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# EPSRC Fellowship Peer Review

EPSRC Reference: EP/S00503X/1

Document Status: With Council

## EPSRC Fellowship - Early Career

### Applicant Details

Applicant	Dr Ohad Kammar	Organisation	University of Oxford
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### Title of Research Project

Type refinement through algebraic effects
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### Review Information

Response Due Date	15/05/2018	Reviewer Reference:	038300687
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### Quality

*Primary criterion. Please comment on the degree of research excellence of the proposal, making reference to:*

- (1) The novelty, relationship to the context, and timeliness;*
- (2) The ambition, adventure, and transformative aspects identified;*
- (3) The appropriateness of the proposed methodology.*

*(For multi-disciplinary proposals please state which aspects of the proposal you feel qualified to assess)*

Although this is packaged as an early career fellowship for Kammar, it is almost a package of three early career fellowships for Kammar, Ahman and Moss, in which they will continue their current work on Algebraic Effects and Higher-Order probabilistic methods, combining the two.

A good case is made for the timeliness of the proposal. The underpinning work has been accepted at the major conferences in the discipline, and the higher-order probabilistic methods, in particular come across as ambitious and potentially transformative. The case for that is indicated, rather than fully spelt out in the proposal.

Generally, I think there is a good case here that the applicants will build on some first-rate existing work in good directions. The issue is that I am not sure they have fully thought through where they might really want to get to. The proposal reads as if it is structured round the individuals, rather than fully thinking through the potential of the material, and structuring a project to fully realise that potential.

### Importance

*Secondary major criterion. Comment on the national importance of the research. How it:*

- (1) Contributes to/helps maintain the health of other disciplines, contributes to addressing key UK societal challenges and/or contributes to future UK economic success and development of emerging industry(s);*

- (2) *Meets national needs by establishing/maintaining a unique world leading activity;*  
(3) *Complements other UK research funded in the area, including any relationship to the EPSRC portfolio.*

I strongly believe that work of this kind is important to maintaining the UK's position as a leader in understanding how to engineer a sound digital infrastructure. The work on probability is unique in this area, and opens a wide range of potentially world-leading applications.

## Impact

*Secondary criterion. Please comment on the pathway to impact identified for this work, particularly:*

- (1) *How complete and realistic are the impacts identified for this work;*  
(2) *The effectiveness of the activities identified to help realise these impacts, including the resources requested for this purpose;*  
(3) *The relevance and appropriateness of any beneficiaries or collaborators.*

The impacts seem realistic for this kind of work, but rather concentrated on the academic programming community... even though there is some reach out to the machine learning one.

## Ability to Deliver

*Secondary criterion. Please comment on the applicant's ability to deliver the proposed project, making reference to:*

- (1) *Appropriateness of the track record of the applicant(s);*  
(2) *Balance of skills of the project team, including academic partners.*

The applicant has a good track record academically. He has publications in key target conferences in his area (LICS, POPL). I have a mild concern that these have been co-authored with more established researchers - not uncommon at this stage of his career. His Balliol fellowship is of a kind that is typically awarded in a cross-disciplinary competition on the basis of a real research submission (possibly a more stringent refereeing and selection process than for this grant). He has held positions in Edinburgh, Cambridge and Oxford, and worked with some of the best staff in his area at each. He has shown leadership in trying to build a community with his work in S-REPLS. He has some community esteem evidence in the form of being invited to give plenary talks and participate in programme committees. He has not held permanent positions, or had much opportunity to run significant projects of his own, and so has no supervisory or formal project management experience.

The academic partners are very strong. Plotkin, Birkedal and Yang are international research leaders. Plotkin and Yang were co-authors on some of the publications leading up to this proposal. But they are not listed as making any specific contributions to any of the work packages. Birkedal's involvement is limited to WP3.2. None of the three has submitted a letter of support, so it is not possible to confirm their understanding of their roles on this project.

The proposed RA's are good recent PhD graduates. Ahman is also a student of Plotkin's, with a good publication record, who has been working on algebraic effects. Moss is a student of Hyland, who has a competitive Junior Research Fellowship in Oxford (comparable to Kammar's), and who has worked with Kammar on quasi-Borel spaces and higher-order Bayesian inference. Both of these are well-suited to the project.

## Research Vision

*Secondary criterion. Comment on the overall research vision and how the fellowship would enable the applicant to achieve their career aspirations.*

Somewhere at the heart of this proposal is a vision that modern semantic techniques for dealing with effects and hence in particular with systems that have a notion of state, can be combined with a promising new approach for higher-order probabilistic reasoning. I think this is highly significant, but only some of the most basic implications are spelt out in this

proposal. That makes the question of the research vision hard to answer. It seems there but not as clearly articulated as it might be. Similarly how the fellowship might enable the fellow to fulfill their career aspirations is not particularly well articulated.

## Leadership Potential

*Secondary criterion. Given the applicant's declared current career stage, please comment on their potential (and the expected timescale) for them becoming an international research leader.*

I don't know Kammar, but given the current state of his career, I would either expect him to make it as an established younger international research leader comfortably within the timeframe of this fellowship if he was then going on to be a significant leader over the course of his career. I don't feel I can really give you a steer on his potential, other than to give you the double negative: I don't think it is obvious that he doesn't have it.

## Resources and Management

*Secondary criterion. Please comment on the effectiveness of the proposed planning and management and on whether the requested resources are appropriate and have been fully justified. Please comment explicitly on any equipment requested, or the viability of the arrangements described to access equipment needed for this project, and particularly on any university or third party contribution.*

The proposal is almost a triple fellowship (three fellows for six years, with Moss coming in formally when his current JRF ends), with the applicant in a slightly more senior role than the two RA's. This simplicity relaxes the management demands. The resources requested seem reasonable. Travel is quite eye-watering but Kammar is a prolific publisher.

## Proposal Assessment

*Please comment on the extent to which this proposal meets each of the criteria laid out in the call document not already covered by your previous answers.*

I hope everything is covered elsewhere.

## Overall Assessment

*Please summarise your view of this proposal*

This is a proposal to build on some first rate underpinning work in what seems like reasonable ways. An excellent team has been assembled to do that. I am particularly impressed with the work on higher-order probability, and its possible impact.

There has been a start to think about the kind of transformative outcomes that might arise. One serious issue for me (and I hope I am not being too harsh about an early career fellowship) is that that process has not been taken far enough, and has not really impacted the structure of the project.

I am also a little concerned (frankly) about the amount of money being requested in what amounts to a first grant. That is somewhat mitigated by the simplicity of the structure and the aims. I'm also a little concerned that there are no letters of support from the academic partners.

Formally, that would place this proposal as 4), but I have chosen to push it higher to 5). There are two reasons for this. One is that I don't know Kammar or his work as well as I would like, and so have less confidence than I would wish in key areas of assessment. The other is that where I do have confidence I can tell that the track records are very good and the work is exciting.

My judgement is that:

- 1) This proposal is scientifically or technically flawed
- 2) This proposal does not meet one or more of the assessment criteria
- 3) This proposal meets all assessment criteria but with clear weaknesses
- 4) This is a good proposal that meets all assessment criteria but with minor weaknesses
- 5) This is a strong proposal that broadly meets all assessment criteria
- 6) This is a very strong proposal that fully meets all assessment criteria

1	2	3	4	✓ 5	6
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My confidence level in assessing this is:

Low	✓ Medium	High
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### Reviewer Expertise

Please indicate your areas of expertise that are relevant to your assessment. Take care not to reveal your identity to the applicant.

I have broad expertise in Theoretical Computer Science, and experience in academic management.