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Oreskes, Naomi

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How Earth Science Has Become a Social Science

Naomi Oreskes[*]

Abstract: *»Wie die Erdwissenschaften zu Sozialwissenschaften wurden«.* Many major questions in earth science research today are not matters of the behavior of physical systems alone, but of the interaction of physical and social systems. Information and assumptions about human behavior, human institutions and infrastructures, and human reactions and responses, as well as consideration of social and monetary costs, play a role in climate prediction, hydrological research, and earthquake risk assessment. The incorporation of social factors into "physical" models by scientists with little or no training in the humanities or social sciences creates ground for concern as to how well such factors are represented, and thus how reliable the resulting knowledge claims might be. Yet science studies scholars have scarcely noticed this shift, let alone analyzed it, despite its potentially profound epistemic – and potentially social – consequences.

Keywords: Physico-social systems, hydrological modeling, climate change scenarios, emessions scenarios, seismology, earthquake prediction.

1. Introduction[1]

In 2007 Paul Forman published a long, detailed, and controversial paper on the historical relations and valuation of science and technology (Forman 2007). Forman argued that while "science" – understood as the search for knowledge about the natural world – was a defining activity of modernity – "technology" – understood as utilitarian material objects and culture – has now replaced science as the defining activity of postmodernity.[2] In modernity, technology was subsumed under science, in postmodernity, science is subsumed under technology. Forman argued that historians had to a great extent missed this shift, because of their own assumptions about the inter-relations and relative value of

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[*] Naomi Oreskes, Department of the History of Science and Department of Earth and Planetary Sciences, Harvard University, Cambridge, MA, USA; oreskes@fas.harvard.edu.

[1] Deep appreciation to Andrea Westermann and Christian Rohr for organizing the conference in which this paper was first presented, to the anonymous reviewer who helped me to clarify the discussions of the IPCC, to Benedetto de Vivo for information on the L'Aquila trial and the ISSO position on deterministic risk assessment, and to Florin-Stefan Morar, Harvard University, for translations.

[2] Forman does not actually define science or technology in this paper; these are my definitions, based on my reading of his implicit understanding of the distinction between these terms and the activities to which they plausibly refer.

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science and technology: historians of science because they assumed the primacy of science, historians of technology because they rejected it. Forman was particularly hard on the latter group, whose desire to elevate technology to a position of equity with science led them to miss a massive alteration in the cultural position of technology, an alteration that has given technology the cultural position for which they previously tendentiously argued.

Forman's paper has been heavily criticized, especially (and not surprisingly) by the historians of technology he criticizes, but it seems to me he is correct in an essential point: that there has been a shift in the cultural valuation of science and technology sometime in the relatively recent past. In this paper, I argue that another shift has also occurred of late, whether emblematic of post-modernity or not: the blurring of boundaries between the natural and social sciences, and the widespread incorporation of human factors into analyses of so-called "natural" systems by natural scientists.

This pattern is particularly clear in the earth sciences (although arguably it is true of much research in the biological sciences as well). Many, perhaps, most, significant topics in earth science research today address matters that involve not only the functioning of physical systems, but the interaction of physical and social systems. Information and assumptions about human behavior, human institutions and infrastructures, and human reactions and responses are now built into various domains of earth scientific research, including hydrology, climate research, seismology and volcanology. In short, there has been a fundamental shift in how earth scientists do what they call 'fundamental' science. Historians of science have scarcely noticed this shift, let alone analyzed it, despite its potentially profound epistemic and social consequences. While many scholars have noted that the earth sciences have always had social *implications*, the incorporation of social parameters *into* earth science research – and the consequences of doing it poorly or wrongly – is something distinct and important that is only beginning to receive significant scholarly attention (Sivapalan, Savenije and Blöschl 2012; Lane 2014; and also Rumsey 2015; Uhrqvist 2015; Westermann 2015, all three in this HSR Special Issue).

2. Example I: Hydrological Modeling

I begin with an example from a field that I examined some years ago in a slightly different context: hydrology. At that time I posed the question: How well do models of natural systems describe those systems? What do we learn when we examine model performance in hindsight? What can we learn from such analyses about the sources of good or bad model performance?

In a paper published in 2001, my colleague Kenneth Belitz and I analyzed the performance of models that attempted to predict the behavior of hydrological systems. Post hoc analyses of the predictive accuracy of models were at that time rather scant, so our sample was small. However, for those systems for which we could find sufficient information to permit post hoc analysis, we found a striking result: that it was common for system behavior to diverge substantially from model predictions, and the most common cause of that divergence was unanticipated changes in the forcing functions of the system (Oreskes and Belitz 2001). That is to say, the factors that were driving system behavior changed in ways that had not been anticipated, and those changes negatively impacted the accuracy of the model forecasts. In several cases, the changing factors involved human behavior and activities.

While these models were physics-based – i.e. based on Darcy's Law, mass and energy conservation, etc. – like all groundwater models they also contained various empirical parameters, some of which, such as groundwater pumping rates and artificial recharge volumes, reflect human activities. The models were also all calibrated against historical data. In one well-documented example in central Arizona, studied by hydrologist Leonard Konikow, a groundwater model that had been calibrated on the basis of 41 years of historical data – an amount of data that most hydrologists would consider good – failed almost as soon as it was implemented.

Why did the model fail? Prior to the model construction, groundwater levels in the modeled regions had been falling and the model predicted that they would continue to fall. Indeed, the model predicted major declines in ground water level throughout the modeled region. Declines did occur, but they were generally smaller than predicted, and in some cases groundwater levels rose.

