a very similar pattern as that shown by Baruník and Křehlík (2018; Figure 1) for long-term frequency connectedness among eleven major financial firms representing the financial sector of the U.S. economy. The starting point of the latter

[20] The end of the EU sovereign debt crisis coincides with a remarkable statement by the ECB President Mario Draghi (2012) at the Global Investment Conference in London on July 26, 2012: "Within our mandate, the ECB is ready to do whatever it takes to preserve the euro. And believe me, it will be enough". Fiordelisi and Ricci (2016) show that the European financial markets started to rally immediately after this statement and that the economic situation began to improve.

period is likely associated with the downgrading of U.S. bonds on August 5, 2011, while the end point can be again related to the "whatever it takes" speech by ECB President Mario Draghi. After that, the long-term connectedness recedes and short- and medium-term connectedness become relatively more influential. As shown in Figure 6, the short- and long-term connectedness are almost perfectly negatively correlated. This is in line with the argument of Baruník and Křehlík (2018) that short-term connectedness characterizes periods of calm markets while long-term connectedness dominates in times of heightened investor uncertainty.

4.2 Contribution to systemic risk

In Hypothesis #1, we ask if a contribution of a bank to systemic risk (expressed by *to*-spillovers) is higher after the announcement or settlement date and if so, at which frequencies or time horizon; results are presented in Table 1. First, we assess reaction in cases when a specific bank receives its own penalty. It seems that the first public announcement about a penalty leads to a realignment of the relative importance of the three frequency connectedness measures. The impact of the short-term and medium-term risk is almost (economically) negligible as the percentage change in median difference of spillovers ranges between 0 and -3%. However, after a penalty is announced, the receiving bank's contribution to long-term systemic risk goes up substantially (+44%). In other words, a penalty-receiving bank begins to make the system more interconnected and riskier from a long-term perspective.

Our results at short- and medium-terms are in line with those of Koester and Pelster (2018) in that we also do not find evidence for transmission of shocks between banks. However, our long-term evidence can be interpreted from the perspective of the frequency decomposition approach that offers finer distinction of the penalties' impact with respect to investment horizons. Degree of connectedness derived from stock price data differs at different frequencies (Baruník and Křehlík, 2018) as investors focus on different investment horizons when forming their investment decisions motivated by their preferences and strategies (Gençay et al., 2010; Conlon et al., 2016; Bandi et al., 2021; Cogley, 2001; Ortu et al., 2013). Hence, increased long-term risk might reflect worries of investors related to long investment horizon as they do not know how long a penalty-to-settlement process might take. In this sense, the risk tends to accumulate over time. On the other hand, from the short and medium investment perspective, once a penalty is announced, portfolio adjustments can be swiftly made. The results and interpretation are also in line with the direct evidence that long-term spillovers dominate in times of heightened investor uncertainty in case of the U.S. financial institutions (Baruník and Křehlík, 2018). The result also correlates with an indirect evidence that uncertainty substantially increases volatility spillovers at long-term in case of interactions between oil and forex markets (Baruník and Kočenda, 2019) or that long-term risk is more pronounced on forex market (Tiwari et al., 2018).

A different type of impact materializes after a settlement between a receiving bank and a U.S. authority is reached. In these circumstances, the long-term systemic risk markedly decreases (-26%) while the two measures capturing the effects at shorter frequencies do not record any statistically significant change (Table 1). This pattern might be interpreted as a relief experienced by financial markets once the enforcement process is over; such finding and interpretation are in line with Flore et al. (2021).

Interestingly, similar findings as above are also obtained when we work with the “other banks’ penalties” vector of announcement/settlement dates. This means that a specific bank – which is not mentioned in the announcement – radiates higher long-term spillovers (increase by 11%) after some other bank has a penalty announced. In other words, a penalty levied on a competitor induces a comparable reaction as if the penalty was levied on a specific bank itself. Similarly, after another bank settles its penalty, the contribution of a bank not receiving a penalty to long-term systemic risk decreases (-17%). The effects for short- and medium-term systemic risk vary but are generally marginal or statistically insignificant when compared with the long-term counterpart whose effect is statistically and economically significant. Economic significance of the above results can be also seen from the perspective of investment horizon: while effects for short- and medium-term systemic risk are negligible, substantial increase (decrease) of the long-term risk is in line with theoretical literature background as they mirror reaction of the investors with long-term strategies (Cogley, 2001; Ortu et al., 2013; Bandi et al., 2021).

4.3 Exposure to systemic risk

In the previous subsection, we established that a bank’s contribution to long-term systemic risk is higher (lower) after the announcement (settlement) date, regardless of if the bank received its own penalty or if a competitor was targeted. Now, we are interested in whether for a specific bank, *from*-spillovers differ after other banks have a penalty announced/settled, as outlined in Hypothesis #2. Aggregated results in Table 1 reveal that short- and medium-term spillovers do not record statistically or economically significant differences. However, a specific bank is exposed to higher long-term systemic risk from the rest of the banking sector after it has its own penalty announced (+84%). This signals that other banks in the system react even if they do not face the possibility of their own penalties. As a result, the system becomes more interconnected over a long period of time. However, after a settlement is reached the specific bank begins to receive less long-term systemic risk from its competitors (-27%).

Next, a specific bank – which does not have a penalty announced – receives higher long-term systemic risk from the banking sector (+22%) after a penalty is announced for a competitor. Similarly, after a penalty is settled for the competitor of the specific bank (that does not face the need of its own the settlement), the specific bank faces lower systemic risk exposure with long-term persistence (-17%).

Overall, it can be concluded that systemic risk is higher after the announcement of a penalty and systemic risk is lower after the settlement. Interestingly, this result is related chiefly to the long-term connectedness measure: the transmission of shocks through the system with higher persistence reflects high uncertainty on the market, which affects the beliefs of long-term investors (Baruník and Křehlík, 2018; Baruník and Kočenda, 2019). After the announcement of a penalty, both long-term *from*- and *to*-spillovers increase, indicating an elevated level of long-term connectedness of the system. On the contrary, we see the opposite development after a settlement – both types of spillovers tend to decrease. Thus, the increased level of connectedness after the announcement of a penalty is not permanent.

