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Forecasting natural hazards, performance of scientists, ethics, and the need for transparency

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Landslides are one of several natural hazards. As other natural hazards, landslides are difficult to predict, and their forecasts are uncertain. The uncertainty depends on the poor understanding of the phenomena that control the slope failures, and on the inherent complexity and chaotic nature of the landslides. This is similar to other natural hazards, including hurricanes, earthquakes, volcanic eruptions, floods, and droughts. Due to the severe impact of landslides on the population, the environment, and the economy, forecasting landslides is of scientific interest and of societal relevance, and scientists attempting to forecast landslides face known and new problems intrinsic to the multifaceted interactions between science, decision-making, and the society. The problems include deciding on the authority and reliability of individual scientists and groups of scientists, and evaluating the performances of individual scientists, research teams, and their institutions. Related problems lay in the increasing subordination of research scientists to politics and decision-makers, and in the conceptual and operational models currently used to organize and pay for research, based on apparently objective criteria and metrics, considering science as any other human endeavor, and favoring science that produces results of direct and immediate application. The paper argues that the consequences of these problems have not been considered fully.

Keywords: natural hazard; risk; landslide; forecast; predictability; accountability performance; ethics; transparency

Introduction

I am a geologist, and I work as a research scientist for the Italian *Consiglio Nazionale delle Ricerche*, the single largest research organization in Italy (CNR − www.cnr.it). Our mission is to design, promote, and execute research in all fields of science, and to transfer and disseminate knowledge to foster scientific, technological, economic, and social development in Italy. My personal research interest is on natural hazards, chiefly landslides, a slippery field − in all senses − when it comes to predictions.

In this paper, I present ideas and considerations on problems that scientists face when attempting to predict natural hazards: landslides in my case. First, I examine the approaches commonly used by scientists to predict natural hazards (including landslides), outlying the conceptual limitations of the approaches. Next, I discuss the role of scientists as advisors of decision-makers, the supposed contrast between basic and applied science, and the possible subordination of science to politics and decision-making. This is followed by considerations on the lack of understanding of the societal role of scientists, and

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the need for specific training on ethics in scientific research. Finally, I criticize current methods to evaluate the performance of scientists based on infometrics, and I call for improved transparency of the science evaluation systems.

The ideas expressed in the paper are personal and subjective. They result from 30 years of work spent detecting, mapping, and predicting landslides, and attempting to ascertain landslide risk. I shaped my ideas executing research to improve the ability of the Italian civil protection system to cope with landslides, a widespread and frequent hazard with significant human and economic consequences in Italy (Guzzetti 2000; Guzzetti and Tonelli 2004; Guzzetti et al. 2014; Salvati et al. 2010, 2014). The Italian (local) perspective has conditioned my thoughts, but I hope that my other activities, including being president of the Natural Hazards division of the European Geosciences Union (EGU − www.egu.eu) between 2002 and 2006, and an editor of the journal *Natural Hazards and Earth System Sciences*, have helped me retaining a broader perspective. Finally, my interest in the philosophy of science and in the societal role of scientists has influenced the ideas expressed in the paper, which were first written in a shorter paper published in the Italian magazine *Ecoscienza* (Guzzetti 2013).

Landslides and their prediction

The paper deals with the prediction of landslides and their consequences. But what is a landslide? A landslide is the movement of a mass of earth, debris, or rock down a slope, under the influence of gravity (Cruden and Varnes 1996; Hungr, Leroueil, and Picarelli 2013) (Figure 1). The definition is clear and simple, apparently. Reality is different, and landslides are complex and diverse phenomena (Guzzetti et al. 2012). Landslides can fall,
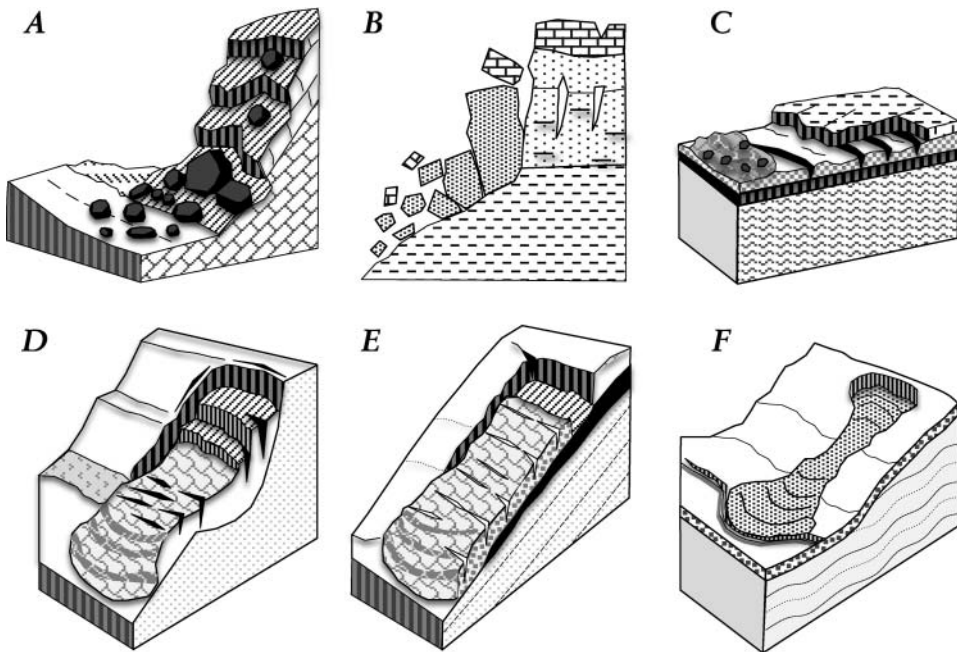

