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Antonello Ciccozzi

Forms of truth in the trial against the Commission for Major Risks: Anthropological notes

«I reassured my family, reassured them until a quarter to one that night, the same night I remember the quake, because I was convinced, also on the basis of decisions, conclusions by the Commission for Major Risks on March 31, that there would not be any strong tremor, I don't mean that there would not be any more tremors, because we had felt the tremors since December, but at any rate the magnitude of those tremors wouldn't be so strong as to cause the catastrophe that later took place».
(From testimony at the trial against the Commission for Major Risks)

Introduction

In this article I present a reasoned synthesis and critical-analytical overview of the cultural anthropological consultation I provided as part of an internationally significant legal case. On the basis of this documentary itinerary, I seek to adopt a theoretical-methodological and interpretive perspective in order to highlight two key issues: the first regards the practicability of using an anthropological approach in juridical contexts to provide cultural expertise about the issue of mental causation; the second is a reflection on the problematic relationship between rational knowledge and irrational beliefs that continues to permeate the social uses of scientific knowledge.

From the former perspective, in the following pages I seek to demonstrate that the role of cultural anthropological expertise might be crucial for understanding the legally relevant events that occur in situations where the value-oriented habitat (the semiosphere, the context of common sense and so on) in which actors move represents a variable that is decisive and hence cannot be bypassed using other approaches; as such, it constitutes an element that must be considered in its entirety and through specific forms of technical expertise if we are to acquire the most cogent possible understanding of the object discussed in the courtroom. This not only means that a reading of the cultural anthropological frameworks in which certain events occur might be useful for understanding the meaning of these events, but that this reading is actually essential for such an understanding. This is because,

ultimately, forcloding the cultural dimension from the mechanisms for explaining anthropologically positioned facts leads not simply to a lower or more approximate degree of understanding but to misapprehensions, misunderstandings and mystifications around the meaning of the behaviors juridical devices set out to assess. Indeed, the Western legal world is currently ever more aware of and interested in understanding the cultural-anthropological variables of human behavior. Although presently limited to the front lines of culturally oriented crimes, this interest may expand in the direction of understanding the cultural motivations and factors that can condition and steer phenomena to a lesser or greater degree – and, therefore, the legally relevant links in the chain of mental causation.

As for the second issue mentioned above, the case addressed in this article shows how, when forms of scientific knowledge are put to social use, they run the risk of generating a mystique of truth more or less removed from the empirical-experimental foundations that would grant them the status of actual science. Indeed, in complex, secularized societies the evocation of scientificity has the potential to constitute a system of cultural persuasion that manifests in two closely-interconnected levels of governmentality. The first is aimed at the population and functions through the production of social representations of reality that may appear misleading in that they are based less on empirically-grounded knowledge and more on unfounded beliefs. In the second level, targeting institutions, the recognition or refutation of scientificity acts as a stamp of approval which legitimizes or de-legitimizes the content in question and thereby serves to ensure that hegemonic apparatuses enjoy a monopoly on interpretations of the truth; these apparatuses, having been invested with an aura of sacredness, impose a disciplinary politics aimed at ordering and normalizing the opinions of the populations, the bodies of individuals and social reality in general. In view of this, it could be argued that an anthropology of institutions capable of examining the social uses of expert knowledge may help in understanding the

ways in which an underground folklore of science bolsters and fuels the production of forms of mystique around what constitutes truth. Precisely by virtue of the pseudo-scientific-ness they contain and convey, these forms of mystique come to be dangerously disconnected from the ideal of knowledge from which they derive their authority.

Juridical truth

After three instances of legal proceedings¹, the trial against the Commission for Major Risks (CMR), also known as the “L’Aquila trial”, concluded on November 20, 2015. The Court of Cassation ruling confirmed the Court of Appeal ruling, which established the legal truth that only one of the seven experts² sentenced in the first instance was responsible for having reassured the inhabitants of L’Aquila through a pseudo-scientific diagnosis according to which the crescendo of daily quakes that had frightened the citizens for months was an «earthquake swarm», that is, a «positive release of energy» capable of diffusing an otherwise disastrous earthquake over time; in the end, however, the earthquake did take place a week after this assessment was made, on the night of April 6, 2009, killing more than 300 people. In other words, Italian jurisprudence essentially acknowledged that an erroneous expert prediction that quickly spread as an element of common sense lowered the risk perception of the population and thereby increased the residents’ exposure to the danger of collapses and contributed to causing the earthquake to prove fatal for several people.

I am caught up with this case both personally and professionally because, in addition to having experienced the earthquake firsthand, I was called on by the Prosecution to provide anthropological consultation aimed at illustrating whether and to what extent the information provided by members of the CMR led the population of L’Aquila to decide to stay in their homes the night of the earthquake despite the two strong tremors that had occurred a few hours before the disastrous final one. In the report that I prepared (titled “Reassurance-ism, an anthropology of scientific communication in the L’Aquila earthquake”), I explained how, through a disastrous act of reassurance, a diagnosis of non-hazardousness promulgated by the sphere of expert knowledge altered the “emic risk perception”³ that would otherwise have generally led residents to act with caution in the face of shocks that were felt quite strongly even if they were not powerful enough to bring down buildings (given that this city had already suffered other telluric de-

struction over the centuries, there was an “anthropological culture” of earthquakes⁴ that augmented local people’s immediate impulse to exit their homes). Whereas in other cases many of them had responded to strong tremors by leaving their homes and stayed outside for several hours, on the night of the earthquake many remained in bed because they had incorporated the reassuring diagnosis according to which the substantial prediction was of a non-earthquake; a diagnosis that owed much of its persuasive weight to the scientific authoritativeness of the sphere from which it was issued.

Against the background of a general principle drawn from the anthropology of risk⁵ – that decreasing the (cultural) perception of risk increases (social) exposure to danger when facing an impactful event (natural, in this case) – I formulated my thesis on the basis of a series of anthropological and cultural assumptions⁶. I drew specifically on the theory of social representations developed by Serge Moscovici⁷, employing these interpretive aids as a general theoretical framework to deductively grant comprehensibility to the specific cases outlined in victims’ relatives testimony about the risk communications issued at the level of expert knowledge. In particular, I sought to use Moscovici to point out that, in contemporary societies, common sense (or, we might say, anthropological culture⁸, understood as a protocol for the everyday and generally uncritical decoding of human experience) tends to be produced by the “reified universes” of the scientific world that are directed toward the “consensual” worlds of everyday life through their function of normalizing the disturbing aspects presented by new events; this process of normalization occurs through procedures of “anchoring” (affixing a term or name to new phenomena) and “objectification” (attributing a meaning to that name). I opted to employ this theory because of its heuristic relevance in relation to the case examined here: Moscovici’s theory served to reveal how the act of anchoring the ongoing telluric phenomenon to the term “seismic swarm” and objectifying it with the reassuring meaning of a “positive release of energy” constituted a social representation of reality that was highly persuasive, capable of altering the conduct of people by changing the common sense – that is, the cultural anthropological habitat of meanings people use to interpret the reality around them – in which L’Aquila locals were immersed on a daily basis⁹.

At the end of the first instance trial, by finding all the experts guilty, the Court fully embraced the argument I put forward as an expert consultant. Indeed, the Court used my argument in two different ways to establish the criminal responsibility

of the defendants through a connection of “mental causation”¹⁰. Specifically, Judge Marco Billi found that the causal chain between the conduct of the defendants (the reassuring diagnosis that the situation did not represent a danger) and the destructive event (remaining in their homes despite two strong tremors that preceded the fatal one) both fell under the deductive-nomological or Covering Law model of scientific explanation¹¹ identified in the theory of social representations and correlated with empirical principles and common sense generalizations made on the basis of ideas about the cultural nature of humans and the persuasiveness of science in Western societies¹².

