Response to Reviewer 2 Comments

Open Review	(x)	I would not like to sign my review report
	( )	I would like to sign my review report
English language and style	( )	Extensive editing of English language and style required
	( )	Moderate English changes required
	(x)	English language and style are fine/minor spell check required
	( )	I don't feel qualified to judge about the English language and style

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	( )	( )	(x)	( )
Are all the cited references relevant to the research?	(x)	( )	( )	( )
Is the research design appropriate?	( )	( )	( )	(x)
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )

The central issue of my earlier referee report was that the method developed in Section 2 to analyse the complexity of individual sequences is not original. The LZ compression method was used exactly to this end in the late seventies.

That the two methods are not rigorously identical does not meet the criticism: presenting a mild variant of LZ compression and using it to the same purpose of complexity analysis for individual sequences does not give a new result. The result might be of interest if the new approach were to yield qualitatively new results, compared to the old method. This does not seem likely, however, and the authors do not engage in any such comparisons. The authors add that their approach allows to compress structures somewhat more general than strings, such as two-dimensional dot patterns. It would be necessary to look at the literature to find out whether such work has already been done.

In any case, however, the extension to dot patterns is not systematically explained, nor does it, in my view, represent a sufficient extension of the LZ algorithm to warrant publication on its own. In any case, however, the article spends a considerable amount of time on an algorithm which should be treated altogether by an explicit reference to LZ, saying that the entire approach championed in Section 2 was described there. This would mean a significant reorganisation of the paper, which the authors do not undertake.

Finally, concerning the ``two axes of complexity'', I still find the authors' views more than a bit puzzling. They say for instance on p.~11:
\begin{quote}
It deserves to mention that we may intuitively say that [vi] is more ``complex'' than [i], because the former looks more random/irregular/difficult to reproduce; while we may also say that [i] is more ``complex'' than [vi], because [i] needs more detailed, complex and delicate mechanisms to generate. However, it is not difficult to realize that these two ``complex'' refer to two distinct directions\ldots
In fact, the two directions correspond to the ladderpath-index λ and the order-index ω , respectively. Therefore, now we are able to distinguish the two axes of ``complexity''.
\end{quote}

But [i] and [vi] have the same size, so that λ and ω add up to the same quantity. In other words, any increase in λ yields an equal decrease in ω, and viceversa. In other words, complexity along the ``first axis'' is exactly the contrary of complexity along the ``second axis''. Simply said, the two axes point in exactly opposite directions. ``Higher complexity'' in the first sense of the word is exactly equivalent to ``lower complexity'' in the second sense of the word. This is hardly a terminology calculated to bring clarity to a difficult subject. And finally, I still find it bizarre to compare the complexity of two signals with different sizes, as well as to think of size as being an ``axis of complexity''.

In my view, the authors have altogether failed to address the core of my remarks which simply states that the bulk of the paper reports results which are not original. The paper should be rejected.