

SB 2017 Scoring grid

1. Candidate: Potential ability to independently undertake doctoral research as an innovation-oriented researcher.

D	C	B-	B	B+	A-	A	A+
0	1	2	3	4	5	6	7
Unacceptable	Below average	Average/Reasonably good		Good/very good		Excellent/Top	
1.1 Potential competence as a doctoral researcher: reasoning skills and critical thinking skills, scientific knowledge and project insight.							
<p>The candidate lacks 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.</p> <p><u>or</u></p> <p>There are clear gaps in the candidate's basic knowledge of the research area. The candidate has virtually no insight into the aim and approach of the project.</p>	<p>The candidate possesses research skills and should, with close supervision, be able to obtain a PhD. Reasoning skills and critical mindset are below average and need to be developed further.</p> <p><u>or</u></p> <p>The candidate has (just) sufficient basic knowledge to undertake the PhD project. He/she has some insight into the relevance of the proposed research approach.</p>	<p>The candidate possesses the necessary research skills and is able to carry out research in a relatively independent manner. He/she still lacks some maturity, but is motivated, has relatively good reasoning skills and a critical attitude.</p> <p><u>and/or</u></p> <p>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.</p>	<p>The candidate comes across as a motivated and potentially competent independent researcher. He/she demonstrates (very) good reasoning skills and a good critical scientific attitude, and knows how to present new concepts in a meaningful way.</p> <p><u>and/or</u></p> <p>The candidate has a very solid basic knowledge within his/her own field of research, but his/her other qualities are rather limited. He/she has a good insight into the relevance of the proposed research approach.</p>	<p>Very convincing and driven candidate with great potential as researcher, very good reasoning skills and ditto critical scientific mindset. He/she is able to present innovative, original concepts in a convincing and substantiated fashion.</p> <p><u>and</u></p> <p>The candidate has an excellent grasp of his/her own field of research, is able to properly position the project and knowledgeable in areas outside his/her field. He/she has a very good insight into the relevance of the proposed research approach.</p>			
1.2 Potential competence as a strategically thinking and innovation-oriented researcher							
<p>The candidate has no or hardly any insight or vision of the economically application potential of the project.</p>	<p>The candidate's insight and vision of potential applications are below average. Additional efforts are needed for the candidate to place his/her doctoral research in a context of economically oriented innovations.</p> <p>The application pays little or no attention to educational aspects with regard to application-oriented research and acquisition of insight into innovation.</p>	<p>The candidate is able to provide a rather good interpretation of the possible applications described in the proposal. His/her insight and vision of the strategic dimension towards an economic finality need to be developed further.</p> <p>The candidate provides sufficient evidence that project, supervision and coaching might enable him/her to develop into a strategically thinking and innovation-oriented researcher.</p>	<p>The candidate has a good insight into the application potential and the possible economically relevant innovations on the basis of the research results. He/she is able to place the strategic importance of the project and the research approach, and has notions of IPR issues, market players in the field, etc.</p> <p>The project, the supervision and coaching, provides an optimal opportunity for the candidate to develop into a strategically thinking and innovation-oriented scientist.</p>	<p>Driven potential 'innovator'. Very good insight and broad vision of the possible applications and their economic relevance. Furthermore, he/she is able to accurately position and substantiate the importance of the project within an innovation context (research questions vs. valorisation, IPR, market situation, and the like)</p>			

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2. Scientific quality and relevance of the research project, and its feasibility over a four-year period

