Risk Measure Inference

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Abstract

We propose a bootstrap-based test of the null hypothesis of equality of two firms' conditional Risk Measures (RMs) at a single point in time. The test can be applied to a wide class of conditional risk measures issued from parametric or semi-parametric models. Our iterative testing procedure produces a grouped ranking of the RMs which has direct application for systemic risk analysis. A Monte Carlo simulation demonstrates that our test has good size and power properties. We propose an application to a sample of U.S. financial institutions using ΔCoVaR, MES, and SRISK, and conclude that only SRISK can be estimated with enough precision to allow for meaningful ranking.

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1 Introduction

Financial risk management is fundamentally based on the comparison of risk measures across different assets, portfolios, or financial institutions. Examples include the comparison of total risk of two portfolios measured by their volatility, of tail risk measured by the Value-at-Risk (VaR) or the Expected Shortfall (ES), of systematic risk measured by the beta, or the comparison of systemic risk scores of two financial institutions and many others. Comparing unconditional risk measures can be done using a variety of parametric or non-parametric tests. However, most risk measures are expressed conditionally on an information set and the corresponding forecasts are generally issued from a dynamic parametric or semi-parametric model. For instance, a (M-)GARCH model can be used to produce conditional VaR or ES forecasts, or a DCC can be used to estimate a dynamic conditional beta (Engle, 2012). As a consequence, the conditional distribution of the estimated risk measure is generally unknown and depends on which estimation procedure is used.

In this paper, we propose a general testing methodology that takes into account estimation uncertainty to statistically test for equality of conditional risk measures for different assets, portfolios or firms at a *single point* in time. We propose two types of tests. The first one is a bootstrap-based comparison test of two risk measures. This test can be applied to a wide class of conditional risk measures and (semi-)parametric models. For example, it can be used to compare conditional measures of volatility, VaR, or ES for two assets or two portfolios at a particular time. It can also be used to test the relative level of systemic risk for two banks for a given day. Additionally, it can be applied in order to test the equality of two conditional risk measures (for instance two VaRs) issued from two different models (say, GARCH and RiskMetrics) for the same asset or the same portfolio.

The second test is a procedure that allows one to allocate a large set of assets, portfolios or firms into groups of elements that are statistically indistinguishable from each other in terms of riskiness, given a conditional risk measure. This method, inspired by the Model Confidence Set (MCS) of Hansen et al. (2011) can be applied to any type of risk measure. However, it is particularly well suited to identify clusters of Global Systemically Important Banks (G-SIBs) that have similar contribution to systemic risk. This is exactly what the

Financial Stability Board (FSB) does each year when it publishes its five-bucket list of G-SIBs in order to set extra capital requirement (Basel Committee on Banking Supervision, 2013). Doing so, the FSB recognizes the inevitable estimation uncertainty in their estimated riskiness and do not fully rely on point estimates.

Many measures of systemic risk have been proposed in the academic literature over the past years, the most well-known being the *Marginal Expected Shortfall* (MES) and the *Systemic Expected Shortfall* (SES) of Acharya et al. (2010), the *Systemic Risk Measure* (SRISK) of Acharya et al. (2012) and Brownlees and Engle (2015), and the *Delta Conditional Value-at-Risk* (ΔCoVaR) of Adrian and Brunnermeier (2014). These measures are designed to summarize the systemic risk contribution of each financial institution into a single figure. The appeal is that there exists a ranking of financial institutions according to their systemic risk measures, that can be displayed on real time with a daily or weekly frequency (see for instance the V-Lab website of the Volatility Institute, NYU Stern). However, claiming that firm A is more risky than firm B because its systemic risk measure is higher, implies that risk is estimated without error. This is certainly not the case, since these measures typically rely on dynamic parametric models that require sophisticated estimation techniques. Even if the model is correctly specified, replacing the true parameters of the dynamic model by their estimates has an impact on the estimation accuracy of the risk measure itself. Indeed, there is convincing evidence that the signal produced by the systemic risk measures are not reliable and are affected by estimation risk (e.g. Danielsson et al., 2011). If this is taken into account it is unlikely that one can discern such an absolute ranking.

To the best of our knowledge, there is only one alternative test for equality of systemic risk measures. Castro and Ferrari (2014) propose a method for testing whether two firms differ in terms of their ΔCoVaR. However, their approach is specific to ΔCoVaR and to the linear quantile regression. In contrast, our method is more general as it works with any conditional risk measure (SRISK, SES, VaR, ES, etc.) and is not specific to any particular estimation method.

Our study is related to the literature on estimation risk in dynamic risk models. The estimation risk is generally assessed through asymptotic confidence intervals. For instance,

Chan et al. (2007) and Francq and Zakoïan (2015) derive the asymptotic confidence intervals for the conditional VaR estimator in the specific context of heavy-tailed GARCH models. Gouriéroux and Zakoïan (2013) consider a different approach based on an Estimation adjusted VaR (EVaR). Alternatively, several papers propose resampling methods to carry out inference on risk measures. Hartz et al. (2006) introduce a bootstrap approach to correct the estimation bias and to improve the VaR forecasting ability of the normal-GARCH model. Christoffersen and Gonçalves (2005) and Pascual et al. (2006) propose a more general approach to assess the estimation error of VaR and ES forecasts. Their resampling techniques allow the computation of bootstrap-based confidence intervals around the risk forecasts issued from historical simulation methods or GARCH-type models. Finally, Escanciano and Olmo (2010, 2011) implement robust backtests for the VaR, using resampling methods.

Unlike previous studies, we do not focus on the inference for a *single* financial asset. Our testing strategy is designed to *compare* the riskiness of two or more assets, given the estimation risk of the corresponding risk measures. In that sense, our study can also be related to the literature on forecast comparison tests (Diebold and Mariano, 1995; Hansen, 2005). However, our null hypothesis and therefore our test differ in some important ways. First, in most cases, we do not compare two models, but the riskiness of two assets, portfolios, or financial institutions, measured with the same measure and the same model. Second, we do not compare a forecast to an *ex-post* observation. Finally, and most importantly, we test for equality of two or more conditional risk measures at time t, for which we have only one estimate. We do not test the equality of these measures over the full sample. In the case where our test is used to compare the forecasts of the same risk measure issued from two alternative models, there are also some similarities with the literature devoted to the volatility forecast comparison (Hansen and Lunde, 2006; Patton, 2011). However, our comparison test does not require the use of a proxy variable since it is not designed to determine the 'best' model.

The paper is structured as follows. Section 2 introduces a general definition for the conditional risk measures and propose some examples. Section 3 presents two types of tests: a comparison test of two risk measures and a bucketing procedure. The bucketing

procedure is clearly a multiple testing problem, and as such it is important to control the number of false rejections. For that, we consider two alternative methods based on the False Discovery Rate (FDR) and the Family Wise Error Rate (FWE). Section 4 discusses the bootstrap implementation. Section 5 presents some Monte Carlo simulation results for both tests. Results suggest that the bucketing procedure has desirable properties, i.e. appropriate size and high power. In Section 6, we propose an empirical application for three systemic risk measures, namely the MES, the SRISK and the ΔCoVaR, based on a panel of 94 US financial institutions. Section 7 concludes and suggests extensions.

2 Framework and Risk Measure Definitions

Consider an asset, a portfolio, or a firm indexed by i and a $\mathcal{F}_{i,t-1}$-conditional risk measure (denoted RM) issued from a dynamic parametric or semi-parametric model, where $\mathcal{F}_{i,t-1}$ denotes the information set available at time $t-1$. Formally, we define RM at time t as follows:

$$RM_{i,t} = f_i\left(\theta_i, \omega; X_{i,t-1}\right), \tag{1}$$

where $f_i\left(.\right)$ denotes a functional form that depends on (i) the risk measure itself (for instance, the VaR) and (ii) the parametric or semi-parametric model used to produce the corresponding forecast (for instance, a GARCH model). $X_{i,t-1}$ is a set of variables belonging to $\mathcal{F}_{i,t-1}$, θ_i is the vector of model's parameters and ω is a vector of parameters specific to the risk measure itself. The latter parameters are determined by the user. For instance, in the case of the VaR, it corresponds to the risk level, generally fixed to 1% or 5% by convention.

The notation for $RM_{i,t}$ encompasses a wide class of (semi-)parametric models and conditional risk measures. For instance, $RM_{i,t}$ can be a measure of price variation (conditional volatility), a systematic risk measure (conditional beta), a tail risk measure (VaR, ES), or a systemic risk measure (MES, SRISK, ΔCoVaR). The model could be a univariate or a multivariate GARCH model, a quantile or a linear regression model, etc. Thus, this notation can be viewed as a generalization of that used by Gouriéroux and Zakoïan (2013) for parametric VaR models.

As examples of the notation we consider (i) a conditional VaR based on a Student-GARCH model, *(ii)* the conditional MES of Acharya et al. (2010) and Brownlees and Engle (2015), (iii) the SRISK of Acharya et al. (2012) and Brownlees and Engle (2015) and (iv) the ΔCoVaR of Adrian and Brunnermeier (2014). These are also the risk measures used throughout the paper.

Example 1 (VaR-GARCH) *Consider a demeaned return process $r_{i,t}$ associated to an asset indexed by i. Assuming a t-GARCH(1,1) model for $r_{i,t}$, the corresponding conditional VaR for a coverage rate $\tau \in [0,1]$ can be expressed as a linear function of the conditional volatility $\sigma_{i,t}$ of the returns as follows:*

$$f_i^{VaR}(\theta_i, \omega; X_{i,t-1}) = -t_\nu^{-1}(\tau)\sqrt{\frac{v-2}{v}}\sigma_{i,t},$$

with $\sigma_{i,t}^2 = \gamma_i + \alpha_i r_{i,t-1}^2 + \beta_i \sigma_{i,t-1}^2$. $t_\nu^{-1}(\alpha)$ denotes the μ-quantile of the standardized Student cdf with ν degrees of freedom. As such $\theta_i = (\gamma_i, \alpha_i, \beta_i, \nu)'$, $\omega = \tau$ and $X_{i,t-1} = \left\{\underline{r}_{i,t-1}\right\}$, where $\underline{r}_{i,t-1}$ is the set of return observations for firm i up to time $t-1$.