Finally, some banks were affected by penalties simultaneously. However, from Table A2, it can be observed that such penalty-related dates constitute a minority of cases as the parallel penalties relate solely to the National Settlement in early 2012 or the settlement of several banks in January 2013. Nevertheless, parallel penalties are included in aggregate results when considering the vector of own penalties (and employing both *from*- and *to*-spillovers). On the other hand, parallel penalties are not included when considering the vector of other banks' penalties (for both *from*- and *to*-spillovers) as the vectors are mutually exclusive. The key observation is that the results based on both types of vectors are very similar, which indicates that occurrence of few parallel penalties does not compromise the results.

4.4 Robustness checks

We perform several types of robustness checks to consider: (i) a restricted set of penalties, (ii) different interval bounds for long-term spillovers, and (iii) an extended control sample of financial institutions. Finally, we also employ an alternative test – the paired sign test – to check the robustness of all reported results derived from using the Wilcoxon test.

First, we revisit the baseline estimation but restrict the set of penalties to include only large penalties that exceed the median penalty value in the sample (penalties over 325 million USD). As we show in Table 2, the key findings remain intact. The finding means that our baseline results are invariant to the penalty size and are not driven by relatively small penalties. We further account for the single largest penalty receiver - Bank of America – that received about 40% of the total volume of penalties. For that, we perform estimation on a group of banks without this particular bank. The results are reported in Table 3 and follow the same pattern as those for the full sample of banks. We conclude that our results are robust with respect to the inclusion of the largest penalty receiver.

We further assess whether the results substantially differ if we assume larger relative penalties instead of absolute ones - larger relative penalties are defined with respect to the total assets of a given bank in the quarter preceding the penalty. In this case, the median value is 0.04% (the absolute value of the penalty divided by the total assets of the bank). The results are very similar to those presented for absolute penalties in Table 2; these are not reported but are available upon request. Hence, we conclude that our results are invariant to whether a penalty is measured in absolute or relative terms.

Second, we test the robustness of our results in terms of long-term spillovers, which constitute a vital part of our analysis. 300 days is the boundary for long-term spillovers used in related studies (e.g. Baruník and Křehlík, 2018; Baruník and Kočenda, 2019). Still, it could be argued that over such a period of time, the distribution of the median values of long-term spillovers can change due to other factors than penalties, for example due to earnings announcements. Therefore, we lower the interval boundary to 80 days, which represents approximately one third of a business year and thus sufficiently accounts for quarterly earnings announcements. Further, the 80-days boundary represents the same proportion in length (4:1) with respect to the medium-term spillovers interval (20 days), as is the length proportion (4:1) of the medium-term spillovers boundary to the short-term spillovers boundary (5 days). The results are presented in Table 4. The magnitude of the coefficients with respect to the baseline case presented in Table 1 somewhat decreased as could be expected due to decrease of the long-term boundary from 300 to 80 days. However, the coefficients associated with both 80-days long-term *to*-spillovers and *from*-spillovers are statistically significant and their signs are same as in the baseline case of 300-days long-term spillovers (Table 1). It should be noted that the results for both 300-days and 80-days boundaries do not materially change with respect to being achieved by the Wilcoxon or an alternative sign test. Finally, the robustness of our results is maintained even when we further decrease the boundary towards the 20-days medium-term boundary; results are not reported but readily available upon request. Hence, based on the detailed robustness check, we conclude that the reduction of the length of the long-term spillovers boundary does not affect our baseline results, and that mortgage-related penalties represent key factors affecting risk propagation among banks.

Third, we broaden our baseline sample of 17 banks, that were subject to mortgage-related penalties, with additional 17 other publicly-traded financial firms operating in the U.S. that are not involved in the mortgage business and for which data is available for the period 2008–2017.[21] The control subsample includes not only banks but also other financial institutions because there were not enough banks that are not engaged in the mortgage business with data available for the entire period 2008–2017. Since we strive to assess a set of comparable financial institutions, we limit the diversity of firm types in this control subsample in order to minimize robustness check results to be driven by company type. For that we include only five insurance companies in the subsample because they might somewhat differ from banks in their exposures to systemic risk. However, Chen at al. (2103) assess the interconnectedness between the U.S. banks and insurers and empirically confirm that insurers do not create significant systemic risk for banks.

Financial firms in the control subsample did not receive a penalty related to mortgage or foreclosure and constitute a suitable control group. We again consider all penalty announcement or settlement dates linked to the 17 banks from our baseline sample. Then we inspect *from-* and *to*-spillovers after the announcement and settlement dates for the control group of financial institutions. Our prior is that since additional financial institutions in control group are not engaged in the mortgage business, they will not discharge risk into the industry and *to*-spillovers might not materialize; on the other hand, those financial institutions might be exposed to the risk due to penalties from the banking industry. The results are presented in Table 5 and provide a rather clear picture. The financial firms unrelated to mortgage business (control group) are exposed to the long-term risk (*from*-spillovers) coming from the system of financial institutions that contains also 17 banks from our baseline sample that did receive

[21] The control subsample includes following companies: American Express Company (AXP), The Bank of New York Mellon Corporation (BK), MetLife, Inc. (MET), Mizuho Financial Group, Inc. (MFG), Capital One Financial Corporation (COF), State Street Corporation (STT), Sun Life Financial Inc. (SLF), Northern Trust Corporation (NTRS), KB Financial Group Inc. (KB), Torchmark Corporation (TMK), Western Alliance Bancorporation (WAL), Sterling Bancorp (STL), American Equity Investment Life Holding Company (AEL), Hilltop Holdings Inc. (HTH), Berkshire Hills Bancorp, Inc. (BHLB), Banco Latinoamericano de Comercio Exterior, S.A (BLX), and Citizens, Inc. (CIA). The control subsample includes not only banks but also other financial institutions because there were not enough banks that are not engaged in the mortgage business with data available for the entire period 2008–2017. In other words, limited availability of the relevant stock price data on banks operating in the U.S. precludes an analysis when one could compare how the announcement of mortgage-related regulatory penalties on a specific bank generates spillovers on other banks that are likely to be subject to similar penalties due to their past mortgage-related lending practices compared to other banks that are not likely to face such penalties.