The model assumed that prevailing trends would continue, but that did not turn out to be the case. The 20-year period prior to model construction was one of relatively uniform downward trends in water levels driven by a consistent upward trend in groundwater pumping. But around the time the model was built, pumping rates began to fall, and the model systematically under-predicted what groundwater levels would turn out to be.

Groundwater levels had been falling throughout the calibration period due to groundwater pumping and the model predicted that they would continue to do so, but the system changed almost as soon as the model was produced (Oreskes and Belitz 2001, 29; see also Konikow 1986). (In hindsight one might speculate that the model was built because of pressures on the system, pressures that were related to the changes that ensued). People began to pump less, and this had a major impact on groundwater levels. That is to say, human behavior was a driving factor in the physical response of the system. When that human behavior changed, the response of the system changed too.

On examination it is obvious that the system being modeled was not simply a physical system but a hybrid physico-social system – a system that involved both physical and social driving forces. The model performed poorly because it failed to capture the social driving forces in that hybrid system. Of course, it is possible that the non-human components of the system – such as average annual rainfall – could have changed in unanticipated ways, but that is not what happened in this case. Changes in the human drivers most impacted the model performance. In hindsight it appears that if hydrologists had wished to build a more accurate predictive model, they would have needed to have paid more attention to potential changes in the social driving forces, and perhaps developed scenarios that explicitly acknowledged the potential impacts of changing human driving forces. Indeed, this is what some climate modelers now do.

3. Example II: Climate Change and Emissions Scenarios

Anthropogenic climate change is arguably the most important earth science issue of social concern facing the world today, both for its potential direct impact on human well-being and biodiversity, and for its potentially aggravating effects on other environmental and social concerns, including poverty, hunger, and inequality (Dasgupta, Ramanathan and Minerath 2014). A great deal of scientific attention has focused on using models to understand and project future climate change.

The Intergovernmental Panel on Climate Change (IPCC) is the world's leading organization addressing climate change, and, in particular, attempting to assess the results of future climate change. While most of the authors of the IPCC reports are scientists, the IPCC is not strictly speaking a scientific organization: it is a hybrid born of a scientific mother – the World Meteorological Organization (WMO) – and a political father – the United Nations Environment Program (UNEP). The founders of the IPCC recognized that climate change was not simply a scientific matter, but a matter of the interaction of human activities and responses with the climatic system. Human activities, such as deforestation and the addition of greenhouse gases to the atmosphere, drive climate change, and climate change in turn has diverse impacts on human activities. Human responses to climate impacts may also mitigate or exacerbate climate change. Thus climate change as a problem simultaneously engages scientific, social, cultural, political, and economic dimensions, and the founders of the IPCC have long recognized this (Bolin 2008).

The IPCC dealt with the intellectual complexity of climate change by creating a tripartite organizations structure, involving three "working groups" (Bolin 2008). Working Group I assesses the "physical science basis," working Group II (WG II) deals with "impacts, adaptation and vulnerability," and Working Group III addresses "options for mitigating climate change," mostly through analysis of "the costs and benefits of the different approaches to mitigation, considering also the available instruments and policy measures." In other words, WG I is focused on physical science (IPCC WG I 2007, 2013), working group II on social and natural science (IPCC WG II 2007, 2014), and WG III on policy (IPCC WG III 2007, 2014).[3]

Like most divisions, this one carries implications of status and hierarchy, reflected not only in the designation of Working Group as number one, but also in the ordinal sequence in which the reports are released, with the reports of Working Groups I always released first, II second, and III third. Epistemologically, the results of Working Group I are described as the "physical science basis," so there is an epistemic assumption built into the IPCC's structure: that physical science is the foundation upon which appropriate social science and policy can and should be based.[4] Reality, however, is more complicated than implied by this schema, because a key part of the IPCC assessment is the projection of future greenhouse gas emissions, which enable scientists to project likely or plausible future climate impacts based on expected levels of greenhouse gases in the atmosphere. And these emissions projections are projections about people.

In the 2007 assessment – AR4 (the most recent assessment at the time this paper was written) two of eleven chapters focused on such projections – one for global and one for regional projections.[5] Portions of two other chapters (2

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[3] The IPCC Working Group I (WG I) assesses the physical scientific aspects of the climate system and climate change. The main topics assessed by WG I include: changes in greenhouse gases and aerosols in the atmosphere; observed changes in air, land and ocean temperatures, rainfall, glaciers and ice sheets, oceans and sea level; historical and paleoclimatic perspective on climate change; biogeochemistry, carbon cycle, gases and aerosols; satellite data and other data; climate models; climate projections, causes and attribution of climate change. Recently WG I has expanded its rubric to work with WG II on a special report on extreme events (IPCC 2013a) and to address questions of near-term climate change and predictability.
The IPCC Working Group II (WG II) assesses the vulnerability of socio-economic and natural systems to climate change, negative and positive consequences of climate change, and options for adapting to it. It also takes into consideration the inter-relationship between vulnerability, adaptation and sustainable development. The assessed information is considered by sectors (water resources; ecosystems; food & forests; coastal systems; industry; human health) and regions (Africa; Asia; Australia and New Zealand; Europe; Latin America; North America; Polar Regions; Small Islands).
The IPCC Working Group III (WG III) assesses options for mitigating climate change through limiting or preventing greenhouse gas emissions and enhancing activities that remove them from the atmosphere. The main economic sectors are taken into account, both in a near-term and in a long-term perspective. The sectors include energy, transport, buildings, industry, agriculture, forestry, waste management. The WG analyses the costs and benefits of the different approaches to mitigation, considering also the available instruments and policy measures.