Example 2 (MES) *The MES measures how firm i's risk taking adds to the financial system risk (measured by the ES). Let us denote the market return as $r_{m,t} = \sum_{i=1}^n w_{i,t} r_{i,t}$, with $w_{i,t}$ the value-weight of firm $i = 1, ..., n$ at time t, and $r_{i,t}$ the demeaned firm returns. The conditional MES is defined by the first derivative $-\partial \mathbb{E}_{t-1}(r_{m,t} \mid r_{m,t} < C)/\partial w_{i,t}$, where C is a threshold. If the vectorial process $(r_{i,t}\ r_{m,t})'$ follows a GARCH-DCC, Brownlees and Engle (2015) show that:*

$$\begin{aligned} f_i^{MES}(\theta_i, \omega; X_{i,t-1}) &= -\sigma_{i,t}\rho_{im,t}\mathbb{E}_{t-1}(\epsilon_{m,t} | \epsilon_{m,t} < C/\sigma_{m,t}) \\ &\quad - \sigma_{i,t}\sqrt{1-\rho_{im,t}^2}\mathbb{E}_{t-1}(\epsilon_{i,t} | \epsilon_{m,t} < C/\sigma_{m,t}), \end{aligned}$$

where $\sigma_{i,t}^2 = \gamma_i + \alpha_i r_{i,t-1}^2 + \beta_i \sigma_{i,t-1}^2$, $\rho_{im,t} = Q_{im,t}/\sqrt{Q_{ii,t|}Q_{mm,t}}$ with $Q_{ij,t}$ the $(i,j)^{th}$ element of the so-called pseudo correlation matrix Q_t, and $Q_t = (1-\alpha_C-\beta_C)\bar{Q} + \alpha_C \epsilon_{t-1}\epsilon_{t-1}' + \beta_C Q_{t-1}$, with $\epsilon_{i,t} = r_{i,t}/\sigma_{i,t}$. Brownlees and Engle (2015) consider a non-parametric estimator (Scaillet, 2005) for the tail expectations of the standardized returns ϵ_t. Then, we have $\theta_i = (\gamma_i, \gamma_m, \alpha_i, \alpha_m, \beta_i, \beta_m, \bar{Q}, \alpha_C, \beta_C)'$, $\omega = (C)$ and $X_{i,t-1} = \left\{\underline{r}_{i,t-1}, \underline{r}_{m,t-1}\right\}$.

Example 3 (SRISK) *The SRISK is defined as the expected capital shortfall of a given*

financial institution i, conditional on a crisis affecting the whole financial system. Acharya, Engle, and Richardson (2012) define the SRISK as follows:

$$f_i^{SRISK}\left(\theta_i,\omega;X_{i,t-1}\right)=\max\left(0\ ;\ k\ D_{i,t-1}-(1-k)\,W_{i,t-1}\left(1-LRMES_{i,t}\right)\right),$$

where $D_{i,t}$ and $W_{i,t}$ denote the book value of total liabilities and the market value of the financial institution,respectively, and k is a prudential capital ratio. $LRMES_{i,t}$ denotes the long-run MES, i.e. the expectation of the firm equity multi-period return conditional on the systemic event. The LRMES can be approximated as $LRMES_{i,t}=1-\exp(-18MES_{i,t})$, where $MES_{i,t}$ is the estimate of the MES for firm i at time t as defined in Example 2 (Acharya, Engle, and Richardson, 2012). Then, we have $\omega=(C,k)'$ and $X_{i,t-1}=\left\{\underline{r}_{i,t-1},\underline{r}_{m,t-1},D_{i,t-1},W_{i,t-1}\right\}$. The vector θ_i is similar to that obtained in Example 2. The individual SRISK is generally expressed as a percentage of the aggregate SRISK:

$$f_i^{\%SRISK}=f_i^{SRISK}/\sum_{j=1}^{n}f_j^{SRISK}.$$

Example 4 (ΔCoVaR) *The $\Delta CoVaR$ is a systemic risk measure based on the CoVaR, i.e. the conditional VaR of market returns given an event $\mathbb{C}(r_{i,t})$ observed for firm i:*

$$\Pr\left(r_{m,t}\leq CoVaR_{i,t}^{m|\mathbb{C}(r_{i,t})}\mid\mathbb{C}(r_{i,t})\right)=\alpha. \tag{2}$$

The $\Delta CoVaR$ is the difference between the VaR of the financial system conditional on the distress firm i and the VaR of the system conditional on the median state of that same firm. Adrian and Brunnermeier (2014) suggest using $r_{i,t}=VaR_{i,t}(\tau)$ as conditioning event and estimating the CoVaR using a quantile regression model, $r_{m,t}=\mu_\tau+\gamma_\tau r_{i,t}$. We then get:

$$f_i^{\Delta CoVaR}(\theta_i,\omega;X_{i,t-1})=\gamma_\tau\sigma_{i,t}\left(F^{-1}(\tau)-F^{-1}(0.5)\right), \tag{3}$$

where $F^{-1}(\tau)$ is the τ-quantile of the standarized returns. Hence $\theta_i=\{\gamma_i,\alpha_i,\beta_i,\gamma_\tau\}$, $\omega=\tau$ and $X_{i,t-1}=\left\{\underline{r}_{i,t-1},\underline{r}_{m,t-1}\right\}$.

Notice that the functional form $f_i(.)$ in Equation (1) is indexed by i. Indeed, even if we consider the same risk measure for two assets i and j, one may use two different parametric models to produce the corresponding forecasts. For instance, the notation allows the comparison of the conditional VaR for Bank of America obtained from a GARCH model, and the conditional VaR for Citigroup using an internal model based on RiskMetrics. On

the contrary, if the functional form $f_i(.)$ is equivalent to that of $f_j(.)$, it means that both firms use the same type of parametric model to produce the risk forecasts. However, in all cases, the vectors of parameters θ_i and θ_j are generally different for $i \neq j$.

3 Hypotheses of Interest and Test

We propose a general framework to statistically test for equality of conditional risk measures obtained for, at least, two different assets, portfolios or financial institutions *at a particular time*. In this section, we present two types of tests: (i) a comparison test of two risk measures and (ii) a bucketing procedure. The latter is a form of sequential testing that allows to allocate assets/firms to multiple buckets of equal risk.

3.1 Comparison Test of Risk Measures

We wish to test whether two assets or firms indexed by i and j respectively, present the same level of risk *at time* t with respect to the conditional risk measure RM_t. Such a risk comparison test may be useful in many contexts. For instance, it allows a fund manager to test the equality of two assets' volatilities at a particular date, in order to implement a risk parity investment strategy. It allows also a risk manager to test if the VaR of say portfolio i is equal to the one of another portfolio j, for a given day. A third example is when a regulator wishes to compare the SRISK for bank i, say Bank of America, and the SRISK for bank j, say Citigroup, on a single day, e.g. on September 15th 2008, given the information set prior to this date.

If there is no model uncertainty, i.e. if the functional forms $f_i(.)$ and $f_j(.)$ are known, this test consists of comparing $RM_{i,t} = f_i(\theta_i, \omega; X_{i,t-1})$ to $RM_{j,t} = f_j(\theta_j, \omega; X_{j,t-1})$, where θ_i and θ_j denote the true value of the parameters. Given the common information set $\mathcal{F}_{t-1} = \mathcal{F}_{i,t-1} \cup \mathcal{F}_{j,t-1}$ for both assets, the two conditional risk measures are observed. Then, the null hypothesis of equal risk at time t can be defined as:

$$H_{0,t} : RM_{i,t} = RM_{j,t}. \tag{4}$$

The null hypothesis is indexed by t, to stress the fact that we are testing the equality of two conditional risk measures on a *single date* t given the information set $\mathcal{F}_{t-1}$. Contrary

to the forecast comparison tests (Diebold and Mariano, 1995) for instance, we do not test for $RM_{i,t} = RM_{j,t}$ over the full sample $t = 1, ..., T$, or over a sequence of out-of-sample forecasts. Thus, the alternative hypothesis $H_{1,t} : RM_{i,t} \neq RM_{j,t}$ means that the risk of asset i is different from the risk of asset j at time t given $\mathcal{F}_{t-1}$, according to the risk measure RM_t.

The need for inference comes the fact that $RM_{i,t}$ and $RM_{j,t}$ are not observed, since the parameters θ_i and θ_j are generally unknown and replaced by their estimators $\widehat{\theta}_i$ and $\widehat{\theta}_j$. So, the null hypothesis is based on the true risk measure implied by $f_i(.)$, $RM_{i,t} = f_i(\theta_i, \omega; X_{i,t-1})$, while the estimated value $\widehat{RM}_{i,t} = f_i(\hat{\theta}_i, \omega, X_{i,t-1})$ is affected by an estimation risk. Our test boils down to the question of whether $(f_i(\hat{\theta}_i, \omega; X_{i,t-1}) - f_j(\hat{\theta}_j, \omega; X_{j,t-1}))$ is big enough relative to parameter estimation error coming from $\{\hat{\theta}_i, \hat{\theta}_j\}$ to reject the null.

Testing the null hypothesis $H_{0,t}$ is challenging, as the conditional distribution of the estimated risk measure $\widehat{RM}_{i,t}$ is generally unknown and may be difficult to obtain depending on the model used to estimate the risk measure. Typically, the estimates are obtained using (M-)GARCH models, whose estimates' distribution is widely unknown. Furthermore, even in the cases where the distribution is known (Chan et al., 2007; Gouriéroux and Zakoïan, 2013), the joint distribution of $\widehat{RM}_{i,t}$ and $\widehat{RM}_{j,t}$ is almost surely not, except for the trivial, but unlikely case of independence between the two risk measures. As a consequence, traditional testing methods are not directly applicable and a new testing procedure is needed. To achieve this goal, we use the assumed data generating process (DGP) to bootstrap the conditional risk measures and obtain their distribution at time t. We propose the following two-sided test statistic:

$$T(\alpha) \equiv \frac{|\hat{x}_{ij,t}|}{c^*_{ij,t}(\alpha)}, \tag{5}$$

where $\widehat{x}_{ij,t} = \widehat{RM}_{i,t} - \widehat{RM}_{j,t}$ and $c^*_{ij,t}(\alpha)$ is the bootstrap critical value obtained from the absolute null-value shifted bootstrap distribution of $\hat{x}_{ij,t}$. The use of the critical value means that the $\alpha\%$ rejection point for all combinations (i, j) is scaled to 1. Rejection thus occurs at the $\alpha\%$ level if $T(\alpha) > 1$. Ex-post, one may draw conclusions on which one is the riskiest based on the sign of $x_{ij,t}$. The bootstrap is assumed to be asymptotically valid for the risk measures considered, in the sense that it correctly reproduces the asymptotic distribution of the risk measure estimator (see Section 4.2).

