# Referee Report Form

**RTA–98**
**March 30 - April 1**
**Tsukuba, Japan**

**Paper Number:** 49

**Title:** An Abstract Concept of Optimal Implementation

**Author(s):** Zurab Khasidashvili, John Glauert

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Rating of the following qualities from 0 to 10, with 10 being the best.

| | |
|---|---|
| **Technical accuracy:** | 8 |
| **Significance:** | 6 |
| **Originality:** | 6 |
| **Quality of presentation:** | 5 |
| **Appropriateness:** | 7 |

**Overall evaluation of the paper:** **6**

10–8 : strong accept
7–6 : weak accept
5–4 : weak reject
3–1 : strong reject
0 : outside the scope of RTA

**Referee's confidence in the evaluation:** 3

**Overall evaluation of the paper by the PC member (if different from reviewer):**

**Justification** (why is the paper rated as above):

The paper present a new and rather technical result in an abstract theory of Lévy's families (the formal definition of sharing originated from Lévy's optimal reductions) that the authors have been developed and widely studied in the last years.

In particular, they prove the equivalence between the generalization of Lévy's zig-zag relation and a notion of separable families (a family is separable when it is impossible to simultaneously create pairs of redexes belonging to the same family) for the affine case of their stable deterministic residual systems (affine means that residuals are never duplicated along reduction).

The main result is the proof that any family relation can be decomposed into a weaker notion of sharing (such as zig-zag) and a non-separable family relation (stronger than zig-zag).

My main criticism to the presentation of the paper is that the authors do not give any help for understanding the intuitive meaning and relevance of this decomposition (and in particular if there is any practical one in the already known optimal implementation techniques).

On the other hand, it is difficult to understand the actual relevance of the systems in which they prove that zig-zag is the only non-separable family. In fact, this result is proved for affine or non-duplicating (in terms of residuals) systems. The authors claim that there are interesting affine systems in which there is duplication; e.g., orthogonal TRS with an innermost reduction strategy. Nevertheless, either I missed something or it is immediate that in this case zig-zag does not correspond to the intended notion of sharing. For example, let us take $\lambda$-calculus with innermost reductions; in $(\lambda x.Wy(xz))I$ (where $W = \lambda xy.xyy$ and $I = \lambda x.x$), the reduction of $Wy(xz)$ duplicates the two virtual redexes $(xz)$; then, the next step transforms both this virtual redexes into a pair of redexes $(Iz)$ that, according to the intended interpretation of sharing, should be in the same family. Therefore, this system seems trivially non-separable.

**Recommendation to the author(s) for possible improvements:**

The subject of the paper is highly technical, therefore, it is difficult to present it in a simple and accessible way. Nevertheless, I think that there is too few effort in giving a more intuitive and readable presentation of the results. I would rather suggest giving a less rigorous and complete sequence of definitions and lemmas integrated with detailed and intuitive examples.

Finally, two general recommendations (maybe, and I hope, they do not apply for the authors are already following them):

- The authors have a lot of theorems on their systems. I think that at this point they should give concrete applications of this abstract theory of optimality.

- It is time that the author start recollecting all their work into one (or more) full paper for a journal, including unabridged examples and explanations of their abstract notions; with particular regards to the possible applications of their theory (e.g., different and clearer proofs of already known results). New results as the one proposed here are getting more and more technical, thus it is more and more difficult to present (and, changing point of view, to understand) them in the limited space of a conference paper.

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Rating of the following qualities from 0 to 10, with 10 being the best.

| | |
|---|---|
| **Technical accuracy:** | 7 |
| **Significance:** | 6 |
| **Originality:** | 6 |
| **Quality of presentation:** | 3 |
| **Appropriateness:** | 7 |

**Overall evaluation of the paper:** **5**

10–8 : strong accept
7–6 : weak accept
5–4 : weak reject
3–1 : strong reject
0 : outside the scope of RTA

**Referee's confidence in the evaluation:** 2

**Overall evaluation of the paper by the PC member (if different from reviewer):**

**Justification** (why is the paper rated as above):

This paper presents a follow-up to the authors' previous work on "deterministic family structures" (DFS). The notion of a family, (introduced by Lévy in the study of optimal reduction in the lambda calculus and extended to other systems), identifies the set of redexes that must be shared in an optimal implementation. DFS provide an abstract notion of a family that generalises the notion of family defined for other systems, which seems new and interesting.

The key results of the paper is the equivalence between the family relation and the zig-zag relation (an alternative characterisation), and the property that the family relation can be decomposed.

I have strong confidence in the technical quality of the paper, but equally I have a lot of reservations about the presentation and the applicability of these results.

A much revised version of this paper would be very appropriate for RTA'98.