[4] This epistemic assumption is so widely assumed that it is difficult for many people to assume any alternative.

[5] This paper was written before the IPCC Fifth Assessment was released, in which the treatment of emissions scenarios was substantially revised. These changes occurred too late for detailed consideration in this paper; the most important aspect for our purposes is discussed

and 9) also present calculations that rely to some extent on model results, which in turn rely on estimates or measurements of greenhouse gas emissions.

Consider what the IPCC had to say in AR4 about the role and importance of these emissions scenarios:

> [E]stimates of future concentrations of LLGHGs [long-lived greenhouse gases] and other radiatively active species are clearly subject to significant uncertainties. The evolution of these species is governed by a variety of factors that are difficult to predict, including changes in population, energy use, energy sources and emissions. For these reasons, a range of projections of future climate change has been conducted using coupled AOGCMs [Atmosphere-Ocean General Circulation Models]. The future concentrations of LLGHGs […] are obtained from several scenarios considered representative of low, medium and high emission trajectories (IPCC WG I 2007, 755).

The scientists involved recognized and acknowledged that scientific forecasts of climate change are based upon estimates of future concentrations of long-lived greenhouse gases (LLGHG) and other relevant atmospheric constituents, and a primary control on these constituents is human activity. So while they are offering a report by physical scientists about physical science, they cannot do their job without considering certain variables whose values hinge on human activities. These variables include the number of people on the planet and the amount and type of energy that they use; the latter requires us to imagine what kinds of lives they are likely to live. They are matters of economics, technology, demographics, culture; they are what we might call "future history" (Oreskes and Conway 2014). One cannot forecast climate change without forecasting the activities and lifestyles of people. So the "physical science basis" of the IPCC assessment has human activity at its core. It is a blend of the physical and the social.

There is, of course, nothing exceptional about collaborations between scientists in diverse disciplines, and it is not a criticism of the IPCC to point out that social scientists are actively engaged in its assessments. But it is to point out a gap between the manner in which the IPCC presents itself and its work – and presumably the manner in which the scientists themselves think about it – and the reality. Working Group I does not present itself as a collaboration between the physical and social sciences. The lead authors of the chapter on climate model projections are physical scientists, as are nearly all the lead authors in Working Group I (and this is true not just for AR4, but for earlier and later reports of WG I as well). These physical scientists did not produce the emissions scenarios on which the AR4 climate projections were built; those were supplied by the vehicle of the IPCC Special Report on Emissions Scenarios, developed by social scientists (IPCC WG I 2007, 749).

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above. Readers may also wish to consider the IPCC SREX 2012, in which WG I and WG II scientists collaborated to integrate the physical and social elements.

How did the social scientists involved develop these emissions scenarios? Future emissions cannot, obviously, be measured. As already noted, their values will depend on a large number of variables, including not only the size of future populations, but where those populations live and what kinds of lives they live, changes in energy efficiency, agricultural practices, patterns of deforestation, and many other matters. If the world responds to climate change by implementing policies designed to change the patterns of human behavior and activity, then this will in turn impact future emissions, so projections of emissions and actual emissions are not independent variables. As the IPCC explains it:

> future emissions, even in the absence of explicit climate policies, depend very much on the choices people make; how economies are structured, which energy sources are preferred, and how people use available land resources (IPCC WG III 2000, summary on the cover; see also discussion in Moss et al. 2010; Manning et al. 2010).

By their own account, IPCC social scientists dealt with this complexity – and the inherent problem that the future cannot be measured – by developing "narrative storylines." They attempted to quantify these "storylines" with the help of six different integrated models from different countries (IPCC WG III 2000, 69-71). The scenarios developed are not predictions in a formal sense, but are better understood as plausible stories. Thus, what we find is that a basic input parameter into the complex numerical simulation models that climate scientists in AR4 used to construct projections of the future climate changes and their impacts was a story about the future.

Strikingly, in AR5 the IPCC changed the way it handled emissions, dropping the emissions scenarios and offering instead a set of "representative emissions pathways (RCPs). In these RCPs, future temperature levels were linked to levels of radiative forcings: 2.6, 4.5, 6.0, and 8.5 W/m2 by the end of the twenty-first century, relative to pre-industrial levels. This, one might argue, is a less social formal of analysis, as it re-focuses attention on a central physical variable: the level of radiative forcing (Moss et al. 2007, 2010; IPCC 2013). At the same time, by placing time frames on when these levels would be reached, and by attempting to calculate what mix of gases and other components (CO_2, CH_4, aerosols, etc.) might produce these pathways, the social drivers remain an essential component of the projections.[6]

Let me reiterate: I offer this discussion neither as a criticism of the IPCC nor of climate modeling in general, but as a matter of record: a descriptive account of the ways and means in which social parameters are embedded in the "physi-

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[6] The 2007 expert report, which first proposed this alternative approach, gives a detailed discussion of the reasoning behind this change. Its analysis is beyond the scope of this paper; suffice it to say that in this discussion, one sees scientists grappling with the best way to handle the interactions between the physical and the social (Moss et al. 2007).

cal" science basis of climate modeling. The sociologist of science Steven Yearley has put it this way:

> the business of predicting greenhouse gas emissions, climate futures and policy responses is critically dependent on social variables such as the choice of technology, regional development policies, consumers' behavior and the performance of the economy (Yearley 2009, 390).

