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## FEATURE ARTICLE

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## Landslide Disparities, Flume Discoveries, and Oso Despair

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### Key Points:

- This article recounts a 40-year effort to understand and predict the sometimes-surprising dynamics of landslides and debris flows
- Key parts of this effort entailed development of the USGS debris-flow flume and of numerical models informed by the flume experiments
- Application of this work to the disastrous 2014 Oso landslide highlighted the rewards and frustrations of using science in the real world

**Abstract** Landslide dynamics is the branch of science that seeks to understand the motion of landslides by applying Newton's laws. This memoir focusses on a 40-year effort to understand motion of highly mobile—and highly lethal—landslides such as debris avalanches and debris flows. A major component of this work entailed development and operation of the U.S. Geological Survey debris flow flume, a unique, large-scale experimental facility in Oregon. Experiments there yielded new insights that informed development of mathematical models that were aimed not only at explaining landslide dynamics but also at evaluating landslide and debris flow hazards. The most sophisticated of these models, called D-Claw, found its first practical application during investigations of the 2014 Oso, Washington, landslide disaster. That event provided indelible lessons about the utility and sociology of science in the real world.

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## 1. Introduction

“Everybody knows that dirt moves downhill. I don't see the point of what they're doing here.” That's what a dump truck driver said to me on a hot summer afternoon in 1993 as he gave me a lift to the top of the U.S. Geological Survey (USGS) debris flow flume near Blue River, Oregon. Dirtied and disheveled, I must have looked more like an itinerant laborer than a scientist, and I didn't admit that I was the person responsible for construction and operation of the flume. Instead, I pretended to share the driver's puzzlement. “They usually mix water with the dirt and turn it into mud before they cut it loose,” I said, “but that shit goes downhill, too.”

Few subjects of scientific study might seem as drab as landsliding. It merely entails stuff going downhill. Nevertheless, gaining a clear understanding of commonplace phenomena has always been more tantalizing to me than investigating phenomena that are inaccessible to direct observations or experimental tests. Newton's theory of gravity-driven motion was purportedly inspired by something as mundane as an apple falling from a tree, and I've been inspired by the potential of using Newton's insights to develop and test new mathematical models of landslide dynamics. Such models have more than academic interest because they can play an important role in landslide hazard mitigation. In fact, application of my work to real-world problems has been the most satisfying aspect of my scientific career.

This memoir summarizes 40 years of field observations, experimental investigations, and model development that in many ways culminated in March 2014, when I stood amid a staggering scene in the North Fork Stillaguamish River valley near Oso, Washington—where an extraordinarily mobile landslide had crossed the ~1-km-wide floodplain and taken the lives of 43 people there (Figure 1). The memoir recounts not only scientific successes but also my failures and frustrations along the way.

## 2. Field Observations and Inspirations

### 2.1. Gros Ventre, Wyoming

I first puzzled over the aftermath of a highly mobile landslide in 1974 while attending the Iowa State University Geology Field Camp near Shell, Wyoming. As part of the camp's itinerary, we visited the site of the 40-Mm<sup>3</sup> Gros Ventre landslide that occurred in 1925 just east of Grand Teton National Park. What most perplexed me there was the moderate slopes of the landslide source area and runout path—they weren't even as steep as a pile of sand. Why had this great mass of rocky debris failed and traveled so far over relatively gentle slopes? Although possible explanations were aplenty, no one really knew (Voight, 1978).

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**Figure 1.** Oblique aerial photograph of the site of the disastrous 22 March 2014 landslide near Oso, Washington, USA. USGS photograph by Mark Reid, 1 April 2014.

