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Development of smart boulders to monitor mass movements via the Internet of Things: A pilot study in Nepal

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Summary

The manuscript by Dini et al, presents a new system for monitoring boulder motion in landslide dominated environments. The work is presented as a case study example, with the system deployed in the very active area of Bhote Koshi catchment northeast of Kathmandu. The system consists of: a) a stack of multi-sensors, which comprise inertial-accelerometers and GPS sensors, and; b) a local LoRa network which is responsible for the wireless collection of the data and their transmission to the GSM network. This configuration demonstrates the potential of continuous monitoring of boulder dynamics and their tracking during a landslide event remotely, from a laptop or a mobile device connected to the internet.

The paper:

- introduces the problem of monitoring landslides it its relationship with monitoring individual boulders
- describes the proposed monitoring system at a high level
- introduces the monitoring area focusing on its high vulnerability to landslides
- discusses the processing and filtering of the accelerometer data
- compares the derived acceleration measurements with complementary measurements of TLS and rainfall data
- evaluates the performance of the sensing system using qualitative comparisons between the observed motions (from the accelerometer) and the complementary measurements.

It is important to note that the monitored boulders range in size and were placed in two neighbouring but different areas: one representative of slowly moving landslides and one faster, debris flow controlled channel. The boulders were also placed at different positions within the landslide (exposed, partially embedded and fully embedded).

Overall Evaluation

This is a truly amazing effort. I know first hand that the IMU technology (accelerometers) is not mature enough for long-term unattended monitoring. The fact that the authors managed to collect data in a "close to real time" manner and demonstrate the use of this technology and its potential to co-exist with both GPS tracking (even if it didn't always work) and the Internet of Things, is remarkable. For this reason, I think that this manuscript can be very useful to the EarthSurfD audience and I want to see it published. At the same time, I have two points of criticism that are, in my opinion, major.

The first is that the paper is too long at places and looses its focus. I believe that the main contribution is the introduction of the sensing system. A little bit less context will benefit the manuscript and allow the reader to focus on the technical aspects of the deployment which are the most difficult (and the most controversial given the maturity of the deployed technology).

The second is relevant to the presentation of the accelerometer data. The authors use a rotation convention which can work in certain electronic engineering applications (when we want to rotate the screen of a smartphone for example), but it is not very relevant to the 3D rotation of the boulder. This is not a matter of semantics, it is important to describe the accelerometer data in more relevant context, even if the focus is not on measuring the actual dynamics but extracting more "qualitative" results or "binary" states (mobility non-mobility, rotation - no-rotation, fast- slow rotation etc.). The authors do not claim that they measure the dynamics accurately (which is very correct), but the way the data are processed and presented makes them very difficult to understand and (more importantly) reproduce, even in isolated laboratory conditions.

I have organised the rest of the document in three sections. The first two are devoted to the problem of analysing the accelerometer data. I attempt to explain where is my main objection and I propose a framework which the authors can use to both shorten the analysis and make it more comprehensive. The last section consists of specific comments on the manuscript.

A disclaimer is needed: I don't claim that what I propose here is the best possible framework to analyse accelerometer data. I only claim that it is probably the most useful given that we are discussing accelerometer measurements only (instead of a full IMU) and that the audience of the journal is not necessarily familiar with the details of this technology yet.

Analysis of the accelerometer data

The sections of the manuscript that refer to the calculation of the rotation angles are not referenced well. My guess is that the authors followed this technical note from

NXP: https://www.nxp.com/docs/en/application-note/AN3461.pdf

which is very useful for the people that make embedded systems but (like most of this type of notes) omits a lot of the necessary theory.

