CLOSURE: CASE STUDY: OSO, Washington, Landslide of 22 March 2014: Dynamic Analysis

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The writers appreciate the thorough discussion provided by Richard M. Iverson, which raises some important points concerning our dynamic analysis of the Oso flowslide. This closure is structured as follows: (1) response to Dr. Iverson's criticisms of our runout analysis methodology, (2) clarification of comments in Aaron et al. (2017) regarding the modelling results of Iverson et al. (2015) and Iverson & George (2016), (3) comparison of the simulations of Iverson et al. (2015) and Iverson & George (2016) with those of Aaron et al. (2017) in light of the present discussion, and (4) summary of our runout analysis.

Criticisms of Runout Analysis in Aaron et al. (2017)

Dr. Iverson states the central scientific problem of explaining the mobility of the Oso flowslide is quantifying the cause-and-effect mechanism by which the landslide dynamics and large undrained strength, i.e., liquefaction, are coupled. This criterion is then used to judge our analysis of the Oso flowslide. Dr. Iverson claims that, because the initial conditions of our model do not correspond to static equilibrium, our modelling results provide no basis to understand the mechanisms acting during the Oso flowslide. Dr. Iverson provides two sets of analyses to demonstrate that our model does not begin from a state of static equilibrium. The first analysis uses a comparison of our average effective friction coefficient to that required for static equilibrium, and shows that our strengths, derived from inverse runout analysis, are too low for static equilibrium. The second analysis uses an energy balance to show that our inverse analysis derived strengths imply that the landslide commenced with a large, instantaneous release of energy.

We fundamentally disagree with the criteria Dr. Iverson has used to judge our analysis. As is elaborated more fully below, we do not think that runout models must start from a statically balanced initial state to provide useful results and the liquefied strength was not operating at static equilbrium. It has long been recognized that, due to the complexity of earth materials, a meaningful geotechnical analysis must balance accurate site characterization with careful use of idealized/conceptual models, all of which must be moderated with judgement (Burland, 1987; Goodman, 1999; Hungr, 2016). The criteria and criticisms presented by Dr. Iverson are heavily weighted towards formulating idealized models, and this comes at the expense of recognizing the complexity of earth materials, and the judgement required to interpret results from idealized models in a meaningful way. As will be discussed in the following sections, our analysis has attempted to balance accurate site characterization with careful use of two semi-empirical runout models, to perform a meaningful dynamic analysis of the Oso Landslide that is in good agreement with field observations.

Dr. Iverson misunderstood the *Analysis of Landslide Mechanism* section presented in Aaron et al. (2017). This is possibly due to the use of different definitions of the word ‘failure’. In Aaron et al. (2017), as well as the present discussion, we use the Hungr et al. (2014) definition, which defines failure as the “single most significant movement episode in the known or anticipated history of the landslide…”(Hungr et al., 2014, p. 167). The shear strength parameters that we derived through our inverse runout analysis correspond to the residual strength acting during the flowslide, discussed below. We never claimed that our model begins from a statically balanced initial state nor do we believe that it does. However, as summarized below, we think that our model can be used to investigate certain questions relating to the runout of the Oso flowslide.

Dr. Iverson’s comparison of our initial conditions to a slingshot and levitation are misleading, as we do not introduce an artificial source of energy because our failure mechanism involves the upper portion of the slope that has a high peotnetial energy. The initial conditions of our model correspond to the point in time when the shear strength on the rupture surface in the water-filled colluvium has been reduced to its minimum or residual value. The initial force imbalance is due to brittle failure of the upper portion of the slope. The landslide accelerates from the at-rest condition due to gravity and internal pressure gradients (as summarized in the equations of motion presented in Hungr (1995) and McDougall & Hungr(2004)) and it impacts the water-filled colluvium.

Our implicit simplification is that this undrained strength loss in the colluvium is instantaneous; a simplification that we believe is justified because we focus only on simulating the runout of the flowslide. As shown on Figure 1, our maximum simulated velocities are similar to the average velocities reported by Iverson & George (2016, p. 181), suggesting that this simplification is justified. This is a well-known and commonly used simplification, and users of semi-empirical runout models are familiar with and acknowledge it. Through careful inverse analysis, models that use this simplification have been a useful tool to investigate certain mechanisms acting during the motion of extremely-rapid, flowlike landslides (e.g. Hungr et al., 2002; Hungr & Evans, 2004; McDougall et al., 2006; Sosio et al., 2008; Aaron & Hungr, 2016; Hungr, 2017). Of course, our model cannot answer the question “Will a large undrained strength loss occur?”, instead it provides insights into the question “Given that a large undrained strength loss has occurred, what is the expected final deposit distribution?". We have only interpreted our model results to answer the second question, and therefore believe that our conclusions are valid because they match field conditions.