What I am underscoring here is that this dependence is expressed not only in work that explicitly addresses social dimensions of climate change, but in the work that provides the (allegedly) physical science basis as well. As an empirical matter, we observe social questions inextricably linked to the production of scientific forecasts of climate change, with estimates of social parameters built into the foundations of those forecasts.

4. Example III: Seismology and Earthquake Prediction

On 6 April 2009, a magnitude 6.3 earthquake struck L'Aquila, the main city of the Abruzzo region in central Italy, destroying thousands of historic buildings and killing 309 people. Three years later, in October 2012, seven men – six scientists and a former government official – were sentenced to six years in prison for involuntary manslaughter in connection with those deaths.[7]

Anglophone news media reacted with outrage. Viewing the trial in the context of recent attacks on climate scientists, American and British scientists saw the trial as yet another example of the "war on science" (Mooney 2005). Calling the trial "perverse" and the sentence "ludicrous" the British journal *Nature* suggested that it revealed "contempt for science" (Nature 2012). The American weekly *Time* magazine interpreted the prosecution and conviction as further proof – if one were needed – that we were living in a dark age of stupidity and scientific illiteracy (Kluger 2012). Various commentators promoted the idea that scientists had been prosecuted for the crime of failing to predict the unpredictable (Pappas 2012). American scientists were distressed: Both American Geophysical Union and the American Association for the Advancement of Science (AAAS), issued statements in support of the Italian defendants. As reported in the journal *Nature*, the AAAS said it was "unfair and naïve" of local prosecutors to charge the men for failing "to alert the population of L'Aquila of an impending earthquake" (Nature 2012).

But the Anglophone media representations and British and American reactions were misleading. The scientists were *not* tried for failing to predict the earthquake, and one finds no evidence to suggest that this case was part of a

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[7] <http://www.bbc.com/news/world-europe-14981921>, 20 September 2011 (Accessed April 18, 2014).

broader pattern of politically motivated attacks on science. The scientists were tried for manslaughter on the grounds that they had given “inexact, incomplete and contradictory” information about the risk that the town faced, particularly in light of the swarm of precursor events that might otherwise have led people to evacuate (Tribunale di L’Aquila 2012, III). By offering misleading reassurances, the indictment alleged, the scientists encouraged many people who might otherwise have evacuated the town to stay in their homes, where they died when those homes collapsed.

The case centered not on the matter of whether or not earthquakes can be predicted, but on political questions about the social obligations of scientists speaking in official advisory capacities, and epistemic questions about the appropriate manner in which risk assessments should be performed. The questions at stake were what information scientists should have offered the public, and how that information should have been communicated. They were not so much matters of scientific facts, but matters of how those facts were rendered and communicated.

5. Background

Throughout the early months of 2009, L’Aquila experienced numerous small tremors. Among diverse public reactions to the swarm of tremors, a retired laboratory technician named Giampaolo Giuliani, whose career included 20 years at the nearby Gran Sasso National Laboratory, began to make measurements of radon gas levels, using home-made radon detectors throughout the region. On the basis of his radon measurements, Giuliani concluded that a large earthquake was imminent.

Perhaps because of Guiliani’s prediction and certainly in light of the large number of minor quakes they had already experienced, residents of L’Aquila were becoming nervous. On 30 March 2009, national civil-protection officials cited Guiliani for *procurato allarme* – instigating alarm – and forbade him from speaking publicly about earthquake risk. The Italian government also asked a group of experts to assess the risk level. The National Commission for Forecasting and Predicting Great Risks (Commissione Nazionale per la Previsione e Prevenzione dei Grandi Rischi) met in L’Aquila on 31 March 2009. Their conclusion was that, in light of the large number of small tremors that had occurred, the earthquake risk was raised, but it was not possible to offer a detailed prediction. “It is extremely difficult to make temporal predictions on the evolution of seismic activity,” the commission explained, and “the simple observation of many small earthquakes does not constitute a precursory phenomenon.” Consistent with general expert opinion on earthquake prediction, they noted that any specific prediction “does not have a scientific foundation” (Tribunale di L’Aquila 2012, IV).

While such meetings are normally closed to the public, this one was attended by several local government officials and other scientists. It was also extremely short: only one hour. Perhaps because of the intense public interest, two members of the commission, Franco Barberi from Rome University and Bernardo De Bernardinis, then vice-director of the Department of Civil Protection, held a press conference following the meeting. Barberi and De Bernadinis reassured the population, reiterating that the prior minor shocks did not presage a major one. Professor Barberi spoke at some length with a reporter from the local television.[8]