My first objection is that the authors calculate the rotations without any information about the initial orientation of the accelerometer sensor. When the manuscript refers to "close to vertical" or "close to horizontal" rotations it is necessary to specify both the frame of reference (horizontal according to what? the global inertial frame?), and which is the accelerometer axis that approaches that level (horizontal or vertical). The fact that the authors moved the time-series around during the plotting, makes this initial orientation even more difficult to understand. In short, there is no guarantee that the accelerometer in the boulder is orientated as shown in figure 5a. As a result, it is not possible to verify the "horizontal" or the "vertical" without calibration *in situ*.

My second objection is that the authors discuss the increased error and the coarser resolution of the axes rotation close to the "horizontal" level, without a clear description of what this is and why it happens. More importantly, it is presented as a sensor/ programming issue which is missleading. The reason the error increases, is called in the theory of rotations "Gimbal Lock". It is the result of the rotations described in the NXP note above as non-commutative, which in plain language means that they are not independent (when one axis changes, the other two change too). There are a lot of useful references for this, one of the most concise and simplified can be found here: http://www.chrobotics.com/library/understanding-euler-angles

My third and final objection has to do with the attempt to record linear accelerations without compensating for gravity. The accelerometer measures the difference between the gravity field and any applied linear acceleration. When the sensor is static it measures any rotation that is not directly aligned with the gravity field. The problem begins when the sensor starts moving (when the linear acceleration is applied). If there is no accurate description of the relative orientation of the sensor with the gravity field (in 3D) available, the two measurements (the static and the "mobile") cannot be decoupled.

To summarise, the presented analysis doesn't offer a true representation of the boulders' movement, despite the fact that 3D accelerometer data are presented. And by "true" I don't imply a fully accurate measurement of the dynamics. The presentation of the data in the manuscript does not allow for a confident observation of the mode of motion (rotation or linear motion), which is critical. This results to a qualitative interpretation of the data which is better than nothing, but a) doesn't explore the full potential of the technology and b) skews even more the already tangled references on the use of accelerometers in the field of geomorphology.

Proposed framework

I strongly believe that there is no real need for a 3D (or even 2D) description of the rotations to make this application successful. It is possible to derive metrics that represent the magnitude of rotational changes and the magnitude of the applied linear acceleration without analysing 3D rotations in their full detail. The direction of rotations is not important in the context of early detection. Robust motion detection can be achieved by calculating the "unit quaternion" and by compensating for gravity using the norm of the raw (non-normalised) accelerometer data only.

The quaternions are a group of complex numbers. They have a long history, but they are in the spotlight at the moment because they simplify the rotations of IMUs. There is incredibly large number of references online and ever more guides to implement them in IMU rotations. However, the vast majority of them assumes the presence of a gyroscope which is not available here. For this application, it is enough to treat quaternions as 4-element vectors.

Valenti at al. (https://www.mdpi.com/1424-8220/15/8/19302) provide a solution for an auxiliary quaternion as a part of an optimised sensor fusion which includes a gyroscope and a magnetometer. This solution is auxiliary because it rotates the acceleration vector to the global (earth) horizontal plane, but doesn't define the orientation in 3D (a magnetometer is necessary for that). However, it provides a global "rotation" metric and it avoids singularities, which is the main issue with the convention followed in this manuscript. The authors can refer to the equation 25 of Valenti et. al, which is the following:

$$ {}^{b}_{i}q = \begin{cases} \begin{bmatrix} \sqrt{\frac{a_{bz}+1}{2}} & -\frac{a_{by}}{\sqrt{2(a_{bz}+1)}} & \frac{a_{bx}}{\sqrt{2(a_{bz}+1)}} & 0 \end{bmatrix}^{T} & , a_{bz} \geqslant 0 \\ \begin{bmatrix} -\frac{a_{by}}{\sqrt{2(1-a_{bz})}} & \sqrt{\frac{1-a_{bz}}{2}} & 0 & \frac{a_{bx}}{\sqrt{2(1-a_{bz})}} \end{bmatrix}^{T} & , a_{bz} < 0 \end{cases} \tag{1} $$

where ${}^{b}_{i}q$ is the quaternion (a 4-element vector for this application), that rotates the accelerometer from to the global horizontal frame, and a_{bx}, a_{by}, a_{bz} are normalised accelerometer measurements.