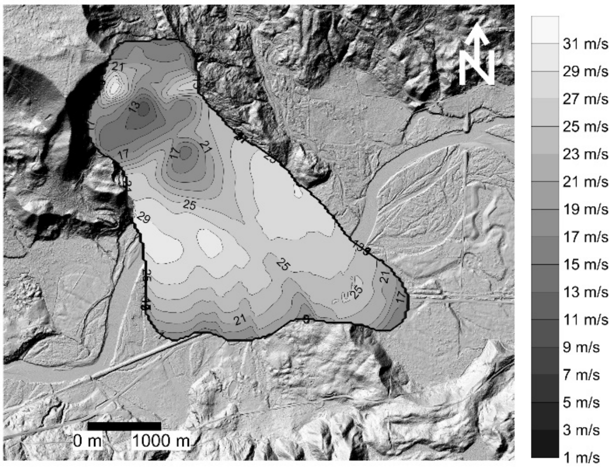

Figure 1: Maximum simulated velocities at each grid node in the computational domain for the Phase A simulation in Aaron et al. (2018).

We parameterize the liquefied strength by using a liquefied strength ratio (Olson & Stark, 2002), an approach that is criticized by Dr. Iverson. Stark & Mesri (1992) introduced the use of a strength ratio to express the shear strength estimated from inverse-analyses of field liquefaction case histories because it was observed that greater pre-failure effective vertical stress yielded greater liquefied shear strengths. After about two decades, the advantages of expressing liquefied shear strength as a normalized strength ratio have been recognized (e.g. Duncan & Wright, 2005; Idriss & Boulanger, 2008).

We agree with Dr. Iverson that liquefied strength results from a complex set of micro-mechanical interactions, however, we do not believe that the strength derived from our inverse analyses is a fixed material property. We have used a well-known empirical relationship to parameterize basal shear strength, and have instead derived 'bulk' or 'apparent' properties based on an inverse analysis of the distal runout behaviour. We believe this to be an appropriate simplification due to the ground profile encountered at the site. This point is discussed in more detail below. In the discussion section of Aaron et al. (2017) we acknowledge that we have not provided a mechanistic description of the strength loss process. We qualitatively suggest some possible mechanisms that could lead to the residual strengths estimated based on our inverse analysis.

Comments from Aaron et al. (2017)

Dr. Iverson has rebutted a number of comments made by Aaron et al. (2017) regarding the models of Iverson et al. (2015) and Iverson & George (2016). Here we comment briefly on this rebuttal.

In Aaron et al. (2017) we state that Iverson & George (2016) assumed that the entire volume of the Oso landslide liquefied. Dr. Iverson rightly points out that his models do not assume the entire volume liquefies, although they do assume that liquefaction could potentially occur everywhere. Iverson & George (2016) then predicted the extent of liquefaction based on their input material properties (discussed below) and "... by gradually increasing the basal pore-water pressure everywhere within the slope..." (Iverson et al. 2015 p. 204). For the set of parameters that they claim "…provides a good match to the Oso landslide's inferred speed and area of inundation…" (Iverson & George, 2016 p. 181) they predict widespread liquefaction, which may be less than the entire volume.

Our site investigation and modelling results suggest that the models of Iverson et al. (2015) and Iverson & George (2016) dramatically overpredict the extent of liquefaction. Our hypothesis, supported by our field and modelling results, is that the large undrained strength loss was confined to the water-filled colluvium derived from previous landslides along the slope, and not the unsaturated and overconsolidated slope materials, which did not liquefy. The models presented by Iverson et al. (2015) and Iverson & George (2016) that best reproduce the impact area of the flowslide predict that the intact slope materials liquefied, a prediction we find unlikely based on field evidence. This field evidence includes the overconsolidated and unsaturated nature of the intact slope materials, as well as the thick accumulation of relatively intact debris deposited in the source zone (Keaton et al., 2014; Wartman et al., 2016; Aaron et al., 2017; Stark et al., 2017).