> Reporter: Can we predict earthquakes?
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> Barberi: Here the answer is very simple, you cannot predict earthquakes, if by prediction you mean telling in advance where, when, [and] what energy [level] an earthquake will produce. There is no technique – there were and there are a thousand studies, attempts, a thousand measures – but we do not have yet a very sure technique, so they are not predictable. Instead what you can do and what is actually done is study where earthquakes occur, what characteristics they have, the frequency of occurrence, what is the maximum energy and based on this, we can determine what is the level of seismic risk, but the temporal prediction is impossible and anyone who says that he/she has the tool to predict the earthquakes speaks nonsense (fesseria), misrepresents things and creates fear in the population.
>
> Reporter: What is the problem with the scientist who tries to predict instead of reassuring?
>
> Barberi: If a researcher is trustworthy, and this person believes he or she has a tool which conforms to the ways of the scientific community, to that extent, he or she must publish results and subject these results to the opinion of the peers, must publish in specialized journals and send the data to an institutional structure of reference, for example the Civil Defense Department (Protezione Civile) first stating clearly on what exactly the prediction was based. This is the ABC of seriousness. If these things are not done, it lacks elementary seriousness.
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> Reporter: What is then the risk in the L'Aquila region?
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> Barberi: Well, this question is not easy to answer. This was the problem analyzed by the Commission. What we can say [...] we can actually put it only in statistical terms. We can say that these are what in the technical language of seismology are called 'seismic swarms.' There are many shocks close-up to each other in time of more or less the same magnitude and are quite frequent. These very rarely evolve into the most critical situations. In most cases they are exhausted without resulting in anything more dangerous. This, however, does not allow us to say that it is mathematically impossible that there will be a stronger shock. If we could say that we would have the ability to making

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[8] The interview at the local TV Channel ABRUZZO24ORE, 31 March 2009, is available on YouTube: <https://www.youtube.com/watch?v=42RdAzpKMSY> (Accessed April 18, 2014)._Translation by Florin-Stefan Morar, Department of the History of Science, Harvard University. For a transcript see also Tribunal di L'Aquila (2012, 90-1).

> predictions, which, as I’ve already said, we don't actually have (Tribunale di L’Aquila 2012, 91).

Barberi’s comments were scientifically correct, but they seemed to imply that because any prediction that was made, including Guiliani’s, was unsubstantiated, it was necessarily wrong. This was, evidently, taken by many citizens to imply that there was no threat. Note as well the contrast drawn by the reporter between prediction and *reassurance*. When Barberi says one cannot predict earthquakes, the reporter draws the inference that the alternative to prediction is reassurance, and Barberi does not contradict that. He thus perhaps gives the impression that his rejection of Guiliani’s worrisome prediction means that one does not need to worry.

This interpretation would have been reinforced by the comments of De Bernadinis, who spoke to reporters both before and after the meeting. His comments were broadcast on Italian television, placed on YouTube, and used in the trial by the prosecution.[9]

On television, De Bernardinis appeared self-confident and relaxed, suggesting that anyone who was worried was being silly. He certainly did not appear to be worried. In response to more than one question on the point, he insisted that the seismic situation in L’Aquila was “certainly normal” and posed “no danger.” In fact, he went further, suggesting that the various minor shocks were dissipating accumulated seismic energy, and so the risk was actually less than it would be were they not occurring. “The scientific community continues to assure me that, to the contrary, it’s a favorable situation because of the continuous discharge of energy.” In response to this, the journalist concludes, “So we should have a nice glass of wine?” De Bernardinis laughs and replies “Absolutely.” Indeed, he specified which wine: Montepulciano.

The reassurances – rather than the failure to predict the earthquake per se – formed the basis of the prosecution case. At the trial it was argued that had the commissioner not so reassured the public, many would have stayed away and fewer would have been killed.[10]

It was also important that the accused were not simply academic researchers, called to task for providing erroneous, inadequate, or incomplete scientific information. If they had been, then their condemnation might have reasonably been interpreted as prosecuting scholars for what they do not know – for failing to predict the unpredictable. But they were members of a government-appointed commission, whose task it was to help ensure public safety and understanding.

Five of the seven were scientists serving in some form of official capacity in the Italian government, one was a government official, and the seventh was

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[9] Interview on 31 March 2009, also available on YouTube: <https://www.youtube.com/watch?v=kLIMHe0NnW8> (Accessed April 18, 2014).

[10] Ibid.; see also <http://www.youtube.com/watch?v=ZpSGXYkf7_s> (Accessed April 18, 2014).

head of a private company developed to assess seismic risk; all had participated in the National Commission panel that publicly refuted claims of significant, imminent risk. The prosecution argument was that, as members of that National Commission, the scientists had an obligation not just to evaluate the risk according to accepted scientific standards, but also to communicate the risk adequately and accurately in the public sphere. While they may have done the former to the best of their abilities, they did not do the later. Instead, the court argued, they provided "incomplete, imprecise, and contradictory information" to a nervous public.

The prosecutor Fabio Picuti, as reported in *Nature*, explained the point:

> I'm not crazy […] I know they can't predict earthquakes. The basis of the charges is not that they didn't predict the earthquake. As functionaries of the state, they had certain duties imposed by law: to evaluate and characterize the risks that were present in L'Aquila." Part of that risk assessment, he says, should have included the density of the urban population and the known fragility of many ancient buildings in the city centre. "They were obligated to evaluate the degree of risk given all these factors," he says, "and they did not" (Hall 2011, 266).

Citizens testified that had it not been for those reassurances, they would have taken better steps to protect themselves. One man, for example, recounted that as a child growing up in the region, his family had the tradition of remaining outside at night when a minor quake occurred. On the night of the earthquake, after feeling a shock, he and his wife discussed whether to go outside, but in light of the Commissioners' comments decided to remain in their home. Four hours later, the main shock hit, the house collapsed, and his wife and daughter were killed (Hall 2011).

Regardless of what one feels about the outcome, the facts of the trial makes clear that much of what was at stake in this case was *communication*. Yet scientists do not often consider communication to be part of science; science graduate programs only very rarely incorporate communication as part of students' training. The general view, among scientists, is that doing science is one thing, communicating it is another. Yet increasingly, earth scientists have become concerned about communication, holding workshops and sessions on communications in their national meetings. While L'Aquila may be an extreme example, it appears that, fairly or unfairly, scientists are being called upon to communicate their results, and if they do so badly, they may be called to account. Seismology in the twenty-first century, it would seem, is not just a matter of learning about earthquakes, it is also about adequately communicating what we have (and have not) learned.

