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EBF response to EBA consultation on *Draft Regulatory Technical Standards on the calculation of the stress scenario risk measure under Article 325bk(3) of Regulation (EU) No 575/2013 (Capital Requirements Regulation 2 - CRR2)*

Q1. What is your preferred option among option A (stress period based extreme scenario of future shock) and option B (extreme scenario of future shock rescaled to stress period)? Please elaborate highlighting pros and cons.

An optimal framework would be a **combination of both approaches** depending on the data quality of risk factors:

- When a risk factor has sufficient data quality, direct determination of the extreme scenario of a future shock for a NMRF over the selected stressed period (Option A) is preferable to any rescaling of a current window shock that could be obtained looking at MRF (B).
- However, when considering the stressed window calibration process associated to the two options, it is also evident that the stressed period calibration associated to option A is computationally too intensive and not pursuable in its current form.
- In case the stressed window calibration approach could be disentangled from the determination of the stressed scenario, option A would be preferable to option B (only for those risk factors with sufficient data quality across the stressed period).

In case alternative proposals to the calibration of the stressed window were not considered (e.g. Q 12) **then option B would be the only implementable solution**.

Q2. What are characteristics of the data available for NMRF in the data observation periods under options A and B?

Option A is only applicable to NMRF with sufficient data quality over a stressed period (which might even mean 12 observations if the sigma approach was allowed) while Option B is more suitable for NMRF without enough observations over the stressed period.

This shows that – over a given stressed window – the two approaches for the determination of the stressed shocks could be contextually used for risk-factors with different data quality.

Q3. Do you think that institutions will actually apply the direct method to derive the extreme scenario of future shock or do you think that given the computational efforts that it requires and considering that the historical method typically provides very similar results it will not be used in practice? As stated in the background section of this CP, the EBA will drop the direct method from the framework if not provided with clear evidence for its need.

We do not believe that Direct Method could be used in practice due to the computational effort it requires, especially in the context of the stressed window calibration.

I.e. since the direct method is only considered in association to option A for the calibration of the stressed window (and the consequent definition of the stressed shock) it will be overly burdensome to turn into a productive implementation.

Admittedly, even leaving aside the stressed calibration window, the calculation of an ES in Full Revaluation for each NMRF will end up requiring many more revaluations than the IMCC calculation (already much more demanding than Basel 2.5 metrics).

Q4. What is your preferred option among (i) the representative risk factor – parallel shift option, and (ii) the contoured shift option? Please elaborate highlighting pros and cons.

The contoured shift option in that more closely represents the characteristics of the individual risk factors embedded in a regulatory bucket.

Q5. What are your views on how institutions are required to build the time series of 10 business days returns? Please elaborate.

The proposed approach does not pose concerns.

Q6. What is your preferred option among (i) the sigma method and (ii) the asymmetrical sigma method for determining the downward and upward calibrated shocks? Please highlight the pros and cons of the options. In addition, do you think that in the asymmetrical sigma method, returns should be split at the median or at another point (e.g. at the mean, or at zero)? Please elaborate.

The sigma method, primarily to stick to an uncontroversial metric that does not open for additional degrees of freedom in its determination (e.g. how to split return).

Q7. What are your views on the value taken by the constant C_{ES} for scaling a standard deviation measure to approximate an expected shortfall measure?

The constant depends clearly on the empirical distribution. Three seems on the conservative side.

Q8. What are your views on the uncertainty compensation factor $(1+C_{UC}/\sqrt{2(N-1.5)})$? Please note that this question is also relevant for the purpose of the historical method.

Uncertainty compensation factor should be 1 if there is full data used (ie 250 data points in the historical method). The uncertainty formula should be adjusted to achieve that.

Q9. What are your views on the fallback method that is envisaged for risk factors that are included in the sensitivity-based method? Please elaborate.

The expectation that the fall-back approach is going to be of limited use under option B, since most risk-factors in a Risk Management model are expected to have more than 12 observations over the current window.

The proposed SBM-fallback method is only applicable to non-modellable risk factors that coincide with SBM risk factors or only differ from SBM risk factors in the maturity dimension. However, NMRF are often basis rather than directional risk factors, the more so as a bank may use the flexibility offered under MAR31.13 footnote 3, to keep in the IMCC the systemic risk associated to the risk factors and include in the NMRF framework only the basis or spread risk. We therefore would see it fit to expand the use of the SBM-fallback method as depicted below.

When a non-modellable risk factor is a basis or a spread between risk factors that coincide with SBM risk factors, the risk weight to be used for the SBM-fallback method is the one that would result in the same SBM capital charge when applied to the basis or spread position in the standardised approach.

In general, the risk weight to be used is a function of the correlation between the two SBM risk factors and the SBM risk weights applicable to each of the SBM risk factors:

$$rw_{NMRF} = \sqrt{{rw_1}^2 - 2 \cdot \rho_{1,2} \cdot rw_1 \cdot rw_2 + {rw_2}^2}$$

Where '1' and '2' refers to the SBM risk factor 1 and 2. However, this generic formula may often be simplified since the applicable risk weight to SBM risk factor '1' and '2' are often identical.