In the second instance trial, the judges instead found only the deputy head of Italian Civil Protection, De Bernardinis, responsible; he was found guilty of having acted of his own volition to reassure the population of L’Aquila before the CMR meeting in an interview in which he described the ongoing telluric event, stating that, according to the scientific community, it was only a release of energy which was not only not dangerous but even beneficial. The other defendants were acquitted on the grounds that they had not participated in any way in formulating either this diagnosis or any other kind of reassuring message. The third and final instance of judgment ended with a confirmation of the Court of Appeal’s ruling.

The legal truth that was established absolved six of the seven defendants on the grounds that, during the meeting, they did not talk about the “energy release” theory, that no other experts had been involved in any way in that particular representation of the situation (and that it could therefore be attributed solely to De Bernardinis, who had outlined it all on his own in an interview before the meeting began). The court reasoned that they had not in any way made reassuring statements concerning the ongoing seismic phenomena and, on the contrary, had only made not-at-all reassuring assessments inspired exclusively by the best science available at the time of the meeting. The concept of “mental causation” was acknowledged, however, and understood as lying at the convergence between consolidated and observable empirical generalizations, on one hand, and the contribution provided by probative evidence and the contingencies of the case on the other.

In making these acquittals, the Italian justice system found that no reassuring function was played by the fact that – as trial documents show – Franco Barberi explicitly asked other members of the CMR to account for the “theory of energy release” during the meeting and none of them refuted it; or that, during a press conference immediately

after the meeting, De Bernardinis stated that «an increase in magnitude is not expected» and none of the three other CMR experts who were present contradicted him; or, again, that over the course of the meeting statements were made such as «this seismic sequence does not herald anything» (Franco Barberi) and «I would rule out that this seismic swarm is preliminary to other events» (Enzo Boschi, after having declared that «we cannot predict earthquakes»)¹³. In this context, in order to locate this series of events within the political frame that gave rise to it we must necessarily recall the shadow cast by the wire-tapped telephone conversation with the head of Civil Protection, Guido Bertolaso, that emerged during the first instance trial. In this conversation, Bertolaso declared that would send «the leading earthquake experts» to L’Aquila to stage a «media operation» in order to «reassure people» – who were alarmed not only by the quakes but also by the forecasts of a local, self-styled scientist – by persuading them it this was «a normal situation, these are phenomena that occur, better 100 tremors measuring four on the Richter scale than silence because 100 shocks serve to release energy, and there will never be a quake, the one that causes damage»¹⁴.

Scientific truth

As for the consultation I provided, the grounds for the second instance ruling later reaffirmed by the Court of Cassation formally rejected the position I put forward; at the same time, however, there was a substantial and unmistakable reaffirmation of the arguments I presented, such as when these same arguments were used to confirm the final guilty verdict against De Bernardinis. The circumstances were thus paradoxical, to say the least. Rising above the annoyance one naturally feels at being obliged to defend one’s scientific respectability, I believe these unusual circumstances deserve to be addressed in detail. Specifically, the Court of Appeal resolved to wholly disregard the deductive-nomological model of scientific explanation put forward by the “theory of social representations”¹⁵ – a theory which was (erroneously) credited to me and depicted as merely the fruit of my own personal experience (271) – on the grounds that it lacked scientific validation by virtue of being deficient in significant criteria of “controllability,” “falsifiability” and “verifiability.” This was in formal agreement with the Attorney General’s office, which publically exposed itself by taking an explicit position in support of the defendants’ innocence and holding only the media responsible for conveying reassuring messages. Indeed, the Attorney General’s office discredited my

expert testimony on the grounds that it was based on a model lacking in a «statistical coefficient determined in quantitative terms» (thereby establishing that such a feature is to be regarded as a *conditio sine qua non* of scientific-ness in the socio-cultural sphere) as well as «lacking any empirical support» (evidently the documents and testimonies were not considered adequate). It furthermore downplayed the “theory of social representations” as an «interpretative category useful for the purposes of an anthropological study, but certainly not a scientific law»¹⁶.

From a logical-theoretical point of view, it should be noted that the work I presented was refuted by avoidance – that is to say, rejected *en bloc* without critically analyzing either the core of the argument or any other element of the theses it entailed – through *argumentum ad hominem* and *argumentum ad verecundiam* rhetorical strategies. While the first focused on delegitimizing the expert testimony I had prepared based on personal experience, the second probably originated from a vague, hasty excommunication pronounced during the hearing by Stefano F. Cappa, an expert consultant for the defense and professor of neuropsychology with a biomedical background. He stated that «I read the expert testimonies with interest, I would say it is a document possessed of impressive narrative impact and, from that point of view, unquestionably to be appreciated; I think [however] that the document lacks references of an experimental and objective type». This excommunication contains a hint of the kind of disciplinary imperialism that manifests in the hierarchical presumption of evaluating anthropological work only on the basis of parameters drawn from the hard sciences. It was subsequently disseminated as if it were a clear repudiation and used as it were a scientific evaluation free of conflicts of interest. Here, I would like to clarify a point that has often been arbitrarily misunderstood: it was neither my job nor a necessary element of my work to epidemiologically quantify the relative incidence of a belief in the harmlessness of the ongoing seismic swarm among the population of L'Aquila as a whole. What I was tasked with doing was making comprehensible the cultural anthropological and psycho-social factors on the basis of which certain individuals developed this belief.

Looking back at the historical (and enduring, albeit in different forms) epistemological tension between nomothetic and idiographic approaches, between naturalism and historicism, between sciences based on explanation (according to general schemes) and sciences based on understanding (beginning from specific phenomena), it seems that certain positions evoke a proto-positivistic reduc-

tionism according to which the human sciences ought to be completely absorbed into the natural sciences. This prejudice has come back into style nowadays thanks to the growing hegemony of the biomedical field at the expense of the psychological field, a supremacy that can be seen in the way neurosciences are spreading in the field of behavioral sciences¹⁷. This prejudice affects the humanistic sphere as well, and can be seen in the confusion between sociometric-type quantitative methods and qualitative methods such as the cultural anthropological approach that guided my consultation¹⁸. According to these precepts, any science that does not fall within laws that can be quantified in physical-mathematical terms and objectified in a strictly empirical sense should be banned from the courtroom. On the other hand, jurisprudence has long employed expert consultancy regarding connections of mental causation which are not, of course, empirically verifiable¹⁹.

All of this gives us cause to reflect on the way that the paradigm of truth ascertainment flaunted by the “hard sciences” sometimes rests not on grandiose empirical-experimental foundations but on «epistemological rituals»²⁰ that stage and spectacularize specific manifestations of authority. These manifestations are often conditioned by the enduring legacy of positivist approaches against the lingering background of the myth of absolute objectifiability that harkens back to the Weberian illusion that we can separate the subject from the object in the field of culture as if the cultural world were a chemistry laboratory²¹. With this in mind, I wonder if or to what extent the method used by the Court of Appeal to strip my work of any character of scientific-ness was itself objective, disinterested or scientific. I wonder, indeed, if this was instead a case of the kind of methodological ethnocentrism and disciplinary imperialism – fostered by the high stakes in the courtroom – that allows some sciences to assert universalistic positions over other sciences based on an implicit principle of superiority that leads them to imagine their categories of evaluation apply in all other fields as well.

As mentioned above, this case was quite singular. First, because the theory of social representations is not the fruit of my own personal experiences; indeed, all I actually did was apply it by matching its postulates with the empirical variables of the specific case of L'Aquila. In reality it was developed by the illustrious scholar Serge Moscovici (who is never indicated in the second instance and Cassation rulings) and has been a pillar of social psychology for twenty years now, appearing in practically all the relevant manuals both within and beyond the discipline. Second and even more surprising is the

fact that, after having de-legitimized my work, the Court of Appeal went on to use precisely the arguments I myself had presents based on Moscovici's theory to establish De Bernardinis' guilt. It did so, moreover, by drawing on these arguments just as they had been formulated by the first instance judge, who quoted my testimony word for word.

