ABI Associazione Bancaria Italiana

# ABI response to the EBA consultation EBA/CP/2020/10

# "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)"

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**POSITION PAPER**

## Introductory remarks

The Italian Banking Association (ABI) welcomes the opportunity to comment on the EBA/CP/2020/10 "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)".

The draft RTS specify the approach that banks shall apply in the calculation of the stress scenario risk measure for non-modellable risk factors (NMRFs), in the context of the IMA under the FRTB framework as transposed in the EU regulation (for reporting purposes).

As a general remark, ABI would highlight that, based on the assessment and initial exercises performed by the banks, the proposed approach appears to be very demanding operationally. The effort required seems to be disproportionate, if we consider that the NMRFs should in principle constitute a residual component of the internal models and the SES is supposed to represent a limited share of the overall market risk measure of a bank.

In ABI's opinion, in drafting the final RTS the EBA should aim to striking the right balance between, on one side, significance and conservativeness of the outcome of the models, and, on the other side, the operational burden for banks.

The response to the questions, that are provided below, should be read in light of the above considerations.

## Response to the questions for consultation

**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.

In principle, direct determination of the extreme scenario of a future shock for a NMRF over the selected stressed period (option A) is deemed preferable.
However, the process required for the stressed period calibration associated to option A is computationally too intensive and appears not viable 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 (at least 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 solution feasible in practice.

**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). Option B is more suitable for NMRF for which enough observations over the stressed period are not available.
Over a given stressed window, the two approaches for the determination of the stressed shocks could therefore 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.

The direct method is expected to be of very limited use 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).
In any case, the calculation of an ES in full revaluation for each NMRF would require a much higher number of revaluations than the IMCC calculation (which is in turn 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 preferred alternative is the contoured shift option, which 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 option that is deemed preferable is the sigma method, as it does not leave rooms for different applications (such as the abovementioned different approaches for splitting returns).

**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 clearly depends on the empirical distribution. The proposed value of $C_{ES}$ =3 is deemed to be rather conservative.

**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.

In ABI's opinion the uncertainty compensation factor should be equal to 1 if full data is used (i.e. 250 data points in the historical method). ABI would therefore suggest adjusting the uncertainty formula accordingly.

**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 is for the fall-back approach 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. It is therefore considered 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 should be 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' refer to the SBM risk factors 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.

In addition, 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 deemed to be needed. 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'.

As the scope of this provision is expected to be limited, in order to avoid adding further complexity to the framework a simpler option is proposed, i.e. 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 the response to 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) – appears 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. The point is explained 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. number of non-modellable buckets for each risk factor (e.g. 3 buckets for CS and 4 buckets for IR; FX is instead modellable → **x7**
  2. Use of Direct Method (→**x250**) vs use of the Stepwise 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 Stepwise 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 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 NMRFs, the computational burden will clearly blow up.

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?

In ABI's opinion, when for a NMRF the maximum loss is non-finite, 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 (instead of the 99.95% proposed in the draft RTS, which would result in adding 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, i.e. 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 others. 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. As the condition under (b) can be too specific, a modification is proposed, i.e. to change clause (b) as follows: "the value taken by the risk factor *should not be systematically correlated with other credit (equity) idiosyncratic factors* ~~shall not be driven by systematic risk components~~"
Accordingly, clause (c) should be modified as follows: "the institution performs and documents the statistical tests that are used to verify ~~the condition in~~ point (b). *This can include tests that prove values taken are not driven by systematic risk components.*

**Q16.** What are your views on flooring the value taken by non-linearity coefficient κ to 0.9? Please elaborate.

**Q17.** What are your views on the definition of the tail parameter $\phi_{avg}$ where a contoured shift is applied (i.e. average of the tail parameters of all risk factors within the regulatory bucket)? Please elaborate.

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

Yes, it is considered beneficial in order to simplify the framework.

**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.

The proposed solution is using for that BRC the scale used to scale the $ES_{F,C}$ in the IMCC portion of the IMA, i.e. $ES_{R,S}/ES_{R,C}$ .