Figure 1. Landslide types: (A) rock fall, (B) topple, (C) lateral spread, (D) rotational slide, (E) translational slide, and (F) flow. Modified after Varnes (1978) and Cruden and Varnes (1996).

topple, slide, and flow, and many landslides exhibit a combination of these simple types of movements (Hungr, Leroueil, and Picarelli 2013), at the same time or through the lifetime of a landslide that spans the range from a few seconds (e.g., in the case of a rock fall) to several thousand years. The area and volume of most terrestrial landslides are in the ranges $10^0 < A_L < 10^8$ m^2 and $10^{-4} < V_L < 10^{11}$ m^3, respectively (Brunetti, Guzzetti, and Rossi 2009; Guzzetti et al. 2009). The Naschitti landslide, in New Mexico, USA, extends for $A_L = 4 \times 10^8$ m^2, with an estimated volume $V_L =$ of 2.2×10^{11} m^3 (Guzzetti et al. 2012), and the Saidmarech or Kabir Kuh landslide in Iran − possibly the largest known sub-aerial terrestrial landslide − extends for $A_L = 2 \times 10^{11}$ m^2. Submarine landslides are generally larger than the terrestrial failures, in the ranges $10^4 < A_L < 10^{11}$ m^2 and $10^2 < V_L < 10^{12}$ m^3 (Haflidason et al. 2004; Guzzetti et al. 2009; Brunetti, Guzzetti and Rossi 2009). Different natural triggers and human actions cause landslides (Guzzetti et al. 2009, 2012). The main natural triggers are intense or prolonged rainfall, rapid snowmelt, earthquakes, volcanic activity, and freeze−thaw cycles. Human causes of landslides include excavations, overloading, construction works, leakage from water or sewage lines, irrigation, deforestation, and traffic. On Earth, a natural trigger can result in a single landslide (or no landslides at all), or in tens or even hundreds of thousands of landslides in periods ranging from a few seconds to a few days. Landslides are not limited to the Earth, and were found on planets and moons (Lucchitta 1978; Lucchitta 1981; McEwen 1989; Quantin et al. 2004; Brunetti et al. 2014). Despite the simple definition, landslides are diversified and complex phenomena. The complexity and variability of the landslides make it difficult to predict landslides, their single and collective properties, and − most important for their societal consequences − the impact of landslides on vulnerable elements, including the population.

Predicting natural hazards

Scientists attempt to predict natural hazards (including landslides) and their consequences using a variety of approaches, methods, techniques, and tools, which − to some extent − depend on the type of the hazard and the information available for the prediction. Despite the differences, all the approaches can be loosely grouped into two broad categories: theoretic (mechanistic, deterministic, physically based, "hard") and empirical (statistical, functional, "soft") approaches. In very general terms, all natural phenomena, including natural hazards, can be classified based on their level of predictability (Prigogine and Stengers 1979; Prigogine 1996; Taleb 2007; Stein and Stein 2014). The motion of a bullet, pendulum, or planet exhibits comparatively little uncertainty, and can be predicted accurately in space and time using models expressed by simple mathematical equations (Figure 2). These natural phenomena share another important characteristic. Their physical behavior can be inferred from repeated observations. For these phenomena, time series and records of past events are important to determine the physical laws that control the phenomena, and for predicting future occurrences.

Other natural phenomena, including geophysical (e.g., earthquakes), meteorological (e.g., hurricanes, rainfall), and geomorphological (e.g., landslides, floods, erosion) phenomena that constantly shape the surface of the Earth (but also biological, ecological, and economic phenomena), are characterized by low predictability (which is different from unpredictability) (Stein and Stein 2014) (Figure 2). These phenomena are typically chaotic, and their low predictability originates in their unstable nature that can evolve catastrophically (Bak, Tang, and Wiesenfeld 1987, 1988; Rundle, Turcotte, and Klein 1996; Turcotte 1997). For these phenomena, repeated observations may not help much in the prediction of future events (Taleb 2009; Blöschl and Montanari 2010), particularly when
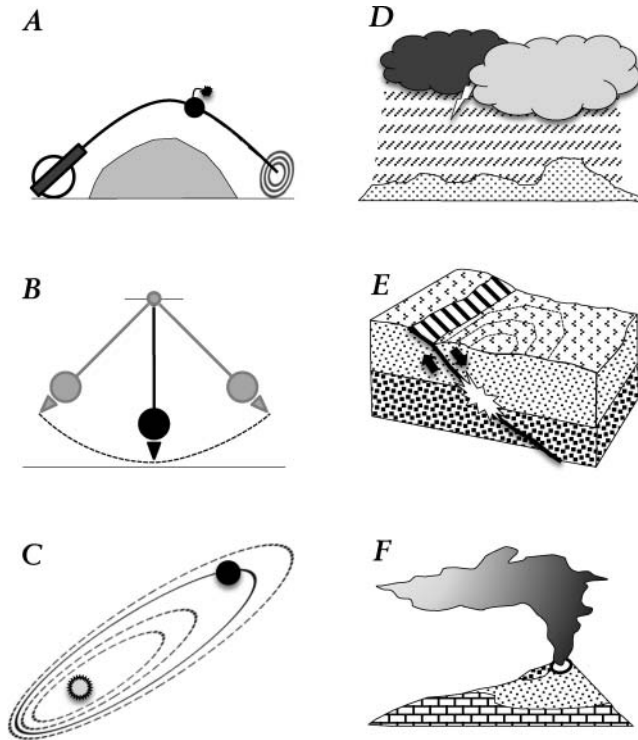
Figure 2.   Natural phenomena and their predictability: (A) trajectory of a bullet, (B) pendulum, (C) orbit of a planet, (D) intense rainfall, (E) earthquake, and (F) volcanic activity. Left column depicts phenomena characterized by high predictability, and right column depicts phenomena characterized by low predictability.