Let us examine this in more detail. The written grounds of the second instance ruling (270, 272) state that, regardless of the «social representations» model, the following assumptions of the first instance ruling were reaffirmed in establishing a causal relationship: «a) the 'credibility' and 'authoritativeness' of a message are proportionate to the source from which it comes; b) in modern Western societies scientific experts enjoy particular authority». These arguments, both the exclusion of the social representations model and the recognition of persuasiveness of scientific authority, also reappeared in the grounds of the Court of Cassation ruling. Indeed, these grounds state that the Court of Appeal was correct in the way it framed the ascertainment of causality (87), recognizing,

rightly so, that, within the horizon of experiential knowledge, there is a principle that tends to grant significant (in that it possesses significant credibility and authority) psychologically conditioning value to messages publicly issued by institutional authorities when they are based on the premise of validation provided by scientific knowledge, and the resulting influential impact can be observed in the behavior of recipients (89).

The point is that, in the part of the grounds of the first instance judgment that was upheld on appeal and confirmed by the Court of Cassation, Judge Marco Billi explicitly drew on exactly my arguments in his effort to clarify these elements (667-671). He stated:

regarding this point, professor Ciccozzi noted that “studies on persuasion have shown that the persuasive value of any allegation is directly proportional to the authority that the recipient attributed to the issuer” [regarding a)]; professor Ciccozzi demonstrated that scientific institutional information has a special quality, asserting, in a completely reasonable and widely accepted argument that, in Western societies, institutional communication from scientific authorities is the one the masses consider to be “the highest expression of authority” and which therefore has “a potential for maximum persuasiveness, which is expressed in the ability of scientific thought to result in social representations that limit collective

responses” [regarding b)].

It is clear that these arguments are essentially the core of the theoretical conclusions I drew from Serge Moscovici's theory of social representations. So, papers in hand, in the second instance and Court of Cassation rulings, the position I presented as a consultant – which served as the foundation for convicting all the defendants in the first instance trial – was formally rejected through partial and erroneous judgments; at the same time, however, it was “covertly” used as a key tool for confirming the conviction of one of the defendants. Through this sentence, moreover, it was also used to assert the rationale behind the trial as a whole (the existence of a causal link between an expert opinion and life-threatening behaviors).

On methodological grounds, I was accused of having published an article on local online media platforms²² before being officially brought in as a consultant that hinted at the arguments I then presented in the courtroom (it was this article, among other things, that triggered the process that led the PM Fabio Picuti to entrust me with the position of technical adviser in the legal proceedings). In this article, I clarified a misunderstanding that was spreading throughout the city, the mistaken tendency to “anchor” the fact of being reassured by the diagnosis that the seismic swarm going on was non-hazardous in the expression “failure to warn”. The core of the thesis I presented was the following: if “failure to warn” means “not predicting a disastrous event,” then “predicting that a disastrous event will not occur” means providing a reassurance that proves to be disastrous when the event in question does actually occur. Since not providing information is quite different from providing incorrect information, not predicting an earthquake (failure to warn) is quite different from predicting a non-earthquake (disastrous reassurance). I hypothesized that this misunderstanding might arise from a semiotic detail: our lexical repertoire does offer a true opposite of the term “alarmism,” a lemma composed of the noun “alarm” (meaning to make [someone] aware of a danger) and the suffix “ism” (which in this case indicates a doctrinal nature, an unfounded fixation often having to do with collective behavior) which together indicate “a tendency to worry in the absence of compelling reasons to do so”. There is no word that means “unfounded reporting of normalcy”: terms like “calming” or “reassurance” do not have the connotations of being unfounded²³. The signifier “reassurance-ism” might therefore be useful in understanding that, in L'Aquila, it was not simply that a lack of alarmism led to a failure to warn but that, going so far as to engage in reassurance-ism, institutional representa-

tives produced a disastrous reassurance whose persuasiveness stemmed from the manifest scientific authority of its source.

While the PM viewed my anthropological interest in this matter as a sign that I would make a good consultant, the Courts of Appeal (271) and Cassation (88) ruled, in line with the defense, that I had selected testimony by individuals who specifically described being persuaded by institutional assurances in order to prove a preconceived thesis. And yet, should a preconceived thesis be considered *false a priori*? In this particular case, I would rather say that, while witnesses who testified to feeling reassured gave a concrete shape (through practices and social action) to theories describing the persuasive ability of authority, on the other hand those theories about the persuasiveness of scientific authority grant a rational, logical content to the personal accounts of those who testified to having incorporated a reassuring diagnosis with disastrous results. It is true, in some respects my thesis was “preconceived”, but it was preconceived on specific theoretical and empirical foundations. This does not detract from the fact that, as I will argue below with the concept of framing, selecting witnesses to include those who were effectively persuaded is not only appropriate but necessary to demonstrate my point: not that this process was pandemic but rather that it took place to a significant degree. The thesis was preconceived in relation to the trial and my being called as a consultant by virtue of the simple fact that I had already been working on this issue. It was thus developed before the events in question, namely the earthquake and risk communication carried out during the catastrophe’s incubation period. And, while on one hand courtroom testimony confirmed this thesis, on the other hand and through a circular effect, the theoretical apparatus I presented made the testimonies attesting to persuasion intelligible in light of a broader framework of understanding. If we apodictically assume that a “preconceived” argument is false, we lose sight of this circular movement between theoretical and empirical dimensions that I have mentioned here for a specific purpose. We lose sight of the fact that my goals as a consultant were not to prove my thesis but to use my research to establish the sense behind certain behaviors (and, it bears repeating, it was not entirely “my” theory but rather a theory inferred from a selection of well-respected theoretical sources). We lose sight of the fact that I did not set out to show that all the citizens had felt reassured (in which case I would indeed have had to consider all the witness statements): rather, my task was to demonstrate that some (some, not all) L’Aquila citizens would have survived had they not been per-

sueded that no destructive earthquake was to occur that night.

Unsurprisingly, I also came under methodological attack for having prepared my report on the basis of a selection of testimony that included both reassuring elements in the experts’ statements and statements from victims’ relatives that confirmed their having assimilated these reassurances in their households, and this fact was held up as grounds for rejecting my arguments. I described my methods from the beginning of my consultation, making it clear that, as an expert witness for the prosecution, my goal was to highlight both the fact that the CMR being convened in L’Aquila produced a specific framework of meaning according to which the overall communicative performance generated reassuring content, and the effect this content had on the population. All the while specifying that, unlike effective scientific communication characterized by unity, consistency, clarity and an unequivocal message, this case involved a diagnosis that was generally cacophonous, disorganized, confused and contradictory. Taken as a whole, this communication simultaneously stated that earthquakes cannot be predicted and that a non-earthquake was predicted. It stated that it was impossible to exclude the possibility of an earthquake but that the experts’ diagnosis foresaw no increase in magnitude or even a positive discharge of energy; it underlined the dangerousness of the area even while conjecturing that the circumstances of the moment presented no particular danger²⁴.

This brings us to the issue of framing strategies²⁵: the selective construction of the context of meaning, that is, the framework that provides the inferential conditions for formulating a judgment. Since there is no narration without selection, however, the question revolves around a specific point: to ensure that science does not degenerate into ideology, argumentative correctness requires an expository selection that does not accidentally become a form of «aberrant decoding». Such aberrations in decoding can even take the form of «semiotic guerrilla warfare» involving a misleading manipulation of the issuer’s message and strategies to delegitimize the issuer²⁶. In this case as well, I was amazed by the fact that, in terms of framing, the same point that had been surreptitiously challenged in relation to my selection methods was then used, just as surreptitiously, to delegitimize my work. It is one thing to select elements of guilt from a series of events with the goal of establishing legal responsibility, it is quite another to claim to disprove a thesis by carrying out a process of selection geared at attacking the person presenting the thesis and his personal experiences. Indeed, in this case the defense raised

questionable objections in terms of methodology all the while avoiding the theoretical core of my argument only to then go on using this same thesis “covertly”. Therefore, considering that my goal was to show that the expert communications had given rise to some reassuring content (not that all its content was reassuring) and that some L’Aquila residents had incorporated that content (not that all of them had incorporated it)²⁷, it seems to me that the accusation of cherry picking – eliminating any significant counterevidence in order to support a specific thesis – might more appropriately be directed to the arguments of the defense.