mortgage-related penalties; long-term coefficients associated with *from*-spillovers are statistically significant. However, non-mortgage-related financial firms do not increase long-term systemic risk (*to*-spillovers) after an announcement of a mortgage-related penalty; the long-term coefficients associated with *to*-spillovers are small and statistically insignificant. On the other hand, the contribution of the non-mortgage-related financial firms to long-term systemic risk is somewhat lower after a settlement is announced for a bank that received a penalty related to mortgages or foreclosures. The finding points to an asymmetric reaction of non-mortgage-related financial firms to the announcement and settlement of mortgage-related penalties. Specifically, non-mortgage-related financial firms do not react to original shocks (penalty announcements) but participate in the systemic risk decrease once the cases are closed. Overall, the findings can be summarized in a way that (i) non-mortgage financial institutions are indeed affected by the turmoil of financial institutions active in the mortgage business caused by mortgage-related penalties but (ii) non-mortgage financial institutions do not contribute to the amplification of the original shock on their own; still they might play some role in the lowering of systemic risk after settlements.

Finally, when we compare results based on the paired sign test (right part of panels in Tables 1-5) and those based on the Wilcoxon test (left part of panels in Tables 1-5) we detect that a few results based on the sign test exhibit lower statistical significance. However, in terms of the outcome the sign-test results are equal to those based on the Wilcoxon test.

4.5 Effectiveness of penalties

In our analysis we focus on the – unintended – impact of penalties on systemic risk. As a complementary issue we explore the effectiveness of penalties in a sense of whether they reduce misconduct (and thus future penalties). The observation from our dataset is that there is a large time lag most of the time between misconduct and penalty. This raises doubts on whether penalties might help in changing banks' behavior. For individual human beings, social psychology teaches that for punishment to being effective, there should be a clear link between crime and punishment - we explore the pattern for financial institutions. Given that many banks in your sample received multiple fines spread out over time, we assess how the timing and severity of the penalties evolve over time. If effective, both the penalties' frequency and their magnitude is expected to decline.

For each bank in our sample, we present the frequency of penalties and their magnitude graphically in Figure A1 (panels a and b). In terms of frequency, individual series for each bank provide very limited evidence and preclude any firm statement. However, the evidence shows that magnitude of penalties increases over time for the majority of banks. Four banks represent exception as penalties levied on them decline with time: Wells Fargo, Morgan Stanley, PNC, and U.S. Bancorp. In sum, the graphical evidence of the time series of penalties' magnitude for individual banks shows that the trend of magnitude (of penalties) is increasing for the majority of banks even though the individual levels are quite diverse. In order to assess the pattern and trend of the penalties' occurrence, we employ a nonparametric test for trend across ordered groups introduced by Cuzick (1985) that is an extension of the Wilcoxon rank-sum test. The test is able to handle our situation in which a variable (magnitude of penalty) is measured for individuals (banks) in three or more (ordered) groups and a non-parametric test for trend across these groups is desired. The detailed results of the test are presented in Table A3, based on which we conclude that the null hypothesis of no trend across ordered groups (of bank penalties) is rejected.

The existing increasing trend in magnitude of penalties over the time span under research suggests that penalties are not effective in a sense that misconduct would decline after penalties have been levied. On the contrary, for majority of banks after a misconduct occurs, it is of more severe nature than before as indicated by the magnitude of a penalty. However, mortgage-related penalties are not exactly the same in nature as penalties levied due to misconduct occurring during more or less standard banking operations. Mortgage-related penalties likely reflect accumulated problems and misconduct whose effects show with a lag; this is documented in our connectedness analysis. As such, mortgage-related penalties reflect a different type of misconduct and our time-span might be too short to reveal a pattern that could help to assess true effectiveness of penalties in general. For that, our results should be taken with a caution and serve more as evidence complementing previous findings how mortgage-related penalties affect systemic risk in the banking industry.

5. Conclusions

In this study, we analyze the link between mortgage-related regulatory penalties levied on banks and the level of systemic risk in the U.S. banking industry. It is generally acknowledged that the subprime mortgage crisis evolved into a global financial crisis. While the main objective of any penalty is arguably to correct the harm caused by a bank's behavior, it can be argued that such action by oversight and enforcement authorities can also destabilize the banking sector if the impact of the penalty travels across the sector and also affects innocent banks. In this sense, our paper contributes to the recent wave of interest in how banks respond to penalties within the industry as we hypothesize that systemic risk might evolve in a different way after penalty announcement or penalty settlement.

We find that after the possibility of a penalty is first publicly announced, long-term systemic risk in the U.S. banking sector tends to increase, indicating high uncertainty among investors with respect to longer investment horizons. Short- and medium-term systemic risk does not play a major role, which is in line with Koester and Pelster (2018) who show that penalties do not significantly affect banks' contribution to systemic risk. We believe that the difference in the above evidence is driven by the frequency-decomposition approach that allows to account for differences in investment horizons. Further, a settlement with regulatory authorities leads to a decrease in the long-term connectedness in the banking industry. This latter pattern is in line with Flore et al. (2021) and might be interpreted as a relief that financial markets experience once the enforcement process is over. Interestingly, we show the same pattern in terms of the contribution/exposure of a given bank to systemic risk regardless if this bank had a penalty announced/settled or one of its competitors did. Thus, rather than having the desired corrective impact on a particular financial institution, the penalty can lead to contagion among banks that increases systemic risk, potentially making the banking sector less stable and more vulnerable. In this sense, our results can be compared to those of Pino and Sharma (2019) who study the contagion effect in the U.S. banking sector in the period from 2001 to 2012 and uncover presence of bank contagion since 2003; the contagion became more pronounced before the onset of the global financial crisis and remained present until the end of the sample period.

In terms of robustness checks, we find that our baseline results are not driven by relatively smaller penalties or interval boundaries for the long-term risk spillovers. We also perform a robustness exercise to demonstrate that financial institutions not engaged in the mortgage business do not discharge higher (lower) long-term spillovers after an announcement (settlement) related to mortgage or foreclosure penalty levied on their competitors. Our results are also robust with respect to testing procedures used.