the future events are outside the range covered by the past observations. Many geophysical, meteorological, and geomorphological phenomena exhibit distinct nonlinear behaviors revealed by their "heavy-tailed" statistics (Rundle, Turcotte, and Klein 1996; Turcotte 1995, 1997). This complicates further the possibility of using past occurrences to predict future events.

It must be stressed that attempting to forecast a natural phenomenon (including landslides) with limited or incomplete information or knowledge is not only inherently difficult, but it may prove dangerous (Prigogine and Stengers 1979; Prigogine 1996; Fenton 2011; Nosengo 2012).

Predicting landslides

If addressed in mechanistic terms, the problem of the prediction of a landslide is relatively simple, or so it may appear. A block that slides along an inclined plane controlled by friction (Skempton 1948; Taylor 1948; Janbu 1954; Hoek and Bray 1977), or a point mass that falls and bounces along parabolic trajectories until it rests when all the energy is lost in impacts (Guzzetti et al. 2002), are reasonable representations of landslides (Figure 3). Both representations are relatively simple to model using basic physics principles, and a number of numerical models and software exist that can be used to predict the behavior of single landslides, or of unstable slopes (Guzzetti et al. 2002; Wyllie and Mah 2005).
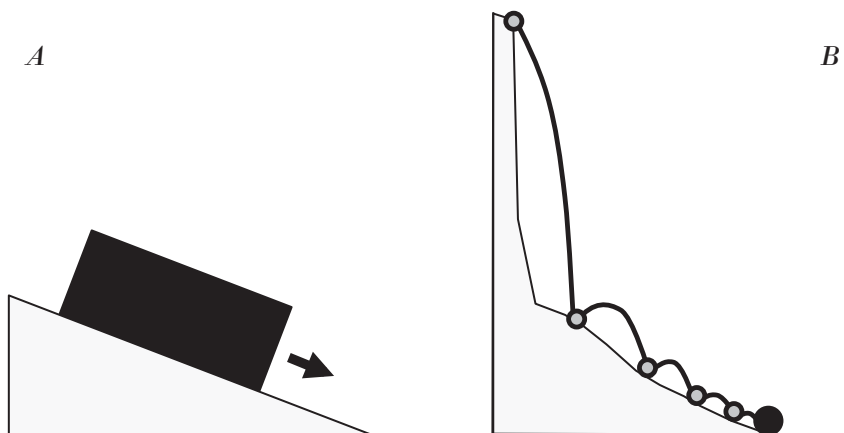
Figure 3. Simple but realistic representations of landslides: (A) a block that slides along an inclined plane is a good representation of a slide; (B) a point mass that falls and bounces along parabolic trajectories until all the energy is lost in impacts is a good representation of a rock fall, a particularly harmful type of landslide.

However, a pure mechanistic approach does not take us far, particularly when attempting to predict populations of landslides or the behavior of multiple unstable slopes over large areas (Alvioli et al. 2014; Raia et al. 2014; Mergili et al. 2014). This is because real landslides are more complex than their representation in the adopted physically based models, and because for most landslides, we do not know with sufficient accuracy (or we ignore completely) the geometrical and mechanical parameters that control the instabilities, from their points of failure to those of arrest. The behavior of a boulder falling from a cliff is impossible to predict "exactly" after just a few bounces, and rock falls are an example of a simple mechanical system whose behavior cannot be predicted even if the initial conditions and the driving force (i.e., gravity) are known (Guzzetti et al. 2002). To a reader unfamiliar with landslides, it may seem odd, but for most of the very many landslides in a typical landscape (Trigila, Iadanza, and Spizzichino [2010] have mapped more than 450,000 landslides in Italy, an average density of 1.5 landslides per square kilometer), we do not know the geometry, i.e., the extent, depth, and shape of the sliding surface, the volume of the instability, and the time or period of failure. For most landslides caused by rainfall or rapid snowmelt, we ignore the amount of water that has infiltrated into the ground causing the slope to fail (Guzzetti et al. 2007). This limits greatly our ability to predict landslides using the deterministic approach.