At this point it makes sense to say a few words about the use of an *argumentum ad hominem* rhetorical strategy to delegitimize my expert testimony. According to the methodology that drove the delegitimization of my work, none of the statements in the thesis I presented were subjected to rigorous critical analysis²⁸, and so the grounds for the Court of Appeal (271) and Court of Cassation rulings (88) dwelt on the details of my personal experience, neglecting and obscuring the work I had conducted on the basis of these experiences. I specified in the courtroom that I, like most of my fellow citizens, had been influenced by those assurances, and, like most of my fellow citizens, I survived together with my family only because the house where we lived, despite having suffered serious damage, stopped just short of collapsing altogether. Therefore the fact that I had personally survived the earthquake was judged to represent an element capable of divesting my work of any possible credibility. It was arbitrarily assumed that being embedded (having experienced the event being analyzed first-hand) necessarily generates bias, an emotional conditioning that leads the observer away from the truth of the facts. The rulings refused to consider that personal involvement does not always or only generate distortions in one’s perspective and that it might instead be a harbinger of new possibilities for understanding that would be difficult to access without having witnessed forms of truth revealed through direct experience²⁹ (which, ultimately, is what grants the ethnographic method its scientific character)³⁰.

Once again drawing a few lines from the article I posted on media platforms, the defense also interpreted my observation that more and more citizens felt dissatisfied with the institutions involved in emergency management as a personal expression of «suspicion and negative opinions about the defendants, almost implying that their conduct had been aimed at benefitting from the future, predictable destructive quake» (271). It is true, I never made any secret of my being engaged in a movement of

grassroots active citizenship mobilized in response to a governmental system of post-earthquake management that took the form of a biopolitics of emergency fueled by the framework of disaster economics in which, among other things, three members of the CMR were immediately tasked with handling enormous amounts of money³¹. At any rate it should be noted that, before being called as a consultant, I addressed these issues – which are not relevant to the legal proceedings but are significant for an anthropological analysis of emergency institutions in the framework of the “shock economy” – in an academic setting and in a manner much more complex than the “suspicion of suspicion” scenario through which my detractors sought to simplify the issue³². Even admitting that I was emotionally involved in the events, I would ask: is this bias, be it real or alleged, really enough to subsume and discredit in its entirety the analytical content of my work without even taking it into consideration, or is it simply a rhetorical device? Is this a case of rebuttal or excommunication? Did this refutation hinge on critical-analytical approaches or gossip? Furthermore, in keeping with the prospective I have just identified, I was accused of having «expressly excluded the possibility that anthropological science be required to submit the theses it put forward to any kind of verification» (271). In the absence of any references whatsoever that would substantiate this insinuation, I do not know what to respond. I do realize that some trials end up becoming wars between expert opinions in which fairness and professional integrity are often not the most effective weapon, so I imagine that this alleged position was the fruit of some objection I raised about qualitative parameters being used to measure quantitative approaches, as outlined above.

Finally, I find it significant that this selective search for external details to be used in assessing the relative scientificness of my work somehow missed the fact that this same work was reviewed by the National Committee for Scientific Qualification, part of the Ministry of Education, University and Research, in 2012 (two years before the Court of Appeal judgment) and earned a unanimously positive response, with the following reviews:

the monograph “Rassicurazione” is particularly noteworthy in terms of quality as well as impact, also outside the academy [...] an interesting and, in some respects, significant work that moves with a certain degree of self-confidence and expository efficacy between the anthropology of disasters, that of science and the media and that of institutions [...] “Rassicurazione” is a commendable document in the anthropological

analysis of institutional contexts [...] the candidate skillfully presents anthropological analyses of debates regarding responsibilities in cases of disasters and phenomena in which institutions and modern mass communications systems play an important role.

In conclusion, I notice the following paradox: using vague procedures of biomedical excommunication cloaked in the authority of scientific to judge my thesis (in which I cite authoritative theories about the persuasiveness of science to explain how this knowledge is often considered a form of supreme and unchallengeable truth in Western culture) as entirely lacking in scientific dignity – even going so far as to launch personal attacks in tabloid-worthy tones – is tantamount to staging an evocative ritual about the charisma of science as the sole source of indisputable truth. It therefore substantiates the thesis I proposed³³. More generally, implicitly assigning an aura of sacredness to science prevents us from considering the extent to which science (especially in its social uses) is caught up with politics. Above all, it prevents us from seeing how much it depends on its own anthropological culture made of rituals and beliefs and permeated by a kind of institutional tribalism that fosters corporatist tendencies to autopoietic maintenance, that is, the self-preservation of its own structures. This ought to help us understand how invoking science in the field of human events sometimes involves a mystique of absolute truth rather than providing effective verification based on logical-experimental principles. This mystique gives rise to a rhetorical strategy aimed at granting plausibility to discourse and opinions in which authoritarianism is camouflaged as authoritativeness. When courts set out to judge scientific validity across the board, then, we have to wonder if, given their social and political weight, these seals of scientificity that the legal system confers on other disciplines might not actually represent a parallel system of scientific evaluation, a system that ends up establishing procedures of theoretical-methodological legitimacy and hierarchies of prestige whose reliability is questionable.

Cultural truth

Examining the social processes of truth construction in this case, we are led to ask whether, to what extent and for how long the legal truth – the only one established in a definitive manner – will influence scientific truth and what implications this might have for the formation of cultural and, consequently, historical truth³⁴. It would be useful, for

example, to clarify the value of the axiom according to which an ongoing seismic swarm absolutely does not constitute a precursor to a disastrous earthquake, in view of the fact that this axiom was legally elevated to the status of science by the juridical judgment stating that the meeting participants (a meeting which, according to the judges, did not count as official³⁵) «did not formulate any assessment that might be said to be scientifically incorrect or unduly reassuring» (183). The question is interesting given that it appears to run counter to the datum – recognized by seismologists all over the world for at least twenty five years now – that a seismic swarm increases the probability of a peak event by 100 to 1000 times³⁶. As we cannot rule out the possibility that this represents an effort to take shelter in a paradigm (in the Kuhnian sense³⁷), I imagine that it will be up to the international seismological community to establish over the next few years whether this assertion is to be considered science or a corporatist way of twisting science to bolster these experts' statements through legal validation. In the latter case, Italian seismology would run the risk of degenerating into an idiolect (if only in order to support or, conversely, hide, the curious theorem according to which there is no chance that an ongoing seismic swarm is the precursor to a destructive earthquake).

Another significant point is that the relationship between the Italian and international seismological communities has been impacted by moves to misrepresent the grounds of the trial. Indeed, distorted depictions of the court case have repeatedly framed it as being about a “failure to warn” and absurd and shameful accusation of “not having predicted the earthquake”. A communicative arc can be observed spanning from the beginning of the legal proceedings to the end. It began with an international appeal signed by 5,000 people, all high-ranking as scientists, convinced by a skewed account of the trial grounds claiming that «the heart of the accusation is that a state of alarm was not issued». The Attorney General's office then took a position, cautioning that «earthquakes are not predictable»³⁸. The arc can be traced through various scientific spokespeople³⁹ to comprise the statement by Enzo Boschi describing the acquittal as «a memorable landmark regarding the fact that no one in the world can predict an earthquake»⁴⁰. Does it really make sense for the “world” to go on being fueled by this mistaken understanding of the grounds of the trial?

