THESIS REVIEWER'S REPORT

I. IDENTIFICATION DATA

Thesis title:	**Sampling-based Motion Planning Using Burs of Free Space**
Author's name:	**Jiri Hartvich**
Type of thesis :	master
Faculty/Institute:	Faculty of Electrical Engineering (FEE)
Department:	Department of Cybernetics
Thesis reviewer:	Dr Martin Rudorfer
Reviewer's department:	Department of Applied AI & Robotics, Aston University, Birmingham, UK

II. EVALUATION OF INDIVIDUAL CRITERIA

Assignment **extraordinarily challenging**

How demanding was the assigned project?

The assigned project is extraordinarily challenging. There is a plethora of existing research on sampling-based motion planning methods, specifically RRT. While this means that there are plenty of resources on this topic, it also means that lot of related works had to be reviewed. Furthermore, it is difficult to make a meaningful contribution within the limited time frame, simply because most "easy" things have been explored already. Nevertheless, Jiri found an angle to approach the problem and make his own contributions, for which he needed to dive deep into the theory and mathematical foundations of the algorithms.

Fulfilment of assignment **fulfilled**

How well does the thesis fulfil the assigned task? Have the primary goals been achieved? Which assigned tasks have been incompletely covered, and which parts of the thesis are overextended? Justify your answer.

I will refer to the tasks from the guidelines of the thesis assignment.

1. **Fulfilled**. Path planning for robotic manipulators has been studied thoroughly; Chapter 2 reviews sampling-based motion planning techniques, specifically RRT and its variants, as set out by the guidelines.
2. **Fulfilled**. IK-RRT has been implemented, no adaptation to mobile manipulators was necessary; BURG toolkit has been used for robot model and environment.
3. **Fulfilled**. Chapter 3.1 conceptually extends RBT to work with prismatic and rotational joints and also includes robot geometry. This fully covers the extension of RBT for mobile manipulators.
4. **Fulfilled**. RBT has been extended in Chapter 3.2 to work specifically in constricted environments, by allowing the links closer to the end-effector to move more freely even if the base of the robot is close to obstacles. The extension of RBT towards multiple taskspace goals has then been described in Section 4.1.
5. **Fulfilled but potential for improvements**. Four different scenarios have been used to evaluate the methods. Statistical evaluation has been conducted by running the (non-deterministic) planners multiple times and averaging over their planning time. Success rate over planning time has been chosen as metric, which is in line with existing works.
   However, I believe this part could have been extended more, as the discussion of the results did not go into detail regarding the difficulty of the scenarios. It has been postulated later, that Burs might not perform well in cluttered environments – this could have perhaps been demonstrated by varying the difficulty of the scenarios even more? I think a larger number of scenarios would have given a better basis for the analysis, especially considering that there were 16 more scenarios readily available.

Methodology **outstanding**

Comment on the correctness of the approach and/or the solution methods.

The proposed generalisations of RBT for mobile manipulators and multiple workspace targets are outstanding and go beyond what would have been expected from an MSc thesis.
The approach is reasoned and skillful, it relies on a large body of background research and demonstrates Jiri's expertise in the domain.

Technical level **A - excellent.**

Is the thesis technically sound? How well did the student employ expertise in the field of his/her field of study? Does the student explain clearly what he/she has done?

At the beginning of chapter 2, the problem of motion planning is formally defined, introducing all the relevant notation. From this, it is very clear what the scope and objectives of the thesis are.

There are only very few, very minor points to note – calling them mistakes would be too much already; it is rather that further explanation or clarification would have been useful to present the aspects unambiguously to the reader. E.g., on page 4 it is mentioned that a grasp directly maps to a configuration, which leaves out that it could be that there are many configurations for a given grasp, or none at all. Or, in Algorithm 1 and 2 it is not obvious where q_new comes from (but it is described correctly in the text).
Similarly, the method section could perhaps be written slightly more comprehensibly. It does involve a lot of mathematical concepts, which to the reader may seem overwhelming at first. Many figures already helped illustrating what was explained, but providing even more explanations on the rationale would have probably been helpful. Perhaps the writer was not aware of how advanced his studies are even to a knowledgeable audience.
Despite these mentions, I choose A – excellent because of the high technical level of the thesis.