Finally, our framework can be extended to test the equality of risk measure forecasts for a horizon $h > 1$, by considering the information set $\mathcal{F}_{t-h}$ rather than $\mathcal{F}_{t-1}$. Since $RM_{i,t} = f_i(\theta_i^*, \omega; X_{i,t-1})$ is stochastic given $\mathcal{F}_{t-h}$, in that case the null hypothesis becomes $H_{0,t} : \mathbb{E}\left(x_{ij,t}|\mathcal{F}_{t-h}\right) = 0$.

3.2 Bucketing Procedure

When considering more than two assets, pairwise comparisons become problematic. One could test for the significance of the difference between each pair, appropriately taking into account the multiple testing problems that arise. However, without adding some additional structure, the set of rejections is unlikely to lead to a cohesive ranking. Instead, we propose an iterative bucketing procedure that can be used to obtain a grouped ranking of assets. The objective is to get a complete ranking by means of a procedure inspired by the Model Confidence Set of Hansen et al. (2011). Our procedure produces buckets of equally risky assets, in the sense that we cannot statistically distinguish the assets within one bucket in terms of their riskiness. This testing procedure can be applied to any type of conditional risk measure, but it has particular application in the context of the systemic risk where the goal is to rank the financial institutions according to their systemic risk contribution.

Consider the set of all financial institutions $\mathcal{N}^0$. We start with the identification of the set of most risky firms, defined at time t as:

$$\mathcal{N}_t^{(1)} \equiv \{i \in \mathcal{N}^0 : x_{ij,t} \geq 0 \ \forall j \in \mathcal{N}^0\}. \tag{6}$$

The goal is to find the set $\mathcal{N}_t^{(1)}$. This is achieved through a sequence of comparison tests where objects in $\mathcal{N}^0$ are removed from the set under consideration if they are found to be less risky. The null we are testing is therefore

$$H_{0,t,\mathcal{N}} : x_{ij,t} = 0 \ \forall i, j \in \mathcal{N}, \tag{7}$$

with $\mathcal{N} \subseteq \mathcal{N}^0$, the subset containing the not yet eliminated firms. The null hypothesis states that all firms in the final set after the elimination procedure should be equally risky. For any set $\mathcal{N}$ this can be tested using an equivalence test and an elimination rule (see Section 3.4.1). If the equivalence test is rejected, we use the elimination rule to remove the most significantly different firm, reducing the size of $\mathcal{N}$, and re-apply the equivalence test.

Our set of most risky firms is the subset of $\mathcal{N}^0$ that contains $\mathcal{N}_t^{(1)}$ with a certain probability which can be controlled. This procedure identifies the most risky set only. To obtain the full ranking, we apply the procedure on the set $\mathcal{N}^0 \setminus \hat{\mathcal{N}}_t^{(1)}$ to obtain a second bucket, $\hat{\mathcal{N}}_t^{(2)}$. This is repeated until all firms have been allocated to a bucket.

3.3 Procedure Implications

Of course, there are many different ways to obtain buckets of equally risky financial institutions, and even to rank them. However, the implications of our procedure are ideally suited to ranking systemic firms.

First, the approach is one directional, which means we only control the Type I error of the null of equal risk, in one direction as well. Since we consider a top-down approach (from the bucket of the most risky firms to the less risky ones), a false rejection leads to a firm being assigned to a less risky cluster in the next iteration. Under-estimating the risk is in our opinion much more hazardous than the reverse, and this is controlled. Moreover, if a false rejection occurs, the procedure has a self-correcting mechanism that minimizes the effect on the relative ranking of the remaining firms. The firm that is falsely rejected, is by definition the most risky firm in the set of firms not yet assigned to a bucket. As such, if the power of our test is reasonable, it will be assigned to a bucket on its own. Even though it is estimated as less risky than it ought to be, it is still deemed more risky than all the remaining, less risky firms.

Second, the Type II error consists of failing to eliminate a firm, and assigning it to a too risky bucket. In practice, what might happen is that a firm with a low point estimate but a high standard error may be assigned to a riskier bucket than a firm with a higher point estimate, but a low standard error. In some sense, these firms are *loose cannons*. Their return series have characteristics that make it difficult to estimate their true risk with accuracy. Again, due to the top-down approach, the resulting ranking will be prudent; in case of large uncertainty, a firm is always put in the most risky bucket.

Finally, we want to emphasize that the number of buckets is not specified ex-ante. This is the main difference with the approach proposed by the BCBS. Ex-post, the number of buckets ranges between one and the total number of firms, depending on the precision of

the estimates. Therefore, our testing procedure strikes a balance between compression and accuracy of the ranking.

3.4 FWE and FDR

The bucketing procedure is clearly a multiple testing problem, and as such it is important to control the number of false rejections. We consider two alternative controlling methods that may result in different allocations (see Bajgrowicz and Scaillet, 2012, for instance).

The Family Wise Error Rate (FWE) is defined as the probability of rejecting at least one of the true null hypotheses. Controlling the FWE requires that the FWE be no bigger than the significance level α, at least asymptotically. In many applications one might be willing to tolerate a larger number of false rejections if there is a large number of total rejections. Instead of allowing a fixed amount of false rejections, we tolerate a certain proportion of false rejections out of total rejections. This is controlling the False Discovery Proportion (FDP). Let F be the number of false rejections made by a multiple testing method, and let R be the total number of rejections. The FDP is defined as $FDP = F/R$ if $R > 0$ and 0 otherwise. Benjamini and Hochberg (1995) suggest controlling the False Discovery Rate (FDR), the expected value of the FDP. A testing method is said to control the FDR at level α if $FDR = \mathbb{E}(FDP) \leq \alpha$, for any sample size T. A testing method is said to control the FDR asymptotically at level α if $\lim_{T\to\infty} \sup FDR \leq \alpha$.

The next two sections will outline the methods to control either the FWE or the FDR. Neither method is superior to the other. However, when the number of hypothesis to be tested becomes very large, the FWE loses a lot of power, making it difficult to reject any hypothesis at all. Romano et al. (2008b) argue that the number of false hypotheses rejected may even tend to zero if the number of hypotheses tested increases. Common practice is to control the FWE in 'small' problems, and control the FDR in 'large' settings. What is small and what is large greatly varies by application. In the simulation exercise we will shed some light on the performance of our newly proposed test.

3.4.1 FWE controlling method

In order to carry out the bucketing procedure we need an equivalence test and an elimination rule. In case of equivalence we have that $x_{ij,t} = 0$ for all $i, j \in \mathcal{N}$. We propose the following test statistic:

$$T^{max}(\alpha) \equiv \max_{i,j \in \mathcal{N}} \frac{|\hat{x}_{ij,t}|}{c^*_{ij,t}(\alpha)}. \tag{8}$$

Here, the need for standardization of the statistic becomes evident, as we want to identify the firm which is most likely to be different from the rest. If there is a significant difference, an elimination rule follows naturally. We eliminate the unit $\arg\max_{j \in \mathcal{N}} \sup_{i \in \mathcal{N}} \hat{x}_{ij,t}/c^*_{ij,t}(\alpha)$, or to put it simply, the most significantly rejected firm. Once we can no longer reject a null hypothesis, all firms are equally risky and we identified a bucket.

The FWE can be controlled by obtaining an appropriate critical value for the $T^{max}(\alpha)$ statistic. Its critical value $d^*_t(\alpha)$ is chosen such that

$$d^*_t(\alpha) = \inf \{x \in \mathbb{R} : P(T^{max}(\alpha) \geq x) \leq \alpha\}. \tag{9}$$

In practice, the probability distribution P is unknown, and we replace it with a suitable bootstrap estimate P^*, discussed in Section 4. The asymptotic results in White (2000) and Romano and Wolf (2005) imply that our bootstrap method controls FWE asymptotically, provided that the bootstrap is asymptotically valid. This FWE-controlling test bears clear similarities to the Reality Check of White (2000), who proposes a method to test whether one of a set of models significantly outperforms a benchmark.

3.4.2 FDR controlling method

Romano et al. (2008a) propose a method to control the FDR in a bootstrap setting. The intuition is as follows. Consider the ordered series of test statistics, denoted $T_{(k),t}$, such that $T_{(1),t} \leq ... \leq T_{(s),t}$, with $H_{(k),t}$ the corresponding null hypothesis. Define $T_{(k:l),t}$ as the k-th largest of the l test statistics $T_{(1),t}, ..., T_{(l),t}$. The idea is to reject all $H_{(s),t}, ..., H_{(s-h^*),t}$, where h^* is the largest integer h satisfying $T_{(s),t} \geq c_{s,t}, ..., T_{(s-h),t} \geq c_{s-h,t}$. Again, controlling the FDR is a matter of choosing the appropriate critical values $c_{k,t}$. Romano et al. (2008a) show that, in order to control the FDR at level α, the critical values are defined recursively

as follows: having determined $\hat{c}_{1,t}, ..., \hat{c}_{h-1,t}$, compute $\hat{c}_{h,t}$ according to:

$$\hat{c}_{h,t} = \inf \Bigg\{ x \in \mathbb{R} : \sum_{s-h+1 \le r \le s} \frac{r-s+h}{r} \times P\left(T_{(h:h),t} \ge x, ..., T_{(s-r+1:h),t} \ge \hat{c}_{s-r+1}, T_{(s-r:h),t} < \hat{c}_{s-r}\right) \le \alpha \Bigg\}, \quad (10)$$

with

$$\hat{c}_{1,t} = \inf \left\{ x \in \mathbb{R} : \frac{1}{s} P\left(T_{(1),t} \ge x\right) \le \alpha \right\}. \quad (11)$$

Again, the probability distribution P will be approximated by a bootstrap counterpart.

Having obtained the critical values, starting with $T_{(s),t}$ and working downwards, we check whether $T_{(r),t} \ge \hat{c}_{r,t}$ and if the null is rejected, we eliminate the significantly less risky firm from the set. The firms that remain after the h^* rejected hypotheses are statistically equally risky, and form a bucket. Romano et al. (2008a) prove that this bootstrap approach asymptotically controls the FDR conditionally on the bootstrap being asymptotically valid.

4 Bootstrap Implementation

This section describes how to obtain $c^*_{ij,T}$ and P^* at particular date T. Consider N assets or firms, and assume a general multivariate DGP for the corresponding returns, $r_t = g(\theta, \epsilon_t | \mathcal{F}_{t-1})$, with r_t and ϵ_t vectors of dimension N, and θ the set of model parameters. We assume $\epsilon_t = (\epsilon_{1,t}, ..., \epsilon_{N,t})$ to be i.i.d. over time-periods, with zero mean and covariance matrix equal to the identity matrix. Notice that this representation allows for non-linear cross-sectional dependence across the $\epsilon_{i,t}$ elements. We define the inverse, $\epsilon_t = g^{-1}(\theta, r_t | \mathcal{F}_{t-1})$, which retrieves the innovations from the observed return process. For instance, consider a single asset ($N = 1$), with demeaned returns $r_t = g(\theta, \epsilon_t | \mathcal{F}_{t-1}) = \sigma_t \epsilon_t$, where σ_t follows a GARCH process with parameters θ. Then, $\epsilon_t = g^{-1}(\theta, r_t | \mathcal{F}_{t-1}) = r_t / \sigma_t$ simply corresponds to the standardized return.