Dr. Iverson states that “Figure 7 of Iverson and George (2016) illustrates clearly that their alternative simulations of the Oso landslide each predicted that some landslide material would be deposited in the source zone, and that the distal deposits of the landslide would be much thinner than the 30 m claimed by Aaron et al. (2017).” (Iverson, 2018 p.8). While this is true, no simulations presented by Iverson & George (2016) *simultaneously* predict significant deposition in the source zone and the geometry of the distal deposits. The observed thickness of the slump block in the source zone is on the order of 50 m (Keaton et al., 2014; Wartman et al., 2016; Aaron et al., 2017; Stark et al., 2017). On Figure 7 of Iverson & George (2016), the only simulation that predicts deposition this thick uses an initial density equal to the critical state density. For this simulation, the distal deposits are not reproduced. As discussed in Aaron et al. (2017), the significant volume of material deposited in the source zone is likely due to brittle failure of the unsaturated and overconsolidated slope material, which did not liquefy or undergo an undrained strength loss. These intact materials remained frictional and stayed on the slope (Stark et al., 2017).

Comparison of the Two Dynamic Models

Based on the discussion above, we think it is relevant to compare the dynamic analysis performed by Iverson et al. (2015) and Iverson & George (2016) to that performed by Aaron et al. (2017). To do this, we first briefly review available site investigation data and then discuss the modelling objectives and results of the two analyses.

Numerous site investigations have revealed that the rupture surface of the Oso flowslide passed through an overconsolidated, varved, anisotropic glaciolacustrine silt and clay with infrequent fine sand laminae (e.g. Keaton et al. (2014) Section 5.1; Wartman et al. (2016) Figure 2; Stark et al. (2017) Figure 2; Aaron et al. (2017) Figure 1). A photo of this unit is shown in Figure 2, which shows the unsaturated and stiff nature of the glaciolacustrine clay. The water-filled, loose colluvium highlighted as susceptible to a large undrained strength loss by many investigators (Keaton et al., 2014; Iverson et al., 2015; Iverson & George, 2016; Wartman et al., 2016; Aaron et al., 2017; Stark et al., 2017) is derived from this glaciolacustrine unit. Sand boils were observed in the deposit (Iverson et al., 2015), however given the absence of a sandy unit in the source zone, we think these are likely composed of alluvium entrained during motion.

Figure 2: Unsaturated and overconsolidated glaciolacustrine silt and clay unit from which the pre-existing colluvium was derived. Photo: J. Aaron.

The purpose of the Aaron et al. (2017) runout analysis was to test the hypothesis that the colluvium underwent a large undrained strength loss/liquefied, whereas the intact slope materials did not. Aaron et al. (2017) parameterized what they interpreted to be colluvium with strengths representative of a liquefied material (after the strength along the rupture surface had been reduced to its residual value), and the intact material with strengths representative of a frictional material with moderate pore-water pressures (i.e, this material is simulated to be moving in a drained condition). Aaron et al. (2017) then estimated the strength parameters that best reproduced the observed deposit through an inverse runout analysis. If Aaron et al. (2017) could reproduce the deposit with liquefied strengths comparable to other liquefied case histories, and drained strengths similar to those measured by Stark et al. (2017), then their model results would support the hypothesis that only the water-filled/saturated colluvium liquefied. This analysis has some discriminatory power, because if Aaron et al. (2017) had parameterized the entire failed mass with a strength typical of liquefied material, then they would not have been able to reproduce the deposit distribution, regardless of the chosen parameters. To meet these objectives, the analysis presented by Aaron et al. (2017) used a realistic rupture surface (see Stark et al., 2017), and carefully considered the site stratigraphy and observed distribution of the slide deposits.

Iverson et al. (2015) states that “A crucial question regarding the Oso DAF concerns whether prevailing conditions at the site made landslide liquefaction and high mobility nearly inevitable …”, and that “…the question can be addressed in a mechanistic way by performing alternative dynamic simulations of the landslide. Our simulations are not intended to recreate the precise details of the slope-failure process at Oso…”(Iverson et al., 2015 p. 204). Based on this, we think the purpose of the dynamic simulations presented in Iverson et al. (2015) and Iverson & George (2016) was not to provide a detailed inverse analysis of the event, instead they focus on inferences into the landslide dynamics that can be gained from their simulations. When interpreting their simulation results for this purpose, we think two important limitations must be acknowledged: (1) Iverson et al. (2015) and Iverson & George

(2016) did not use a realistic rupture surface and (2) the material properties used by Iverson et al. (2015) and Iverson & George (2016) do not represent the site stratigraphy.