As for other natural phenomena, when the mechanistic approach fails to provide good results (for whatever reason, including the lack of a theory, of adequate knowledge, or of sufficient information) scientists revert to "soft" approaches that typically analyze the existing empirical observations using numerical (statistical) correlation analysis methods. Through the analysis of empirical observations, statistics is used in the attempt to identify patterns or relationships that can then be used to make predictions. However, as pointed out, for example, by Dondi and Moser (2014) in this volume, correlation does not prove cause−effect evidence, necessarily. In a recent paper, Guangmeng and Jie (2013) have used satellite images showing "cloud anomalies" observed hours before an earthquake to "predict" three earthquakes in Bulgaria, Iran, and Italy. Unfortunately, the authors have failed to consider events for which similar "anomalies" were observed and earthquakes did not occur, and to demonstrate the cause−effect evidence needed for predictions,

something that was attempted by Harrison, Aplin, and Rycroft (2014). Lack of evidence of a cause−effect relationship obtained through statistical correlation of empirical data is a potentially significant limitation of "soft" approaches, and it should always be considered when attempting to predict future events based on the results of "soft" modeling approaches. Further, "soft" methods require proper understanding of statistics and probability, and a correct interpretation of the mathematical results (D'Agostini 2011; Fenton 2011).

A benefit of statistical ("soft") models lays in the fact that they can measure the uncertainty associated with a predictive model, allowing to determining how much or how frequently a prediction is expected to be right or wrong. This can also be obtained with deterministic models, perturbing the initial conditions (e.g., like in ensemble forecasting, Leutbecher and Palmer [2008]) or by sampling randomly from known or inferred distributions of the parameters controlling the deterministic models (e.g., Raia et al. 2014; Mergili et al. 2014). The model uncertainty has different sources, including deficiencies in the model formulation, the approximate numerical methods used to solve the equations, and errors introduced using imperfect initial and other modeling conditions. Overall, this part of the uncertainty measures the lack of knowledge necessary for an accurate prediction. The uncertainty also measures the unpredictability of the phenomenon. Unpredictability and lack of knowledge are problematic to disentangle, and hard to communicate. The latter has many reasons, including the fact that scientists have to admit their lack of knowledge (i.e., ignorance) and cognitive limitations. Recognition of a (current) limit of science may be seen in contrast with the expectation that science can be used to predict all aspects of nature (Prigogine and Stengers 1979), including those related to natural hazards and their consequences. This may result in a loss of credibility by the general public. However, I maintain that there is an ethical duty to communicate the uncertainty associated to a prediction (Pozzati 2004). I further maintain that much work is needed to improve the ability of scientists (and science) to communicate their individual and collective ability (or inability, or the impossibility) to predict future hazardous events, and their consequences.

This is not an easy task, and it is complicated by the assortment and the sophistication of modern media, the increased demand of scientific information, and the inherent difficulty in communicating uncertain information. It is also unclear who should communicate, i.e., government (official) institutions, academic institutions, individual scientists, or independent organizations. The idea that only official (government) sources communicate reliable information may prove ineffective, particularly when the information is not delivered timely, and where other (independent) sources of information exist and are effective.

Scientists, decision-makers, and society

This brings me to the relationship between scientists, decision-makers, and the society; an old problem. Decision-makers, once represented by emperors, kings, and other monarchs, and today by presidents, prime ministers, ministers, legislators, governors, and policy and decision-makers, who decide and act on behalf of all of us, regardless of their rank and role have always sought the advice of scientists, and many scientists have advised decision-makers. Ludovico Sforza ("il Moro"), duke of Milano, asked Leonardo da Vinci to build for him machines, innovative weapons, and defensive structures. Isaac Newton served the British government as Warden and Master of the Royal Mint. The US President Franklin D. Roosevelt asked scientists of the caliber of Enrico Fermi, Richard P. Feynman, and J. Robert Oppenheimer to help the USA and its allies win World War II.

He also asked Venner Bush, director of the wartime Office of Scientific Research and Development, "how the lessons that had been learned (...) could be applied in the days of peace". Bush (1945) responded with the famous report "Science, the endless frontier". It is the rationale behind the saying "when the going gets tough, the toughs have to get going".

As a civil servant working for the largest research organization in Italy, I do not have a problem (moral, ethical, or else) with scientists working for, or advising decision-makers. I am convinced that science (and scientists) can be useful to decision-makers, and beneficial to the society. Otherwise, I would not have accepted to be part of the "*Commissione Nazionale per la previsione e la prevenzione dei Grandi Rischi*", a group of experts that collectively advise the Prime Minister and the Italian National Department for Civil Protection on natural and human-induced risks. However, I perceive a number of problems in the relationship between scientists, decision-makers, and the society. Some have always existed; others are new or were exacerbated recently (Sidle et al. 2013; Bilotta, Milner, and Boyd 2014; Nursey-Bray et al. 2014).