If the authors derive a ${}^{b}_{i}q$ for each one of the boulders, then the norm of this quaternion is calculated using the following equation:

$$ n(q) = \sqrt{q_2^2 + q_1^2 + q_3^2 + q_4^2} \tag{2} $$

where q_1, q_2, q_3, q_4 are quaternion elements. I state this calculation here because most of the quaternion operations are different to the typical vector ones. The n(q) of each boulder can be used to check the stability of equation 1 (this is a unit quaternion, the norm should be approximately equal to 1). The ${}^{b}_{i}q$ of each boulder is an unambiguous metric of orientation change. A time-series of ${}^{b}_{i}q$ will show when the boulders have rotated. And the component derivatives of ${}^{b}_{i}q$ can give a quantified metric of the speed of this rotation.

As for the linear acceleration, it is enough to just subtract the gravity norm from the raw acceleration norm. It will be much easier to identify major impacts and understand the noise threshold of the accelerometer when it is static. The manuscript states that the sensor at "maximum" settings did not record anything larger than 1g. If that is the total acceleration and there is no compensation for gravity, then the boulders did not move (according to the accelerometer), or the data did not transmit at all during transport events for the reason I discuss in my third objection on the analysis of the accelerometer data.

My final suggestion is to try this framework (or at least try to extract global metrics or norms instead of accelerometer axes metics) and then evaluate the use of the moving average filters applied in this work. Most of the IMU errors are not linear, but the reasoning of the filtering methodology used in the manuscript is not unfounded. I think that after the calculation of more global or normalised metrics, many spurious values will be punished or clearly identified in the noise threshold of the sensor which makes them easy to remove.

Specific comments on the manuscript

Line 21 ".. and sudden rotations"

Those are difficult to distinguish using the accelerometer measurement only.

Line 21, end of paragraph discussing RFID tags

A general note here is that all those techniques work in a "before/ after" event manner. Not suitable for warning.

Line 91 "Recently, the use of IMUs (Inertial Measurement Unit) has been tested for different applications in 91 the field of geomorphology (e.g. Caviezel et al., 2018 and references therein)."

If the scope here is to refer to previous IMU deployments in geomorphology, I would argue that the first one was from Akeila et al. 2010. And the first milestone from Frank et al., 2014. Those refer to fluvial and coastal transport respectively, and the implementations are quite detailed.

Line 95 "..to reconstruct the path and movement of individual particles in a laboratory flume"

This is not accurate. Unfortunately, it is not possible to reconstruct the path of a stone using a standalone IMU.

Line 109 "...accuracy and precision of the measurement itself, the latter requiring further development"

Very important comment this one. I sincerely appreciate it.

Line 116 "... the energy thresholds required for remobilisation of different grain sizes"

The term "energy" here is quite misleading. Is this a reference to kinetic energy? I think re-wording this to "forcing" will clarify this sentence

Lines 132-138

There are paragraphs like this that they need to be summarised more. I know the advantage of a wireless semi-automated warning system is clear. I think that there is a little bit more space on arguing about the benefits of the deployed system that it is required.

Line 142 "We also demonstrate for the first time the use of this technology in the field of geomorphology, and in a field setup, to monitor the movement of boulders embedded within a landslide and in two debris flow channels."

That is a very strong statement. There are few applications of IMU sensors in fluvial environments. Unless the authors mean the wireless transmission of data through the local network, which is, to my knowledge, a widely applied technology for environmental studies too.

I think that the sections 2.2 and 2.3 can be summarised. I understand the need to describe the site, but if this paper is more about the deployment of the system, then the focus must be on the method and its validation. This amount of background information just shifts the focus, in my opinion at least.