The dynamic analysis presented in Aaron et al. (2017) uses a compound rupture surface above the water-filled colluvium, which is appropriate for the anisotropic glaciolacustrine unit (Stark et al., 2017) and supported by numerous surface and subsurface observations (Aaron et al., 2017; Stark et al., 2017). The dynamic analyses presented by Iverson et al. (2015) and Iverson & George (2016) use a deep-seated rupture surface based on a logarithmic spiral, which is only relevant for homogenous and isotropic materials (Stark et al., 2017). No compelling site investigation data exist to support this interpretation of the rupture surface. This logarithmic spiral rupture surface is also steeper than our compound rupture surface, and therefore results in a greater imbalance between driving gravitational forces and resisting forces, once liquefaction has occurred in their model. This, combined with their prediction of widespread liquefaction, is the likely reason that the models of Iverson & George (2016) cannot explain both the significant deposition in the source zone and the geometry of the distal deposits.

The material properties used by Iverson et al. (2015) and Iverson & George (2016) "…were inferred from laboratory testing of sediment mixtures used in landslide and debris-flow experiments that involved uncompacted materials similar to the predominantly sandy material observed at Oso (Iverson et al., 2000, 2010)." (Iverson & George 2016, p.181). As summarized above, the Oso flowslide was not composed of a predominantly sandy material. It is not obvious to us that an accumulation of water-filled/saturated, fine grained colluvium along the slope toe derived from the grey glaciolacustrine silt and clay would behave like a sandy material similar to that experimentally tested by Iverson et al. (2000 and 2010). As discussed in Aaron et al. (2017), the colluvial material at Oso is more likely composed of disaggregated blocks of overconsolidated glaciolacustrine silt and clay with water filling the cracks and fissures between the blocks.

Given the discussion above, we think there are significant limitations to the simulations presented by Iverson et al. (2015) and Iverson & George (2016). However, with appropriate use of judgement, these models can provide important insights. They support the hypothesis that a large undrained strength loss occurred at the site (although, as summarized above, they likely overpredict the extent of liquefaction), elucidate the process of pore-pressure dissipation at the margin of flowslides, and demonstrate the sensitivity of undrained strength to initial porosity. In interpreting these results, however, it is important to realize that details of the site stratigraphy were not known at the time Iverson's simulations were performed, so the physical relevance of the parameters used (if interpreted as true material properties) requires further justification. The concepts of in-situ and critical state density for a heterogeneous, overconsolidated, and anisotropic glaciolacustrine silt and clay unit need to be meaningfully defined. The dynamic model used by Iverson et al. (2015) and Iverson & George (2016) can only simulate the liquefaction of a loose granular material, a limitation that must be disclosed and does not represent the majority of the soils involved in the slide.

Summary

We believe that a meaningful geotechnical analysis must balance site characterization and use of idealized models, all moderated with judgement. With this in mind, numerical dynamic models should be used within a complete analysis framework that carefully considers the site stratigraphy. This is how we set-up and interpreted our dynamic modelling results. The shear strengths we have derived from inverse analyses correspond to an apparent residual strength attained by the water-filled colluvium along the slope toe during the runout phase of motion.

The inverse analysis results, based on a geotechnically feasible rupture surface and a detailed interpretation of the site stratigraphy and the landslide deposits, support the hypothesis that two distinct mechanisms occurred during the Oso flowslide (Keaton et al., 2014; Wartman et al., 2016; Aaron et al., 2017; Stark et al., 2017). One mechanism is a large undrained strength loss of the water-filled/saturated colluvium that mantled the slope toe due the impact of a slide mass further upslope. This colluvium was derived from an overconsolidated, varved, glaciolacustrine silt and clay layer. The second mechanism is brittle failure of unsaturated, overconsolidated, and non-liquefiable intact slope material including glacial till and dense outwash sands. Our numerical modelling results do not provide a mechanistic description of the large undrained strength loss of the water-filled/saturated, fine grained colluvium. However, we do suggest some potential mechanisms that could have resulted in the observed undrained strength loss of the colluvium. Further research into this topic is warranted.

Our analysis equally weighs consideration of the ground profile and results from idealized models. We have made careful use of empiricism to support our conclusions, while acknowledging the limitations of our analysis. Due to this, we think we have identified one of the key unanswered questions regarding the Oso flowslide: How can an accumulation of colluvium, derived from overconsolidated, varved, glaciolacustrine silt and clay, undergo a large undrained strength loss/liquefy? This is not a material conventionally recognized as liquefiable, and it is not obvious how the mechanics of liquefaction of loose, saturated sands apply to this material as Iverson claims. As this conclusion acknowledges the complexity of the site stratigraphy, it is useful for researchers and practitioners trying to predict the future behavior of similar slopes.

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We were greatly saddened by the passing of Oldrich Hungr last year. He was an insightful researcher, a patient advisor, and a great mentor. He approached scientific debate with respect, an open mind, and good humour, and we agree he would have welcomed this discussion. He was an integral part of this work, and his insight into the present discussion is greatly missed.

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