D	C	B-	B	B+	A-	A	A+
0	1	2	3	4	5	6	7
Unacceptable	Below average	Average/Reasonably good		Good/very good		Excellent/Top	
2.1 Scientific quality, relevance and challenge, innovativeness and inventiveness							
The project for the most part or completely lacks an intellectual (PhD-worthy) challenge and/or in-depth research question.	The research question and challenge is limited, or less relevant. The PhD worthiness of the research is on the low side. The project fully adds little to the international state-of-the-art. The targeted research results are moderately original and their innovative character is only incremental.	The project proposal is scientifically relevant and of rather high quality, and sufficiently challenging as PhD-worthy research. The added value of the project with respect to the international state-of-the-art is less pronounced. The project features original and innovative aspects.	The project comprises high-quality basic research and offers significant scientific challenges that call for an approach at doctoral level. The project fully builds upon the international state-of-the-art. The targeted research results are innovative and rather original.	The project is scientifically quite challenging, and has a high-risk profile. It provides an unmistakable added value with regard to the international state-of-the-art, thereby placing the project within the international leading group. The targeted research results are quite innovative (pioneering) and based on inventive and challenging ideas, concepts and research strategies.			
2.2 Quality of the research approach and feasibility of the project							
The quality of the research approach and planning is below par; <u>or</u> the research activities are too limited for a four-year grant period; <u>or</u> the project is not feasible because it involves too many planned activities.	The project approach and project planning are flawed. The intrinsic feasibility is low <u>or</u> the objectives are formulated in insufficiently concrete terms, making it difficult to evaluate their feasibility.	The research approach and project planning are reasonably well elaborated, but not always properly substantiated. Given some adjustments and risk control, the project implementation appears to be feasible.	The research approach is adequate to achieve the targeted results, the planning of the project implementation is clearly formulated. The proposed project implementation is realistic and feasible within the four-year time frame.	Cf. '(very) good', but the project proposal also includes a precise identification of the risks with alternative strategies and fall-back options.			

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3. Strategic nature of the research project with regard to the long-term potential for innovative applications with economic added value

D	C	B-	B	B+	A-	A	A+
	<50%	>50%	>40%	>30%	>20%	>10%	>5%
0	1	2	3	4	5	6	7
Unacceptable	Below average	Average/Reasonably good		Good/very good		Excellent/Top	
3.1 Strategic importance of the research approach for the anticipated applications (= relevance)							
<p>The project proposal lacks a strategic dimension. There is no orientation towards an economic finality;</p> <p><u>or</u></p> <p>There is an apparent mismatch between the identified possible applications on the one hand, and the scientific objectives and/or the project implementation on the other hand.</p>	<p>The project includes a strategic dimension for economically relevant applications. However, the project is not fully adapted to the realisation of the application(s).</p> <p><u>and/or</u></p> <p>The strategic dimension is based on an assumption for which there is as yet little concrete evidence.</p>	<p>The strategic focus of the project on economically relevant innovations is elaborated and substantiated in rather broad terms. The research approach is sufficiently geared to the anticipated applications.</p> <p>The proposal pays attention to its positioning with respect to the broader (possibly new) goals of the research group in the area of strategic basic research with long-term innovative applications.</p>	<p>The strategic focus on economically relevant innovations is clearly and fully substantiated in the proposal. The chosen project approach is suitable to allow the anticipated application(s) to be achieved.</p> <p>The proposal is well integrated and positioned within the broader strategic basic research goals of the research group, aimed at long-term innovative applications. This embedding increases the likelihood of the actual implementation of the anticipated applications.</p>	<p>The strategic dimension, including the anticipated innovation, is realistic and thoroughly substantiated. The proposed project approach is the best possible one to achieve the anticipated applications. The anticipated applications are clearly the driving force behind the implementation approach.</p>			
3.2 Strategic importance of the potential applications for possible users (= impact)							
<p>The anticipated application is not relevant for possible users and does not fit in with the VRWI transition priorities;</p> <p><u>or</u></p> <p>the proposed impact among possible users is unrealistic;</p> <p><u>or</u></p> <p>the project is too strongly embedded in the strategic R&D horizon of a single company.</p>	<p>The anticipated applications are to a certain extent relevant for the identified possible users <u>and/or</u> the VRWI priorities. The anticipated economic impact is less realistic or there are doubts regarding its feasibility.</p>	<p>The anticipated applications are relevant, they have a potential impact on companies, sector(s), or they fit in with the VRWI transition priorities. The proposal exhibits certain flaws or gaps in the identification and/or elaboration of the (potentially present) applications.</p>	<p>If successful, the project is very likely to effectively contribute to economically relevant innovations within the identified companies and/or sectors, and/or fits in well with the VRWI transition priorities. These are clearly defined and interpreted.</p> <p><i>If the innovations offer a potential added value for Flanders, a higher score may be assigned this criterion.</i></p>	<p>If successful, the project could play a key role for disruptive innovations and result in a great economic added value for the identified companies, sectors and/or VRWI priorities, also in Flanders. Moreover, this goal is realistic.</p>			