This approach should be restricted to instances where the basis or spread relates to the difference of two strongly related SBM risk factors (or two SBM risk factors that differ only in the maturity dimension). Hence, we see this approach fit only for a bucket basis risk or spread risk to a SBM tenor, everything else equal. For example the 12 to 10 year spread risk on an IR curve shall be risk weighted at:

$$rw_{12-10\,Yr\,spread} = rw_{10\,Yr} \cdot \sqrt{2\left(1 - \rho_{10,12\,Yr}\right)} = rw_{10\,Yr} \cdot 0.11.$$

From there onward, the SBM fallback method will apply as depicted in the consultation paper.

Specifically, we do not consider that this approach is applicable to the basis risk between a single name credit spread and a credit index or a single equity stock and an index. Indeed, either the index is a diversified index fulfilling the criteria of MAR21.31 and assigned to an index bucket, in which case the basis will be across buckets, or the index is assigned to the single name credit or equity bucket but, given that the SBM approach does not recognise the higher correlation that should prevail with an index, the correlation will be understated and the risk overstated. Hence, we consider that the case of basis risk between a single name credit spread and a credit index or a single equity stock and an equity index will be better addressed using the fallback "same type of risk factor" method.

Finally, since the SBM risk weights have been calibrated to be conservative for most SBM risk factors within a bucket, including those with limited observability, no uncertainty multiplier is needed: the SBM risk weights are already conservative, i.e. overstated for the majority of risk factors to which it applies. Hence, it is our view that the uncertainty multiplier of the SBM-fallback method shall be set to 1 when the NMRF coincide with a SBM risk factor (or is a basis or spread between two NMRF risk factors that coincide with SBM risk factors).

Q10. What are your views on the fallback method that is envisaged for risk factors that are not included in the sensitivity-based method? Please comment on both the 'other risk factor' method, and the 'changing period method'.

Not to additionally complicate the methodology for such corner cases, we would favor a simpler option, a mapping to one of the SBM RW.

Q11. What are your views on the conditions identified in paragraph 5 that the 'selected risk factor' must meet under the 'other risk factor' method? What would be other conditions ensuring that a shock generated by means of the selected risk factor is accurate and prudent for the corresponding non-modellable risk factor?

See Q10

Q12. What are your views on the definition of stress period under option A (i.e. the period maximizing the rescaled stress scenario risk measures for risk factors belonging to the same broad risk factor category)? What would be an alternative proposal?

The algorithm for the identification of the stress period under option A (maximization of the losses stemming from the direct method for each Broad Risk Category (BRC) is simply not manageable and drives the choice to Option B.

Indeed the number of instrument revaluations required to calibrate the stressed window for each BRC can quickly become unmanageable. We explain the point through a comparison with the UES calibration approach used for the ES part of IMA.

- From 2007 to today there are about 3500 10-day returns
- The calibration of UES for a Bond in (e.g.) CZK for an EU Bank requires **3500** revaluations of the Bond which are then aggregated in 250-sets to identify the one with the highest ES.
- The calibration of SES for the same bond depends on:
  1. The number of non Modellable buckets for each RF (e.g. 3 buckets for CS and 4 buckets for IR; FX is instead Modellable → x7
  2. Use of Direct Method (→ **x250**) **vs** the used of the Step Wise Method (**x6 grid-points**)
- This could hence result in either 3500 revaluations for each NMRF aggregated in 250-sets to identify the set with the highest ES (Direct approach: **3500x7** revaluations of the bond) or 6 revaluations within each of the stressed periods that can be identified between 2007-today; for historical return method, due to the

overlap between periods it is conceivable that this results in 3250*5% windows, where 5% represents the tails over which ES is computed in each window. As a result the number of revaluations of the bond could be **3250*5%x6x7**.

- For sigma method, there is not overlap as even a 1-day change of the period changes the Stdev of the returns and as a result the number of revaluations of the bond could be **3250x6x7**.
- While the calibration through the step-wise method could look computationally lighter than the UES calibration, it nevertheless shows a linear dependence to the pairs **InstrumentWithNMRF x NMRF** that can quickly become larger than the overall number of Instruments in the portfolio. In this example the number of revaluations is milder than the UES calibration but it is due to the fact that we are considering a single instrument and 7 NMRF. For a real life portfolio with hundred thousands of instruments and thousands of NMRF the computational burden will clearly blow out.

In order to substantially reduce the computational needs to a manageable level, firms could be allowed to use a sensitivity approach to determine the stress window even though those risk factors may be modelled for capital (ES, SES) under a full revaluation approach.

Alternatively, a proposed approach would be to use a RF based approach as is used in Option B to identify the stressed period per BRC and to make the assumption that a worse stress period for the modelled risk factors is a suitable period to use for the SES for that broad risk class.

Q13. What are your views on the definition of maximum loss that has been included in these draft RTS for the purpose of identifying the loss to be used as maximum loss when the latter is not finite? What would be an alternative proposal?