A first problem lays in the selection of who should advice the decision-makers. Ideally, decision-makers should seek for the "best" scientists in the pool. But who decides who is the "best", i.e., who is authoritative (influential) and who is not? The question is not trivial, particularly if one considers that often the problems for which scientists are asked for their advice are outside the fuzzy boundary of consolidated science, and require the scientists' "educated" or "informed" opinion. In the past, the difficult and uncertain selection was based (largely, if not entirely) on the *cursus honorus* of the scientist, i.e., on the results he/she had obtained in his/her career, demonstrated by the scientific work accomplished and the papers published, summarized in the CV. The opinion of the peers (i.e., the other scientists) was important to determine who was influential (authoritative) in a specific field, and who was not. The system was far from perfection, and established scientists could (and did) abuse of their authority. An example for all: Isaac Newton considered impossible, in face of contrary evidence, that the problem of determining the longitude at sea could be solved with (then) innovative mechanical clocks, a breakthrough in navigation (Sobel 1995). Here, I maintain that the track record of a scientist is important (mandatory) but that his/her future record is unpredictable. The fact that a scientist has obtained significant results — or has made successful predictions — in the past is no guarantee that the same scientist will be right on the next prediction.

I feel that in the recent years the relevance of the *cursus honorus* (the track record) and the opinion of the peers have diminished, significantly. Others contribute to deciding who is authoritative (influential) in a specific field. Further, when a scientist is (or is considered to be) an authority in a field, it is easy to consider her/him an authority in (apparently) related fields. Thus, an (true) expert in climate can be considered an expert in meteorology, and even in the various impacts of intense rainfall including floods, flash floods, landslides, and erosion. This, in spite of the fact that climate and, say, landslides are different phenomena, investigated using rather different methods and techniques. But who are the "others" who decide on authoritative scientists? The media — and chiefly TV and the Web — play a significant role. With an increased number of laws regulating medical, environmental, ecological, civil protection, and other personal and collective safety issues, the legal responsibilities of individuals and organizations have increased, or have materialized where they did not existed before. The judiciary system (e.g., courts of laws, judges, prosecutors, lawyers) exploits science (and scientists) more than ever before (Fenton 2011), and it is also (implicitly, and possibly unwillingly) contributing to the selection of influential scientists.

Politicians and policy and decision-makers use the advice of scientists, and contribute to decide who is authoritative and, implicitly, who is not. The later can generate a (potentially dangerous) short circuit, with decision-makers selecting scientists that fit their needs and expectations, and scientists fulfilling the needs and the expectations of the decision-makers. In Italy, many regional and even local administrations rely on experts working for local or nearby universities and research centers. However, it is unrealistic to think that in all the local universities and research centers, one can find leading experts and influential scientists. It is not sufficient to be employed by a university or a research center, or to publish in a scientific journal, to be an expert or an influential scientist. Further, there are excellent scientists employed by public, non-academic administrations and by private enterprises and businesses, who do not publish regularly in international journals. I maintain that universities and research centers, and the society as a whole, should acknowledge this evidence.

A second problem consists in the potential (or factual) increased subordination of scientists to policy and decision-makers. In my broad field of research (i.e., understanding and predicting natural hazards), the relationship between scientific knowledge and its exploitation by policy and decision-makers is changing, rapidly. On the one hand, more than ever before scientists are becoming advisors (or consultants) to decision-makers. A consultant serves the interests of the client, and when the "consultant" is a scientist and the "client" is a decision-maker, there is a subtle, but not negligible, risk that the scientist can lose the status of an independent and "free" scholar. On the other hand, decision-makers, and particularly elected officials, tend to lay the burden of hard and often unpleasant decisions on their advisors (i.e., the scientists). This is unfair and inadequate, because the roles and responsibilities of decision-makers and scientists are different. It is of paramount importance that scientists always perform their job of "advisors" to their "clients" adopting the highest possible standards and the best available knowledge. This will contribute to mitigate the problem, but will not resolve it, totally. Also, we should acknowledge that the highest standards and the best knowledge might not be accessible to all scientists, always and everywhere. This is known, for example, in the medical field.

I acknowledge that the different roles and responsibilities are often difficult (or even impossible) to separate. However, I maintain that a role of scientists in modern societies is also that of "whistle blowers" (Shrader-Frechette 2007; Benchekroun and Pierlot 2011). In sports, referees and umpires blow the whistle or raise a flag when a fault is committed. Similarly, scientists should blow their whistles and raise their flags when policy and decision-makers do not act, or do not act properly, or effectively. This can prove useful to prevent and mitigate risks (Dondi and Moser 2014). In sports, good referees and umpires are the independent referees and umpires. The same is for science, and scientists must be independent to be good and respectable referees who blow the whistle only when it is necessary.

It is worth investigating the reasons for the changing balance between science and decision-making. I maintain that it is simplistic to blame politics and the decision-makers. Certainly, in Italy − as in other countries − politics has percolated deeply into the structure of the modern society. The niche of the society represented by scientists and their organizations, including universities and research institutes and centers, is no exception and the influence of politics on science has increased, undoubtedly. In Italy, the government appoints the presidents and the administration boards of all the major public research organizations, including the National Research Council (CNR). I am not arguing this is negative, necessarily. I notice that this top-down approach − that does not stop at

the level of the presidents and the administration boards − is not balanced by bottom-up (e.g., "elected") counterweighs, that existed in the past and were eliminated.