**Recommendation to the author(s) for possible improvements:**

You really must improve the presentation of this paper. There are a lot of potentially very interesting results here, but the reader is not given much help in extracting the essential ideas. As a very general comment I suggest that you cut down the technical content and motivate the ideas more. For instance you could replace a lot of your definitions by examples (or at least support your reasoning by examples).

The quality of the text is quite high (with respect to accuracy), but this is offset by lack of motivations.

I have a few specific comments.

1. Most of the paper is concerned with recalling definitions and concepts from your earlier work. I wonder if it is really necessary to be so formal, in this case, to set all this up for your contribution?

2. In the introduction you mention labeling. Do you have a labeled version of SDRS? This would

(at least to me) give some additional understanding to your paper.

3. I fail to understand your comment (in the abstract, and at the end of page 2) about sharing being "compositional". Could you clarify this?

4. I didn't understand your discussion about "shift" very well. (Section 3, before Def. 3.1.) Maybe you could re-write this part?

5. I found the paper slow to read because of the high number of abbreviations which I had to keep going back to check what they meant. This is a style adopted my many authors, but I find it quite obstructive.

6. At the end of page 5 you state that there are only a finite number of reductions in $STV(P)$. Could you justify this with a proof, or at least an informal argument.

7. I could not follow your discussion in the last paragraph of Section 2. Either it is wrong or it is badly explained. Please be more precise here.

8. The technical results of this paper seem convincing after a lot of deciphering. I found the proofs very hard to follow. One gets a strong feeling that could be done in a more "crisp" way.

9. What about real implementations of SDRS using Lamping's combinators (or similar)? Do you have anything in mind?

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|---|---|
| **Technical accuracy:** | 7 |
| **Significance:** | 5 |
| **Originality:** | 7 |
| **Quality of presentation:** | 4 |
| **Appropriateness:** | 9 |

**Overall evaluation of the paper:** 6

10–8 : strong accept
7–6 : weak accept
5–4 : weak reject
3–1 : strong reject
0 : outside the scope of RTA

**Referee's confidence in the evaluation:** 3

**Overall evaluation of the paper by the PC member (if different from reviewer):**

**Justification** (why is the paper rated as above):
In previous papers the authors have introduced deterministic family structures (DFSs) which can be considered as an axiomatisation of Lévy *labelling*. Lévy labels serve to indicate which redexes must be shared in an *optimal* implementation of a rewrite system. Redexes which must be shared are said to be in the same *familiy* Lévy also gave two other characterization of families, via *extraction* and *zig-zag*, and showed all three to be equivalent for the lambda-calculus. This has been extended to other types of rewrite systems.

DFSs accommodate rewrite systems where labelling families are in general strictly larger than the other two. The paper shows that for affine (non-duplicating) DFSs which are *separable* the equivalence is restored. To that end first the equivalence between extraction and zig-zag families for non-duplicating deterministic residual structures is shown.

Then it is shown that every rewrite system can be turned into an affine one by considering whole families as single redexes, and that the resulting structure can be turned into a separable DFS. This yields a factorization theorem for DFSs: every DFS can be factorised as an affine separable DFS (i.e. for which all three family notions are equivalent) and a stronger non-separable sharing.

Although the rewriting techniques in the paper are interesting enough for RTA, I wonder about their applicability and have much criticism on the presentation.

1. The paper is the culmination of a long series of papers by the authors. In fact it concludes (as one learns from the conclusion) the first chapter of the line of research pursued (only) by the authors. To make the paper reasonably self-contained several definitions and results from previous papers have been included. This takes up more than half of the paper. This is too much. It's good mathematical practice to base yourself on earlier work, but the interface to it should be small and precise, only using some main results and definitions (and possibly giving some intuitive explanations). Having a journal paper for reference would make things much better. The results deserve it.

2. The paper is badly presented. Structure, examples, and proofideas are missing. There are

some discussions of related work but the authors fail to link many of their results and proofs, to those. For example, the proof of equivalence between extraction and zig-zag (the topic of section 3) is already almost abstract in the 'syntactic' setting. Moreover, the way to show equivalence between zig-zag and separable DFSs closely resembles existing proofs of equivalence in the 'syntactic' setting.

3. I fail to see what one gains from the factorization theorem, I wouldn't expect that non-separable affine families (one of the factors) are any simpler to study than what we started with (I also don't think that non-separable families are interesting.) The real interest here would be, I think, to consider the duplicating case, but it is very much an open question whether the techniques in this paper are useful also in that case.