To obtain the bootstrap distribution, we employ a multivariate version of the methodology suggested by Pascual et al. (2006) and Christoffersen and Gonçalves (2005) for GARCH forecasts. The approach is as follows. First estimate θ on the original series r_t for $t = 1, ..., T-1$. Generate bootstrap series, r^*, using $\hat{\theta}$, and innovations drawn

with replacement from the empirical distribution of the centered residuals. Estimate the same model on the bootstrap series, to obtain $\hat{\theta}^*$. The bootstrap risk measure forecasts, $RM^*_{i,T} = f^*_i\left(\widehat{\theta}^*, \omega; X_{i,T-1}\right)$ is computed for each asset $i = 1, ..., N$, based on the original past return series $\underline{r}_{T-1}$ and bootstrap parameter estimates $\hat{\theta}^*$. The use of the original return series in $RM^*_{i,T}$, instead of the bootstrapped ones, ensures that the current state of the returns is taken into account in the bootstrap RM forecast. Then, the bootstrap only measures the estimation uncertainty.

4.1 Bootstrap Algorithm

Hence, we propose the following algorithm:

1. Estimate the models to obtain $\hat{\theta}$. Use the parameter estimates to forecast $\hat{x}_{ij,t}$, for all pairs $(i, j) \in \{1, ..., N\}^2$.

2. Compute the residuals $\hat{\epsilon}_t = g^{-1}(\hat{\theta}, r_t|\mathcal{F}_{t-1})$ for all $t = 1, ..., T-1$.

3. Draw $s_1, ..., s_{T-1}$ i.i.d. from the uniform $\mathcal{U}_{\{1,T-1\}}$ distribution and construct the bootstrap errors from the centered residuals $\epsilon^{*b}_t = \hat{\epsilon}_{s_t}$, $\forall\ t = 1, ..., T-1$.

4. Construct the bootstrap return series $r^{*b}_t = g(\hat{\theta}, \epsilon^{*b}_t|\mathcal{F}_{t-1})$.

5. Estimate the model on the bootstrapped series to obtain $\hat{\theta}^{*b}$. Compute $\widehat{RM}^{*b}_{i,T}$ using $f_i(\hat{\theta}^{*b}_i, \omega; X_{i,T-1})$ and similarly for $\widehat{RM}^{*b}_{j,T}$ to obtain $\hat{x}^{*b}_{ij,T}$.

6. Repeat steps 3 to 5 B times, obtaining bootstrap statistics $x^{*b}_{ij,T}$, $b = 1, ..., B$.

Two remarks have to be made concerning this bootstrap algorithm. First, note that in Step 3, we re-sample cross-sectional vectors of residuals. The time-concordant sampling ensures that the potential cross-sectional dependence in the innovations is preserved. Second, the critical values $c^*_{ij,T}$ and $d^*_{ij,T}$ are obtained as the α-quantiles of the 'null-value shifted" series $|\hat{x}^{*b}_{ij,T} - \hat{x}_{ij,T}|$ and $T^{max*b}_{ij,T} - T^{max}_{ij,T}$, respectively. Romano et al. (2008b, p. 412) argue that using these "null-value shifted" series is equivalent to inverting bootstrap multiple confidence regions, and therefore a valid approach. For a detailed description on how to obtain the bootstrap critical values in the FDR procedure from the bootstrap distribution, we refer to Romano et al. (2008a).

4.2 Bootstrap Validity

A formal proof of the asymptotic validity of the bootstrap – in the sense that the bootstrap correctly reproduces the asymptotic distribution of the risk measure estimator – is outside the scope of this paper, as the general setup for the risk measures cannot be treated uniformly with regards to the bootstrap. Instead, bootstrap validity has to be considered for each case separately, and doing so explicitly would complicate the paper needlessly. Instead, we provide some general guidelines for checking bootstrap validity. First, the most important condition for the validity of the bootstrap, is that it correctly replicates the asymptotic distribution of the estimators of the parameters θ. If the parametric model assumed to estimate θ is correct, and the estimators of θ are "well-behaved", for instance by being $\sqrt{T}$-consistent and asymptotically normal, then it can typically be shown that the bootstrap is asymptotically valid for these parameters. For instance, Shimizu (2010) explicitly derives the bootstrap validity for the parameters of stationary ARMA-GARCH models.

Our setting contains two additional difficulties. First, the distribution of the model parameter estimators is only an intermediate step in obtaining the distribution of $\widehat{RM}_{i,t}$. As argued by Francq and Zakoïan (2015), given the distribution of these parameter estimators, an application of the Delta method permits to derive the asymptotic distribution of the risk measure estimator. The same Delta method argument can be applied to the bootstrap and suggests that validity of the bootstrap parameter estimators suffices for establishing bootstrap validity of the risk measure. However, a formal proof requires one to deal with the subtleties involved with conditioning on the past for constructing the conditional risk measure. Second, we need the joint distribution of $\widehat{RM}_{i,t}$ and $\widehat{RM}_{j,t}$, which may be more difficult to obtain even if the univariate distributions are known. For these two reasons we believe that formal proofs of bootstrap validity for a general class of risk measures deserve separate attention and are outside the scope of the paper. In what follows, we therefore work under the assumption that the bootstrap method chosen for a particular risk measure is appropriate. For our specific choices of bootstrap methods and risk measures, we return to this issue in the simulation study where we study their small sample performance. The results we find there do not give us a reason to doubt the validity of our bootstrap approach.

5 Simulation Study

We use Monte Carlo simulations to study the properties of both the single test and the bucketing procedure. The Monte Carlo simulation is performed on 1,000 replications. For the bootstraps we generate $B = 999$ samples. We always compare the conditional risk measures at time T and estimate them over the sample 1 to $T-1$. We apply the comparison test to the VaR, and both the single test and the bucketing procedure to the MES, as defined in Examples 1 and 2 respectively. All the results are generated using Ox version 7.00 (see Doornik, 2012) and the G@RCH package version 7.04 (Laurent, 2013).

5.1 Simulation Design

For the VaR, we consider two assets, indexed by $i = 1, 2$, and the following DGP:

$$r_{i,t} = \sigma_{i,t}\epsilon_{i,t} \tag{12}$$

$$\epsilon_{i,t} \overset{i.i.d.}{\sim} ST\left(0, 1, \nu_i\right), \tag{13}$$

where $\sigma_{i,t}^2$ follows a GARCH(1,1) model with parameters $(\gamma, \alpha_1, \beta_1) = (0.05, 0.10, 0.85)$ for both return series. The innovations follow a Student distribution with zero mean, unit variance and degrees of freedom ν_i. Under the null, the τ-VaRs are equal for both series, $VaR_{1,T}(\tau) = VaR_{2,T}(\tau) \iff t_{\nu_1}^{-1}(\tau)\sqrt{(\nu_1-2)/\nu_1}\sigma_{1,T} = t_{\nu_2}^{-1}(\tau)\sqrt{(\nu_2-2)/\nu_2}\sigma_{2,T}$. To impose this equality, we simulate processes and re-scale the returns ex-post such that the volatilities at time T, $\sigma_{1,T}$ and $\sigma_{2,T}$, imply the equality of both VaRs. We consider two cases in which the degrees of freedom ν_1 and ν_2 are equal or different. In the former, the volatility at time T is equal for both firms, in the latter case the volatility will be higher for the firm with a higher degree of freedom. For the case with equal degrees of freedom, we set $\nu_1 = \nu_2 = 5$. We set $\sigma_{1,T} = 2$ and define $\sigma_{2,T}$ relative to that as $\Delta\sigma = \sigma_{2,T} - \sigma_{1,T}$. We use $\Delta\sigma = \{0.0, 0.1, 0.2\}$ to simulate under the null hypothesis ($\Delta\sigma = 0$) and alternatives ($\Delta\sigma > 0$). In the case of different degrees of freedom, we set $\nu_1 = 5$ and $\nu_2 = 7$. Again $\sigma_{1,T} = 2$. We scale $\sigma_{2,T}$ such that the VaRs at time T have the same value under the null, i.e. $\sigma_{2,T} = \frac{t_5^{-1}(\tau)}{t_7^{-1}(\tau)}\sqrt{21/25}(\sigma_{1,T} + \Delta\sigma)$. In all cases, the coverage rate for the VaR is fixed at 5%, i.e. $\tau = 0.05$.

For the MES, we consider the general DGP proposed by Brownlees and Engle (2015),

i.e.

$$\begin{aligned} r_{m,t} &= \sigma_{m,t}\epsilon_{m,t} \\ r_{i,t} &= \sigma_{i,t}\left(\rho_{i,t}\epsilon_{m,t} + \sqrt{1-\rho_{i,t}^2}\xi_{i,t}\right) \\ (\epsilon_{m,t}, \xi_{i,t}) &\sim F, \end{aligned} \tag{14}$$

where $\sigma_{m,t}$ and $\sigma_{i,t}$ follow GARCH processes, while $\rho_{i,t}$ follows a DCC as described in Example (3). F is a general zero mean, unit variance distribution, with unspecified non-linear dependence structures. For the Monte Carlo simulations, we restrict the model to a multivariate Gaussian conditional distribution and constant correlations, i.e. $\rho_{i,t} = \rho_t$ (CCC model). Of course, both assumptions will be relaxed in the empirical application. Notice that, we have done simulations using DCC correlations for a few parameter settings with a small number of replications and found very similar results to those reported here.

Since the innovations are i.i.d. and all dependence between firms and the market is captured by the correlation, then the MES can be written as:

$$MES_{i,t}(\tau) = \beta_{i,t}ES_{m,t}(\tau), \tag{15}$$

where $\beta_{i,t} = \rho_i\sigma_{i,t}/\sigma_{m,t}$ denotes the conditional beta of the firm i and $ES_{m,t}(\tau)$ is the ES of the market returns. Under the normality assumption, the ES has a closed form expression. Denote by $\phi(.)$ and $\Phi(.)$ the standard normal univariate pdf and cdf, respectively. The MES can be written as follows:

$$MES_{i,t}(\tau) = \beta_{i,t}\sigma_{m,t}\lambda(\Phi^{-1}(\tau)) = \rho_{i,t}\sigma_{i,t}\lambda(\Phi^{-1}(\tau)), \tag{16}$$

where $\lambda(z) = \phi(z)/\Phi(z)$ is the Mills ratio. Therefore, the MES solely depends on the volatility of the firm and its correlation with the market. Under these assumptions, two firms have equal MES if the product of conditional volatilities and conditional correlations with the market at time T is equal. We use this result to control the relative risk of simulated firms.