Any propagation of risk affects investment decisions, and our results show that a risk impact due to penalties is primarily reflected in the behavior of investors with longer investment horizons. Thus, our results offer implications for portfolio selection and investment strategies on financial markets since asset pricing in the frequency domain allows to capture the price of risk at different frequencies, e.g. different investment horizons. Hence, our results are in line and support arguments of Dew-Becker and Giglio (2016) who demonstrate importance of asset pricing in the frequency domain.

Further, our analysis is relevant both to authorities imposing the penalties and those in charge of financial stability. Specifically, it raises the question on the apparent trade-off there may be between microprudential and macroprudential regulation and supervision. From the microprudential perspective, after the global financial crisis individual banks have faced numerous legal settlements that have frequently resulted in substantial penalties levied by regulatory bodies. These penalties affect both performance and valuation of the penalized banks since they represent direct costs and lower their profitability (Köster and Pelster, 2017) and reputation (Fiordelisi et al., 2014). However, from a macroprudential perspective, penalties levied on a specific individual bank also do impact other banks in the industry, which results in increased systemic risk. In addition, the risk tends to accumulate over the time as our long-term evidence shows.

The originally intended objective of the penalties, which is to correct the social harm inflicted by an individual bank (microprudential dimension), is then in contrast with potential danger related to the stability of the entire banking sector (macroprudential dimension). Regulatory bodies should take the macroprudential dimension into account and scrutinize the penalties and risk associated with misconduct because the heightened risk in the banking industry comes as an unpleasant side-effect of imposed penalties.

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Table 1: Aggregated baseline results

Vector of penalties	Type of a date	Wilcoxon test						Paired Sign test					
		To-spillovers			*From*-spillovers			*To*-spillovers			*From*-spillovers		
		Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term
Own penalties	Announcement	-1%[a]	-3%[a]	+44%[a]	-1%[b]	-1%[a]	+84%[a]	-1%[b]	-3%[a]	+38%[a]	-1%[a]	-1%[b]	+115%[a]
	Settlement	0%	0%	-26%[a]	0%	0%	-27%[a]	0%	+1%	-26%[a]	0%	+1%	-23%[c]
Other banks' penalties	Announcement	0%[c]	-1%[a]	+11%[a]	0%	-1%[a]	+22%[a]	0%	-1%[a]	+6%	0%	-1%[a]	+8%[c]
	Settlement	0%	-1%[b]	-17%[a]	0%	-1%[a]	-17%[a]	0%	0%	-16%[a]	0%	-1%[a]	-16%[a]

Note: We assess whether the magnitude of spillovers before and after the announcement/settlement date differs by testing the null hypothesis that the median difference of spillovers is equal to zero. The numbers in the table show the percentage change in the value of the median difference of the spillovers before and after the announcement or settlement. When null hypothesis is rejected, symbols of a, b, and c denote statistical significance of the median difference at the 1%, 5%, and 10% levels, respectively. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

Table 2: Large penalties in absolute terms (robustness check)

Vector of penalties	Type of a date	Wilcoxon test						Paired Sign test					
		To-spillovers			*From*-spillovers			*To*-spillovers			*From*-spillovers		
		Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term
Own penalties	Announcement	0%[b]	-3%[c]	+48%[a]	0%	-1%[a]	+87%[a]	0%	-3%[c]	+43%[a]	0%[b]	-1%[c]	+123%[a]
	Settlement	0%	-1%	-23%[c]	0%	+0%	-15%	0%	-2%	-26%	0%	0%	-7%
Other banks' penalties	Announcement	0%	-1%	+16%[a]	0%[c]	-1%[b]	+26%[a]	0%	-1%	+10%	0%	-1%[b]	+8%
	Settlement	0%	-1%[c]	-14%[a]	0%	-2%[a]	-15%[a]	0%	-1%	-11%[a]	0%	-1%[a]	-13%[a]

Note: We assess whether the magnitude of spillovers before and after the announcement/settlement date differs by testing the null hypothesis that the median difference of spillovers is equal to zero. The numbers in the table show the percentage change in the value of the median difference of the spillovers before and after the announcement or settlement. When null hypothesis is rejected, symbols of a, b, and c denote statistical significance of the median difference at the 1%, 5%, and 10% levels, respectively. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

Table 3: Sample of banks without Bank of America (robustness check)

Vector of penalties	Type of a date	Wilcoxon test						Paired Sign test					
		To-spillovers			*From*-spillovers			*To*-spillovers			*From*-spillovers		
		Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term
Own penalties	Announcement	-1% [b]	-2% [b]	+55% [a]	-1% [b]	-2% [a]	+84% [a]	-1%	-2% [b]	+38% [a]	-1% [b]	-1% [b]	+112% [a]
	Settlement	0%	0%	-23% [a]	0%	0%	-27% [b]	0%	+1%	-16% [a]	0%	+1%	-23% [c]
Other banks' penalties	Announcement	0%	-1% [b]	+21% [a]	0%	-1% [a]	+36% [a]	0%	-1% [b]	+11% [c]	0%	-1% [b]	+26% [a]
	Settlement	0%	-1% [b]	-17% [a]	0%	-1% [a]	-18% [a]	0%	0%	-16% [a]	0%	-1% [c]	-16% [a]

Note: We assess whether the magnitude of spillovers before and after the announcement/settlement date differs by testing the null hypothesis that the median difference of spillovers is equal to zero. The numbers in the table show the percentage change in the value of the median difference of the spillovers before and after the announcement or settlement. When null hypothesis is rejected, symbols of a, b, and c denote statistical significance of the median difference at the 1%, 5%, and 10% levels, respectively. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

Table 4: 80-days boundary for long-term spillovers (robustness check)

Vector of penalties	Type of a date	Wilcoxon test		Paired Sign test	
		To-spillovers	*From*-spillovers	*To*-spillovers	*From*-spillovers
		Long-term			
Own penalties	Announcement	+16% [a]	+24% [a]	+10% [a]	+20% [a]
	Settlement	-11% [a]	-14% [a]	-11% [a]	-8% [b]
Other banks' penalties	Announcement	+4% [b]	+5% [a]	+4% [a]	+4% [b]
	Settlement	-3% [a]	-5% [a]	-2%	-1%