When for a NMRF the maximum loss is non-finite loss, banks should be allowed to provide an alternative expert based stress scenario using qualitative and quantitative information calibrated to be at least as conservative as a 97.5% stressed ES and not the 99.95% proposed by the draft RTS, which would result in another element of conservativism to the framework.

Q14. How do you currently treat non-pricing scenarios (see section 3.2.5 of the background section) if they occur where computing the VaR measures? How do you envisage implementing them in (i) the IMA ES model and (ii) the SSRM, in particular in the case of curves and surfaces being partly shocked? What do you think should be included in these RTS to address this issue? Please put forward proposals that would not provide institutions with incentives that would be deemed non-prudentially sound and that would target only the instruments and the pricers for which the scenario can be considered a 'non-pricing scenario'.

In the ES or VaR model shifts to curves or surfaces are applied in a scenario consistent way, ie all the points on that curve or surface are jointly shifted according to the historical realized dynamic. Therefore, pricing issues resulting from the application of large shifts to only one portion of the curve/surface are not really frequent so that at the moment the affected instrument is removed by that particular scenario.

On the contrary for SES calculation a stress shift is applied to only one part of a curve or surface so this is an important point to consider.

However in practice NMRF will be decomposed into a portion that is included in the ES model and a basis that is used in the SES. The fact that the SES basis shifts will be smaller than the outright RF shifts already embeds a natural mitigation. It is however conceivable that shifting only a portion of a curve/surface will still lead to pricing errors. Therefore it would be useful to introduce mechanisms and safety valves that could be applied and give resilience if this does arise.

The fundamental problem, that can occur when a small portion of a curve is shifted by a large amount and the other parts are left constant, is that the shift size amount is unrealistically large versus the parts that are not shifted and this breaks the consistency of the curve or surface that is applied in a stress (and what is applied, is economically meaningless).

When a non-pricing scenario is identified for certain product/pricer combination, the banks should be allowed to adjust the scenarios for the product/pricer combination in question. Such adjustment includes e.g. de-arbitration, imposing floor/cap, and etc. The adjusted scenario should be permitted as long as banks can provide sufficient documentation on the methodology and evidence of the case when this adjustment is applied should be tracked and made available to competent authorities.

Q15. What are your views on the conditions included in these draft RTS for identifying whether a risk factor can be classified as reflecting idiosyncratic credit spread risk only (resp. idiosyncratic equity risk only)? Please elaborate.

The risk factors reflecting idiosyncratic credit spread risk and idiosyncratic equity risk are aggregated with zero correlation. NMRF basis will be created by decomposing NMRF into a component that is suitable to represent the RF in the ES model and a residual basis. This choice of decomposition will be driven by getting as a good representation of the RF in the ES model. The residual basis should not be correlated. The condition under (b) can be too specific, so propose a modification.

Change clause (b) as follows: "the value taken by the risk factor should not be systematically correlated with other credit (equity) idiosyncratic factors"

Change clause (c) as follows: "the institution performs and documents the statistical tests that are used to verify point (b). This can include tests that prove values taken are not driven by systematic risk components.

Q18. Would you consider it beneficial to set the tail parameter ϕ to the constant value 1.04 regardless of the methodology used to determine the downward and upward calibrated shock (i.e. setting ϕ = 1.04 also under the historical method, instead of using the historical estimator)? Please elaborate.

Yes, to simplify the framework a bit.

Q19. Do you agree with the definition of the rescaling factor $m^{i}_{s,c}$, under option B or do you think that the rescaling of a shock from the current period to the stress period should be performed differently? Please elaborate.

The scaler in its current definition is prone to spikes in those cases where a BRC is dominated by MRFs with a very low standard deviation over the current period. The trimming could help in this context however the effectiveness depends on the relative presence of such types of risk factors **among the MRFs of the BRC**. In case the difference in volatility between current and stressed period is the result of a change in market regime (e.g. negative rates) such extreme re-scaling would not be necessarily appropriate, especially because it would then affect any other risk factor in that BRC.

A relevant example can be identified with EUR rates in particular over the short term pillars where the insurgence of negative rates has also caused a significant compression of the standard deviation over the current window. Current trimming at 1% can be effective in reducing such extreme cases only to the extent that these risk-factors represent 1% of the MRF for the affected BRC. For a portfolio dominated by EUR this might not be the case.

In order to reduce the effect it would be beneficial to allow a **higher trimming for those BRC** (i.e. IR) where this effect is visible to an amount that reflects the relative presence of these types of RF among the MRF of that BRC. The refinement of the trimming confidence level would have to be documented.

Q20. The scalar $m^{i}_{s,c}$, is obtained by using data related to modellable risk-factors in a specific risk class (i.e. the class i). As a result, such a scalar is not defined where an institution does not have any modellable risk factor in this risk class. How do you think the scalar $m^{i}_{s,c}$, should be determined in those cases? Please elaborate.

We would propose using for that BRC the scale used to scale the $ES_{F,C}$ in the IMCC portion of the IMA, i.e. $ES_{R,S}$ /$ES_{R,C}$

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