One may compare this to the IPCC treatment of risk communication, including the famous "burning embers diagram," which in the Third Assessment Report vividly conveyed the degree of diverse risks associated with climate change. Interestingly, this was removed in the Fourth report as too "incendiary," but reinstated in the Fifth, suggesting that the IPCC also struggles with the correct degree of "alarm" or "reassurance" that it should convey (IPCC, WG II 2013, Summary for Policymakers; Smith et al. 2009; Revkin 2009a, b; Mahoney and Hulme 2012). While no one has accused the IPCC of being inappropriately reassuring, it has been argued that the general tendency of earth scientists to eschew dramatic claims has led them to tend to understate rather than overstate the risks associated with anthropogenic climate change (Brysse et al. 2013).

6. Seismic Risk Assessment

There was also a second issue at stake at L'Aquila, one that received almost no media coverage in the US, but was extensively discussed by seismologists in Italy and elsewhere. That was the matter of the correct way to do scientific risk assessment.

Risk assessment as a scientific practice has existed since at least the 1970s. In the United States, the Environmental Protection has undertaken risk assessments since its establishment in the 1970s and has formally required them since the 1980s (U.S. Environmental Protection Agency 2012). The European Union also now requires risk assessments (European Environment Agency 1998). Not surprisingly, there is a large scientific literature on how risk assessment should be done: in the U.S., the National Academy of Sciences has issued an official guide to risk assessment, known informally as the Red Book for the color of its cover (U.S. National Academy of Sciences 1983). First published in 1983, it has been updated twice since then, in 1994, and 1996. Both of these updates discuss incorporation of social factors, including the role of expert judgment, uncertainty, and communication in democratic decision-making (National Research Council 1994, 1996). European Union guidelines also stipulate that risk assessments should include considerations of values, social costs, cultural heritage, and other non-technical elements, and that scientists involved need to be mindful of the need for transparency and effective communication (European Environment Agency 1998).[11]

The existence of formal guidelines does not, however, mean that all experts agree on those guidelines, or on how they are used and implemented, and behind the legal dispute at L'Aquila there was also a technical dispute about risk assessment. The prosecution alleged that the scientists involved

> did not meet the duties of risk assessment connected to their job of risk management and their duties of clear, correct, and complete communication; under the circumstances of the violent earthquake, they caused and can be held re-

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[11] See also <http://ec.europa.eu/health/dialogue_collaboration/policy/index_en.htm (Accessed April 14, 2014).

> sponsible for the deaths of [follows a list of names of the victims]" (Tribunale di L'Aquila 2012, IV).

What are the duties of scientists engaged in risk assessment? Experts do not all agree. One group of scientists, associated with the International Seismic Safety Organization, supported the prosecution for reasons they articulated in a Position Statement on Earthquake Hazard Assessment and Design Load for Public Safety, published on 6 August 2012, and made public via the internet (ISSO 2012a).

The motto of ISSO is "Be prepared for the largest unpredictable earthquake," and this encapsulates their differences with conventional risk assessment. The complaint involves a precautionary approach linked with the statistical problem of "long tails" – how do you calculate the risk of a low probability but high impact event? Most conventional risk assessment is based on the principle of calculating a probability distribution function over a defined recurrence interval, and using that to calculate likelihoods of various levels of earthquakes. One multiplies the magnitude of the earthquake by its frequency to get the risk of an earthquake of that magnitude over that time period. Summing over all possible magnitudes produces an estimate of the total risk. Most earthquakes are small and do little or no damage; the rare big earthquakes do the most damage and kill the most people. So the probability distribution function has a long tail. Calculating probabilities on the tail is difficult precisely because those events are so rare, particularly if the time frame of analysis is short. The events that matter most are the hardest to evaluate.

The ISSO group argued that this conventional approach underestimates the actual threat. If you underprepare for the risk of a small quake, say magnitude 3 or 4, little or no harm is done because small quakes hardly ever kill people. But if you underprepare for the risk of a major earthquake, magnitude 6, 7 or higher, then the damage may be enormous. So, the ISSO scientists argued, the conventional approach does not adequately protect people. Risk assessment should focus on the largest earthquake that is physically plausible, and protect for that.

The ISSO thus defended the conviction and rejected the suggestion that the indictment was an attack on science. On the contrary, they argued that "the purpose of this trial was to ascertain the truth for the triumph of justice, certainly not to intimidate science. […] By interpreting it as an attack on science and scientists, the detractors distort the reality of the facts" (ISSO 2012b, cover letter). What was the trial about, in their view?

> [W]e are convinced that the decision of the judge has stressed precise responsibilities of the CGR members, who were accused not for not having been able to predict the earthquake, but for having wanted to corroborate a forecast of "no risk" in progress, although some of these scientists had previously published articles in which they sustained the opposite position on the situation in L'Aquila. In addition, the lack of independence of judgment by the CGR, based on released declarations in line with the Department of Civil Defense

> (as documented in their phone call recordings published in the website of "La Repubblica"), shows that the interactions between scientific community and the institutions in charge for preserving public safety require significant improvements […] [W]e believe that this process is extremely important to stimulate researchers to "to apply science" in a responsible and impartial way, particularly when dealing with investigation of natural phenomena, like earthquakes, that are not predictable with precision and are susceptible to extremely serious consequences (ISSO 2012b, cover letter).