Note: We assess whether the magnitude of spillovers before and after the announcement/settlement date differs by testing the null hypothesis that the median difference of spillovers is equal to zero. The numbers in the table show the percentage change in the value of the median difference of the spillovers before and after the announcement or settlement. When null hypothesis is rejected, symbols of a, b, and c denote statistical significance of the median difference at the 1%, 5%, and 10% levels, respectively. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

Table 5: Control group of financial firms unrelated to mortgages and foreclosures (robustness check)

Vector of penalties	Type of a date	Wilcoxon test						Paired Sign test					
		To-spillovers			*From*-spillovers			*To*-spillovers			*From*-spillovers		
		Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term	Short-term	Medium-term	Long-term
All mortgage- and foreclosure-related penalties	Announcement	0%	-3%	+5%	0%	-4% [c]	+10% [b]	0%	0% [a]	0%	0%	-4% [a]	+6% [a]
	Settlement	0%	0%	-6% [c]	0%	-2%	-8% [a]	0%	0% [a]	-6% [a]	0% [c]	0% [a]	-4% [a]

Note: We assess whether the magnitude of spillovers before and after the announcement/settlement date differs by testing the null hypothesis that the median difference of spillovers is equal to zero. The numbers in the table show the percentage change in the value of the median difference of the spillovers before and after the announcement or settlement. When null hypothesis is rejected, symbols of a, b, and c denote statistical significance of the median difference at the 1%, 5%, and 10% levels, respectively. Positive (negative) sign indicates increase (decrease) of volatility spillovers.

Figure 1: Gross volumes of penalties to banks in the United States (2010–2016)

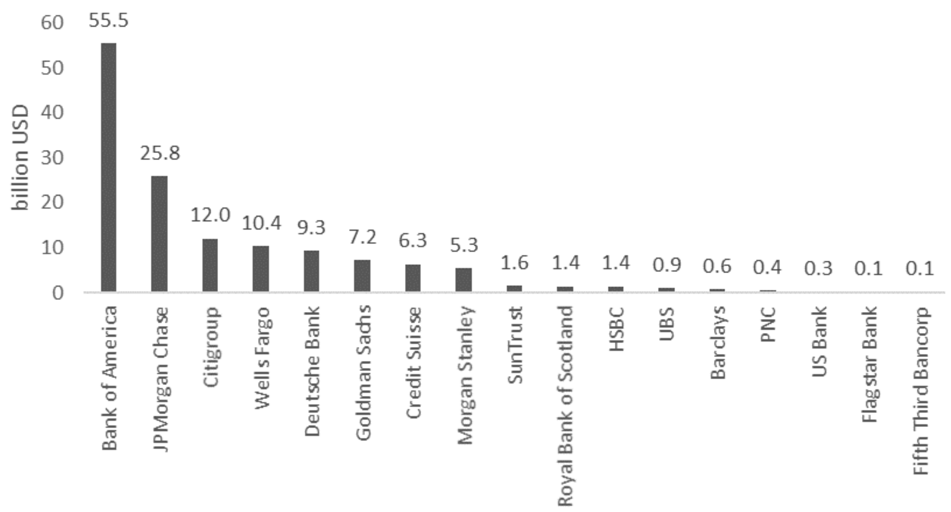


Figure 2: Yearly distribution of penalties to banks in the United States (2010–2016)

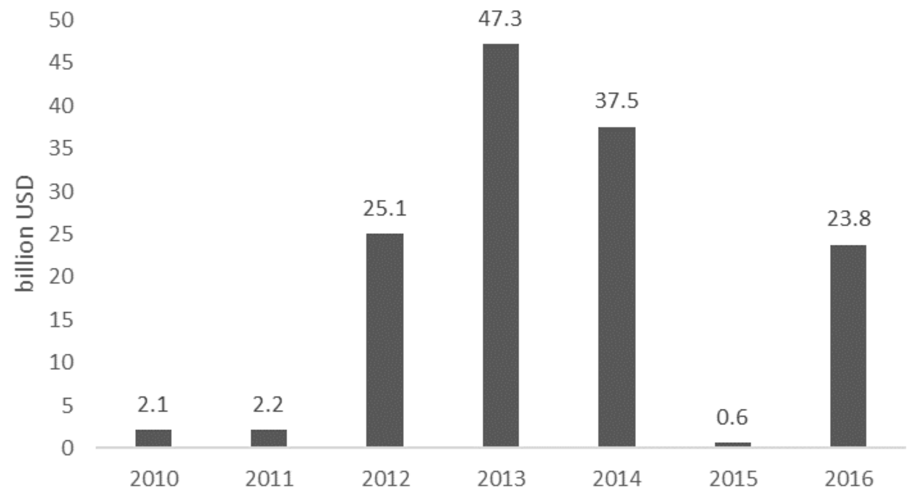


Figure 3: Size of penalties (2010–2016)

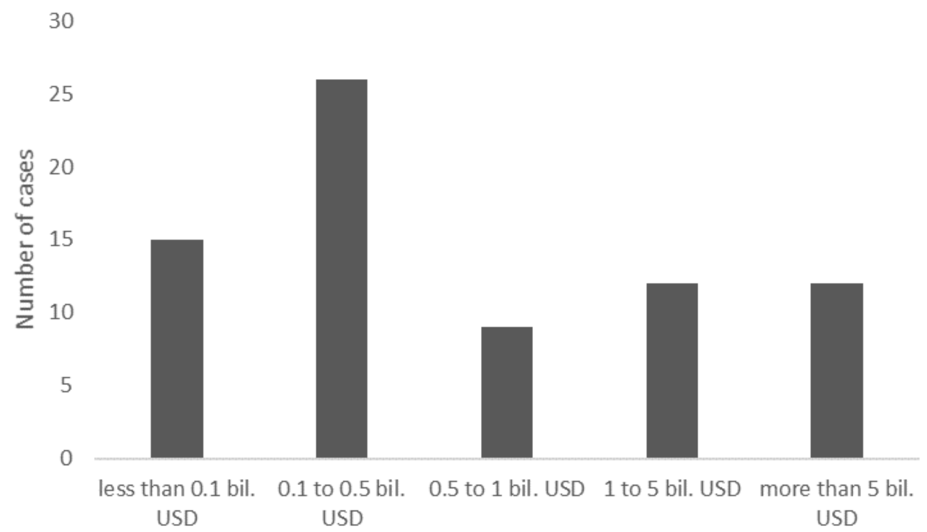


Figure 4: Length of the enforcement process (2010–2016)