I argue that others and more important reasons exist for the increased, and not necessarily all healthy, links between scientists, politics, and decision-makers. The first − and possibly the most important − is that to execute research one needs adequate resources (in other words "funding"), and politics and decision-makers control the public resources, entirely. Again, I am not arguing that this is negative, necessarily. What I want to point out is that how research is executed should not be decided by politics or the decision-makers, but by the scientists. On the other hand, scientists should not interfere directly on the adoption (or lack of adoption) of scientific findings and innovation by the policy and the decision-makers. I acknowledge that complete separation is difficult, but I maintain that in a healthy society, the roles of science (and scientists) and of politics (and decision-makers) are − and should remain − different.

A measure of the changing balance between those who provide the resources (politics, decision-makers, funding agencies) and those who use (consume) the resources (the scientists) is a − not so novel anymore − paradigm (model) for the organization and the execution of research. Nowadays, (almost) all the funding is given to execute "committed" research, i.e., research (more or less) explicitly requested by a "costumer", e.g., a funding agency or an administration that has requested a specific "result". I call it "hired science", without any negative or diminutive implications. Research is now bounded by legal contracts (see, e.g., the Grant Agreements regulating the projects funded by the European Commission that, together with their annexes, explain in great detail what shall be produced (the "deliverables") and when (the "milestones")). Supposedly, advantages of this model to organize research include (1) an increased competition among scientists, who are stimulated by the need to obtaining resources (a consequence of the dreadful rule of "publish or perish"), (2) an improved evaluation of the results obtained ("accountability"), which are compared to the results expected (e.g., promised), and (3) a straightforward exploitation of the innovations obtained (if any). This organizational method illustrates the outspoken modern interest towards "useful" science, which generates results for immediate application, preferably with commercial implications. The European Horizon 2020 flagship research and innovation program (Council of the European Union 2013) is framed around this paradigm. I maintain that scientists share the responsibility for the adoption of this paradigm. Scientists have often used significant resources without control, and not providing reasonable results and feedbacks. This was bad. Also, scientists have contributed − in various ways − to conceiving and implementing the paradigm, allegedly in an attempt to improve their accountability. I acknowledge the importance of accountability, but I am not convinced this is a way to obtain it.

I see a number of drawbacks arising from the implementation of the described (and prescribed) method of organizing and funding science. The interest of scientists to collaborate has decreased significantly. Individual scientists, research teams, and even research organizations pool together to compete for (and participate in) projects to obtain resources (i.e., to get funded), and not for a driving (and healthy) interest in the collaboration with their peers. This is exemplified by the increasing number of scientists from the same team or laboratory that participate to competing proposals in response to the same call, with the sole scope of getting the grant. For many scientists, the main scope is getting the grant, and not doing the research. This attitude, justified by the shortage of resources (compared to the number of scientists), has another drawback. An increasing number of research groups − and even of individual scientists − pretend (or attempt) to be knowledgeable and experienced in fields for which they are not. Every competing group

attempts to have all expertizes internally, reducing the needs for collaborations, and contributing to generate a self-referenced system.

The obsessive need to demonstrate the ability to obtain quick results has increased the tendency to produce results which are minor, marginal, and repetitive (Errami and Garner 2008), and of (almost) exclusive interest to the "client" of the specific research. There is also a propensity to overestimate (and overemphasize) the relevance and the applicability of the results obtained. A related problem is a tendency to ignore (unwillingly) or to hide (voluntarily) the problems encountered during a research, the workarounds adopted, the limitations of the results obtained, and the uncertainties inherent in any scientific endeavor. I maintain that the medium- to long-term consequences of these drawbacks have not been considered sufficiently. A consequence that is becoming manifest is the increasing lack of proper scientific discussion. The peer review systems that lays at the base of the self-evaluation of the quality of science is becoming less effective, with an increasing number of papers with wrong or irrelevant results published in peer-reviewed journals.

Basic or applied science?

This brings me to the − presumed or concrete − contrast between basic (i.e., theoretic, fundamental) and applied (i.e., problem solving, hired) science. I acknowledge that differences exist between basic and applied science. Applied science cannot exist (or does not have much sense) if it does not rely on theoretic principles discovered by fundamental science. Applied science can also serve as a stimulus for basic science (for landslide studies, see, e.g., Picarelli [2009]). Differences exist (or emerge) in the missions of the institutions that perform science. In many countries, the mission of universities is focusing on basic (fundamental) research (in addition to education), and the mission of research councils (including the Italian CNR) and of national institutes and centers is concentrating on applied science (in addition to transfer and exploitation of knowledge for the common societal and economic benefit). Despite these differences, I maintain that in the modern world the distinction between basic and applied science is irrelevant, and the sole relevant difference is between good and poor science. This is not a popular opinion, today.

The common opinion − particularly among decision-makers and funding organizations − is that "good" science is "useful" science. I have nothing against applied science (quite the contrary), and I welcome efforts to exploit scientific results and technological innovations for practical purposes. I also welcome educational activities and outreach efforts that help bring science "out of the labs". As I wrote before, I am firmly convinced that science can be useful and beneficial to the society. What I am noticing is that in my country − and in others too − decision-makers, including the highest levels of governments, are unable to ask scientists difficult questions relevant to the society. They are tailing the new discoveries and innovations, rather than stimulating the scientific community with challenging questions that may (and will) result in new discoveries and advancements. This is different from the approach followed, for example, by the UK government in 1714 when it offered the "Longitude Prize" for anyone who could find a practical solution for the precise determination of longitude at sea (Sobel 1995), a challenging problem for navigation at the time. The approach of challenging scientists with hard problems is well exemplified by the speech given by the American President John F. Kennedy at the Rice University, in Houston, TX, on 12 September 1962. To endorse the new space program, Kennedy said, passionately:

> We choose to go to the Moon in this decade and do the other things, not because they are easy, but because they are hard, because that goal will serve to organize and measure the best of our energies and skills, because that challenge is one that we are willing to accept, one we are unwilling to postpone, and one which we intend to win, and the others too.