The GARCH parameters $(\gamma, \alpha_1, \beta_1)'$ are set to $(0.05, 0.10, 0, 85)'$ for each series. In order to simulate the returns under the null and the alternative, we re-scale the simulated process such as to control for the value of the MES at time T. For the single test, we generate the returns for two firms and the market. The market has $\sigma_{m,T} = 1$. The first firm has

Table 1: Rejection frequencies of the single test of equal VaR

	VaR					
	T=1,000			T=2,000		
$\nu_2\backslash\Delta\sigma$	0.0	0.1	0.2	0.0	0.1	0.2
5	0.045	0.523	0.652	0.049	0.613	0.846
7	0.052	0.544	0.721	0.050	0.671	0.844
	MES					
	T=1,000			T=2,000		
$\Delta\rho\backslash\Delta\sigma$	0.0	0.1	0.2	0.0	0.1	0.2
0.0000	0.046	0.414	0.763	0.048	0.592	0.850
0.0125	0.069	0.612	0.854	0.112	0.789	0.891
0.0250	0.199	0.791	0.888	0.310	0.877	0.920

Note: The table contains the rejection rates of a single test of equal VaR and MES. Nominal size is 5%.

$\sigma_{1,T} = 2$ and $\rho_1 = 0.4$. We vary the volatility and correlation of the second firm. We choose $\Delta\sigma = \{0, 0.1, 0.2\}$ and $\Delta\rho = \{0, 0.0125, 0.0250\}$, where $\Delta\rho = \rho_2 - \rho_1$. The distance between the MES of firms 1 and 2 is therefore a function of the parameters $(\Delta\sigma, \Delta\rho)$. For instance, setting $(\Delta\sigma, \Delta\rho) = (0.1, 0.0125)$ results in $MES_{1,T} = 1.650$ and $MES_{2,T} = 1.787$. The null hypothesis of equal MES is obtained for $(\Delta\sigma, \Delta\rho) = (0, 0)$.

For the bucketing procedure, we generate the returns for N firms and the market. In order to obtain firms that satisfy the null hypothesis of equal systemic risk, we give all firms within the same bucket identical variance and correlation. To illustrate the trade-off between the FWE and FDR tests, we simulate $N = 10, 20, 40, 60, 80, 100$ firms. In each simulation there are $c = N/5$ buckets, each containing five firms. The market again has $\sigma_{m,T} = 1$. All firms i in bucket 1 have $\sigma_{i,T}^{(1)} = 2, \rho_i^{(1)} = 0.4$. All firms i in bucket $k = 2, ..., c$ have $\sigma_{i,T}^{(k)} = 2+(k-1)\Delta\sigma$ and $\rho_i^{(k)} = 0.4+(k-1)\Delta\rho$. The difference between two successive buckets in terms of volatility and correlation is therefore equal to that between the two firms in the single test of equal MES. We also take the same values for $\{\Delta\sigma, \Delta\rho\}$.

5.2 Pairwise Comparison Test

Table 1 reports the rejection frequencies of the null hypothesis of equal VaR and equal MES for $T = 1,000$ and $2,000$ observations at the 5% significance level. The empirical size of the test corresponds to the case $\Delta\sigma = \Delta\rho = 0$. Results suggest that for both risk measures

and for all the DGPs we consider, the test does not suffer from any size distortion. Indeed the rejection rates are remarkably close to the nominal size even for $T = 1,000$.

The other entries in Table 1 correspond to power. We first consider the VaR. When the second VaR is 5% bigger than the first one ($\Delta\sigma = 0.1$), power already exceeds 50%, and it is close to 70% when the difference is twice as big. Power is increasing with the sample size, and interestingly, power is bigger when the two series have different distributions.

Power for the MES test is comparable to the power of the single test for the VaR. Keeping $\Delta\rho = 0$ the values are very close to those of the VaR. Small changes in the correlation are more difficult to precisely estimate than changes in volatility, and as such, power is much lower in the direction of increasing correlation compared to increasing volatility. But the differences do stack up: when both $\Delta\rho$ and $\Delta\sigma$ are large, power exceeds 90%.

5.3 Bucketing Procedure

In order to save space, we only report the results for $T = 2,000$ and choose a significance level for both the FDR and FWE of $\alpha = 0.05\%$. It is difficult to evaluate the bucketing procedure in terms of size and power. This is mainly due to the fact that an error in any of the iterations has an impact on the next steps. Indeed, the composition of the second bucket will be affected by the composition of the first one, and so on. Moreover, we may overestimate the number of buckets if, for instance, the first bucket is split up into two separate buckets, such that the third estimated bucket is in fact the second bucket implied by the DGP. As such, we do not expect to always have a one-to-one correspondence between the generated ranking and the estimated ranking.

We therefore summarize the performance of our bucketing procedure in five numbers, three based on the first bucket only, and two on the full ranking. The first two are the actual FWE and FDR, computed on the first bucket. Next we consider the power of the test, defined as the fraction of less risky firms that are successfully rejected. Finally, to assess the accuracy of the complete ranking, we present the Spearman rank correlation between the true and estimated rankings, as well as the total number of buckets found. The latter should be close to $N/5$ when the bucketing procedure has an ideal trade-off between Type I and Type II errors.

Table 2 presents the results of the simulation. Each panel has one of the five performance criteria, with the results for the FWE (resp. FDR) controlling procedure in the left-hand (resp. right-hand) panel. First, both the FWE and FDR approaches control their respective error. When the difference between buckets is small or the number of firms is large, the FWE procedure tends to over-reject a bit, but the FWE is quite well controlled when the difference between buckets is large. The FDR is too high when the number of firms is small, and there is little difference between buckets. There is slight under-rejection when the number of firms becomes very large, but the FDR is nicely around 0.05 when the buckets are furthest apart. Of course, when the FDR is controlled, the actual FWE will be above 0.05, as the number of correct rejections is far larger than the number of true hypotheses. Similarly, the FDR of the FWE controlling procedure is generally below 0.05 for the same reason. Finally, as expected, the FDR procedure is uniformly more powerful than the FWE procedure.

Next, consider the statistics on the complete ranking. First, the Spearman rank correlation gives an indication on how good the ranking is. Importantly, even if the complete ordering is correct, that is, firms are ranked above or at the same level as all firms that are less risky, the Spearman correlation still penalizes if they are not in the correct bucket. As such, when a bucket is split up into two estimated buckets, the rank correlation will go down. The rank correlation of the FWE buckets is generally higher for $N = 10, 20$ and the FDR has higher rank correlation with $N = 40$ and up. This is in line with general practise where the FDR is often used as the number of hypothesis becomes large and power of FWE controlling procedure drops. This is further evidenced by the final panel which shows the number of buckets. The FDR procedure generally estimates a greater number of buckets, as it rejects more null hypotheses by construction. The FWE generally has far too few buckets. For instance, for $N = 100$, even in the case where the distance between buckets is large, the average number of buckets is only 6.597. Interestingly, the FDR procedure comes very close with an average of 19.103 buckets, when there are 20 true buckets.

We want to stress that we do not necessarily advocate the use of either method. The FWE and FDR control are different statistics, and it is up to the user which one to control. The FWE is always more conservative, but it loses power when the dimension of the problem

becomes large. However, depending on the application, one might choose to control the FWE even in large dimensions, if the cost of the false rejection is high.

6 Empirical Application

In this empirical application we apply the bucketing procedure to a panel of 94 large U.S. financial firms. The dataset we use is identical to the panel studied by Acharya et al. (2010), Brownlees and Engle (2015) and many other papers on similar topics. It contains daily returns and market capitalizations retrieved from CRSP and quarterly book value of equity from Compustat. The data covers the period between January 3, 2000 and December 31, 2012, for a total of 3,269 daily observations. The market return is approximated by the CRSP market value-weighted index return. Market value is determined by CRSP daily closing prices and number of shares outstanding. Quarterly book values of total liabilities are from Compustat (LTQ). This results in a dataset containing all U.S. financial firms with a market capitalization greater than 5 billions USD as of the end of June 2007. A full list of ticker symbols and firms is given in Appendix A.

The objective of this empirical application is twofold. In a first section, we apply our pairwise comparison test for the MES. We consider a subset of financial institutions in order to emphasize the time profile of the systemic risk and the need for a comparison of *conditional* risk measures. In a second section, we apply the bucketing procedure to the full sample, contrasting the FWE with the FDR approach. Then, we estimate buckets for the MES, %SRISK and ΔCoVaR.

The estimation of the three systemic risk measures is done according to the same methodology as that recommended by their authors. The MES is estimated using $C = VaR_{m,t}(0.05)$ and a DCC-GJR-GARCH model (estimated by QML). We check for possible dynamics in the mean by minimizing the Schwarz Information Criteria for the individual ARMA(m,n)-GJR-GARCH(1,1) models over $m, n = 0, ..., 3$. We test for the presence of serial correlation in the standardized residuals and their squares, and fail to reject the null for all series. As such the bootstrap for serially uncorrelated returns described in Section 4 will suffice. For the %SRISK, we fix the capital ratio k at 8%, following Brownlees and Engle (2015). We only consider the series with strictly positive SRISK estimates. Finally,