The hoopla about a “trial against science” even compared it to the trial against Galileo, thereby hinting at the mystique of its origins by evoking the historical figure of one of its founding heroes. On

the basis of this *réclame*, the indictment of seven experts turned into a story about all science through a metonymic process driven by the poetics of absolute authority, which is capable of being highly persuasive at the level of common sense (to put it another way, in our secularized cultural sphere no one would react to a priest accused of pedophilia by talking about an attack on religion or God). In view of this transfiguration from entities to ideals, from individuals to forms of knowledge, an article written by two administrators at the INGV struck me not so much for its content as for its title: “The arguments of science in the L’Aquila trial”. In this publication the authors, complaining of «accusations addressed to the entire scientific community», excommunicate my work on the grounds that the «scientific community» had questioned its lack of «rigor in the collection and selection of data». On the basis of this data, they claim, I argued that people received the reassuring message from the experts and not, as they assert, exclusively through distorted representations generated by the media coverage⁴¹. Through this use of rhetorical metonymy, the authors of the article explicitly raise their arguments to the level of the “arguments of science”; they cast themselves as prophets embodying and giving voice to divinity and depict the seven defendants as the entire scientific community. In the face of this rhetoric, I feel obliged to note one more time that the legal truth has confirmed the validity of my interpretation, albeit while finding only a single expert responsible (a fact which does not affect the substance of the arguments I presented).

This obstinacy in misrepresenting the case is surprising because at the indictment, during the trial and in the grounds of the sentence of first instance it was repeated *ad nauseam* that no one was accused of not having predicted the earthquake⁴²; rather, the accusation was of having made an «approximate, generic and ineffective» assessment of seismic risk «with regard to [their] duties of prediction and prevention» by providing «incomplete, incorrect and contradictory information» that ended up reassuring the population and convincing them to stay at home despite the severe tremors that preceded the deadly quake. The first instance ruling does not contest the fact of not sounding the alarm (predicting the earthquake) but, on the other hand, it goes beyond «generic reassurance-ism»: it states that the conduct of the defendants in terms of risk prediction was enough to convict them, specifically their not having alerted people about the danger of the situation and going so far as to reassure, that is, to predict an absence of risk (363-365). The Court of Appeal, in contrast, found this distinction between «prediction of risk» and «pre-

diction of the actual earthquake» to be «artificial» (203). In my opinion this is one of the most questionable elements of the second instance ruling, the move to equate the risk of an occurrence with the occurrence itself. This framing blurs the difference between the (scientific) possibility of making probabilistic forecasts and the (pseudoscientific) pretension of making deterministic predictions. It entails the brazen move – as legally authoritarian as it is scientifically unfounded – of razing the entire scientific domain of risk analysis to the ground⁴³.

Moreover, the stereotype of a trial for “not having predicted the earthquake” is reminiscent of an allegation that has been leveled against the citizens of L’Aquila on various occasions: they have been accused of trying to make blameless scientists into scapegoats in order to shift their own guilt for having structurally unstable buildings. Indeed, there is a “scientific” saying that represents a sort of totemic motto in the seismological community: “earthquakes don’t kill, buildings do”. Now, if we move beyond its glib and propagandistic uses, the concept of “scapegoat” indicates (as discussed in the second hearing) a sacrificial victim who is surreptitiously loaded with all the blame for a disastrous event and then killed to purify the community⁴⁴. With this in mind, it should be noted that, while the prosecution never ascribed all the blame for the deaths to an expert diagnosis of non-hazardousness (people died *in part* because they had been reassured), a recurring rationale for discrediting the trial was that it assigned all responsibility for the deaths to the structural vulnerability of the buildings (people were dead *solely* because the houses collapsed). It turns out, however, that L’Aquila’s vulnerability had been overstated (in fact, the incidence of death was sporadic that night because, although almost all the buildings in L’Aquila suffered serious damage, they ended up saving the people who remained inside them, often on the basis of reassuring expert assessment) and was used as a scapegoat to take on all the blame for the deaths.

In keeping with the prosecution’s case, the first instance ruling based its rationale on the basic principle of disaster studies, namely that a disaster results from the intersection of an impactful agent and factors of vulnerability and exposure ($D=I \times V \times E$). In this case the disaster was the result of earthquake as agent combining with the factors of vulnerability represented by the buildings’ seismic resistance and factors of exposure comprising the reasons that led people to stay inside their homes even after two strong tremors. Just as some experts failed to understand that, in reality, “it is earthquakes, houses and being inside them that kills”⁴⁵, so they missed the fact that decreasing people’s per-

ceptions of risk increases their exposure to danger. The point is that, on the night of the earthquake, the official reassurance may have played a role akin to that of a structure in violation of anti-seismic building codes in exacerbating the earthquake's disastrous impact. Of course, generally speaking, taking measures to reduce the seismic vulnerability of housing is always the first priority of prevention in any high seismic risk area, but this should not justify an act of technocentric reductionism that leads the sciences guiding risk prevention policies to overlook factors of exposure when conducting their analyses.

The last and perhaps most important question in terms of constructing historical truth about this case concerns the confluence of two forms of responsibility, scientific and moral. Did the experts always express themselves in a scientifically correct way, never unduly reassuring the population, as the legal truth would indicate? Was excluding a possible connection between seismic swarms and earthquakes, stating that there is no reason to expect an increase in magnitude and not refuting the "energy release" theory, scientifically correct and therefore morally irreproachable? The question of whether certain pronouncements may be regarded as autonomous units of meaning, if and to what extent they should be contextualized in a broader communicative frame, and whether or not they might be perceived as having a reassuring meaning and with what social consequences are not the purview of seismology. Rather, they are a phenomenon to be investigated using the tools of semiotics and the anthropology of risk, ranging into semantic, pragmatic and cultural fields. This is because the natural sciences are imbricated in language and, therefore, in society: they engage with acts of speech that have variously collective consequences. It should be clear, therefore, that experts might talk "nonsense" not only when basing their communication on mistaken theories, but also when communicating on the basis of accurate theories yet in an inappropriate way. The content, theories and models of seismology are the rightful concern of seismologists, but the use and social consequences of these theories cannot concern seismologists alone.

In terms of this point, as we move outside the immediate orbit of a strictly corporatist consensus it seems clear that we cannot be satisfied by the legal truth. We can look, therefore, to the meaning of some of the many diagnostic utterances, confusingly made that day, to find the thematic crux of moral responsibility. Reducing the issue to its essence in a logical-philosophical sense and in view of the fact that «an agent is responsible if he does not try to interrupt the inertia of a course

of events»⁴⁶, I would ask: could the experts have refuted the energy release thesis? Could they have formulated some of their statements in a different way? As a matter of fact, they could have repudiated the "energy release" thesis when it was presented at the meeting, but they did not⁴⁷. It would have been sufficient to say: "energy release? That's ridiculous!". And indeed this affirmation was made after the fact, during the first instance trial, when they declared before the judge that they never heard this absurdity uttered at the time (despite its being recorded in the minutes) or even that they had interpreted it as a joke (139-141, 259). As a matter of fact, it would have been sufficient to correct De Bernardinis when he declared publicly and in front of three other members of the CMR that «an increase in magnitude is not expected». Or, if Boschi had added a negation to his conclusions, the message would have been that "I would DO NOT rule out the possibility that this seismic swarm is preliminary to other events", as opposed to his actual statement, «I would rule out that this seismic swarm is preliminary to other events»⁴⁸. At this point, keeping in mind that a science that seeks to affirm both a proposition and its negation at the same time is pseudoscience, which of these statements – between seismology and semiotics⁴⁹, and in the careful perspective of the anthropology of risk – is actually correct?