## 2.2. Mojave Desert, Southern California

In 1977 I began graduate studies at Stanford University and soon found myself conducting accidental debris flow research in California's Mojave Desert. The purpose of the Mojave work was to investigate the physical impacts of off-road vehicle traffic on arid lands (Iverson et al., 1981), but during reconnaissance trips through places such as Panamint Valley, it was impossible to miss the conspicuous deposits of debris flows on alluvial fans bordering the mountain fronts. I had learned a bit about debris flows while taking Arvid Johnson's famous class, "Physical Processes in Geology" (Johnson, 1970), and I was excited to see such striking evidence of the real thing. I was intrigued even more, however, when I witnessed the formation and motion of miniature debris flows during rainfall simulation experiments that Bern Hinckley and I conducted in Jawbone Canyon. Our experiments were designed to study erosion that accompanies rainfall runoff, but on some slopes steeper than  $24^\circ$ , rain-soaked sediment began to move not when it was entrained by running water but instead by mass failures (Iverson, 1980). The mobilizing mixtures of sediment and water transformed into miniature debris flows, complete with lobate granular fronts, lateral levees, and watery tails. Each of the flows involved less than  $100 \text{ cm}^3$  of material, but they were fascinating nevertheless. I wanted to learn more about how they worked.

## 2.3. Minor Creek, Northern California

My interest in debris flows took a back seat for a few years as I finished a pair of MS degrees and then embarked on a PhD project that involved monitoring and modeling the behavior of the slow-moving Minor Creek landslide just southeast of Redwood National Park in northwestern California (Nolan & Janda, 1995). From my perspective, I had the best PhD project ever. It was almost entirely unsupervised yet was generously funded owing to great trust placed in me by John Bredehoeft of the USGS. Bredehoeft had a keen interest in the connection between groundwater hydrology and earthquake triggering, and he saw landslide hydrology as an accessible analogue. I saw it not only as a wonderful scientific opportunity but also as a chance to learn to maneuver and operate a trailer-mounted drill rig on a steep, rugged slope.



I recruited my friend Hinckley to help, and over the course of a summer we peppered the landslide with nearly 100 boreholes in which we took samples and installed instrumentation.

As I accumulated data over the next several years, it became apparent that the most important scientific puzzle at the Minor Creek landslide was, in a sense, precisely the opposite of the puzzle at Wyoming's Gros Ventre slide. The Minor Creek landslide moved downslope every winter in response to rising groundwater pressure at its base, but it moved slowly and almost steadily even in the presence of widely fluctuating rainfall patterns (Iverson & Major, 1987). Its top speed never exceeded  $10^{-7}$  m/s (1 cm/day). The extensive groundwater and soil properties data I'd collected explained why the landslide started and stopped moving annually as the water table rose and fell, but the data held few clues about what regulated the landslide's speed as it moved. Faced with the need to finish my PhD thesis, I addressed this question by developing a model that was quite elaborate mathematically but rather unsatisfactory physically because it invoked soil viscosity as the property that regulated landslide speed (Iverson, 1985, 1986a, 1986b). (Disclosure: slowly deforming soil behaves predominantly as a frictional material that lacks significant viscosity.) I suppose I was a callow youth—and I was unquestionably a youth who was strongly motivated to finish his PhD. I convinced both myself and my committee that my fancy math represented reality, but I was wrong. I was lucky, however, because I later had the opportunity to use experiments and theory to understand regulation of slow, stable landslide motion more satisfactorily. It took me only 20 years to do it (Iverson, 2005; Schaeffer & Iverson, 2008).

#### 2.4. Mount St. Helens, Washington

In 1984 I finished my PhD and moved to Vancouver, Washington, to begin work at the U.S. Geological Survey's Cascades Volcano Observatory (CVO). I knew nothing about volcanoes, but the USGS wanted to hire me, and the only opportunity was at CVO, which had as its primary task the monitoring of Mount St. Helens (MSH). The volcano had continued to erupt intermittently following its big blast on 18 May 1980, and it was an exciting place to be. (My first day of field work at MSH involved flying into the crater to conduct an early season snowpack survey and watching as my friend Roger Denlinger disappeared into a collapsing, snow-roofed pit the instant he stepped off the helicopter. Roger was OK, fortunately, and on subsequent trips we brought skis on the helicopter and used them to travel more safely within the crater and then descend to the Pumice Plain north of the volcano at the end of a day's work. We dubbed it Mount St. Heliskiing.)

