Referee Report
on the paper
"The Complexity of Deterministic Guaranteed Automatic Algorithms: Cones, Not Balls"
by Y. Ding, N. Clancy, C. Hamilton, F. Hickernell and Y. Zhang

This is an interesting paper about the worst case setting of information-based complexity (IBC). It is assumed that

$$S : F \to H \tag{1}$$

is a given solution operator with the property that $S(cf) = cS(f)$ for $c \geq 0$. The goal is to compute $S(f)$ with an error of at most ε. Algorithms are allowed to use only a finite information, given by finitely many functionals. It is well known and stressed by the authors that, in most cases, this is not possible without a restriction of the set F to some "set of inputs" which I call $\widetilde{F}$ for simplicity.

It is very popular in IBC to assume that $\widetilde{F}$ is a ball, i.e., $\widetilde{F} = \{f \in F \mid \|f\|_F \leq c\}$ for some $c > 0$. The authors argue that it is often interesting to consider another kind of "a priori information" on $f \in F$ and they suggest to study cones of the form

$$\widetilde{F} = \{f \in F \mid \|f\|_F \leq \tau \|f\|_G\}, \tag{2}$$

where G is another space with $F \subset G$. It is then reasonable and necessary to study algorithms for the cone $\widetilde{F}$ with "varying cost", i.e., the cost may depend on $\|f\|_F$.

This whole idea seems to be a very good one. The authors present several interesting results along this line and also two specific examples:

- Numerical integration for univariate functions with $F = W_1^2([0,1])$ and $G = W_1^1([0,1])$. See Section 5.
- Approximation in the L_∞ sense for the spaces $F = W_\infty^2([0,1])$ and $G = W_\infty^1([0,1])$. See Section 6.

The algorithms and the results are quite convincing. The whole topic of the article is very much in the spirit of IBC and fits to the Journal of Complexity.

In spite of all these positive and interesting good news, I also want to criticize the paper. The paper is very long, 39 pages, and the presentation is a bit slow, with many repetitions and too many details, some of them are not really relevant. I would guess that a more focussed paper would get more attention and would be more attractive. The first math result (Lemma 1) is on page 12, after the last result (Theorem 8) there are another 6 pages with remarks and discussion. I guess that it is ok for such a paper to include many comments and remarks, but my feeling is that the authors exaggerate a bit. I guess they should read the paper again and hopefully agree with me and shorten it.

Specific comments and remarks

Title: it should be "cost of an algorithm" and "complexity of a problem". In the text the authors mainly use this terminology. Why guaranteed? Why automatic? The real difference compared to "classical IBC" is in the set $\widetilde{F}$: Cones, not balls. I would prefer a title such as "The cost of deterministic algorithms: cones, not balls".

I am not sure whether I like the list on page 2:

Of course you want an error of at most ε and of course an algorithm is "automatic". Is this special? You say that you *want* an adaptive algorithm which I find strange. To me it seems that a kind of adaption *is needed* because you replace balls by cones.

Optimality: You mention two (different?) requirements. The first requirement does not make sense, however, since "complexity of the problem" does not make sense. Complexity is defined only for a *class* of problems, such as $\widetilde{F}$. If $\widetilde{F} = \{f\}$ then the algorithm simply has to output the (constant) result $S(f)$. The sentence starting with "Furthermore" makes sense, but this is not another requirement. [There are 2 typos: "is not given" and "does know"].

Tunable: I am not sure whether it is good to discuss issues of the implementation in such a paper that deals with the foundations. Is the "maximum allowable computational cost budget" really important for *this* paper?

page 3, -10: "However, this algorithm is not adaptive." Why should it be adaptive? Adaption is not an advantage or an disadvantage per se, it is *needed* if $\widetilde{F}$ is a cone.

There is a typo on p3, -7: but not on local information

p4, -11: I guess there is a typo and $G(f)$ should be replaced by $G_n(f)$

Figure 2 shows a diagram for a typical algorithm in the case where $\widetilde{F}$ is a ball; Figure 3 similarly shows a typical algorithm for the case that $\widetilde{F}$ is a cone. I find the words "non-adaptive", "guaranteed", "adaptive" and "automatic" rather irrelevant.

On page 5 (last line) you mention the survey paper of Novak [12]. (There is a typo on p5, -1: "in for some problems" (?)). You claim that this paper only contains positive results for adaption in the average case and randomized settings. This is not correct. Novak [12] also reports about positive results for adaption in the worst case setting, on page 202 there is a list of such results and there are more than 10 references. Therefore the wording "By contrast" on page 6, line 3, seems to be misleading. Again: What is new in this paper is the study of cones $\widetilde{F}$, see above.

p6, 16. Two typos: describe and provide

In (5) you only allow algorithms with the property $\phi(cy) = c\phi(y)$ for $c \geq 0$. This restriction is not common in IBC. Is it needed? I guess this assumption simplifies the analysis of adaptive algorithms.

Formula (7) should end with a point.

Page 9 and elsewhere: Is this discussion of "practical automatic algorithms" with $N_{\max}$ and the "Boolean warning flag" really needed? I guess that this is a distraction from the more interesting issues.

Formula (14b): I guess that G_0 is not defined.

p13, -15: typo: the sample size needed to

p15. First display formula should end with a point.

Formula (26) should not end with a point.

p19, l -21, typo: delete "by the"

Just after (39) a typo: delete "consider algorithms"

p25, middle of the page: "According to (15)": something seems to be missing here.

Page 27: The whole page is about the integration problem, but you write three times about the approximation of functions. Please correct!

Table 1: How is the observed success rate defined? If you put $\varepsilon = 10^{-8}$ then any error bigger than this is considered as "wrong result" or do you allow some tolerance?

p30, -11: It should be (43).

On page 33 (-3) you claim that algorithms based on balls are not adaptive. This is a strange statement since in IBC we allow *all* algorithms, adaptive or not. It is a math *result* that says that, sometimes, *adaption is not needed*. Again I find this stress on "automatic" and "guaranteed" a bit strange, not at all specific for your approach. Again: Cones, not balls, this is the main difference compared to more standard IBC.

page 36, Section 7.4. It seems that this discussion is too long. Every cone (in the sense of the authors, i.e., $x \in K$ and $c \in \mathbb{R}$ implies $cx \in K$) different from the whole space that contains a ball is not convex.

Proof: Take a ball $B(x, d) \subset K$ and $y \notin K$ and consider the midpoint of $x + dy$ and $-x + dy$. This midpoint is $2dy$ and is not in K.

page 37: "We believe that more complex problems also deserve to have automatic algorithms with guarantees of their success." Again: The error ε is guaranteed for $f \in \widetilde{F}$ and one may study different input sets, such as balls or cones, or other sets (for example sets that are convex, but not symmetric). We know that without such an assumption, $f \in \widetilde{F}$, most problems cannot be solved.

To say it again: This is a very interesting paper. In my opinion, it should be shortened, and should be more focussed to the main questions. Then it would make for an excellent contribution to the Journal of Complexity.