Table 2: Simulation Results Bucketing Procedure

		FWE Controlling Procedure						FDR Controlling Procedure					
N		10	20	40	60	80	100	10	20	40	60	80	100
$\Delta\rho$	$\Delta\sigma$						FWE						
0.0125	0.0	0.098	0.144	0.250	0.291	0.332	0.375	0.186	0.225	0.324	0.358	0.404	0.436
0.0250	0.0	0.074	0.096	0.111	0.182	0.274	0.291	0.228	0.304	0.348	0.444	0.496	0.481
0.0000	0.1	0.055	0.062	0.064	0.083	0.100	0.084	0.133	0.185	0.334	0.437	0.532	0.553
0.0125	0.1	0.053	0.059	0.064	0.079	0.093	0.082	0.154	0.186	0.414	0.501	0.621	0.636
0.0250	0.1	0.052	0.054	0.062	0.071	0.083	0.078	0.185	0.208	0.406	0.583	0.587	0.653
0.0000	0.2	0.047	0.047	0.048	0.068	0.088	0.080	0.134	0.272	0.437	0.530	0.601	0.647
0.0125	0.2	0.048	0.047	0.048	0.065	0.078	0.077	0.164	0.336	0.503	0.565	0.657	0.679
0.0250	0.2	0.048	0.047	0.048	0.062	0.071	0.074	0.194	0.417	0.547	0.649	0.747	0.752
							FDR						
0.0125	0.0	0.254	0.187	0.139	0.087	0.063	0.048	0.259	0.186	0.137	0.086	0.064	0.050
0.0250	0.0	0.122	0.084	0.054	0.044	0.041	0.031	0.112	0.100	0.081	0.074	0.061	0.048
0.0000	0.1	0.123	0.053	0.013	0.009	0.008	0.005	0.160	0.076	0.067	0.040	0.036	0.029
0.0125	0.1	0.098	0.042	0.012	0.007	0.009	0.006	0.107	0.064	0.050	0.041	0.035	0.031
0.0250	0.1	0.063	0.041	0.012	0.008	0.006	0.004	0.088	0.059	0.051	0.045	0.036	0.044
0.0000	0.2	0.055	0.038	0.013	0.007	0.005	0.004	0.094	0.072	0.061	0.048	0.039	0.034
0.0125	0.2	0.053	0.044	0.010	0.005	0.003	0.003	0.081	0.061	0.055	0.050	0.041	0.042
0.0250	0.2	0.032	0.032	0.009	0.003	0.002	0.001	0.058	0.052	0.051	0.047	0.048	0.051
							Power						
0.0125	0.0	0.090	0.156	0.227	0.283	0.340	0.369	0.178	0.239	0.301	0.353	0.410	0.437
0.0250	0.0	0.106	0.166	0.499	0.513	0.675	0.591	0.212	0.248	0.370	0.482	0.789	0.737
0.0000	0.1	0.141	0.323	0.565	0.689	0.758	0.781	0.265	0.503	0.829	0.915	0.946	0.955
0.0125	0.1	0.166	0.375	0.571	0.761	0.786	0.818	0.348	0.610	0.886	0.944	0.982	0.986
0.0250	0.1	0.254	0.448	0.649	0.806	0.811	0.831	0.446	0.705	0.929	0.981	0.983	0.992
0.0000	0.2	0.236	0.541	0.720	0.788	0.825	0.842	0.407	0.826	0.940	0.961	0.974	0.978
0.0125	0.2	0.283	0.613	0.795	0.803	0.840	0.881	0.469	0.874	0.966	0.986	0.977	0.995
0.0250	0.2	0.377	0.645	0.828	0.867	0.883	0.926	0.480	0.904	0.972	0.991	0.993	1.000
							Spearman Rank Correlation						
0.0125	0.0	0.643	0.431	0.371	0.318	0.278	0.267	0.613	0.409	0.352	0.312	0.289	0.279
0.0250	0.0	0.706	0.464	0.395	0.366	0.333	0.350	0.660	0.430	0.434	0.389	0.368	0.354
0.0000	0.1	0.726	0.579	0.613	0.733	0.799	0.827	0.705	0.570	0.702	0.777	0.839	0.878
0.0125	0.1	0.798	0.644	0.676	0.800	0.847	0.836	0.797	0.587	0.789	0.791	0.899	0.934
0.0250	0.1	0.816	0.690	0.687	0.832	0.870	0.846	0.808	0.615	0.794	0.839	0.932	0.938
0.0000	0.2	0.772	0.697	0.783	0.848	0.874	0.899	0.742	0.691	0.834	0.903	0.933	0.948
0.0125	0.2	0.863	0.699	0.842	0.871	0.925	0.928	0.747	0.697	0.925	0.937	0.969	0.959
0.0250	0.2	0.910	0.722	0.869	0.918	0.937	0.978	0.769	0.702	0.956	0.985	0.999	0.989
							Number of Buckets						
0.0125	0.0	1.480	1.802	2.022	2.110	2.200	2.266	1.534	1.844	2.110	2.212	2.326	2.388
0.0250	0.0	1.502	1.914	2.248	2.435	3.063	4.140	1.560	2.214	2.564	3.524	4.623	3.899
0.0000	0.1	1.508	2.004	2.582	3.166	3.816	4.200	1.586	2.128	3.190	4.374	5.612	6.740
0.0125	0.1	1.547	2.071	2.704	4.093	4.121	6.146	1.662	2.543	4.446	4.468	9.080	10.411
0.0250	0.1	1.615	2.120	3.036	4.974	5.519	6.450	1.719	2.647	4.702	6.771	9.840	15.215
0.0000	0.2	1.622	2.202	3.240	4.166	4.996	5.678	1.748	2.790	4.916	7.094	9.304	11.362
0.0125	0.2	1.672	2.354	3.576	4.423	5.559	6.224	1.821	2.924	4.917	7.290	12.190	16.129
0.0250	0.2	1.739	2.436	3.975	5.044	5.704	6.597	1.882	3.058	5.560	9.117	12.520	19.103

Note: The table contains simulation results for various parameter settings $\{\Delta\sigma, \Delta\rho\}$ and number of firms N. Each column gives the results for N firms, which are allocated to $N/5$ buckets of five firms each. The left- and right-hand side give the results for the FWE and FDR controlling procedures respectively. The first two panels give the FWE and FDR computed on the first bucket only. The power is the fraction of firms successfully rejected for the first bucket. The fourth panel gives the Spearman Rank correlation between the true and the estimated ranking, and finally we provide the number of estimated buckets.

for the ΔCoVaR we consider a risk level τ equal to 5%.

6.1 Time Series Properties of Risk Measures

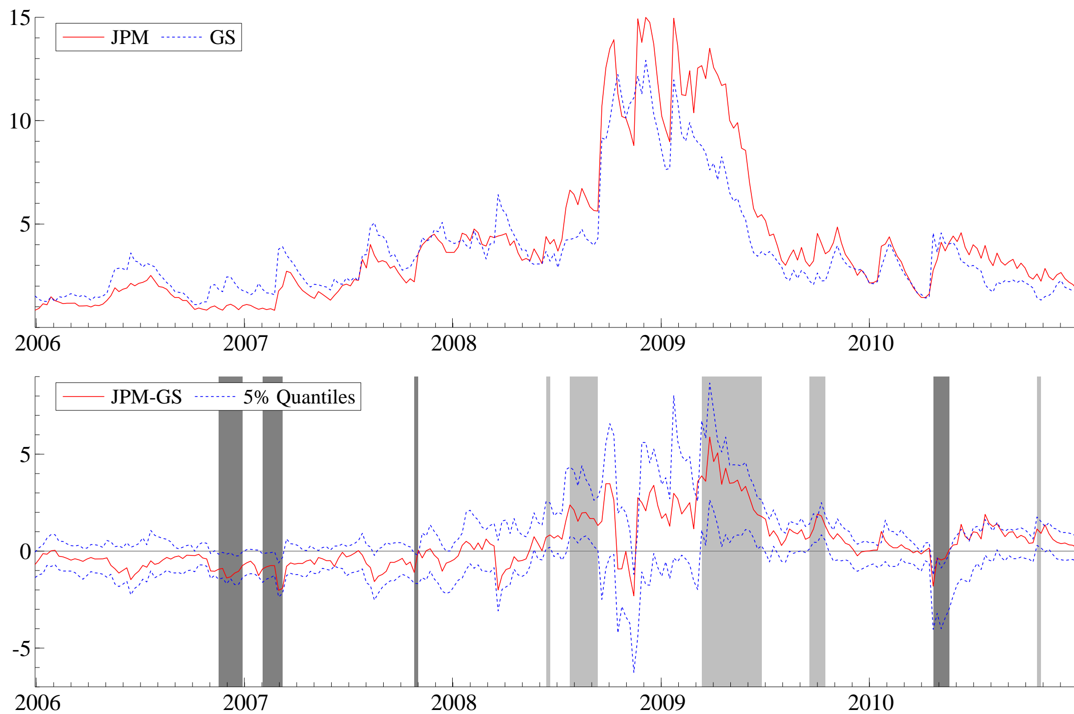
In this section we restrict our analysis to the subset of the sixteen most risky firms which were designated as Global and Domestic Systemically Important Banks (G- and D-SIBs) in 2009 by the Stress Tests of the Federal Reserve and kept that status through 2014. On every Friday of our sample, we estimate the conditional MES for each firm and we obtain the estimates' distribution by means of our bootstrapping procedure. Then, for all pairs of firms, we test for equality of MES at these dates.

As all these financial institutions are systemically important, they are all risky. Their time-series average MES ranges between about 2.43 for US Bancorp to 4.60 for Morgan Stanley. The average standard deviation of MES over the sixteen firms equals 3.80, suggesting that there is considerable time-variation in the MES.

To put our conditional approach into perspective, we contrast it with the unconditional approach of a DM-type test (Diebold and Mariano, 1995). Using a DM test on the time-series of MES forecasts, we reject the null of equal average risk for about 44% of the pairs. However, this assumes that we are only interested in the average risk over a time-period of twelve years. If we perform the DM test on single years, we reject on average 6.8% of the null hypotheses. The reduction in sample size leads to far fewer rejections. Overall, these results illustrate the limits of the risk comparison based on an unconditional approach. It has good power to distinguish firms when the sample size is long enough, but the relative risk of firms is too dynamic for the average risk over long periods to be of interest.