MSH is a paradise for landslide and debris flow research. It was, and still is, the site of the largest terrestrial landslide in recorded history. The 2.3-km<sup>3</sup> landslide that decapitated the volcano and triggered the explosive eruption of 18 May 1980 was remarkably well documented (Glicken, 1986). Nevertheless, like Wyoming's Gros Ventre landslide, the MSH landslide had exhibited great mobility that was incompletely explained (Voight et al., 1983). In addition, most of the major stream valleys draining the slopes of MSH had been inundated by debris flows (i.e., lahars) on 18 May 1980. When I arrived in 1984, their imprint on the landscape was still fresh. Even today, small debris flows occur routinely at MSH when they're triggered by adequate rainfall or snowmelt.

Despite the ripe setting for research, my first two attempts to do landslide dynamics science at MSH were both colossal failures. The first effort took place on a steeply sloping segment of the deposit of the great 18 May 1980 landslide. I recruited electronics wiz Rick LaHusen to help me establish a field laboratory there, complete with a large umbrella tent, gas-powered generator, desk, portable computer (which had a mass >20 kg in those days), and a huge quantity of sensors, cables, pumps, hoses, and plumbing fixtures. We blanketed the slope with instrumentation and installed subsurface well points in several locations where our pumps injected groundwater. Our goal was to trigger an artificial landslide and measure its internal dynamics to help us understand how landslides like the giant MSH landslide of 18 May can be so mobile. We pumped and pumped into our injection wells, but nothing much happened. The ground was resolutely stable, so we eventually surrendered to its obstinacy, collected our gear, and left.

Our next effort was in Loowit Channel, which drained most of the crater of MSH and had a history of conveying small debris flows. The stream in Loowit Channel dropped more than 50 m over a bedrock-lipped waterfall as it exited the crater mouth, and the channel just upstream from the waterfall was flooded by bedrock, too. The substrate and channel geometry there were ideally suited to our purpose of installing an array of electronic sensors intended to measure the degree of liquefaction in flowing

debris (i.e., the degree to which the weight of moving debris is supported by basal pore fluid pressure). No one had done this before, and such data could test the hypothesis that the great mobility of debris flows resulted from liquefaction—but a different style of liquefaction than that caused by earthquakes. LaHusen and I eventually got our sensor array and data logger working, and we returned to CVO to await the arrival of the next debris flow. It came just a few weeks later, but it was 10 times bigger than we'd anticipated. It simply swept all our instrumentation away, never to be seen again. A change in tactics seemed warranted.

### 3. Experiments in Japan

A big opportunity for a tactical change presented itself in 1987 when I was invited to spend a few months at the National Research Center for Disaster Prevention (NRCDP) in Tsukuba, Japan. (The same organization is now known as the National Research Institute for Earth Science and Disaster Resilience.) NRCDP operated the world's largest and most sophisticated rainfall simulator, which hung from the roof of a building roughly the size and shape of a hangar for storage of jumbo jets. The goal of my stay at NRCDP was to use the rainfall simulator to conduct large-scale landslide experiments with Japanese colleagues, and LaHusen came along to help. The Japanese handled the major logistics of the experiments, including the use of heavy construction equipment to place 40-m<sup>3</sup> prisms of sandy sediment onto a roughened, 30° concrete slope. LaHusen and I installed an array of electronic sensors in the sediment to make high-frequency measurements of pore pressure, downslope displacement, and shear strain as the slopes failed. The experiments were a big success, and we began to understand how motion-induced changes in pore pressure can cause some landslides to liquefy and mobilize as they fail (Iverson & LaHusen, 1989).

More importantly, the experience LaHusen and I gained in Japan opened our eyes to the scientific potential of closely controlled, large-scale laboratory experiments. Such experiments are reproducible, and they eliminate many uncertainties that can complicate interpretation of data collected in the field. In addition, big experiments can benefit from an economy of scale resulting from the use of industrial equipment to move materials instead of equipment specially fabricated for use in a conventional lab. Why spend megabucks to fabricate a one-of-a-kind sediment conveyor when you can buy a bulldozer instead?

