הקרן הלאומית למדע
المؤسَّسة الإسرائيلية للعلوم
Israel Science Foundation

כ״ח בתמוז, תשפ״ב
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לכבוד
פרופ׳ שרונה טל לוי
ייעוץ והתפתחות האדם, מדעי הלמידה וההוראה
אוניברסיטת חיפה

לפרופ׳ לוי שלום רב,

הנדון: בקשתך למענק מחקר בנושא:
<u>הפיכת למידה לעמידה: מסלולים ארוכי-טווח של למידה מבוססת-מידול על מערכות מורכבות במדע</u>

הצעת המחקר אשר הגשת לקרן הלאומית למדע לא נכללה, לצערנו, בין ההצעות אשר זכו במענקי מחקר השנה.

מצ״ב עיקרי חוות הדעת.

לתשומת לבך, החלטות הנהלת הקרן מתקבלות על סמך סיקור עמיתים ודיונים בוועדות מקצועיות. ראוי להדגיש כי הוועדה המקצועית אשר בחנה את הצעת המחקר התבססה על מכלול ההערות והציונים שהתקבלו מסוקרים חיצוניים ומחברי הוועדה שהם מומחים בתחום, ונתנה משקל, בדיוניה, רק לאותן ביקורות והערכות שהיו מקובלות עליה.

מאחר שכספי ההקצבה השנתית מחולקים עד תום, החלטות הנהלת הקרן הן סופיות ואינן ניתנות לשינוי.

אנו מאחלים לך הצלחה בהמשך דרכך המדעית.

בכבוד רב,

ד״ר תמר יפה-מיטווך
מנכ״ל

העתק: רשות המחקר, אוניברסיטת חיפה

מוזמנים לבקר באתר קול המדע https://kolhamada.isf.org.il/ - מיזם של הקרן הלאומית למדע, המגיש מידע מדעי מחזית המדע, לציבור ולקהילה המדעית בישראל.

Reviewer No. 1

Originality and Innovation:

This is a very ambitious grant proposal that coordinates a number of aspects of innovative approaches to science learning. It focuses on agent-based modeling as an approach to learning about chemistry and physics. Students would learn to think about systems, modeling, and the specific science concepts. It draws upon block-based modeling which is helping to bring modeling to younger learners. The focus on longitudinal learning of the concepts makes the proposal particularly valuable.

The proposal is innovative compared to current classroom approaches and combines innovations in the research community (including those involving the PI) and practicality in applying these approaches to classroom contexts. The proposal refers to normative curriculum comparisons. Assuming that this refers to Business as Usual, it would be most helpful to have the normative comparison articulated so that he work is more broadly useful to the field beyond those who are already familiar with the comparison curriculum (and assuming that it has some heterogeneity.

This work leverages an earlier award that has been very productive and positions the investigators to continue to advance the work and to mine the work that has already been carried out in doing so.

Project Importance and Contributions to Scientific Knowledge:

The project incrementally advances work on understanding how agent-based modeling can support learning of chemistry and physics concepts. It is difficult to tell how embedded the work is in the literature of the particular concepts. It would be good to put it in relation to, for instance, Talanquer's work in chemistry and also the very rich literature on how students understand and barriers to understanding physics concepts (such as electrical circuits).

The proposal doesn't address some of the limits suggested by the literature such as Chi's focus on the ontological incoherence/difficulties of building understanding of emergence from models defined by different components at lower levels or Cuzzolino et al.'s work on how agent-based models can detract from systems conceptions. It does adequately address the disconnect between modeling and Knowledge in Pieces conceptions. Ultimately, it would be helpful for the work to lead to clear guidance in education in relation to the puzzles raised by the work in ontological coherence and emergence.

Adequacy of Methods:

With the interviews and learning logs, the study should provide rich data for analysis. Questionnaires appear to be already developed.

The proposal discussed preliminary work that has been done for the automated analysis of students' articulated ideas. This work sounds very promising and inspires confidence in how deeply grounded it is in knowledge elements and hand-coding of protocols. The proposal is a little lean on specifics related to each subject area, yet this is not surprising given the scope of the project and the page limitations of the proposal. It would be helpful to have more information in judging the project. How, for instance, will each topic be analyzed? With automated analyses, but the difficulties are often in the details and quite a lot is known about the situated knowledge and barriers to understanding for the topics under

consideration. Existing, older work considers what makes it difficult to understand some of these topics from a systems perspective (for instance, White and Frederiksen, Chi, Clement, Smith, Grotzer, etc.)

The focus on longitudinal study is an important aspect of this work. It would be helpful to know more about the mechanisms that enable insights from the longitudinal aspects. It seems that longitudinal growth analysis might be helpful. It was good to see mention of microgenetic analysis as it should be helpful in characterizing what "upward shifts" actually means.