To illustrate our conditional approach, we plot the MES of J.P. Morgan and Goldman Sachs, along with their difference and its 5% confidence bounds in Figure 1. Significant differences are marked by shaded regions, dark indicating GS is more risky than JPM and light shading indicating the reverse. This figure illustrates that the MES of the two firms are highly correlated. Until 2008 the point estimates for GS are generally higher than those for JPM, and this order is reversed after 2008. However, although the point estimates may be different, they are not frequently significantly different. GS is more risky on 8.5% of sample days, while JPM's risk exceeds GS' on just 5.9% of days, so that the parameters

Figure 1: MES of JPM and GS

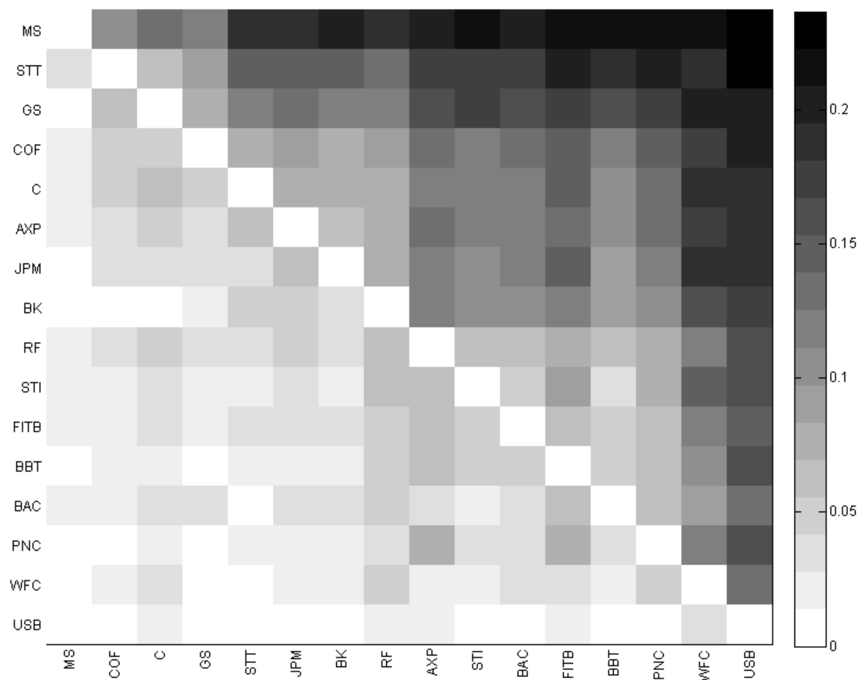


Note: The top panel shows the estimated MES of JPM and GS in the period 2006-2011. The MES is estimated every Friday. The bottom panel shows the difference, along with bootstrap confidence bounds. The shaded regions represent a significant difference between the two. When the shading is dark, GS has significantly higher MES than JPM, when it is light the reverse is true.

can only be estimated precisely enough on about 14.4% of the days to truly distinguish the two banks. Importantly, significant rejections are clustered with an autocorrelation of 0.7, meaning that the single days where one firm is more risky than the other, are rare.

The results for all other pairs are summarized in Figure 2. This figure plots the rejection frequencies for each pair, where the color corresponds to a value determining the frequency at which the firm on the y-axis is found to be more risky than the one on the x-axis. The heatmap shows that even the firms with highest MES are only significantly more risky (at 5%) than firms with the lowest MES about 20-25% of the time. On average, across pairs, we find a significant difference between firms on 16.4% percent of the days. Different significance levels do not change the relative picture much, but at 10% the highest rejection frequencies approach 50%.

Figure 2: Significant difference MES



Note: The heatmap plots the rejection frequencies over the full sample of the hypothesis that $H_0 : x_{ij,t} = 0$ vs $H_1 : x_{ij,t} > 0$, with i on the y-axis and j on the x-axis. A value of 0.25 means that the firm on y-axis had significantly higher MES than the firm on the x-axis on 25% of the days.

Table 3: Bucket Allocation – Top 10

MES				%SRISK				ΔCoVaR			
Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.
30-06-2008											
LEH	1	1	10.287	C	1	1	0.152	FITB	1	1	3.480
MBI	1	1	9.781	BAC	2	2	0.091	HBAN	1	1	2.874
CIT	1	1	8.111	JPM	2	3	0.081	LEH	1	1	2.685
WM	1	1	7.459	MER	2	3	0.078	KEY	1	1	2.372
PFG	1	1	6.563	MS	3	4	0.073	RF	1	1	2.330
ABK	1	2	7.806	FRE	4	5	0.065	C	1	1	2.277
FITB	1	2	7.733	FNM	4	6	0.063	STI	1	1	2.034
C	1	2	5.816	AIG	4	7	0.057	BBT	1	1	2.017
FRE	1	2	5.713	GS	5	7	0.056	AIG	1	1	2.008
MER	2	2	6.248	LEH	6	8	0.052	MI	1	1	1.951
30-01-2009											
STT	1	1	22.188	JPM	1	1	0.153	AFL	1	1	9.049
C	1	1	20.884	C	2	2	0.142	PNC	1	1	8.266
HBAN	1	1	20.775	BAC	3	3	0.129	STT	1	1	6.891
FITB	1	1	19.821	WFC	4	4	0.093	FITB	1	1	6.414
PNC	1	1	19.817	AIG	5	5	0.063	BAC	1	1	5.974
AFL	1	1	19.499	GS	5	6	0.061	ACAS	1	1	5.537
LNC	1	1	19.032	MS	6	7	0.046	ALL	1	1	5.487
BAC	1	1	18.491	MET	6	8	0.036	WFC	1	1	5.399
HIG	1	1	17.415	PRU	7	9	0.034	STI	1	1	5.258
PFG	1	1	17.097	HIG	8	10	0.022	C	1	1	5.139

Note: This table provides the ranking estimated by the FWE and FDR controlling methods, based on the MES, %SRISK and ΔCoVaR risk measures. We show only the top 10 of firms sorted by assigned bucket.

6.2 Buckets

In this section, we apply the bucketing procedure to the 94 financial institutions for three systemic risk measures, the MES, the %SRISK and the ΔCoVaR, which were defined in Examples 2, 3 and 4. By applying the bucketing procedure, we test whether an absolute ranking can be distinguished. If no absolute ranking can be distinguished, we want to test whether we can, at least, identify buckets of firms that are indistinguishable from each other within the bucket but distinguishable from firms belonging to lower ranked buckets. The three systemic risk measures are differently affected by the estimation risk, and are also likely to differ in the ordering of their point estimates (Benoit et al., 2012). As a consequence, different risk measures can lead to different rankings.

We estimate the bucket allocation for the MES, %SRISK and ΔCoVaR on eight predetermined dates coinciding with those considered in Brownlees and Engle (2015). A firm is included in the ranking at a certain date, if the firm still exists and furthermore, there are at least 1,000 observations up until this date. Table 3 displays the results of the bucketing

procedure, with $\alpha = 0.05$, for two days. The results for the remaining days are deferred to Appendix B. The firms are first ranked in terms of their bucket, and within buckets we order the firms in descending value of their risk measure estimate, even though there is no statistical evidence that their risk is statistically different. We then report the ten highest ranked firms, as is done in Brownlees and Engle (2015). For each firm, we report the point estimate, as well as the allocated bucket according to the FWE and the FDR method.

The results suggest that it is indeed difficult to find significant differences between the estimated risk measures. Although point estimates may vary considerably, they are not necessarily statistically different. In general, in line with theory, the FDR method is more powerful, and we obtain smaller buckets compared to the FWE. In June 2008, the precision of the MES estimates allows for a division of the top 10 risky firms into two buckets. The size of the most risky bucket using FDR is five firms, compared to nine for the FWE. The procedure rejects more frequently for the %SRISK, allowing for six or eight buckets for the top 10 firms. The reason for this is that the liabilities and the market value of the firm, introduced in the definition of the SRISK (see Example 3), add variability between the different point estimates, without adding additional estimation risk. In fact, in January 2009 we find an absolute ranking using the FDR method. Like for the MES, it is difficult to statistically distinguish firms based on ΔCoVaR. The ΔCoVaR is defined as the product of a conditional VaR and a quantile regression parameter (see Example 4). Most of the estimation risk comes from the quantile regression. For instance, the highest point forecast of ΔCoVaR is 9.05 for AFL, but its bootstrap standard deviation is close to 4. In an unreported simulation, we find that even if the true DGP is exactly the one assumed here, the standard deviation of the ΔCoVaR is still on average over 40% of its value. These results are in line with those obtained in another context by Guntay and Kupiec (2015). Replacing the quantile estimate γ_α of Example 4 with an OLS estimate significantly reduces the uncertainty, leading to buckets of sizes in between those of MES and %SRISK.

In Table 4, we investigate the sensitivity of the bucketing procedure to the significance level chosen. We report the total number of estimated buckets on each of the eight days, at five different significance levels. The Model Confidence Set (Hansen et al., 2011), on which our procedure is based, is commonly estimated using confidence levels upwards of

Table 4: Number of estimated buckets

Date	#Firms	Significance Level									
		FWE Controlling Procedure					FDR Controlling Procedure				
		20%	10%	5%	2.5%	1%	20%	10%	5%	2.5%	1%
		MES									
30-03-2007	83	5	4	4	3	3	7	6	4	4	3
29-06-2007	83	5	3	3	3	3	5	4	4	3	3
31-12-2007	81	5	4	4	4	3	10	8	5	5	5
29-02-2008	82	5	5	4	4	4	12	8	5	5	4
30-06-2008	82	5	5	5	5	5	11	7	6	6	5
29-08-2008	81	8	7	6	5	5	16	10	8	7	7
30-01-2009	73	6	5	4	4	4	15	9	7	6	5
30-06-2010	75	4	3	3	3	3	7	5	5	4	4
		%SRISK									
30-03-2007	14	6	6	5	5	4	7	6	6	6	5
29-06-2007	13	6	5	4	4	4	10	7	6	6	4
31-12-2007	36	17	13	11	11	11	21	17	15	15	12
29-02-2008	37	16	15	13	12	12	25	19	17	16	14
30-06-2008	39	18	17	15	13	12	37	26	21	19	17
29-08-2008	36	15	14	13	11	10	33	25	18	16	15
30-01-2009	53	31	29	29	29	29	49	45	39	34	31
30-06-2010	37	18	16	15	13	11	31	22	20	18	15
		ΔCoVaR									
30-03-2007	83	1	1	1	1	1	2	2	2	2	1
29-06-2007	83	1	1	1	1	1	1	1	1	1	1
31-12-2007	81	2	1	1	1	1	2	2	2	2	2
29-02-2008	82	2	2	2	2	1	2	2	2	2	2
30-06-2008	82	3	2	2	2	2	3	2	2	2	2
29-08-2008	81	2	2	2	2	2	2	2	2	2	2
30-01-2009	73	2	2	2	2	2	4	4	3	3	3
30-06-2010	75	1	1	1	1	1	2	2	2	2	2

Note: This table reports the sensitivity of the procedures to the level of FWE and FDR that is controlled. We show the total number of firm and the number of buckets they are assigned to.

20%. We generally have enough power to be more prudent and consider 20, 10, 5, 2.5 and 1%, for both FWE and FDR. As a reference, the second column of the table gives the total number of firms under consideration, providing a cap on the number of buckets possible.

As rejection occurs more frequently with lower significance levels, the number of buckets is increasing with the significance level. We confirm that the FDR procedure is uniformly more powerful than the FWE procedure. For instance for the MES, the FDR estimates up to twice as many buckets than the FWE. With the %SRISK, the FDR procedure using high confidence levels comes close to absolute rankings, with the total number of buckets only slightly lower than the number of firms. Even at very stringent levels, we get interesting rankings with buckets that do not contain more than three or four firms. Finally, significance levels of 20% still do not help with disentangling the ΔCoVaR of different firms.

