



**SDI Review Form 1.6**

Journal Name:	<a href="#">Advances in Research</a>
Manuscript Number:	2014_AIR_11388
Title of the Manuscript:	Crack-growth on canvas paintings during transport simulation monitored with digital holographic speckle interferometry
Type of the Article	Original Research Article

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This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

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**PART 1: Review Comments**

This paper reports a series of experiments to investigate the vibration impact in paintings, more precisely in the generation and growth of surface cracks. The article is an interesting read and is adequate for publication. However I feel that some kind of revision is needed before publishing.

	<b>Reviewer's comment</b>	<b>Author's comment</b> <i>(if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)</i>
<b>Compulsory</b> REVISION comments	<p>I had a lot of problems understanding the objective of the paper, which is a bit frustrating. The introduction states that the paper aims to record the vibration impact during the process of generation of cracks in real time, presumably to determine under which conditions it becomes a danger to the painting or to better understand how it degrades.</p> <p>However, after reading the paper it seems that only an exponential dependency of the number of cracks with time and/or vibration acceleration is obtained. I am not sure if this result is of any importance, because I found no relation with similar studies or previous assumptions in this field. In addition results vary quite a lot from one test sample to another and too few points are used to fit the equation <math>y = \exp(a+bx+cx^2)</math> (why this and no other exponential? Is that of any significance?) to extract any useful conclusion. I am sure all this is of importance, but I failed to understand why.</p> <p>The discussion section is, in my opinion, poor. It fails to convey the major findings of the investigation and why they are important, and includes a very complex section which I think should not be there, just to introduce the last paragraph. Probably it is just because I found all this part quite difficult to follow.</p> <p>The system is supposed to work in real time, as the author's state in the introduction, but it is using a 5-frame algorithm as a basis, which requires the sample to remain static and under a controlled environment for the time it takes to obtain the 5 images. Wouldn't this be a problem for real time operation? I see that the experiments are</p>	



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	<p>performed so that the map is obtained *after* each vibration cycle, in a static state; thus real-time operation here is no more needed. What am I missing here?</p> <p>I failed to understand the need of the first image after thermal treatment too.</p> <p>The sensor itself is not described in enough detail. I am sure that the authors have published this data in previous papers, but a quick summary would be important (including some schematics and working principle, how interference fringes are obtained, how the phase is calculated, etc.). The fact that they state that DHSPI is well-known is not enough. Only Tornari et al use this name for the technique, so it is difficult to understand the difference with Digital Holography or conventional ESPI methods.</p>	
<b>Minor</b> REVISION comments	<p>A deep revision of the grammar, style and general writing is needed. Some sections and paragraphs are obscure and difficult to understand. I found most problems in the description of the samples and the methodology, for instance lines 183-190.</p> <p>There are other style issues such as the citation [16] at line 42, which I am not sure why it is there.</p> <p>I had problems with some figures, missing the arrows or overlapping with some text (fig 9) or poor resolution (fig 10, for instance). Maybe it is my copy of the PDF, though.</p>	
<b>Optional/General</b> comments	<p>The authors have made an excellent work with the design of the simulator and the making of the samples. They seem to use a quite novel technology, though they don't explain well how it works or its benefits when compared to other similar techniques. Their study seems to be of interest and a lot of work has been put in this investigation. In my opinion they should clarify the objectives and the major findings better to help the reader understand the relevance of their research.</p> <p>I really think clarifying the above points would improve this article a lot.</p>	

**Note: Anonymous Reviewer**