While it would have been helpful to have concrete examples of measures, etc., this is an investigator known for thoughtful and detailed work and her recent publications show that she has been advancing work in this area (with Zohar for example). This inspires confidence that the work will be methodologically rigorous.

Suitability of the Investigator's Scientific Background to the Project:

The PI is very well-positioned to take on (and continue as is the case here) this project. She has assembled a strong team to support her with the requisite skills to carry out the work. Her previous work is thoughtful, innovative, and advances our understanding. She seems to have strong partners on the ground in the school of focus who will help to ensure the success of the project.

The number of years required for completion seems appropriate given the rich data that the project will generate and the longitudinal aspects of the study.

Summary:

This is an ambitious grant proposal for a project that brings together important targets of understanding related to modeling, systems thinking, computational thinking, and the specific disciplinary knowledge. Aspects of the proposal that are lean do not, in this reviewer's mind, signal concerns about the project as much as they convey the scope of what is to be accomplished and the page limitations of the proposal. This is a qualified project team that will do a thoughtful job with the details. The strengths of the project include longitudinal analysis, deeply situated contexts, focus on modeling and CT, and the rich data that can be mined. The automated analyses may also progress to the extent that they make an important contribution. Positioning the work in the broader literature on systems thinking, causality, and the specific concepts and an openness to addressing potential challenges/limitations to understanding related to agent-based modeling as a primary approach would strengthen the proposed project.

Reviewer No. 2

Professor Sharona Levy has proposed a novel and ambitious project to study the impacts of model-based learning in science classes across multiple years. The project builds upon previous work by the Levy group and by technical work on NetLogo and NetTango out of Northwestern.

Professor Levy's Much.Matter.in.Motion conceptual framework is a powerful starting place for this work. They already have experience using the framework in chemistry and physics. They know practical lessons learned like the importance of physical experiences to ground the model representations. The flexibility of this framework is astonishing. The proposed work starts in gas simulations, then continues using the framework in electricity and even to models of the solar system. The work described is very exciting.

Professor Levy argues that we don't have longitudinal data about learning with modeling. I think she's right. The closest study I know that has a similar structure was the work by Idit Harel and Yasmin Kafai where third graders were consumers of software about fractions, fourth graders were designers of the software, and fifth graders (who had been in the previous roles) served as consultants to the fourth graders. The Harel and Kafai work is powerful in creating a narrative that creates social cohesion between the different groups of students. There are useful lessons there, but it's not about modeling. (Kafai extended the original math focus of the work into science after Harel, but did not explore modeling.) No one has done what Professor Levey proposes.

I have two concerns about the proposal. The first is vagueness about affect. The keywords "self-efficacy" are listed for this project, and that term appears in the body of the proposal as an objective of study, but with no methods or theoretical framework associated. Self-efficacy is a critical variable in getting students to engage in modeling or programming activity, and I'm concerned that the project might fail because students lack confidence in their ability to program. I would generally like to see some attention to motivation and student goals (perhaps with a framework like Eccle's Expectancy-Value Theory). If a student decides in 6th grade that they are not interested in programming or science, how successful will a Learning by Modeling approach be?

The second concern is that teachers are barely mentioned in the proposal. We're told that teachers will collaborate in developing materials, and that teachers and researchers co-taught in earlier studies. The co-teaching model is unlikely to work across three years. Developing the materials collaboratively doesn't guarantee Fidelity of Implementation (see Jeanne Century's work). I am concerned that poor teaching in 7th grade (e.g., a teacher not excited about modeling, or who does not teach in the way that the researchers imagine) might ruin the whole multi-year study because the students would have negative associations with the whole project. I recommend the work of Line Have Musaeus at Aarhus who has developed ways to structure teachers' design activities with model-based learning.

Summarizing based on the criteria given:

1) **Originality & innovation:** Highly original and innovative. I've not seen a similar project anywhere.

2) **Project importance & implications:** The project would contribute a new perspective on model-based learning in science classes, because of its longitudinal timescale and because of its breadth across multiple scientific disciplines.

3) **Adequacy of methods:** The cognitive goals for the project are well-supported by the selected methods and likely to succeed. I have more concern about the affective goals.

4) **Suitability of investigators' scientific background to the project:** I don't know of any investigators who are better situated to take on such a challenging and noteworthy project.

Reviewer No. 3

Originality & innovation

The proposed project fills a gap, as identified by the authors, in the science education literature. In particular, the approach they have outlined with regard to long term impacts of the learning by modeling program is definitely innovative in that there are no such studies that I am aware of at this time.

The actual intervention they are studying, the MMM framework, is more difficult to judge in terms of innovation. It appears quite similar to other agent based programmable modeling tools out there (including others using NetLogo) for these content areas. The authors assert that it allows for the systems to be explored across varied content with the same set of tools, but with the information given it is difficult to ascertain how novel the platform is. This is a limitation of proposals like this because the authors have very little space to describe their design. As a result, I do not feel equipped to answer the question of innovation of the intervention based on the descriptions offered in this proposal.