## 4. Experiments at the USGS Debris Flow Flume

### 4.1. Flume Beginnings

Almost as soon as I returned from Japan, I began lobbying for construction of the world's first large, outdoor debris flow flume. Debris flow science was languishing as a result of a dearth of high-quality data, and controlled experiments in an appropriately scaled flume held promise for providing such data. Nevertheless, my proposal was a tough sell. Some USGS managers stated rather bluntly that the idea was ridiculous. Fortunately, my immediate supervisor, John Costa, and his supervisor, John Conomos, thought that the idea had merit and told me to proceed—at least through the planning stages. They reckoned that they could find roughly \$220,000 to spend on flume construction because that much money would soon be freed up by the retirement of a senior scientist and his project.

I knew that the flume needed to be large in order to produce debris flows that were dynamically similar to field-scale flows. Miniature debris flows created in benchtop experiments generally don't act like the real thing, in part because the viscosity and shear strength of the fluid phase in sediment-water mixtures have a disproportionately large effect on shear resistance at small scales (Iverson, 2003, 2015). Back-of-the-envelope calculations told me that we could likely afford to build a reinforced concrete flume roughly 100 m long, 2 m wide, and 1 m deep, and I figured that such a flume could convey debris flows as large as 20 m<sup>3</sup>. Also, partly on the basis of experience gained in Japan, I felt that the flume should be inclined roughly 30° to strike the right balance between realism and practicality.

With those criteria in mind, the big question was where and how to build such a flume. The USGS owned no land with a suitable building site. Moreover, it lacked the funds to buy land or build roads, and it had little experience with designing and overseeing large construction projects. Indeed, prospects for building the flume seemed pretty dim when I allowed myself to stop and think about it—but I tried not to do that.



At this point I'll digress a bit to repeat a quote that's commonly attributed to Johann von Goethe but was actually penned by the Scottish mountaineer, William Hutchison Murray (1951). It was printed on a card that I'd kept at my desk since I was in graduate school:

Until one is committed, there is hesitancy, the chance to draw back, always ineffectiveness. Concerning all acts of initiative and creation, there is one elementary truth the ignorance of which kills countless ideas and splendid plans: that the moment one definitely commits oneself, then providence moves too. All sorts of things occur to help one that would never otherwise have occurred. A whole stream of events issues from the decision, raising in one's favour all manner of unforeseen incidents, meetings and material assistance which no man could have dreamt would have come his way.

Whenever I got discouraged I reread this quote. Now, as I view my debris flow flume experience retrospectively, I am amazed by the sequence of "unforeseen incidents, meetings, and material assistance" that came my way and allowed the flume to be planned beginning in 1989, constructed beginning in 1991, and used beginning in 1992.

The first providential event was a discussion I had with Fred Swanson of the U.S. Forest Service (USFS) regarding the question of where we might build the flume. He thought there could be a suitable site within the H.J. Andrews (HJA) Experiment Forest, a research watershed near Blue River, Oregon, that's jointly administered by the USFS and Oregon State University. Shortly thereafter, Art McKee, the director of HJA, was onboard with the flume concept and suggested that there might be a good location just east of the HJA headquarters compound. LaHusen and I quickly made a trip there to survey the site, thrashing through the underbrush of the second-growth forest with a compass, measuring tape, and Abney level. It was perfect! The slope stretched roughly 100 m between two existing roads and was inclined an average of 31°. Moreover, it was situated on federally owned land only 3 hr from CVO and had housing for researchers nearby. A bonus was that Lookout Creek, which ran through the heart of HJA, could easily provide the large volume of water we'd need to generate manmade debris flows.

The next steps were to design the flume and navigate the complexities of the permitting, bidding, and construction process. I had a specific flume architecture in mind, but I lacked any experience with designing concrete structures. To enlist the necessary expertise, I sought the services of Franz Rad, a professor of structural engineering at Portland State University who specialized in reinforced concrete and did consulting work. Rad was enthused with the project, and he and I had many meetings to pore over details of the flume design. He ultimately generated a set of blueprints and construction specifications that laid the groundwork for what was to come.

With a flume plan in hand, new bureaucratic hurdles started to appear, but USFS staff again came to my rescue. The HJA Experimental Forest is located within the McKenzie Ranger District of the Willamette National Forest, and construction projects within the district must go through its formal permitting and approval process. USFS district rangers have good reason to be skeptical of any unusual requests for permits, and building a huge concrete flume on a steep slope certainly fit that description. Nevertheless, district ranger Lynn Burditt listened thoughtfully to my rationale and agreed to issue a USFS Special Use Permit for construction and operation of the flume—contingent on an agreement to remove the flume when use of the facility was discontinued. More than 30 years later, that time has not yet come.

