**PHD FELLOWSHIP STRATEGIC BASIC RESEARCH EVALUATION/ score grid with scoring descriptors - INTERVIEW**

**PHD FELLOWSHIP: SCORING DESCRIPTORS CRITERION “CANDIDATE” (INTERVIEW)**

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<thead>
<tr>
<th>0</th>
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<tr>
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During the interview, candidates are assessed on their potential to develop towards an independent researcher with proper reasoning skills and a critical mindset. Scientific knowledge and project insight are also key elements in the evaluation. Descriptions in the score grid ("potential", "competent", "knowledge", "skills", "mindset", ...) implicitly also take into account the evaluation findings of the preselection phase. SB candidates should as well reveal potential competences on strategic innovation oriented thinking, including some economic insight and positioning of their research.

### 1.a Potential competence as an independent doctoral researcher (reasoning skills and critical mindset, scientific knowledge and project insight)

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<tr>
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<tr>
<td>□ Lack of the inherent qualities required of a doctoral researcher. Reasoning skills and critical scientific mindset are below par. Not even strict guidance or supervision would allow to adequately compensate for this;</td>
<td>□ Research skills are present: with close supervision, able to obtain a PhD. Reasoning skills and critical mindset below average and to be developed further;</td>
<td>□ Research skills present, candidate is able to carry out research relatively independently. Lacks some maturity, but is motivated. Relatively good reasoning skills but less critical attitude.</td>
<td>□ Motivated and (potentially) competent independent researcher. (Very) good reasoning skills and a good critical scientific attitude. Presents new concepts in a meaningful way.</td>
<td>□ Very convincing and driven candidate with great potential as researcher, very good reasoning skills and ditto critical scientific mindset. Presents innovative, original concepts in a convincing and substantiated fashion.</td>
</tr>
<tr>
<td>□ (just) sufficient basic knowledge to undertake the PhD project. Limited insight into the relevance of the proposed research approach.</td>
<td>□ The candidate has sufficient basic knowledge within the field of research. He/she has a rather good insight into the relevance of the proposed research approach.</td>
<td>□ Solid basic knowledge within own field of research, but less knowledgeable outside this field. Good insight into relevance of proposed research approach.</td>
<td>□ Excellent grasp of own field of research, knowledgeable in areas outside. Excellent insight into the relevance of the proposed research approach and positioning of project.</td>
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### 1.b Potential competence as a strategically thinking and innovation-oriented researcher

| □ No insight in, or vision of the economically application potential of the project. | □ Limited insight and vision of potential applications. Additional efforts are needed for the candidate to place his/her doctoral research in a context of economically oriented innovations. | □ Rather good interpretation of the possible applications. Insight and vision of the strategic dimension towards an economic finality needs to be developed further. | □ Good insight into the application potential and possible economically relevant innovations. Able to place the strategic importance of the project and the research approach. Notions of IPR issues, market players in the field, etc. | □ Driven potential ‘innovator’. Very good insight and broad vision of the possible applications and their economic relevance. Able to accurately position and substantiate the strategic importance of the project, taking into account valorization, IPR, market, ... |

**Description**

- **Weak**
  - Limited basic knowledge within own field of research
  - Virtually no notion of IPR, market players in the field, etc.
  - Notions of further concepts

- **Fair/Reasonable**
  - Limited basic knowledge within own field of research
  - Good notion of IPR, market players in the field, etc.
  - Present concepts

- **Good/very good**
  - Solid basic knowledge within own field of research
  - Good notion of IPR, market players in the field, etc.
  - Present concepts

- **Excellent/outstanding**
  - Excellent grip of own field of research, knowledgeable in areas outside
  - Excellent insight into the relevance of the proposed research approach and positioning of project
PHD FELLOWSHIP: SCORING DESCRIPTORS CRITERION “PROJECT” (PRESELECTION + INTERVIEW)

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### 2a Scientific quality, relevance and challenge, originality

A PhD project is scientifically challenging and relies on a proper and focused research question. It should significantly contribute to the current international state-of-the-art. To what extent is the proposal original and will it generate knowledge that goes beyond the state-of-the-art (e.g., novel theories, concepts or approaches, new methods, ...)?

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<td>□ The project is out of scope: it does not comply with the scope of the panel it was submitted to. <em>(preselection only)</em></td>
<td>□ Research question and challenge limited or less relevant,</td>
<td>□ Scientifically relevant project, rather high quality, and sufficiently challenging as PhD-research. The research is less well focused.</td>
<td>□ Original and significant contribution to the international state of the art.</td>
<td>□ Highly ambitious and original project of potentially groundbreaking nature and large scientific impact.</td>
</tr>
<tr>
<td>□ Project lacks an intellectual (PhD-worthy) challenge: an in-depth research question is missing</td>
<td>□ the research objectives lack focus. PhD worthiness is on the low side,</td>
<td>□ The project brings less pronounced added value to international state-of-the-art.</td>
<td>□ High-quality basic research, with significant scientific challenges (doctoral level).</td>
<td>□ Very high level of scientific risks. Clear inventive and challenging ideas, novel concepts and strategies.</td>
</tr>
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### 2b Quality of the research methodology and feasibility of the project

To what extent is the proposed research methodology appropriate to achieve the goals laid down in the research project? To what extent is the outlined scientific approach feasible, bearing in mind a personal grant with a duration of four years? Finally the fit in the research team may be of importance (guidance and access to expertise).