The driving idea is that research fosters economic growth. I share this idea, deeply, but I maintain that the link between science and economic growth is complex and not linear. Today, science is considered as any other venture. I respect entrepreneurs, for their ability to produce innovation, for their willingness to take chances, and for their vision and wisdom. I am happy when a new product or service makes my life better, or easier. However, I do not think that scientists should be (or attempt to be) entrepreneurs, necessarily. I maintain that there is more to science than venture, and that considering science as any other endeavor may not be such a good idea.

With some exception (e.g., Rees 2014), it is self-evident that today politics and decision-making react to, and do not anticipate the ongoing or the expected societal, economical, and environmental changes (and challenges). It is perhaps less evident that this is happening in science as well. I will give an Italian example. Italy is exposed to most (if not all) natural hazards, and is the country in Europe with the largest number of human consequences produced by natural hazards. In the 64-year period, 1950−2013, more than 7000 people have died, went missing, or were injured by landslides and floods alone. In the same period, the number of evacuated or homeless people due to geo-hydrological hazards exceeded 700,000 (Guzzetti, Stark, and Salvati 2005; Salvati et al. 2010, 2014). Between 1944 and 2012, the direct economic damage caused by all natural hazards in Italy was estimated to exceed €240 billion (2011), a yearly average of €3.5 billion. Landslides and floods accounted for 25% of the total, i.e., €61.5 billion (2011) (ANCE-CRESME 2012). With significant uncertainties (Guzzetti et al. 2014), the figures are increasing, and measure the societal and economic problem posed by natural hazards, and geo-hydrological hazards, in particular, in Italy. Despite the problem being manifest, Italy does not have a scientific program on natural hazards, their consequences, and for the design of sustainable mitigation and adaptation strategies. There is no coordination of the research activities on natural hazards. The government National Research Program 2014−2020 ignores natural hazards and their societal and economic consequences. The government has announced an investment of €2 billion to mitigate geo-hydrological hazards. This unique effort will be conducted without any scientific support. This is not a matter of basic vs. applied science, or of good vs. poor science. This is evidence that politics and decision-making in Italy are shortsighted, and not interested in exploiting science and its potential contribution to the society. The general public should be aware of this shortsightedness. I maintain that in Italy a shift in paradigm is necessary to bring science at the core of the solution of the problems posed by natural hazards and their associated risk.

A matter of speed and ethics

Information is more abundant today than in any other period. Abundant information contributes to democracy, because informed decisions are more conscious and, in general, better decisions. Not only information is more abundant, it also circulates faster. Almost everything is faster today than it was in the past, and scientific research is no exception. I take 1991 as the year to mark a significant change in pace. In 1991, the European Organization for Nuclear Research − CERN − announced the World Wide Web, and today it is

(nearly) impossible to execute relevant research disconnected from the Internet. The Internet gives individual scientists and research groups fast and constant access to the existing knowledge and new information (e.g., through publications, conference proceedings, web seminars). Scientists receive through the net new data to analyze, and they process vast amount of data using innovative network technologies (e.g., grid and cloud technologies, search engines). Once obtained, scientists publish their results on the Internet that disseminates the new findings quickly and effectively.

Although research goes fast, it still proceeds slower than desired. The medical and the pharmaceutical fields are good examples. Progress is made virtually every day in these fields, but not fast enough to fulfill the needs and expectations of individuals and the general public, which are growing at a faster pace. Despite the efforts and the resources invested, in many fields of medicine, progress remains slow and an increasing number of people turn to untested and unreliable treatments. Lack of ethics and of full understanding of the societal role and responsibility of science and scientists (Shrader-Frechette 1994), and poor understanding of how science works, chiefly by the media and the general public, favor the sprouting of "miraculous" solutions. There is increasing impatience for "fast" scientific results, not considering that the inability (or the impossibility) to obtain immediate results does not mean that scientists are inadequate or unable, necessarily. Reality is different, and sufficient time is required to design and execute experiments, to verify results, and to falsify the proposed models and theories. The (apparent) slowness of science contrasts with the inclination of the "customers" (including the general public) who are unwilling to wait, or to postpone the adoption of results that were not peer reviewed or validated or, even worse, failed to pass proper scientific validation. The issue is manifest in the medical and the pharmaceutical fields, but it can − and will − touch other fields of science, including those related to the prediction of natural and environmental hazards and their consequences.