Formal oversight of the competitive bidding and construction processes was also handled by USFS staff, mostly based at the Willamette National Forest headquarters office in Eugene, Oregon. Key individuals included Gerry Foster, Jerry Stevenson, Lisa Anheluk, and John Cissel. I held my breath when the bids from prospective builders came in and let out a sigh of relief when the low bid put the construction project within our price range. Foster told me not to fret about hiring the low bidder because on such a unique project the low bid commonly comes from the contractor who's most competent to do the work. So we went with it, and Foster was right. The project soon got underway, and by the fall of 1991, a great mass of concrete had been poured on our 31° slope. It was fun but a little frightening to watch the construction process unfold. A lot of time and money had been invested, and the return on investment was unknown.

#### 4.2. Flume Findings

By April 1992 the flume was ready to use. It's an extreme understatement to say that our first experiment did not go well. Only three of us, Tony Bequette, LaHusen, and I, were on hand to witness it, but a video record of the debacle exists (Logan et al., 2018). In a steady rain we loaded the hopper at the top of the flume with 20 m<sup>3</sup> of pea gravel and then opened the flume head gate to let it go, but it hardly went. Instead, the moist pea gravel drizzled down the slope, retarded by the surface tension of water adhering to the grains. It left the entire flume bed coated with a layer of sediment averaging more than 0.1 m thick. The three of us spent the ensuing week cleaning roughly 40,000 kg of material out of the flume by using shovels, rakes, and a primitive dredge improvised out of scrap iron. The experience was painful in more ways than one.

Our luck soon started to improve, however, and by July 1992 we'd figured out how to make realistic debris flows in the flume by adding water in a strategic way to 10-m<sup>3</sup> prisms of initially dry, loosely packed, poorly sorted sediment. Soon thereafter, a stream of useful data started to pour in. In fact, data flowed so abundantly that we initially reported only some rudimentary results and instead concentrated our efforts on pressing our experimental program forward (Iverson et al., 1992; Iverson & LaHusen, 1993).

By 1997 our flume scientific team had grown and we had begun to publish some significant findings about the structure and mechanics of debris flows (Iverson, 1997a; Iverson et al., 1997; Major, 1997; Major & Iverson, 1999; Reid et al., 1997). The most important of these findings was firm evidence that debris flow bodies were, indeed, largely liquefied, and that motion of debris flows was resisted mostly by friction within unliquefied, coarse-grained surge fronts. This basic structure developed within several seconds as our experimental debris flows started moving, and it persisted even as the flows decelerated and formed deposits. Experiments with diverse debris mixtures showed that the effects of this structure were accentuated by the presence of suspended silt-and-clay sized sediment, which inhibited drainage of fluid and thereby enhanced liquefaction and debris flow mobility. This effect was precisely the opposite of that envisaged by many previous debris flow researchers, who thought the main role of fine sediment was to contribute yield strength that helped resist downslope motion.

Findings that were more nuanced—and commonly more counterintuitive—emerged over the course of many years of subsequent experiments at the debris flow flume. The sophistication of these experiments grew with time and benefited greatly from the addition of Matthew Logan to our staff in 1998. Like LaHusen, Logan was best described as an inventor. (Sadly, the USGS lacks “inventor” as an official position title.) One of Logan's most valuable skills was in devising and fabricating unique mechanical apparatuses that allowed us to perform various difficult tasks that we hadn't even attempted before.

The most formidable task that Logan spearheaded was roughening the flume bed in the year 2000. Previously the bed had a smooth, broom-finished concrete surface like that of standard sidewalks, but we believed that flume debris flows would behave more realistically if the bed were greatly roughened so that it better emulated the condition of most natural beds. To accomplish this, we used an elaborately choreographed sequence of operations to anchor specially fabricated concrete tiles to nearly the entire 31° flume bed. The tiles were designed so that poorly sorted sediment contacting them exhibited a quasi-static basal friction angle very similar to the sediment's internal friction angle. Over the course of a week our small crew hauled and installed roughly 35,000 kg of tiles. Although they've shown some wear, none of the tiles has failed over the ensuing 20 years.