Conclusions

Going beyond this specific case, the question remains as to what function anthropological and cultural knowledge might play in the field of legal consultation in terms of detecting cultural variables in phenomena of mental causation, under what circumstances and to what extent. To play such a role, anthropologists would need to achieve two forms of recognition: they would need to find ways to be formally recognized in an institutional role by "other" forms of expert knowledge, which implies the preliminary move to recognize this possibility in "ourselves".

Pursuing this end would also involve an epistemological commitment the nature of which I can only briefly mention here, a move to address the outcomes and excesses of a decades-long and well-established historical-methodological approach based on a radical rejection of the culturalist paradigm. Although it was based on a laudable attempt to move away from a series of deterministic twentieth-century postulates that often proved to be harbingers of social stereotypes more than scientific knowledge, the fact that this rejection was

carried so far has often led contemporary cultural anthropology to reject, scorn and be intimidated by the prospect of investigating and understanding the connections between individual behavior and anthropological culture. This position is closely caught up anthropology's embrace of an often radically antiessentialist disciplinary aesthetic which, yielding to the temptation of absolute indeterminism, has ended up crippling the heuristic potential of cultural anthropology. Although I would never suggest a return to the positivist temptation to essentialize local cultures or interpret the relationship between cultures and individuals in a deterministic way, we should remember that the link between people, cultures and places does exist in some way, albeit in ways that are different than those conceptualized around the second half of the 1900s. The relationship exists in nuanced forms: it does not follow the implicit Aristotelian logical principles of identity, non-contradiction and excluded middle; rather, it follows causal trajectories and procedures of objectification, better described using fuzzy logic⁵⁰, that invoke a vibrant intertwining of relations and entities, of bodies and worldviews. Here, in a constitutive dimension of fluidity and in the spaces that open up between probabilism and fuzziness, we might have a chance to grasp the meanings of social action.

Notes

¹ In addition to the initial lower court, the Italian legal system involves two appeals courts: a lower court in which cases focus on the merits of the case (Court of Appeal) and a supreme court in which cases focus on a point of law (Court of Cassation).

² The defendants were (accompanied, in parenthesis, by the position each held at the time of the events): Enzo Boschi (president of INGV - Istituto Nazionale di Geofisica e Vulcanologia, National Institute of Geophysics and Volcanology), Franco Barberi (deputy chairman of the CMR), Bernardo De Bernardinis (deputy head of the technical department of the Civil Protection Department), Giulio Selvaggi (director of the National Earthquake Center), Gian Michele Calvi (director of Eucentre), Claudio Eva (physics professor at the University of Genoa) and Mauro Dolce (director of the Seismic Risk Office at the Civil Protection Department).

³ The expression "emic risk perception" refers to local conceptions of risk in a given culture (Ligi 2009).

⁴ The Aquilan historian Alessandro Clementi noted that the devastating earthquakes that struck the city over the centuries (in 1315, 1349, 1456, 1461, 1462, 1498, 1646 and 1703 respectively) have «produced, at the level of collective consciousness, an earthquake 'culture'» (Clementi 2009: 153).

⁵ Thanks to a socio-anthropological approach to risk analysis, an awareness has been spreading for several decades now that disasters result from the correlation between natural factors and human, social and cultural factors (see Quarantelli 1978; Douglas, Wildavsky 1982; Oliver-Smith 1986). This basic premise gives rise to a series of combinatorial formulas that set out from different starting points to converge in addressing risk by focusing on these variables or, more specifically, on the different weight these factors take on in each different disastrous circumstance.

⁶ I employed a number of general theoretical premises about the cultural nature of humans to clarify the link between social communication and individual behavior (Ciccozzi 2013: 79-83), drawing in particular on the semiotic definition of culture formulated by Clifford Geertz (Geertz 1973). This was intended to show the Court that humans are immersed in a habitat of meanings which – at various, more or less localized levels – serves to pre-codify the way we experience the world, influencing our perceptions, decisions and actions (this was intended to refute a naive model of free will presented by the defense in which individual choice was depicted as wholly independent of social frameworks and the individual as completely impervious to cultural conditioning).

⁷ See Moscovici 1984.

⁸ It should be clarified that Moscovici engages the theoretical field of social psychology and this does not explicitly speak of anthropological culture, but his concept of common sense refers to what is understood as anthropological culture in complex societies (Herzfeld 2001). It goes without saying that, since these disciplines share several fundamental aspects in terms of their object of study, the boundaries between cultural anthropology and social psychology often blur, fuzzy and overlap. Well-reasoned interdisciplinary crossings useful for understanding the phenomena under investigation can therefore be epistemologically stimulating and heuristically profitable.

⁹ The concepts of "anchoring", "objectification", "reified universe", "consensual universe" and the "normalization" of disturbing elements are borrowed directly from the theory of social representations (see Moscovici 1984; Jodelet 1991; Grande 2005). In a nutshell, Moscovici's theory might be said to show that, in complex

societies, common sense (and thus anthropological culture) is permeated to a large extent by the runoff of scientific knowledge which flows from the reified universes of expert knowledge into the consensual universes of the world of everyday life.

¹⁰ In the Italian legal field *causalità psichica*, mental causation, comes into play when there is a nexus or connection in the form of an etiologic link of conditioning between the communicative conduct of those issuing the communication and the behavior, active or by omission, of those receiving the communication (see Brusco 2012). As recently noted, the Court of Cassation ruling «has, among other things, affirmed the configurability of so-called ‘mental causation’ even in the case of crimes of negligence, a causality to be reconstructed on the basis of established and maximum generalizations drawn from experience, which must necessarily be followed by rigorous and timely critical feedback from probative evidence and the contingencies of the individual case» (<http://www.giurisprudenzapenale.com/2016/04/01/terremoto-laquila-la-sentenza-della-cassazione-sulla/>). Clarifying that in this case the generalizations drawn from experience concern forms of common sense collectively consolidated through social representations that spread through local anthropological culture, it can be argued that the L’Aquila case shows how mental causation can arise from cultural anthropological variables. It is also necessary to make a clarification about the reason why in this article I have translated the Italian legal concept of “causalità psichica” as “mental causation”: in relation to this recent field of legal reflection, it should be noted that the choice to use the adjective “psychic” instead of “mental” in the Italian context is perhaps not entirely appropriate given that – in addition to straying from an already substantial scientific literature on mental causality – the term “psychic” refers more to the field of personality than that of consciousness.

¹¹ Given that, in the field of jurisprudence, a defendant can only be considered guilty of a crime if and only if a certain behavior (active or by omission) is causally related to an adverse or dangerous outcome, in the contemporary criminal law (deterministic or probabilistic) deductive-nomological laws are used to establish the existence of this causal link. These laws are derived from scientific theories which are considered relevant for the task of proving a regular succession of antecedents and consequences. In this sense, the criterion of being subsumed under laws of science (which occurs by combining a scientific generalization based on abstract principles with the concrete elements of the particular court case) is a tool that can be used to identify the causal links in solving doubtful cases, in order to formulate rulings about criminal responsibility (See Stella 1975; Palazzo 2005).

¹² In relation to this issue, during the trial the lawyer Petrelli invoked Popper to challenge me regarding the idea of the authority and persuasiveness of science; he argued that, today, the prevailing view of science among educated people is characterized by skepticism. I do not doubt that a segment of the well-educated Western population, having read Popper, Kuhn, Feyerabend or Latour, begin from the scientific truth asserted by these authors to view science in terms of skepticism, anti-dogmatism, uncertainty, fallacy and so on. The point is that the epidemiology of such beliefs is far from pandemic; conversely, especially for a large part of the population with an average level of education, the word of science is, often rightly, considered the hegemonic source of truth. To cite an example: there are many more people who – almost always to their benefit – uncritically follow the doctor’s directions than who advance more or less sensible objections (and, in contrast to those who have read and understood Popper, there are people who uncritically associate the whole of official science to various plots and rant about curing cancer by drinking carbonated lemonade). I did not deny the existence of forms of skepticism, nor did I resort to claiming that all the inhabitants of L’Aquila were convinced by a reassuring diagnosis presented as scientific. What I did was illustrate, on the basis of Moscovici, the process of cultural conditioning that a part of the population had experienced as a result of persuasion of experts who presented themselves as authoritative scientists; I sought to highlight the anthropological and cultural consequences of a situation in which a pseudo-scientific diagnosis of non-hazardousness appeared to be cloaked in the aura of the “word of science” (Ciccozzi 2013; Ciccozzi 2014).