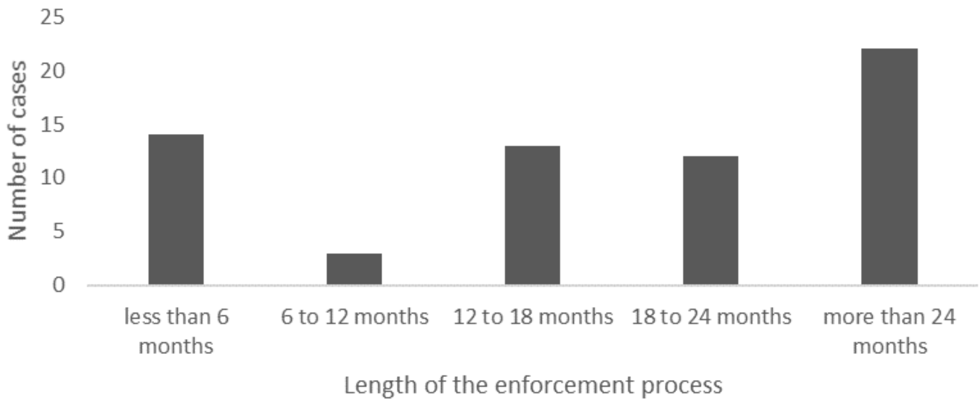


Figure 5: Test for the effect of penalties (in days)

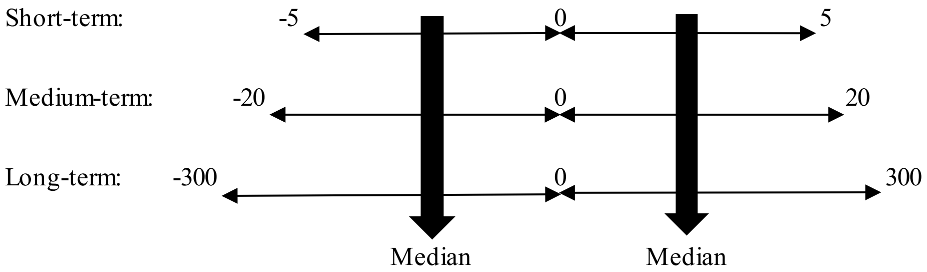
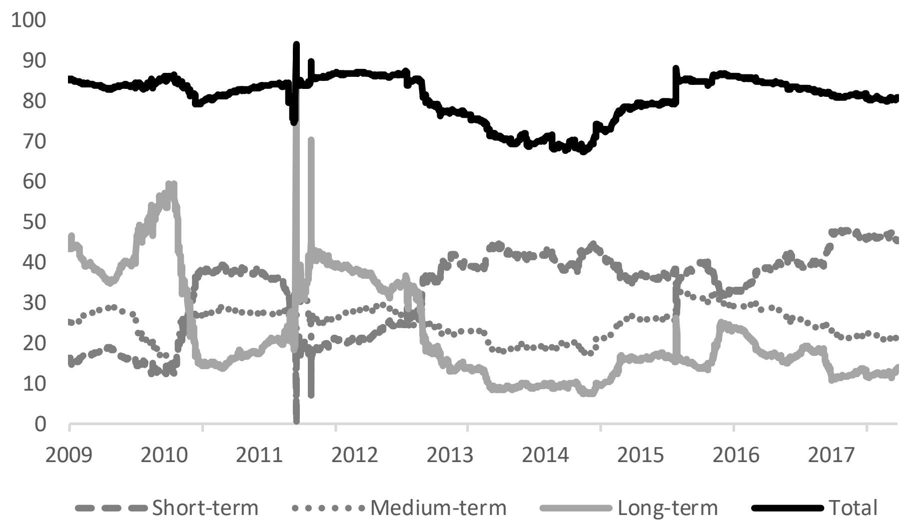


Figure 6: Total and frequency connectedness (2009–2017)



Appendix

Table A1: Summary statistics of the daily volatility data

Bank	Ticker	Mean	Median	St. dev.	Skewness	Kurtosis
Bank of America	BAC	0.314	0.216	0.322	4.044	25.210
Barclays	BCS	0.251	0.175	0.261	4.737	35.700
Citigroup	C	0.321	0.209	0.373	4.912	37.321
Credit Suisse	CS	0.208	0.155	0.184	3.802	21.657
Deutsche Bank	DB	0.231	0.177	0.186	3.211	15.612
Fifth Third Bancorp	FITB	0.351	0.220	0.432	5.007	38.245
Flagstar Bank	FBC	0.531	0.340	0.565	3.931	27.998
Goldman Sachs	GS	0.247	0.183	0.229	4.747	35.093
HSBC	HSBC	0.140	0.106	0.117	3.419	17.612
JPMorgan Chase	JPM	0.253	0.180	0.238	3.495	16.786
Morgan Stanley	MS	0.330	0.233	0.373	7.163	85.800
PNC	PNC	0.256	0.174	0.272	5.419	56.983
Royal Bank of Scotland	RBS	0.260	0.184	0.285	7.035	90.872
SunTrust	STI	0.326	0.220	0.333	3.784	20.847
UBS	UBS	0.213	0.153	0.196	3.418	16.745
U.S. Bancorp	USB	0.233	0.159	0.242	4.075	24.704
Wells Fargo	WFC	0.262	0.172	0.277	3.426	14.886

Note: The table contains annualized daily percentage volatility data.