Line 249 " The sensors are equipped with an accelerometer configured to sample at 2 Hz, as well as a GPS module"

The first question that pops up to mind is " is that enough?" I think we are talking about very gradual motion (before the long intense event). A little bit more discussion about the sampling frequency is necessary.

Line 250 "When movement is detected by the accelerometer, so that tilt or acceleration exceed defined thresholds,.."

This means that the sensor is programmed in a "sleep - wake" routine. What is the frequency of the measurement for the "sleep" state? I assume that the 2Hz sampling frequency refers to the "active" state.

> Line 261 "The depth of the hole allowed for the emplacement of the C-cell batteries and the sensor. After placement, each hole was filled with epoxy resin, sealing the cavity, thus protecting the device from tampering and from the elements..."

Is there any consideration regarding the orientation of the sensor in respect to the frame (the 3D volume) of the boulder?

> Line 277 "The devices deployed in the 2019 season were programmed to not store the data, but to send it immediately, causing the data transmitted during gateway offline time to be lost."

Very common issue, I have been there and it is a very hard lesson to learn. It hurts even if you can repeat the experiment in the next hour.

> Line 304: "... with an accelerometer event for which activation thresholds can be set for impact forces and for angular variations."

Detail is needed for this threshold

> Line 309: "In the first case, the values of the three axes are normalised and the measurements essentially represent the static angle of tilt or inclination, thus the projection of the acceleration of gravity, g, on the three axes, ranging between 0 (for a horizontal axis) and $\pm$ 1 (for a vertical axis)."

Question a: normalised with the acceleration norm I guess, needs to be clarified.
Question b: how the normalised measurement is a direct measure of the tilt angle? Many references are needed here.

> Line 321 "...and m is accelerometer value recorded on the same axis in g"

Is this a normalised measurement?

> Line 322 ".. = 0.016 g the corresponding angular variation is of 0.9 if the axis is vertical, but 5.5 if the axis approaches horizontal"

I think the authors use the normalised accelerations and only positive angle 2D changes. This is a very small subcategory of 3D rotations. In addition, it is necessary to describe more equation 1. And the authors need to explain why the resolution changes according the initial orientation of the sensor.

> Line 327 "...variability in accelerometer measurements around a stable value, rather than true movement, with this effect becoming more important in sensors programmed with the coarser scale."

The way this is written, it implies that the error and the scaling are programming/ sensor issues. They are not. The differences in scaling appear because the authors used Euler angles (yaw and pitch from what I can understand) which result into singularities. This could be avoided with the use of quaternions.

> Line 339 "Measurement variability and errors related to the sensors led to spurious data, given the relatively small angular threshold assigned for the highest detectable maximum of 16 g. In other words, given that the step of accelerometer measurement is as high as 0.186 g, a spurious angular variation of more than 5 is often detected even when the boulder is stable, due to intrinsic measurement variability (up to 2 bits)."

This may be an artefact of calculating subsequent orientations and the integration of those. It is very difficult to tell from this presentation.

> Line 346 "within ± 0.186 g of the data point immediately before the window. If any of the values lie outside the ± 0.186 g threshold.."

This averaging corresponds to the "near vertical scenario". What if the sensor is on the "near horizontal" state?

> Line 350 "This would mean that a high value would likely be followed by a change in the static angle of tilt of the three axes"

Here, the case of a linear motion without rotation is not captured.

> Line 351 "Therefore, it is unrealistic to have a peak value followed by a value equal to that observed before the peak."

If the sampling frequency is at 2Hz, this is potentially true for the larger boulders. But not safe to assume for the smaller ones.

> Line 359 "The accelerometer readout in the current version of the software is tied to a GPS acquisition, this means that although the accelerometer is measuring as soon as movement is detected, the acquisition is obtained only when the GPS has successfully retrieved the position."

This is not an issue, as long as there is a clear description of when the movement is detected (time and acceleration threshold)

> This is not an issue, as long as there is a clear description of when the movement is detected (time and acceleration threshold for the "wake up" of the sensor.