¹³ It seems obvious that these specific claims have been proven inaccurate by the “reality test” (namely the earthquake that devastated L’Aquila a week after the meeting). Moreover, as I will argue at the end of this article, clarifying the meaning and social consequences of certain lexical expressions is a task not for seismology but for semiotics and the anthropology of risk. I might also be useful to keep in mind that, a few months after the L’Aquila earthquake Boschi, in disagreeing with Bertolaso, released the following statement: «the idea that I could ever have excluded [the possibility of] severe shocks in Abruzzo at any moment during my professional life is simply absurd» (from the letter written by Boschi to Bertolaso, dated September 16, 2009, published in the newspaper *L’Espresso* in December of 2009).

¹⁴ Bertolaso has denied responsibility for making this unequivocally reassuring diagnosis, testifying in the first instance trial that he heard it «from the scientific community». On September 30, 2016, the trial commonly referred to as “Major Risks, the encore” with Bertolaso as defendant ended in an acquittal. This precluded the possibility of clarifying the details and significance of

this “buck passing” regarding what agency (the INGV or Civil Protection) had authored this pseudoscientific diagnosis of “energy release”. Already beginning with the Court of Cassation ruling in the “L’Aquila trial”, the justice system had definitively determined that the experts attending the meeting were not aware of what Bertolaso intended to do. Lastly, I would point out that this diagnosis overlaps to a large extent with the one media outlets attributed to Concetta Nostro, head researcher at the INGV, in an article in the local newspaper *Il Centro*. In this article, published over a month before the meeting, the theory of “energy release” made its first appearance in L’Aquila. According to the interview, «a [seismic] swarm, no matter which one or how long it lasts, is never and I repeat never a precursor to large-scale seismic events [...], better many small movements than one big and abrupt one that results in considerable damage, even destruction and death. In a certain sense these sequences that last so long should reassure people because it means that the underground energy is being released spread out over time». In the hearing, Nostro distanced herself from these statements (asserting that the concept of “energy release” is a «thesis widely agreed-upon in the scientific world» but only in reference to the geologic faults in California which, «however, are characterized by therefore having small earthquakes and no large ones»). Nonetheless, according to her testimony, although at the time she did read the article right after it was published, she did not move in any way to retract such a reckless and pseudoscientific diagnoses disseminated dangerously in the public sphere: she expressed dissatisfaction with the journalist through an email telling him that she would have been more specific if she had known it would be published, but she did not make any statement clarifying that she did not subscribe to the theory of “energy release”.

¹⁵ The Court of Appeal fully affirmed the argument presented by the defense lawyer Alessandra Stefano who, criticizing the judge in the first instance trial for having failed to take into account the neuropsychological opinions presented by the defense, asserted that «the so-called model of social representations» was an anthropological theory «lacking any evidence that would grant it a minimum of scientific validity based on the criteria of verifiability, method, falsifiability, submission to verification by the scientific community, knowledge of the error rate and general acceptance; on the contrary, it displays elements that clearly divest it of any scientific character, without addressing the credibility and reliability of the technical advisor for the prosecution who had supported that model» (135). Hence the Court concluded that to condemn De Bernardinis «it is not necessary to resort to the theory of social representations» (270), which «is derived from the consultant’s personal experience» (271).

¹⁶ Grounds of the Court of Appeal ruling, pp. 158-165.

¹⁷ Regarding neurobiological-type naturalistic reductionism in the cultural sphere, it is significant that – in a version of the L’Aquila case according to which the statement that “earthquakes cannot be predicted” was «the only shared element in a flow of contradictory information reported by the press» – Stefano Cappa argues that analyses of peoples’ decision to stay at home the night of the earthquake despite two severe shocks can be reduced in some way to «the conditions of a laboratory experiment». According to this argument, while methodologies that «measure the volume of gray matter in a specific brain region» can «predict the profile of risk propensity», on the other hand, «little is known about possible external influences on decision-making mechanisms in conditions of risk». The scholar concludes (without, however, citing a source) that «social psychological studies have shown that factors of a cultural nature, such as media communication, are certainly capable of producing intense emotional involvement, but they are not enough to produce profound and long-lasting cultural changes such as those that are required to prevent and effectively deal with natural hazards such as seismic events» (Cappa 2015: 221-226). Given that nowhere in the indictment does it suggest that a profound and long-lasting culture change would be required for people to take on a belief in “energy release” in the span of a single week, the underlying thesis – that, essentially, cultural factors are not enough to produce cultural changes – is rather astonishing.

¹⁸ What statistical explanations and biomedical tests would be needed to assess the cases of parents who claim to have lost their children because the night of the earthquake they stayed at home despite two strong quakes owing to the fact that they had been persuaded by a reassuring expert diagnoses? On what possible basis could an ethnographic understanding of those lived experiences and the anthropological culture of the local context, developed on the basis of proven theoretical apparatuses, be judged wholly devoid of scientific value?

¹⁹ See Holden 2011: 1-37 for a discussion of the status of cultural expertise in the legal sphere and the problems generated by the tension between cultural discourse and legal discourse.

²⁰ See Gargani 2009.

²¹ See Borutti 1999 regarding the issues inherent in social scientific procedures of objectification.

²² This article, titled *Il valore dei termini: mancato allarme oassicurazione disastrosa* (The value of terms: failure to warn or disastrous reassurance-ism), was published in June of 2010 on the sites www.abruzzo24ore.tv and ilcapoluogo.com

²³ In the report I drafted, I provided a few examples to help readers better understand this subtle but crucial difference, one of which was the example of the traffic light: a failure to alarm is a broken traffic light that does not light up (lack of information), while a disastrous reassurance is a broken traffic light that shows green when it should show red (the presence of incorrect information). It should be noted that, while an unlit traffic light suggests caution, a traffic light displaying the opposite signal decreases people's perception of risk, thus increasing their exposure to danger. In short, a disastrous reassurance consists of providing a reassuring signal in a dangerous situation.

²⁴ While those who defended the decision to acquit the experts alluded to the "uncertainty of science" (Greco 2015) in judging what transpired during the meeting to be scientifically above reproach, it should be noted that the information the experts communicated to the population in L'Aquila consisted largely of contradictory certainties rather than uncertainty as such. This information was expressed through ambiguous and cryptic communication, the polar opposite of what would constitute scientific correctness (in the minutes of the meeting and the draft communication, Enzo Boschi simultaneously reminded people that earthquakes cannot be predicted and, in addition to ruling out the possibility that the ongoing seismic swarm might be a precursor to a strong earthquake, specified that «the state of knowledge» enabled him «to make certain affirmations». He was accompanied by Franco Barberi, who stated that «there is no reason to say that a sequence of low magnitude shocks can be considered a precursor to a strong event»). It is simply incorrect and misleading to confuse contradiction with uncertainty.

²⁵ Goffman 1974.

²⁶ Eco 1994; Volli 2006.

²⁷ The trial did not involve all of the over 300 victims of the earthquake, it concentrated on the deaths of 29 people and the injuries of four. The fact that someone in town might not have felt reassured does not detract from the fact that others did feel variously reassured, and that this belief may have led to a deadly outcome for some of them. Likewise, the fact that some of the pronouncements set forth in the experts' evaluative-communicative performance were scientifically accurate do not exclude the pseudoscientific nature and, above all, the danger, of the inaccurate ones. In fact, a large part of the report I prepared was dedicated to explaining how the belief of non-dangerousness both manifested among the expert diagnoses and spread and propagated among citizens in a way that was not homogeneous and uniform but rather blurry, fuzzy (See Ciccozzi 2014; Kosko 1993).