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<td>□ Quality of research approach and planning is below par.</td>
<td>□ Methodology and planning are flawed. Intrinsic feasibility is low, or the objectives are formulated too vaguely to evaluate feasibility.</td>
<td>□ Research methodology reasonably well elaborated, but less well substantiated. Given some adjustments and risk control, project implementation appears to be feasible.</td>
<td>□ Adequate, substantiated research methodology to achieve targeted results, logical set-up and realistic planning: feasible within the four-year time frame.</td>
<td>□ thorough identification of the research risks, with alternative research strategies and “fall back” research options.</td>
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<td>□ Research activities are too limited for a four-year grant period.</td>
<td>□ Project does not fit to an individual PhD project.</td>
<td>□ Adequate, substantiated research methodology to achieve targeted results, logical set-up and realistic planning: feasible within the four-year time frame.</td>
<td>□ Good fit of project in research group activities, giving candidate access to necessary expertise.</td>
<td>□ thorough identification of the research risks, with alternative research strategies and “fall back” research options.</td>
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<td>□ Project not feasible because of too many planned activities.</td>
<td>□ Ties with/dependence of other researchers, groups or external partners may jeopardize feasibility.</td>
<td>□ Adequate, substantiated research methodology to achieve targeted results, logical set-up and realistic planning: feasible within the four-year time frame.</td>
<td>□ Good fit of project in research group activities, giving candidate access to necessary expertise.</td>
<td>□ thorough identification of the research risks, with alternative research strategies and “fall back” research options.</td>
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**PHD FELLOWSHIP STRATEGIC BASIC RESEARCH EVALUATION/ score grid with scoring descriptors - INTERVIEW**

**PHD FELLOWSHIP SB: SCORING DESCRIPTORS CRITERION “APPLICATION POTENTIAL” (PRESELECTION + INTERVIEW)**

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**Strategic basic research** in the context of a PhD grant stands for challenging and innovative research (at PhD level), which, if successful, may in the longer term lead to innovative applications with economic added value (for specific companies, for a collective of companies, or a sector, or in line with the Flanders 2025 transition areas (socioeconomic benefits). Societal impact should always be linked to a (in)direct (macro)economic benefit. E.g. cost reductions in health care, higher education level, environmental impact... should be positioned in an economic context.

### 3.a Strategic importance of the research approach for the anticipated applications (= relevance)

**Does the research --if successful- contribute to the (on the long term) realization of the anticipated applications? Is the research approach the proper one to this purpose?**

One or more of the following items apply:  
- Strategic dimension is lacking, no orientation towards an economic finality;  
- apparent mismatch between application potential and project content.

One or more of the following items apply:  
- Strategic dimension is present, but project is not well adapted to the anticipated utilization.  
- Strategic dimension based on an assumption for which there is as yet little concrete evidence.

The strategic focus of the project on economically relevant innovations is substantiated in rather broad terms. Research approach is reasonably or partially geared to the anticipated applications.

All of the following items apply:  
- The strategic focus on economically relevant innovations is clear, and well substantiated in the proposal. Suitable project approach to allow the anticipated utilization.

Requirements as in “very good”,  
AND  
- Best possible approach to achieve the intended applications. The latter are clearly the driving force behind the implementation approach.  
AND  
- Project fits well in broader strategic basic research goals of the research group.

### 3.b Strategic importance of the potential applications for possible users (= impact)

**Assuming the research approach is effectively geared towards applications: is there a significant impact for industry and economy, for possible (end-)users? Is the impact of the intended applications described in the project application credible and achievable?**

One or more of the following items apply:  
- The anticipated application is not relevant for possible users nor is the proposed impact realistic;  
- the project is too strongly embedded in the strategic R&D horizon of a single company (cfr. Baekeland programme at Flanders Innovation & Entrepreneurship - VLAIO).

- [Frank De Winne SB only] out of scope: no strategic importance for users in the space economy value chain.

- Application potential may be real but of less economic relevance and limited impact for the identified possible users.

- The anticipated applications are economically relevant, they have a potential impact on possible users. The proposal exhibits certain flaws or gaps in the identification and/or elaboration of the (potentially present) applications.

- If successful, the project is very likely to effectively contribute to economically relevant innovations within the identified companies and/or sectors, or even new economic activities. These are clearly defined and interpreted.

Requirements as in “very good”,  
AND  
- If successful, project could play a key role to disruptive innovations, implying substantial economic added value. Moreover, this goal is realistic.  
AND (score 7):  
- A successful project may lead to a substantial economic added value for Flanders