In their view, scientists had succumbed to pressure to reassure the public, rather than basing their statements on the best scientific information, including, in some cases, their own earlier scientific work. How should science be done when the phenomena are not predictable and the stakes are high? They continued:

> We finally stress that, even if earthquakes are not predictable with precision, civil defense policy can be effectively oriented also by results of the most recent studies in the field of both seismology and seismic engineering, taking into consideration the expected maximum event that can be estimated in a "robust" way, for both the short- and long- term policy (ISSO 2012b, cover letter).

The crux of the scientific point was this latter one: the idea of paying heed to the maximum event that could be expected, based on available information, what they labeled the "Maximum Credible Event" (MCE). Given that earthquakes cannot be predicted – a point on which all experts agree – how should one prepare for them? Conventional risk assessment focuses on the normal, in the sense of what do we normally expect, given the known (or possible) distribution of earthquakes? These scientists were arguing for a different approach: what should we prepare for given the known worst-case event?

The actual damage caused by an earthquake depends on the magnitude of the ground motion, and this is independent of how often an earthquake of that magnitude occurs. Therefore, if you discount the risk of large quakes on the grounds that they are rare, you may lead communities to underprepare for the large magnitude vents:

> [W]hen an earthquake of a certain magnitude occurs, it causes a specific ground shaking hazard that does not take into account whether the event is rare or not. Therefore, ground motion hazard parameters for risk mitigation should not be scaled by the occurrence frequency (ISSO 2012b, cover letter).

In place of Probabilistic Seismic Hazard Assessment (PSHA), they advocated Deterministic Seismic Hazard Assessment (DSHA), using the notion of Maximum Credible Event to guide officials in preparation for large events with the potential to do great damage.

> [S]tructures should be designed and constructed to withstand the largest of Maximum Credible Earthquake (MCE) events that include or exceed [known] historic events [in geologically comparable situations] and the public should be advised to be prepared and ready for such possible events […] These are

> the most dangerous and destructive events that can happen at any time regardless of their low frequencies or long recurrence intervals (ISSO 2012a, 1).

Stressing the low frequency of major events gives the public the impression they need not prepare for them – and the scientists involved in L'Aquila had done just that.

> Although seismic hazards and the risk in L'Aquila were already known to be very high, the CGR came to conclude that a larger earthquake was 'improbable,' overlooking and even in direct contradiction and scientific betrayal of their knowledge […]. Regardless of how long is the recurrence interval […] the consequences from a possible seismic event should always be considered; specifically (a) the largest earthquake that can be expected, (b) the strongest one that can be scientifically assessed, or (c) at the very least, the size of the strongest one that has happened in the past.

In other words, scientists should articulate how bad it could be, and communities should prepare for the worst. "Using a long recurrence interval or low frequency argument as a basis for [discounting] 'improbable' earthquakes leads to a false and unjustified sense of security" (ISSO 2012a, 3). The 2011 Tohoku earthquake off the coast of Japan, which led to the devastating events at the Fukushima nuclear power plants, was a case in point. PSHA had led engineers to plan and design for earthquakes up to 8.5 creating tsunamis up to 5.2 meters; the actual event involved a magnitude 9 quake and a tsunami that in places reached 40 meters in height. Yet historical events suggested that such events had occurred in the past; if scientists had heeded that evidence and engineers used it, "it would definitely have helped to reduce considerably the damage" (ibid., 4).

The approach advocated by the ISSO scientists would be far more protective, but it would also be far more expensive. No doubt some would argue that, given the realities of official budgets, it simply is not realistic. Others might ask how money spent on a maximum level of earthquake preparedness would benefit the relevant publics as compared with funds spent on education, health care, or other public goods. The point here is not to say which approach is better or more justifiable, but simply to point out that this is a choice, one that cannot be made on purely technical grounds. The issues involved are inescapably linked to questions of cost – both the monetary cost of reinforcing buildings, evacuating towns, and caring for the injured and traumatized, as well as the non-monetary costs of anxiety and psychic trauma associated with loss of loved ones and loss of cultural heritage. Once one begins to discuss the difficult trade-offs involved, one is moved immediately into the realms of preferences and value – out of the technical and into the social.

Earthquake safety has never been simply a matter of geophysics, but most earthquake scientists, acting qua scientists, have traditionally understood their job to be to study how, when, and why earthquakes happen, and only to a lesser extent (if at all) how to communicate that knowledge to engineers and officials responsible for mitigation, or to the general public (Geschwind 2001; Clancey 2006; Smith 2015, in this HSR Special Issue). The idea that they might be responsible – morally and legally – if people died because an earthquake was not predicted, or the risk not adequately communicated – strikes most Anglophone seismologists as startling, if not offensive; hence their outraged reaction to the L'Aquila prosecution. But in the contemporary world, the inter-relationship between knowledge and safety is not easily disentangled. Seismology is no longer simply a matter of geophysics, if it ever was. It involves consideration of ethics, values, and monetary and social costs. L'Aquila shows that scientists can no longer ignore the social factors that affect and even control how damaging a particular earthquake may be. Earthquake prediction is a social science.[12]

7. Does This Matter?

I began this paper with a discussion with Paul Forman's efforts to bring to historical attention developments that we historians seem not to have fully or properly acknowledged and addressed. That was the cultural position and perceived relations of science and technology in the modern and post-modern worlds. In this paper, I have described a change has been taking place in the relations between the natural and social sciences. I have shown how social matters have become an essential part of the work of earth scientists in hydrology, climate science, and seismology. In each case, the work done by people who identify themselves as physical scientists involves social considerations, and relies on information and inputs that come from the social sciences or socially-produced sources of information.