Formal and language level, scope of thesis **A - excellent.**

Are formalisms and notations used properly? Is the thesis organized in a logical way? Is the thesis sufficiently extensive? Is the thesis well-presented? Is the language clear and understandable? Is the English satisfactory?

The formatting and layout of the thesis are flawless, good use is made of figures, tables and equations to illustrate the line of thought. They are of high quality as well; it is clear that strong efforts have been made to create good visualisations. The writing is concise and there are virtually no mistakes in grammar/spelling.

The thesis follows a logical structure and addresses all relevant parts with adequate emphasis. Some parts are in the result section that I would probably put into the Methods section – e.g. the algorithmic parts, extension of RBT to J+RBT, etc. – but that may be down to personal preference.

The presented document adheres to professional standards.

Selection of sources, citation correctness **A - excellent.**

Does the thesis make adequate reference to earlier work on the topic? Was the selection of sources adequate? Is the student's original work clearly distinguished from earlier work in the field? Do the bibliographic citations meet the standards?

The thesis introduces the motion planning problem, and specifically reviews the two most popular algorithms of sampling-based motion planning, namely RRT and PRM. PRM is only mentioned briefly, as the project focuses on tree-based methods. Other motion planning methods are not mentioned, they can be considered out of scope.
The review first focuses on extensions of RRT for being able to plan paths towards workspace goals. Then, it explores RBT – which is at the heart of this thesis – and reviews its extensions as well. The review of RBT and its extensions is excellent. It evidences critical analysis and insight into the algorithm and its underlying assumptions beyond what is just written in their papers.

The sources are well chosen and adequate references are made. All reviewed material directly contributes to the aim of the project. The student's own work is clearly distinguished from existing works. Bibliographic citations adhere to professional standard.
There are very few aspects where it would have been nice to provide further evidence. E.g., I would have found it interesting to expand on the exploration properties of RRT. In some places the report says that RRT is biased towards exploring the free space, in others it says that it is slow at exploration. A clarification would be interesting, as this point is made to motivate the use of RBT.

Additional commentary and evaluation (optional)
Comment on the overall quality of the thesis, its novelty and its impact on the field, its strengths and weaknesses, the utility of the solution that is presented, the theoretical/formal level, the student's skillfulness, etc.

The student should not be sad that the proposed modifications did not yield novel state-of-the-art results. The undertaken work was able to shed light on open research questions, which is an extraordinary achievement for an MSc thesis. It is the nature of research, that the answer is not always what we want it to be. What is important is the critical analysis – which has been done in the form of analysing the differences in implementations and timing the individual methods that took up most of the computation time. Perhaps another avenue would have been to explore more diverse scenarios for being able to characterise more precisely in which scenarios RBT has an edge over the other methods, and in which it does not.

III. OVERALL EVALUATION, QUESTIONS FOR THE PRESENTATION AND DEFENSE OF THE THESIS, SUGGESTED GRADE

Summarize your opinion on the thesis and explain your final grading. Pose questions that should be answered during the presentation and defense of the student's work.

The thesis at hand tackles a very difficult problem and yet managed to contribute to open research questions. This is an outstanding achievement. The final grade is based on the detailed assessments above.

The grade that I award for the thesis is **A - excellent.**

Potential questions for the defense:

- One outcome of your thesis is that IK-RRT outperforms other approaches in most scenarios. However, one drawback of IK-RRT is that the IK may be overly restrictive especially for highly redundant robots. Do you see any other ways how to mitigate this problem?
- Can you explain exactly what we can observe in Fig. 4.4?
- The final assessment of RBT seems rather destructive. Do you see any scenarios in which RBT would be superior to other methods? Do you think it valuable to further research possible extensions of RBT, to make the idea work better?

Date: **29.5.2024** Signature: Martin Rudorfer