This reaffirms the uncertainty in the quantile regression estimates.

7 Conclusion

This paper introduces a bootstrap-based comparison test of two risk measures, as well as an iterative procedure to produce a grouped ranking of $N > 2$ assets or firms, given their conditional risk measures. These tests can be applied to a wide variety of conditional risk measures, while taking into account their estimation risk. Simulation results on VaR and MES forecasts suggest that the pairwise comparison test has good properties in finite samples, both in terms of size and power. Since the bucketing procedure is clearly a multiple testing problem, we propose two versions, one controlling the FWE rate, and one controlling the FDR rate. Simulations show that both set-ups do control their respective rates, and illustrate the trade-off of using either method depending on the size of the problem.

In the empirical application, we apply the pairwise comparison test to the MES estimates of sixteen U.S. G- and D-SIBs. This application points out the advantages of the comparison of *conditional* risk measures. We observe a great time-variation in conditional MES estimates, and from one week to the next, firms' relative ranking often changes. While unconditional tests of equal MES between any of the pairs is rejected in 44% of the days, the conditional test only rejects equal risk 6.8% of the time, showing that, on most days, we cannot distinguish firms in terms of their riskiness.

We applied the bucketing procedure for three popular systemic risk measures, namely the MES, the ΔCoVaR and the SRISK. We find that for both versions of the procedure, the MES and ΔCoVaR are estimated with too much uncertainty to reject equality often. For most of the eight dates considered in the application, the first thirty firms belong to the same bucket of riskiest firms. Consequently, ranking firms on the basis of point forecasts of MES and ΔCoVaR seems hazardous. However, when applied on %SRISK, our bucketing procedure is able to identify a meaningful ranking of buckets containing equally risky firms in each bucket. This results is mainly due to the differences observed in the liabilities and the market value of the financial institutions over the period 2000-2012. Since the liabilities and market values are not estimated, these differences add cross-sectional variability in the systemic risk measures, without adding additional estimation risk. Our results clearly

illustrate the importance of taking into account the estimation risk when establishing a ranking of the financial institutions according to their systemic risk.

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A Company Tickers

Depositories(29)		Insurance (32)	
BAC	Bank of America	ABK	Ambac Financial Group
BBT	BB&T	AET	Aetna
BK	Bank of New York Mellon	AFL	Aflac
C	Citigroup	AIG	American International Group
CBH	Commerce Bancorp	AIZ	Assurant
CMA	Comerica inc	ALL	Allstate Corp
HBAN	Huntington Bancshares	AOC	Aon Corp
HCBK	Hudson City Bancorp	WRB	W.R. Berkley Corp
JPM	JP Morgan Chase	BRK	Berkshire Hathaway
KEY	Keycorp	CB	Chubb Corp
MI	Marshall & Ilsley	CFC	Countrywide Financial
MTB	M&T Bank Corp	CI	CIGNA Corp
NCC	National City Corp	CINF	Cincinnati Financial Corp
NTRS	Northern Trust	CNA	CNA Financial corp
NYB	New York Community Bancorp	CVH	Coventry Health Care
PBCT	Peoples United Financial	FNF	Fidelity National Financial
PNC	PNC Financial Services	GNW	Genworth Financial
RF	Regions Financial	HIG	Hartford Financial Group
SNV	Synovus Financial	HNT	Health Net
SOV	Sovereign Bancorp	HUM	Humana
STI	Suntrust Banks	LNC	Lincoln National
STT	State Street	MBI	MBIA
UB	Unionbancal Corp	MET	Metlife
USB	US Bancorp	MMC	Marsh & McLennan
WB	Wachovia	PFG	Principal Financial Group
WFC	Wells Fargo & Co	PGR	Progressive
WM	Washington Mutual	PRU	Prudential Financial
WU	Western Union	SAF	Safeco
ZION	Zion	TMK	Torchmark
		TRV	Travelers
		UNH	Unitedhealth Group
		UNM	Unum Group

Broker-Dealers (10)		Others (23)	
AGE	A.G. Edwards	ACAS	American Capital
BSC	Bear Stearns	AMP	Ameriprise Financial
ETFC	E-Trade Financial	AMTD	TD Ameritrade
GS	Goldman Sachs	AXP	American Express
LEH	Lehman Brothers	BEN	Franklin Resources
MER	Merill Lynch	BLK	Blackrock
MS	Morgan Stanle	BOT	CBOT Holdings
NMX	Nymex Holdings	CBG	C.B. Richard Ellis Group
SCHW	Schwab Charles	CBSS	Compass Bancshares
TROW	T.Rowe Price	CIT	CIT Group
		CME	CME Group
		COF	Capital One Financial
		EV	Eaton Vance
		FITB	Fifth Third bancorp
		FNM	Fannie Mae
		FRE	Freddie Mac
		HRB	H&R Block
		ICE	Intercontinental Exchange
		JNS	Janus Capital
		LM	Legg Mason
		NYX	NYSE Euronext
		SEIC	SEI Investments Company
		SLM	SLM Corp

B Bucket Allocation Top 10

MES				%SRISK				ΔCoVaR			
Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.
				30-03-2007							
LEH	1	1	3.540	MS	1	1	0.212	LEH	1	1	1.094
BSC	1	1	3.469	FRE	2	2	0.146	AGE	1	1	1.053
MS	1	1	3.446	FNM	2	2	0.134	MS	1	1	0.979
AMTD	1	1	3.427	MER	2	3	0.105	BSC	1	1	0.952
ETFC	1	1	3.260	LEH	3	3	0.104	BEN	1	1	0.904
AGE	1	1	3.221	GS	3	3	0.103	MER	1	1	0.900
JNS	1	1	3.159	BSC	4	4	0.091	GS	1	1	0.894
GS	1	1	3.158	MET	5	5	0.034	LM	1	1	0.844
BEN	1	1	3.155	HIG	5	5	0.019	C	1	1	0.831
MER	1	1	2.816	PRU	5	5	0.018	BBT	1	1	0.821
				29-06-2007							
AMTD	1	1	3.009	MS	1	1	0.179	MBI	1	1	0.944
BSC	1	1	2.799	FRE	1	2	0.164	GS	1	1	0.848
MER	1	1	2.630	MER	1	2	0.155	C	1	1	0.814
MBI	1	1	2.619	BSC	2	3	0.111	LEH	1	1	0.800
SCHW	1	1	2.601	LEH	2	3	0.109	MER	1	1	0.790
GS	1	1	2.544	FNM	2	3	0.094	JPM	1	1	0.762
LEH	1	1	2.431	GS	2	4	0.092	BSC	1	1	0.754
ETFC	1	1	2.350	MET	3	5	0.033	SCHW	1	1	0.728
TROW	1	1	2.235	PRU	3	5	0.026	EV	1	1	0.712
MS	1	1	2.197	HIG	3	5	0.021	HRB	1	1	0.685
				31-12-2007							
ETFC	1	1	9.406	C	1	1	0.166	MBI	1	1	3.173
MBI	1	1	9.288	MER	2	2	0.102	ABK	1	1	2.377
ABK	1	1	7.528	MS	3	3	0.092	ETFC	1	1	2.288
FRE	1	1	6.960	FRE	3	4	0.084	SLM	1	1	2.123
CFC	1	1	6.036	FNM	3	5	0.077	NCC	1	1	1.741
WM	1	1	5.815	GS	3	5	0.076	WM	1	1	1.731
CITa	1	1	5.378	LEH	4	6	0.061	C	1	1	1.688
FNM	1	1	5.276	JPM	4	6	0.060	FRE	1	1	1.621
HBAN	1	1	4.629	BAC	4	7	0.040	FITB	1	1	1.579
MS	1	2	5.148	BSC	5	7	0.034	BBT	1	1	1.548
				29-02-2008							
ABK	1	1	8.266	C	1	1	0.151	AIG	1	1	1.774
MBI	1	1	7.644	MER	2	2	0.089	CNA	1	1	1.715
LEH	1	1	5.279	MS	2	3	0.083	MI	1	1	1.674
CIT	1	1	5.279	FNM	2	4	0.078	MER	1	1	1.666
WM	1	1	5.150	JPM	2	4	0.077	C	1	1	1.575
MER	1	1	5.105	BAC	2	5	0.074	RF	1	1	1.571
FRE	1	1	4.816	FRE	3	5	0.074	LEH	1	1	1.561
CNA	1	1	4.783	GS	3	6	0.061	JPM	1	1	1.503
MI	1	1	4.613	LEH	4	7	0.045	EV	1	1	1.467
BSC	1	1	4.607	BSC	5	8	0.038	HBAN	1	1	1.402

MES				%SRISK				ΔCoVaR			
Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.	Tick	FWE	FDR	Est.
29-08-2008											
FRE	1	1	13.593	C	1	1	0.134	AIG	1	1	2.972
FNM	1	1	13.399	JPM	2	2	0.097	LEH	1	1	2.495
ABK	1	1	12.942	BAC	2	2	0.096	MI	1	1	2.491
LEH	1	1	12.422	MER	3	3	0.074	FRE	1	1	2.429
MBI	1	2	9.642	FRE	3	3	0.074	MER	1	1	2.306
AIG	1	2	8.656	AIG	3	4	0.072	RF	1	1	2.304
RF	1	2	7.682	FNM	4	4	0.070	KEY	1	1	2.049
MER	2	2	8.633	MS	5	5	0.067	FNM	1	1	2.006
BAC	2	2	7.227	GS	6	6	0.060	SNV	1	1	1.967
WM	2	2	6.969	LEH	7	7	0.051	C	1	1	1.939
30-06-2010											
ABK	1	1	7.617	C	1	1	0.164	MTB	1	1	1.927
CBG	1	1	6.968	BAC	1	1	0.161	BEN	1	1	1.882
MI	1	1	6.792	JPM	2	2	0.138	TROW	1	1	1.788
JNS	1	1	6.752	AIG	3	3	0.086	EV	1	1	1.674
ETFC	1	1	6.680	MS	4	4	0.071	MI	1	1	1.655
ACAS	1	1	6.619	WFC	4	5	0.048	AFL	1	1	1.609
LNC	1	1	6.568	MET	5	5	0.046	AXP	1	1	1.575
PFG	1	1	6.319	GS	5	5	0.044	CINF	1	1	1.512
MBI	1	1	6.229	PRU	5	6	0.042	GS	1	1	1.492
AMP	1	1	6.188	HIG	6	7	0.031	SCHW	1	1	1.472