There is no discussion of implementation, teacher professional development, or pedagogy in the proposal. In fact, I'm not even sure how many teachers will be involved after reading the plan. I'll say more about this later in the review, but with regard to originality and innovation, I'm quite concerned that the lack of discussion around implementation of the intervention signals a lack of attention to it, which in turn is a troubling sign for innovation. In my experience, if you want students to reason in complex and robust ways, it is definitely not enough to simply provide them with new or different tools. The entire context, the epistemological game if you will, must be significantly altered or the pull of traditional, right-answer/rote approaches will hamper student reasoning.

Project importance and contribution to scientific knowledge

My notes about implementation are relevant when considering importance and contribution as well. It is not the case that new tools are all that we need and if those tools are developed and implemented in ways that are not scalable then the contribution is limited. The work in a known school context increases the chances that the research program comes to fruition while simultaneously reducing the generalizability of the results.

Adequacy of methods

Methods is another area that is sometimes difficult to judge because of the space constraint in proposals. The design of 4 inter-related studies is a strength. However, the most interesting of these is the longitudinal one and there is little information on how the content instruments will be developed. This is critical as a core feature of this study is looking for cumulative effects. This is extremely difficult to do with content instruments across 6 different areas so more detail on that process would have been useful.

I'm also a bit concerned about the sample sizes for the various studies. The total of 770 is misleading as there are 4 distinct studies, each with more than one condition. It would be helpful if the table on pp 9-10 delineated the number of students in each condition. A matrix that shows data by study including more info on instruments would be useful.

This proposal does not contain much detail about the analytic approaches the investigator will use with the data. One concern here is that "time to upward shift in mental models" is listed as a dependent variable. I'd argue that time is not the most important aspect of the learning process

and would expect that further exploration of “learning process” might be possible with the qualitative data the team is planning to collect.

Suitability of investigators' scientific background to the project
The investigator seems well-equipped to engage in this program of research and the documentation of prior work and how this proposal builds upon that work is compelling.

Summary (strengths / weaknesses of the proposal)
Overall, there are many strong aspects of this proposal and further investigation of the MMM platform and the attempt to generalize from it seems like a fruitful avenue to pursue. The field does need more research into ways of engaging students in reasoning about complex systems. I do worry about the narrow focus on this tool without attending to the pedagogy and implementation issues that the tool is completely reliant upon. Also, note some underspecified aspects of the research plan.

Reviewer No. 4

1. Originality & innovation

This project focuses on timely and interesting topics in science education such as Learning by Modeling and computational thinking. As long-term or longitudinal studies are few in science education, this project will contribute to the field by investigating long-term modeling-based learning about complex systems in science.

2. Project importance and contribution to scientific knowledge

Four studies will be conducted in the project (however, in Figure 2, only three are presented). The studies of students' learning in single and multiple learning units could provide in-depth information about how students develop an increasingly durable and robust understanding of science. The research instruments developed in the project including the automated scoring technique will be useful for teachers and researchers.

3. Adequacy of methods

Some methodological issues need to be resolved or more information is needed. First, it is unclear how the longitudinal study will be conducted and whether the same cohort will be followed up for three years (Table 1). That is, will the experimental group 7 involve the same 200 students in three years? Secondly, Figure 3 indicates that experimental groups 1-6 and 8-12 have no prior modeling experiences. However, will some students participate in different units from 7^{th} to 8^{th} grade? Then these students have some modeling experiences in 8^{th} grade. Thirdly, what research instruments will be used to examine the constructs identified in Figure 2? What is the purpose of the interview? Which learning outcome will be examined or addressed in the interview? Finally, the automated scoring is mentioned but no further information is offered to explain how it will be conducted and which performance will be scored automatically

4. Suitability of investigators' scientific background to the project

The PI is a productive researcher and has published research on complex systems and modeling in high-impact journals. This project follows her previous project of MMM and preliminary results have been generated and

published. She has a strong background in learning sciences and technology and is suitable for running the project.

5. Summary (strengths/weaknesses of the proposal)

Strengths of the project: The PI has a strong background in the topics being investigated. The project is theoretically grounded and conceptual frameworks have been developed and applied. Some components of the modeling learning environment such as the technology platform and learning activities have been designed. Preliminary results have been generated and suggested the feasibility of the project.

Weaknesses of the project:

1. Consistency of the proposal: Several learning outcomes/constructs have been mentioned throughout the proposal but they are not part of the research objectives. For example, computational thinking and systems thinking are mentioned and discussed in the early sections but they are not shown in the framework in Figure 2. Are computational thinking and systems thinking involved in modeling practices or understanding complexity?
2. Theoretical background of mental models is missing. How is a mental model defined and assessed?
3. Some methodological issues need to be resolved.