Many geoscientists are unaware of the role, and of the societal and ethical consequences of their work (Shrader-Frechette 1994; Wyss and Peppoloni 2014). This is largely due to a lack of education and specific training, more than the absence of interest. Training in philosophy, and on the ethical problems faced by geoscientists, and scientists in general, should be introduced as part of all university courses. Research institutes and professional organizations should adopt ethical codes. Matteucci et al. (2012) have proposed for geologists an oath similar to the "Hippocratic Oath" that binds medical doctors to a code of ethics. The full content and exact scopes of an ethical code for geoscientists are a matter of discussion (Wyss and Peppoloni 2014), but the design and the acceptance of a shared code of ethics may (1) promote the social responsibility of geologists as scientists and professionals, (2) increase the awareness for the expectations and needs of citizens, decision-makers, and the society, (3) favor the acceptance and use of geoscientific and environmental knowledge and information, and (4) raise awareness for the social mission of geoscientists. Although I have no naïve illusions, I argue that the introduction of a code of ethics, associated with proper deontological training, can contribute to the improved use of geoscientific knowledge, and to an ethical approach to the management of natural hazards and their consequences.

Performance and transparency

In the recent years, in Italy and elsewhere in the World, universities, research organizations, and funding agencies are increasingly using "infometrics" (or "scientometrics") (Tague-Sutcliffe 1992; Hood and Wilson 2001; Bar-Ilan 2008; Harzing 2011) to measure

and rank the performances of individual scientists. I am not against the use of metrics to evaluate performances, including the performances of scientists, groups, and research organizations. However, I maintain that (1) it is illusory to use solely infometrics/sciento-metrics for the evaluation of scientists, and (2) scientists should be told beforehand what are the rules and metrics used to evaluate their performances. The latter has not occurred in Italy.

It is worth asking why these numerical "metrics" were introduced, and are now used (and abused). They were introduced in an attempt to measure and rank "objectively" the performances, first of scientific journals and next of individual scientists and research organizations. But why was this needed? It became necessary (or deemed necessary) when older evaluation and ranking methods based chiefly on the subjective judgment of peers failed to work. I argue that the failure was largely a result of a lack of ethics among the peers, and not an inherent flaw in the evaluation system. Scientists started to judge and rank their peers partially, with a more benevolent eye toward their academic friends, and a more critical eye for their academic opponents. Partiality and unfair judgment resulted in a loss of credibility in the evaluation system. I argue that the attempt to solving the problem of a reduced credibility by introducing "objective" metrics does not solve the problem. Quite the opposite, as it can foster the growth of lobbies and pressure groups that attempt to bend the system to their benefit. Also, no one that knows how research works can argue that a single metrics, or even a collection of metrics, can capture all the multifaceted aspects of the performance of a scientist, or of a research institute or organization.

What we need to judge science and rank scientists are authoritative, experienced, and independent reviewers, and a more transparent evaluation system. Transparency is a key, but it is difficult to implement. A number of international journals, including most of the journals of the EGU are adopting an "open discussion" system that allows reviewers and other peers to comment publicly on papers before they are published in the journals. The system is far from perfection, but it contributes to fair reviews and to the quality of the publications and the peer review system. I maintain that funding organizations should also embrace transparency in their evaluations. The evaluation system adopted by the European Commission does not shine for transparency, and some of the evaluators have never coordinated a European project. One wonders how these referees can properly judge the proposals they are asked to evaluate.

Concluding remarks

Like other natural hazards, landslides are difficult to predict. The difficulty arises from the poor understanding of the phenomena that control landslides, and from the inherent low predictability of landslides, a result of their complexity and chaotic nature (Turcotte et al. 2002). However, low predictability is different from unpredictability (Stein and Stein 2014), and efforts should be made to improve our ability to forecast landslides and their consequences. Low predictability results in large uncertainties in the forecasts, and new efforts are needed to determine and to communicate the uncertainties. I argue that the latter is an ethical responsibility of scientists.

Attempting to predict landslides is a problem of scientific interest and of societal relevance, particularly in Italy where landslides are widespread (Trigila, Iadanza, and Spizzichino 2010) and pose a severe threat (Guzzetti et al. 2007; Salvati et al. 2010). Scientists experimenting landslide forecasting are exposed to issues inherent to the complex interactions between science, politics, and the society. A first problem consists in

deciding who is authoritative (influential, reliable, trustworthy), and who is not (non-influential, unreliable, untrustworthy). The difficulty is in the fact that even an excellent track record is no guarantee of the future performances of a forecaster. A second problem is the increasing subordination of science to politics and decision-making. For multiple reasons, scientists are turning into "advisors" or "consultants" to decision-makers, often without a complete understanding of their societal and ethical responsibility. A third problem lays in the way science is organized, executed, and funded, based on apparently objective criteria and metrics, and considering the scientific endeavor as any other human venture. The pressure to obtain quick results of immediate application has increased the tendency to produce results which are minor, marginal, repetitive, and of limited general interest. A related problem is a tendency to ignore or to hide critical issues and limitations of the results obtained.

I argue that the consequences of these problems are serious, and have not been considered sufficiently. Finding a solution to these problems in not trivial, and requires the collaboration of all the parties involved. Training on ethics in scientific research (Shrader-Frechette 1994; Wyss and Peppoloni 2014), the adoption of research and professional ethical codes (Matteucci et al. 2012), and more transparent evaluation and review systems, may contribute to solve − or at least to face − the problems. Further research on these and related issues is needed.

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Disclosure statement

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