After the flume bed was roughened, we conducted a series of experiments that compared the behavior of debris flows on the rough bed with the behavior of similar debris flows we had previously run on the smooth bed (Iverson et al., 2010; Logan et al., 2018). Unsurprisingly, flows on the rough bed were slower and deeper than those on the smooth bed. Almost paradoxically, however, debris flows on the rough bed traveled considerably farther than those on the smooth bed did when they discharged from the flume mouth and ran out across a nearly horizontal surface. This behavior was a consequence of grain-size segregation, which led to formation of coarse-grained lateral levees that confined the debris flows and focused their momentum. Debris flows on rough beds developed the most pronounced grain-size segregation and consequently formed the most prominent lateral levees and most far-traveled deposits. Subsequent experiments at the flume revealed many details of the grain-size segregation and levee formation process (Johnson et al., 2012).

Another fruitful set of flume experiments focused on how sediment porosity influences landslide acceleration from initially static states (Iverson et al., 2000; Reid et al., 2008). We used one of Logan's devices to compact the sediment very precisely as we placed it behind a retaining wall at the top of the flume, and then gradually wetted the sediment with artificial rainfall and groundwater. In this way we triggered a series of landslides with initial porosity differences of only a few percent. Loosely compacted landslides liquefied rapidly as they failed and underwent runaway acceleration, whereas landslides that were only slightly denser exhibited either slow, quasi-steady slip or episodic stick-slip cycles. These differences in behavior are clearly evident in archived video recordings made in 1998 and 1999 (Logan et al., 2018).

One of our most challenging but enlightening sets of experiments examined debris flow interactions with erodible bed sediment (Iverson et al., 2011; Reid et al., 2011). In these experiments we lined the rough bed of the flume with a nearly uniform layer of sediment averaging about 0.1 m thick and then used sprinklers to wet the bed sediment to varying degrees before releasing debris flows onto the beds (Figure 2). The results showed that debris flows interacting with bed sediment can grow explosively in mass and momentum—provided that the bed sediment is wet enough and loose enough to liquefy when it is compressed and sheared by the overriding mass. In contrast, debris flows interacting with drier bed sediment simply bogged down and lost momentum. These effects are very conspicuous in paired video recordings that contrast the behaviors of flume debris flows interacting with different substrates (Logan et al., 2018).

Other sets of debris flow flume experiments focused on a wide variety of topics, including debris flow motion through channel bends (Iverson et al., 1994), debris flow run-up on vertical barriers and adverse slopes (Iverson et al., 2016), hydrologic triggering of landslides and debris flows (Reid et al., 1997), debris flow containment by flexible steel barriers (DeNatale et al., 1997), breaching of debris dams by rising water levels (Walder et al., 2015), basal stress states in static debris masses (Iverson & George, 2019), radiation of seismic energy by debris flows (Allstadt et al., 2019), and tsunami generation by debris flows entering bodies of water. Many additional topics await future experiments.

## 5. Mathematical Modeling

As our debris flow flume experiments progressed, I undertook parallel efforts to develop physically based mathematical models that were informed by our findings. I'd always embraced the view that, unless you can express something mathematically, you don't fully understand it. Moreover, I'd taken to heart a challenge that Richard Feynman (1965) posed to anyone with a mathematical model that makes a successful prediction: What *else* does your model predict? I consequently aimed to develop models that were consistent with well-established physical theory yet made new, testable predictions in diverse contexts.

A principal goal of my models was to account for the coupling between deformation of granular materials and evolution of pore fluid pressure that mediates intergranular friction as landslides move. For soils or rocks undergoing small, elastic deformations, the role of similar coupling was established by Terzaghi's (1923) consolidation theory and elaborated by Biot (1941, 1956). The conceptual problem I struggled with was how to revise this theory to make it compatible with the mathematical structure of depth-integrated models of landslide and debris flow dynamics, in which deformations may be large, rapid, and irreversible. I focused on depth-integrated rather than 3-D models because I wanted to develop tools that were readily applied to real-world problems.