Table A2a: List of penalties (2010–2016)

Announcement	Settlement	Bank	Regulator	Value (mil. USD)	Announcement	Settlement	Bank	Regulator	Value (mil. USD)
n/a	2010-06-25	Morgan Stanley	SA/AG	102.7	2011-04-05	2013-01-07	JPMorgan Chase	COMP	1958
2010-04-16	2010-07-15	Goldman Sachs	SEC	550	2011-04-05	2013-01-07	PNC	COMP	180
2009-05-28	2010-07-29	Citigroup	SEC	75	2011-04-05	2013-01-07	US Bancorp	COMP	208
2010-12-15	2010-12-31	Bank of America	FMCC	1350	2011-04-05	2013-01-07	Wells Fargo	COMP	1991
2010-12-15	2011-01-03	Bank of America	FNMA	1520	2011-09-02	2013-01-07	Bank of America	FNMA	11600
2011-04-04	2011-04-05	Wells Fargo	SEC	11	2011-04-05	2013-01-16	Goldman Sachs	FED	330
2011-04-14	2011-06-21	JPMorgan Chase	SEC	153.6	2011-04-05	2013-01-16	Morgan Stanley	FED	227
2011-09-15	2011-10-19	Citigroup	SEC	285	2011-04-05	2013-01-18	HSBC	COMP	249
2011-03-23	2011-11-15	Citigroup	NCUA	20.5	2011-03-23	2013-03-29	Bank of America	NCUA	165
2011-03-23	2011-11-15	Deutsche Bank	NCUA	145	2011-09-02	2013-05-28	Citigroup	FHFA	250
n/a	2011-11-28	Royal Bank of Scotland	SA/AG	52	2011-09-02	2013-07-01	Citigroup	FNMA	968
2011-04-13	2012-02-09	Wells Fargo	HUD	5350	2011-07-28	2013-07-23	UBS	FHFA	885
2011-04-13	2012-02-09	Citigroup	HUD	2205	2011-03-23	2013-07-31	UBS	SEC	50
2011-04-13	2012-02-09	JPMorgan Chase	HUD	5290	n/a	2013-09-10	Barclays	SA/AG	36.1
2011-04-13	2012-02-09	Bank of America	HUD	11820	2011-09-02	2013-09-25	Citigroup	FMCC	395
2012-02-29	2012-08-14	Wells Fargo	SEC	6.5	2011-09-02	2013-09-27	Wells Fargo	FMCC	869
2012-02-29	2012-11-16	Credit Suisse	SEC	120	2011-04-13	2013-10-10	SunTrust	HUD	968
2012-02-29	2012-11-16	JPMorgan Chase	SEC	296.9	2012-06-07	2013-10-10	SunTrust	FNMA	373
2011-04-05	2013-01-07	SunTrust	FED	163	2012-06-07	2013-10-10	SunTrust	FMCC	65
2011-04-05	2013-01-07	Bank of America	COMP	2886	2011-09-02	2013-10-25	JPMorgan Chase	FNMA	670
2011-04-05	2013-01-07	Citigroup	COMP	794	2011-09-02	2013-10-25	JPMorgan Chase	FHFA	4000

Source: Financial Times, Wall Street Journal, Factiva; SA/AG = state attorney / attorney general, SEC = Securities and Exchange Commission, FMCC = Federal Home Loan Mortgage Corp. (Freddie Mac), FNMA = Federal National Mortgage Association (Fannie Mae), NCUA = National Credit Union Administration, HUD = Department of Housing and Urban Development, FED = Federal Reserve, COMP = Office of the Comptroller of the Currency, FHFA = Federal Housing Finance Agency; DofJ = Department of Justice, FDIC = Federal Deposit Insurance Corporation.

Table A2b: List of penalties (2010–2016)

Announcement	Settlement	Bank	Regulator	Value (mil. USD)	Announcement	Settlement	Bank	Regulator	Value (mil. USD)
2011-09-02	2013-10-25	JPMorgan Chase	FMCC	480	2011-09-02	2014-03-21	Credit Suisse	FHFA	885
2011-09-02	2013-11-06	Flagstar Bank	FNMA	121.5	2011-09-02	2014-03-26	Bank of America	FHFA	9330
2011-09-02	2013-11-06	Wells Fargo	FHFA	335.23	2011-09-02	2014-04-24	Barclays	FHFA	280
2013-09-23	2013-11-19	JPMorgan Chase	SA/AG	298.9	2011-09-02	2014-06-19	Royal Bank of Scotland	FHFA	100
2013-09-23	2013-11-19	JPMorgan Chase	SA/AG	19.7	2011-09-02	2014-06-30	HSBC	DofJ	10
2013-09-23	2013-11-19	JPMorgan Chase	DofJ	6000	2014-04-25	2014-07-14	Citigroup	DofJ	7000
2013-09-23	2013-11-19	JPMorgan Chase	FDIC	515.4	2014-02-25	2014-07-24	Morgan Stanley	SEC	275
2013-09-23	2013-11-19	JPMorgan Chase	FHFA	4000	2014-02-25	2014-08-20	Bank of America	DofJ	16650
2013-09-23	2013-11-19	JPMorgan Chase	SA/AG	100	2011-09-02	2014-08-21	Goldman Sachs	FHFA	1200
2013-09-23	2013-11-19	JPMorgan Chase	SA/AG	34.4	2011-09-02	2014-09-12	HSBC	FHFA	550
2013-09-23	2013-11-19	JPMorgan Chase	NCUA	1400	n/a	2015-10-06	Fifth Third Bancorp	DofJ	85
2013-09-23	2013-11-19	JPMorgan Chase	SA/AG	613.8	n/a	2015-10-19	Barclays	NCUA	325
2013-11-06	2013-11-22	Fifth Third Bancorp	FMCC	26	n/a	2015-12-10	Morgan Stanley	NCUA	225
n/a	2013-12-10	US Bancorp	FMCC	56	2015-06-05	2016-01-15	Goldman Sachs	DofJ	5100
2013-08-01	2013-12-12	Bank of America	SEC	131	n/a	2016-02-02	Morgan Stanley	FDIC	63
n/a	2013-12-12	PNC	FMCC	89	2015-06-05	2016-02-04	Wells Fargo	DofJ	1200
2011-09-02	2013-12-20	Deutsche Bank	FHFA	1925	2015-06-05	2016-02-05	HSBC	DofJ	470
2011-09-02	2013-12-27	Flagstar Bank	FMCC	10.75	2015-06-05	2016-02-11	Morgan Stanley	DofJ	3200
2011-09-02	2013-12-30	PNC	FNMA	140	n/a	2016-09-28	Royal Bank of Scotland	NCUA	1100
2011-09-02	2013-12-30	HSBC	FNMA	83	n/a	2016-10-03	Royal Bank of Scotland	SA/AG	120
2011-09-02	2013-12-30	Wells Fargo	FNMA	591	2015-06-05	2016-12-23	Credit Suisse	DofJ	5300
2011-09-02	2014-02-04	Morgan Stanley	FHFA	1250	2016-09-16	2016-12-23	Deutsche Bank	DofJ	7200