A quantification of how "rough" is this estimate here would be useful.

Line 386 This data is used in a qualitative way for comparison with and validation of the accelerometer data obtained with the wireless devices and, despite the qualitative approach, this data provided a quite detailed overview of the days in which movement occurred.

It is important to stay here how the authors associate the geomorphic change with the accelerometer data. It is necessary to recognise that the comparison refers to two vastly different time resolutions.

Line 421: "The values of each axis are recalculated to show the deviation from the original position for visualisation purposes, rather than the actual values measured (hence all raw data curves begin at 0, and the smoothed curves around zero, due to the smoothing)."

This can be very misleading. The initial orientation is crucial for interpreting the accelerometer data.

Line 434 "Fig. 4 and 5 show that the movements of boulders within the landslide not only differ in the magnitude of the angular variations recorded, which is an order of magnitude higher for B A226 and B 9A41 in comparison to other boulders, but also in the evolution with time."

There needs to be an objective, quantified metric for this comparison. There is plenty of data.

Line 461 "These boulders were programmed to retrieve actual g values (as opposed to normalised values) and forces up to 16 g."

This needs be highlighted and clarified much earlier in the manuscript

Line 473 "We do not observe forces > 1 g for any of the sensors programmed with the maximum settings, despite the ability of the sensors to detect up to 16 g. This is consistent with a lack of debris flow activity recorded by cameras or seismometers, the more prolonged activity of which would have generated sustained boulder movement, beyond the time needed for GPS acquisition as explained below"

This is not compatible with the detection of linear accelerations. Compensation for gravity is required.

Lines 458-459 require further quantification

Lines 508-515

I know that those deployments are extremely difficult and they don't always go to plan. But the discussion of the GPS data here is not very useful. It is both too long and not directly feeding to the interpretation of the data. My honest opinion is that the paper would benefit if the GPS data were not discussed in the main body. It is probably useful for an appendix to demonstrate the deployment, but there is no clear quantitative information that can be extracted from here.

Line 543 "The movements observed for the boulders scattered on the landslide body and embedded within the material can be described as small angular variations that occurred gradually during the season"

Those are the type of statements that require further quantification.

Line 550 "...show higher magnitude of the angular variations with respect to other boulders (Fig. 5F-G)."

How much higher?

Line 552 "..follows a spatial and temporal pattern"

This is a very strong statement. I would definitively require some statistical justification.

Line 588 "What controls this behaviour is not the fact that the sensors were programmed to detect the maximum force or the static tilt respectively, but rather the scale that was chosen and associated with the two settings types."

That can be true, but it is not the only reason for increased noise. My first guess would be de-callibration or humidity. Those sensors are very temperamental. And most of the noise is traditional, random AC-DC circuit noise.

Line 602 "Future improvements of the accelerometer accuracy, resulting for example from the activation of the 9-axes IMU present in the hardware of the devices, could reduce this problem"

This is very true. Especially the gyroscope measurement will be very useful for this type of measurement.

Line 616 " The high positional errors and the important battery expenditure make the current GPS module not fit for the purpose of tracking boulders in rugged terrains."

That is also very true. We need to investigate or come up with alternative tracking techniques for remote areas.

Line 630 " This may explain why, although the boulders in the channel were programmed to detect high forces, they never show accelerometer values higher than 1 g (either negative or positive)."

This is true, but I think there is also an artefact of the processing followed here. I am not sure the authors will pick up very high inertial force. The boulders are quite large and heavy. Maybe 1g is too small though (see notes above).

The last section of the discussion (5.1) is useful but I think it could be summarised a lot. A table of prons and cons would be a good addition.

Line 730 "...but that in the future are expected to replace the need for an accurate GPS."

I would strongly oppose that. There is no evidence that the available IMUs will be suitable for standalone tracking anytime soon. Unless, the authors refer to military grade optical sensors which cost £10k each. If that is the case it is necessary to provide some specs.

GM