The progression of resulting models is described in papers by Iverson (1997a, 1997b), Iverson and Denlinger (2001), Denlinger and Iverson (2001), Savage and Iverson (2003), Iverson (2009), George and Iverson (2011), Iverson and George (2014), and George and Iverson (2014). Without the contributions of Roger Denlinger, Stuart Savage, and David George, many improvements in the sophistication of these models would have happened more slowly—if they happened at all.

Our current computational model is named D-Claw, short for Debris Conservation Laws (George & Iverson, 2014). The nucleus of this model consists of five conservation laws expressed as hyperbolic partial differential equations that are solved simultaneously using a shock-capturing finite-volume method with adaptive mesh refinement—a method previously implemented in GeoClaw to model tsunamis using





**Figure 2.** Photographs of a USGS debris flow flume experiment in which a debris flow entrained wet bed sediment that partially liquefied as it was overrun. Inset photo shows a close-up view of the agitated flow front interacting with bed sediment about 15 m downslope from the flume head gate. USGS photos by Matthew Logan, 21 June 2007.

shallow-water equations (LeVeque et al., 2011). However, the lowest-order approximation of the more elaborate D-Claw equations expresses the coupling between shear-induced volumetric deformation (i.e., dilatancy) and pore pressure change, so that's where some very essential physics resides (Iverson & George, 2014).

We first tested D-Claw against experimental data from our debris flow flume experiments, in which initial and boundary conditions as well as material properties were well constrained. We found that the model could predict differing styles and rates of landslide acceleration during the onset of motion as well as dynamics and depositional patterns that developed after landslides evolved into debris flows (George & Iverson, 2014). This gave us confidence that the model was ready for real-world applications. What we didn't anticipate was that our first real-world application of D-Claw would occur under great duress, when the model's output would be used to help guide recovery efforts following the disastrous Oso landslide of 2014.

## 6. Oso Despair

On the morning of 22 March 2014, a historically unstable bluff near the community of Oso in Snohomish County, Washington, collapsed in unprecedented fashion. It spawned a 9-Mm<sup>3</sup> landslide composed mostly of glacial and proglacial sediments, which transformed into a debris avalanche that sped across the floodplain of the North Fork Stillaguamish River and demolished a bucolic riverfront neighborhood there (Figure 1). As the scope of the devastation became clear, Snohomish County made a request for help, and my USGS colleague Jonathan Godt and I were dispatched to the site. Our purpose was not to conduct scientific investigations, but rather to assist and inform state and county officials who were managing the disaster. Our most pressing objective was to evaluate continuing hazards that might threaten those who were engaged in search-and-rescue operations.

When Godt and I arrived at the landslide site, we were met with a gut-wrenching scene. The landslide's distal deposit was a wood-laden chaos strewn with abundant evidence of obliterated human habitation. It looked like a debris field generated by a powerful tornado, except that much of it was encased in mud or quicksand that could swallow you up to your neck. Drizzling rain added to the gloom.

Hundreds of first responders were onsite, including EMTs, chainsaw operators, excavator operators, search dogs, and support crews who provided food, drink, and shelter for those working amid the horrific muck. Initially, the goal of the responders was to find survivors, but within several days the effort transitioned to one focused on body recovery. It was brutal work, which left even the search dogs traumatized because they detected hints of human remains in many locations at once. Meanwhile, David George was working nonstop at CVO running D-Claw with preevent lidar topography to generate landslide transport-vector maps. Soon these maps were distributed to recovery workers who used them to help guide their search. Nevertheless, it took months for officials to arrive at the landslide's final death toll of 43.

Although Godt and I hadn't arrived at Oso to do research, as scientists we couldn't ignore the most striking feature of the landslide: Some of the mass had traveled more than 1 km across the flat floodplain—an enormous distance considering that the landslide originated on a bluff only 180 m high (Figure 1). With the help of Snohomish County geologist Jeff Jones and geotechnical engineer Dale Topham, we made our way to the most distal point of the landslide deposit. There I used my Abney level to take a sighting on the landslide headscarp, and when I read the instrument's scale I was stunned. It showed that the inclination of a line between the landslide's headscarp and its toe was only 6°, implying that the ratio of descent height  $H$  to horizontal length traversed  $L$  was 0.105. I'd been studying landslide dynamics most of my life, and the value  $H/L = 0.105$  indicated a degree of mobility that was unprecedented for a historical landslide of this size and composition crossing a nearly flat, unconfined runout surface.