Source: Financial Times, Wall Street Journal, Factiva; SA/AG = state attorney / attorney general, SEC = Securities and Exchange Commission, FMCC = Federal Home Loan Mortgage Corp. (Freddie Mac), FNMA = Federal National Mortgage Association (Fannie Mae), NCUA = National Credit Union Administration, HUD = Department of Housing and Urban Development, FED = Federal Reserve, COMP = Office of the Comptroller of the Currency, FHFA = Federal Housing Finance Agency; DofJ = Department of Justice, FDIC = Federal Deposit Insurance Corporation

Table A3: Detailed results for the non-parametric test for trend

Bank	Number of observations (penalties)	Sum of ranks
Bank of America	9	561
Barclays	3	77
Citigroup	9	455.5
Credit Suisse	3	146
Deutsche Bank	3	165
Fifth Third Bancorp	2	16
Flagstar Bank	2	103
Goldman Sachs	4	216
HSBC	5	133.5
JPMorgan Chase	16	807.5
Morgan Stanley	7	247
PNC	3	63
Royal Bank of Scotland	4	98
SunTrust	4	131.5
UBS	2	59.5
U.S. Bancorp	2	36
Wells Fargo	8	425.5

Note: We perform a Wilcoxon-type test for trend designed by Cuzick (1985). The null hypothesis of no trend across ordered groups is rejected based on the *p*-value of 0.07. This translates into evidence that there is an increasing trend in magnitudes of penalties across banks during the time-span under research.

Figure A1a: Time series of penalties for individual banks

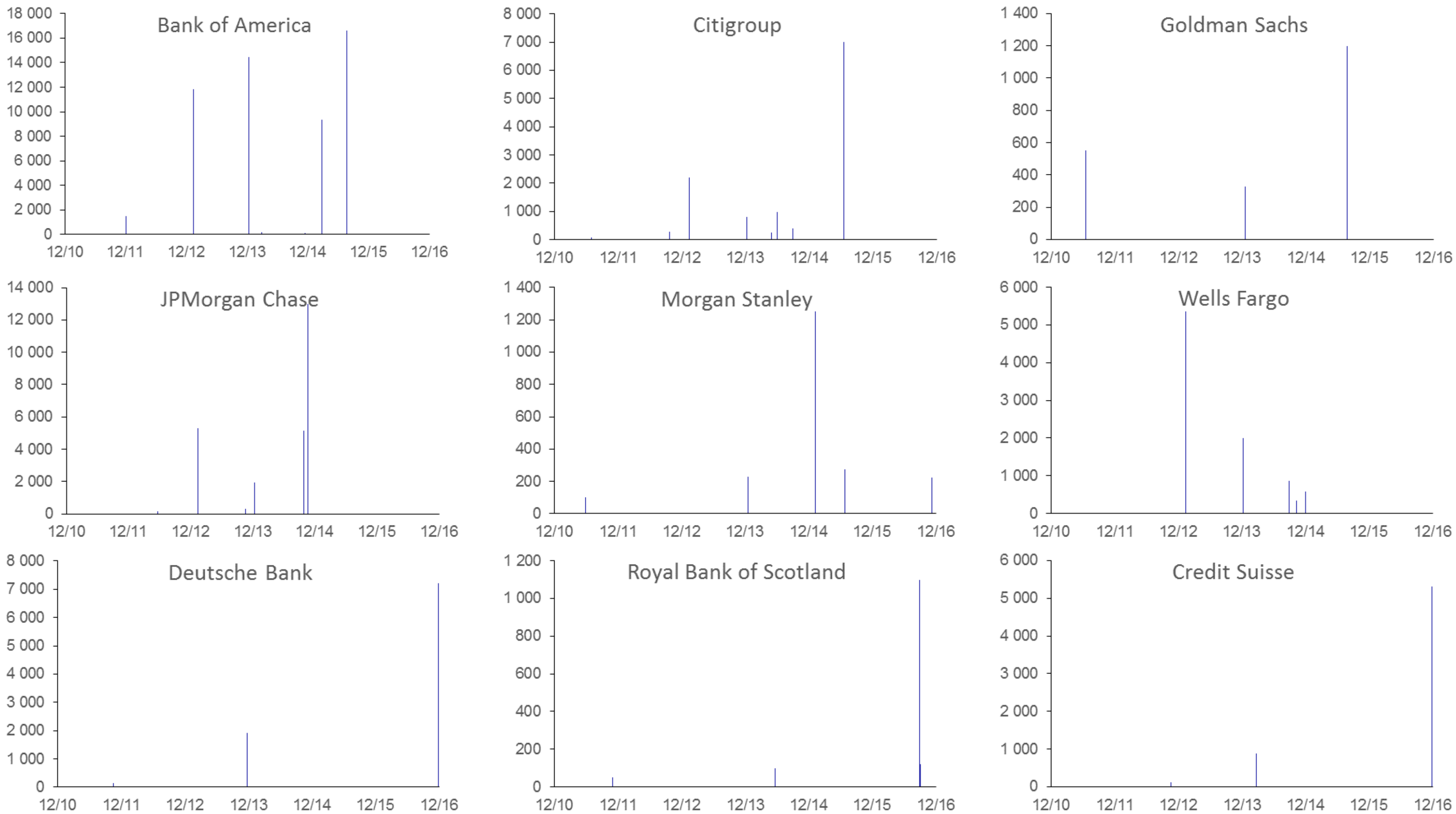


Figure A2b: Time series of penalties for individual banks