²⁸ Clearly, a methodologically sound process of refutation would have addressed the content of the expert testimony, its key assertions beginning with the ones incorporated into the grounds of the first instance ruling (a few pages out of a total of 120 comprising the entire report, pages containing the arguments that, it bears repeating, were paradoxically used at a later moment to justify the condemnation of De Bernardinis).

²⁹ See Rosaldo 1989 for a discussion of the «value of personal experience as an analytical category».

³⁰ If I did develop at some point before the trial the idea that forms of expert knowledge held a moral responsibility in the matter of the disastrous reassurance conveyed to L'Aquila's population, this idea did not derive from an irrational frenzy in search of scapegoats, but from a critical analysis in which I examined data drawn from both documents and direct experience in light of a theoretical-interpretative apparatus. Operating on the premise that legal truth is different from scientific truth and that, especially in the cultural sphere, it is difficult if not inappropriate to define this value in absolute terms (especially in cases involving political interests that affect the historical processes through which this truth is socially constructed), I self-ethnographically discussed aspects of my personal involvement in terms of "observant participation" in Ciccozzi 2013: 21-34, 169-180.

³¹ The total cost of managing the immediate post-earthquake emergency in L'Aquila was approximately 3 billion euros.

³² I analyzed the elements of the shock economy inherent in post-earthquake emergency management in L'Aquila in Ciccozzi 2010 and Ciccozzi 2011, and also dealt with this issue in the book I published shortly after the first instance judgment (Ciccozzi 2013: 180-186).

³³ Overall, I had the impression that the effort to discredit the thesis I presented at the trial was pursued by launching a complex and systematic process against me personally that in many ways resembled a degradation ritual, that is, a practice that essentially involves redefining the social identity of an individual by lowering his or her social status (see Benadusi *infra*).

³⁴ It is worth noting that, at the completion of the trial proceedings, the legal truth – which fully confirms the substance of the accusations even while restricting liability to a single defendant – was covered by the international mainstream media in a misleading way, under the prevailing banner of "scientists acquitted" (thus erroneously suggesting that the charges driving the trial had been refuted *in toto*). In the collective imaginary, therefore, the depicted trial (the one against the scientists who had failed to predict the earthquake, according to the

pattern of a failure to warn) overshadowed the actual trial (the one against the experts who had predicted a non-earthquake, according to the pattern of disastrous reassurance).

³⁵ From the Court of Appeal ruling onward, it was asserted that the meeting in question had not been a meeting of the CMR (and therefore not part of a framework of shared institutional responsibility) but rather a private consultation by members of the CMR with equally private content. This assertion was made despite the fact that the event was presented to the citizens as an official meeting of the CMR (which suggests not a mitigating factor but rather an instance of influence peddling). It also failed to address the question of why such a private meeting was not only not held not in Rome (which would have been more convenient) but was actually accompanied by public fanfare around the experts' relocation to L'Aquila, the precise place undergoing the seismic phenomenon they were to evaluate. This fanfare had a disruptive symbolic impact thanks to the ceremonial significance people granted to the epiphany of the great scientists who had been brought in specially from the capital. The population, panicked and hungry for explanations, responded with visceral interest (not to mention that these explanation were provided at a press conference held to publically communicate the outcome of the private forum through the diagnosis that there would not be an increase in magnitude, that is, by reassuring the population with the prediction of a non-earthquake).

³⁶ I address this point in Ciccozzi 2015 from the perspective of cultural perceptions of risk; for other discussions of the issue, see Console 2001; Console, Montuori, Murru 2000; Console, Murru 2001; Grandori, Guagenti 2009 (specifically regarding the L'Aquila earthquake); Jordan, Chen, Gasparini *et al.* 2011; Kagan, Jackson 1991; and Wyss, Console, Murru 1997.

³⁷ From Kuhn onwards it has been demonstrated that groups of scientists tend, more or less consciously, to deviate from the anti-dogmatic and skeptical principles that ought to guide them. Then, similar to the behavior of tribal communities immersed in magical-religious horizons, they tend to autopoietically defend their research habitat. That is, they seek to bend representations of reality to correspond to the theoretical tradition they have produced, which tends to take on the form of a founding myth that delimits both a cultural horizon of value operability and a domain of political and economic interests (see Kuhn 1999).

³⁸ <http://www.ilfattoquotidiano.it/2014/10/18/terremoto-laquila-lavvocatura-dello-stato-assolvete-i-7-della-grandi-rischi/1159796/>

³⁹ For example, in a recent text on the relationship be-

tween science and society that devotes all of three or four lines to the L'Aquila trial, the CMR was supposedly condemned «for not having recommended the city be evacuated» (yet another variant of the “failure to warn” cliché) as part of a «search for scapegoats» (Corbellini 2013: 115-116). False conclusions drawn from false premises.

⁴⁰ Statement taken from a public interview in “Il Tempo” (November 11, 2014).

⁴¹ See Amato, Galadini 2013. Speaking of rigor as well as sociometric and quantitative methods, I assume that, in order to feel justified in referring to “the entire scientific community”, the authors surveyed the mood of the inhabitants of this “village” one by one before issuing a sort of tribal call to arms.

⁴² Regarding this point see De Marchi 2013.

⁴³ Regarding the distinction between “event prediction” and “risk prediction”, the following example (which I introduced in the second hearing and which can also be found in the grounds of Judge Billi's ruling, 310) may be useful: a cardiologist cannot predict the exact moment when a heart attack will come but, according to a number of indicators, he or she can predict a patient's level of risk. A good cardiologist would never tell an overweight patients with alarming blood values that the pain in their left arm is a positive sign of stress relief, advising them to go for a jog. This can help clarify why distinguishing between the event and the risk is anything but contrived: on the contrary, it is the foundation of risk analysis.

⁴⁴ See Girard 1982.

⁴⁵ The point is that the seismologists' saying “Earthquakes don't kill, Buildings do” is pseudoscientific insofar as it reduces a multifactorial phenomenon to a single-cause explanation, subsuming the variables I and E under the variable V (since $D = I \times V \times E$ and not $D = V$).

⁴⁶ Gozzano 2013: 56.

⁴⁷ Although the Court of Appeal found that the experts did not interject regarding the notion of “energy release” during the meeting, it should be noted that, especially in a formal setting such as a technical meeting, not responding to a question (and not just any question, but the most crucial one) nonetheless constitutes a communicative act. We would do well to recall the first axiom of human communication: «One cannot *not* communicate. Activity or inactivity, words or silence all have message value: they influence others and these others, in turn, cannot *not* respond to these communications and are thus themselves communicating» (Watzlawick, Beavin, Jackson 1967: 49).

⁴⁸ It seems appropriate to point out that, at the same hearing in which she distanced herself from the concept of “energy release”, Concetta Nostro of the INGV declared that «a seismic swarm is a series of earthquakes that can also include a strong earthquake». Comparing this statement with that of Boschi, we can see how critical the boundary between seismology and semiotics really is. Indeed, the same is true of the dividing line – along with the specific hierarchical ranking it gives rise to – that we continue to claim to draw so clearly between the natural sciences and social sciences.

⁴⁹ It is telling that, in order to acquit the defendants, the judges established a principle of semiotic authority according to which the diagnostic utterances pronounced as part of the CMR meeting which exhibited an apparently reassuring meaning (at this point) did not actually have a reassuring meaning. According to this perspective it is scientifically accurate to reject any link between an ongoing seismic swarm and a disastrous earthquake, and if anyone hearing this diagnosis were to perceive it as reassuring it is because they misunderstood its meaning.

⁵⁰ Kosko 1993.

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