Thus, within hours of arriving at the Oso site, I knew that the cause of the severe devastation there was not merely that a large landslide had occurred, but rather that the landslide was extraordinarily mobile. I also suspected that basal liquefaction was responsible for the mobility, partly because of the abundance of quicksand amid the landslide's distal deposit and partly because of knowledge we'd accumulated during decades of experiments at the debris flow flume. Reporters and the public were clamoring for this sort of information, but I could communicate it to no one outside of an inner circle consisting of Godt, Topham, Jones, and geologist Stephen Slaughter of the Washington State Department of Natural Resources. The reason was that we'd been asked to comply with a gag order issued by Washington State officials, apparently because accusations were flying that mismanagement of state-owned land in the source area caused the landslide.

When media interest is intense and good information is lacking, less informed opinion will move in to fill the void. Each evening when I returned to my motel room in nearby Everett, I would turn on the TV and see experts pontificating about the Oso landslide. I had long known each of these people, some for more than 30 years. All were bright, but none had done much work on landslide dynamics or mobility, and none had visited the Oso site subsequent to the 22 March event. Some claimed that the landslide disaster was entirely predictable. Others were simply unaware that the mass fatalities were largely a consequence of the landslide's extraordinary mobility. Having to bite my tongue under these circumstances was extremely difficult. I eventually was able to tell my story to feature writers for *Science* magazine (Stone & Service, 2014), but by then a lot of misinformation about the landslide had already spread.



The flow of misinformation continued for months, causing me considerable despair. Shortly after the landslide occurred, a National Science Foundation-funded Geotechnical Extreme Events Reconnaissance (GEER) team sought to visit the site to gather data. However, emergency managers at Oso temporarily prohibited their access to the site to avoid any interference with ongoing search operations. Meanwhile, the USGS rotated crews of landslide specialists in and out of the site to inform the search effort, deploy instrumentation, collect perishable data, and interview eyewitnesses. By the time the GEER team arrived in late May and spent 4 days at the Oso site, USGS staff had already logged hundreds of person-days there. However, we had released almost none of our technical findings because we followed the usual protocols for scientific peer review and publication. In contrast, the GEER team rapidly prepared a 175-page interpretive report on the event and posted it online on 22 July, without any peer review (Keaton et al., 2014). They certainly deserved credit for speedy work, but in science the speediest work is not always the best work, and the GEER report shaped much of the subsequent discourse about the Oso event.

This is not the place to adjudicate the differences between the interpretations of the GEER team, USGS landslide scientists, and other groups who later worked at Oso because that would require many pages of text, data, and diagrams. Rather, it's best left to readers of pertinent technical publications to weigh the evidence themselves. The first Oso landslide publication by USGS authors appeared online in December 2014 and was formally published in February 2015 (Iverson et al., 2015). Subsequent publications by USGS staff fleshed out the details of numerical modeling and field investigations that informed our interpretations (Collins & Reid, 2019; Iverson & George, 2016). A peer-reviewed distillation of the GEER team's findings, with some revisions, was subsequently published, too (Wartman et al., 2016). Readers of any report on the Oso landslide ought to read the others as well.

## 7. Conclusion

"Everybody knows that dirt moves downhill." Perhaps owing to this observation, landslide science has long taken a back seat to other Earth sciences with respect to academic prestige and funding priority. Nevertheless, in my experience the challenges and rewards of landslide science are great. A serious challenge exists because models and interpretations of landslide behavior must withstand direct testing against irrefutable observations and experimental data. This property distinguishes landslide models from models of geological phenomena that are partly hidden by the veil of deep Earth or deep time. Rich rewards can be reaped in landslide science because valid models and interpretations are not merely vehicles of academic advancement. Rather, at their best, they can be applied in practical ways that benefit society at large.

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