**Recommendation to the author(s) for possible improvements:**

p. 1
- Compositional? More *decompositional* or even better *factorizable*,
- Does the recent negative result of Asperti and Mairson influence the viability of the subject?

p. 2 Please use Asperti & Laneve's notation for their example.

p. 3
- Hindley's notion of ARS is non-standard, use Klop's. Why does the set of redexes have to finite? In this way you exclude infinitary rewriting (i.e. the unfolding morphism from cyclic term graph rewriting to term rewriting cannot be stated as a morphism of ARSs)?
- Don't use 'term' or 'redex' since these have by definition structure which ARSs do not have. (Redexes are subexpressions of the expression to be rewritten. They *induce* but *are not* reduction steps.) Use relation composition (juxtaposition) for concatenation of reduction sequences.
- Give a reference to Church's standard notion of residual.
- Make a distinction between discarding and erasing already in Definition 2.1.
- Formally speaking, your definition of residual relation for reduction sequences is not complete. To make it complete, you either have to *specify* a concrete order for developing sets of residuals or work everywhere up to the order (also Huet and Lévy forget this).

p. 4
- ..., and that *Lévy*...? This is not a sentence.
- Observe that $P$ might be external to $Q$ even if they contract a residual of the same redex in the initial term, according to your definition. I.e. your formal concept 'external' does not correspond to the intuitive meaning of external, or more seriously $P$ might be external to some $U$ considered as a development, while it is not external to $U$ considered as a set! (This might happen when the combined effect of $P_i$ and $Q_j$ is needed to erase the residuals $u_i$ and $v_j$ of the same redex $w$ in the initial object $t_0$.)

p. 5
- Omit everything related to essentiality. It is used only once in the rest of the paper as far as I can see. There (thm. 3.7) it's not essential.
- There are clearly ... This is not really obvious (since not all reductions in the same equivalence class have the same length due to erasure. It would only be obvious for linear systems), but cf. your page 8.

p. 6 Can [initial] and [separable] be seen as corresponding to the base case and induction case for creation of families by a reduction sequence?

p. 8 Usually extraction is kept distinct from standardisation. You combine them. Why?

p. 9 A duplicating version of Lemma 3.5 (that is, for the lambda calculus) has been shown in Lemma 4 of 'Take Five' [$\lambda xxx.x\&xxx$,IR-406,Oostrom96]. Could you prove 3.6 also like p. 91 [PhDthesis,Klop80] (showing uniqueness of normal forms for extraction using a diagram)?

p. 12 I think some cases are missing in the proof of the main Theorem 5.4. Why does one reduction have to be a prefix of the other as you suppose? Theorem 5.5 is intuitively trivial. In non-duplicating separable systems redexes can never end up in the same zig-zag family.

p. 13 Theorem 6.5 is a factorization theorem, hence you should consider a notion of 'product' (composition) on DFSs. The present formulation of the theorem is rather cumbersome. You state that what we have gained from this is that studying sharing in an SDRS can be reduced to studying zig-zag and studying non-separable affine familes. Is this a real progress?

1. Please use LLNCS style.

2. Please replace idiosyncratic notions by more standard ones.

3. Please add concrete examples. You should consider replacing some proofs by proofs by example.

4. You should be careful with some comments you make.

   p. 1 There was no other way ...? Why not. One can well imagine other ways. Lévy just proposed *a* (seminal) solution.

   p. 1 Was thought to require ...? By whom? Not by me.

   p. 2 Labellings are not so important themselves, they tend to be rather arbitrary. It's the properties they bring about that matter. Therefore omit the part about the history of labelling.

   p. 3 'more syntactically oriented'? I don't think so. Omit this qualifier or substantiate it.

   p. 3 The statement 'minimal axiomatization of ...' seems a formal statement, but is it?

   p. 4 From your comment ('duplicating syntactic ...') one is supposed to understand that observing redexes is abstract and observing other things is syntactic?

   p. 6 What then is the essence of sharing?

   p. 7 This goes beyond Lévy's concept of sharing... Which one? It certainly doesn't go beyond his concept of sharing via labelling. It mainly shows that it goes beyond the concept of sharing via residuals (i.e. beyond your abstract approach).

5. Add structure and explanations to your paper. For example, section 4 is lacking any structure whatsoever, it's just a series of lemmas. Moreover proofs usually are technically ok, but lack any explanation. It would help the reader a lot if you would give a proofidea as well. Just state what you're going to do and link this to proofs in literature.

6. Many of your abbreviations don't have great mnemonical value (c.f. your introduction of $ST, STV, STA, \langle P\rangle_S$). Think of better ones or don't use them at all (the best notation is no notation).

7. Please relate Section 3 to the standard proof of equivalence between zig-zag and extraction as much as possible.

8. State which results essentially depend on the non-duplicating restriction, and which don't.

9. Most of the proofs are conceptually easy, yet they look complicated. Most of them work by commuting steps in diagrams, so diagrams are the proper tool to present them. The only proof which comes close to being well presented is the proof of Lemma 3.5 in the appendix.

10. Thinking of sharing as an implementation technique it is very restrictive to consider only orthogonal systems. Would it be possbile to extend your work to non-orthogonal rewriting as well?

11. 'Factorizing' the family relation in HORS by following thier structure, yields a decomposition into a linear and a non-lnear part. Could you have such a factorization theorem for DFSs?