My argument, put simply, is that within the world of earth science – and perhaps other areas of natural science as well – the relationship of the technical and the social has changed. Of course, social considerations have always been implicit in the earth sciences, concerned, as they have been, with satisfying the resource needs of societies for minerals, fuels, and water, and understanding geophysically-based natural hazards such as earthquakes, volcanoes, and tsunamis. Still, for most of the history of these sciences, the social dimensions have been considered secondary rather than primary, "applied" rather than "fundamental" (Oreskes and Doel 2002). The discussion presented here suggests that these social elements have now become explicit, primary, and fundamental. We might say that there has been a fundamental change in how scientists do "fundamental" science.

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[12] Note added in proof: In November 2014, the convictions of the L'Aquila scientists were overturned by an Italian judge. The families of the victims have pledged to appeal.

This, I would suggest, is an empirical phenomenon worthy of note, and possibly significant of not just perceived changes in the inter-relationship of the social and the natural worlds, but actual changes in the inter-relationships of the social and the natural worlds. As historian Dipesh Chakrabarty has argued, the world around us has changed, and the social and the natural can no longer be analyzed as separate realms (Chakrabarty 2009; see also Latour 1993; Oreskes 2007; Lane 2014). Men and women have become geological agents, and this means that our analyses of geological processes necessarily must include human activities. And by this, I mean "geological" in the broadest sense: our use of groundwater changes hydrological systems, our use of fossil fuels changes the climate system (and acidifies the ocean), and waste water re-injection from shale gas drilling is now causing if not large earthquakes, then at least small to moderate ones (Cherry et al. 2014).

Recognizing this requires a very large change in scientific and cultural thinking. As Stephen Jay Gould, Martin Rudwick, and other historians have argued, it was long viewed as the major accomplishment of geology, qua geology, to have demonstrated the insignificance of human time in comparison to the long stretch of geological time (Gould 1987; Rudwick 1992).

But time is one thing, impact is another. To their credit, natural scientists have increasingly recognized this, and have begun to incorporate social scientific matters into their analyses, recognizing that the world in which we live is increasingly one in which the systems that concern us – in some cases gravely – are neither wholly social nor wholly natural, but complex composites of the social and the natural.[13] On the other hand, the incorporation of these social factors has often not been fully acknowledged, nor managed in a satisfactory way.

Another way to put this is to stress that many of the systems we are studying are coupled earth and social systems, in which the social components are as dispositive as the physical ones, but many (if not most) of our models reflect this only incompletely. Models may incorporate parameters reflecting social variables – as in the example of the hydrological models – but in a static, and therefore empirically inadequate, manner. Modelers may be well aware – as in the L'Aquila example – of the social dimensions of risk, yet these dimensions may nonetheless be inadequately incorporated into their risk assessments and communications. In the modeling of emissions scenarios, scientists have recognized that crucial social drivers will change, but the level of scientific attention to these drivers has not matched the level of attention to the physical ones.

This, of course, reflects larger social patterns, prevalent since the Cold War, in which the physical sciences have in general (and with the possible exception of economics) been far better funded than their social counterparts. So it is not

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[13] The recent suggestion of the term 'Anthropocene,' to name a new geological epoch, is an indication of this recognition. See Rosol (2015, in this HSR Special Issue) and <http://quaternary.stratigraphy.org> (Accessed December 10, 2014).

surprising that, while physical scientists have made tremendous strides in understanding the questions such as climate sensitivity, we have made rather less progress in standing crucial human drivers and dimensions of climate change. This matters, because in the end, the planetary impacts of climate change will depend both on climate sensitivity and on the levels of emissions and degrees of deforestation that humans produce. And no matter how well we understand the climate system, we will not prevent dangerous anthropogenic interference in it if we inadequately understand the anthropogenic dimension.

We may say something comparable about hydrology and seismology. We know a good deal about how to protect groundwater resources, but we are inconsistent in implementing what we know. We know well where earthquakes occur, but we seem to know less well how to persuade governments and individuals to prepare adequately for them. Yet our future health, well-being, and prosperity may be supported or undermined by our ability to prepare for earthquakes, to anticipate volcanic hazards, to use our water resources sensibly, and to mitigate or adapt to climate change. In short, our well-being will depend on our ability to consider the physical and the social as parts of integrated and interacting systems.

So, just as many commentators now follow Bruno Latour in talking of technoscience (Latour 1987), we might begin to speak profitably about socio-physical systems – or at least to search for a gracious term to describe such a phenomenon. We might even hope that as our natural scientific colleagues increasingly recognize that the performance of their models may depend on their success in capturing the social components of the systems they study, that they might also begin to develop a capacious – and more generous – appreciation for the need for social scientific study and analysis. But the blending of social and physical phenomena in the world of earth scientists is not simply of social or semantic significance. It has, I would argue, epistemic implications as well.

One implication involves the predictability of human behavior. It is not news that humans are unpredictable, yet many models in the natural sciences implicitly assume consistency in human behavior. As our hydrology example shows, they follow persistence forecasting: they assume that current trends will continue. When the system being examined is a closed system with stable boundary conditions, controlled by physical and chemical laws of nature, persistence forecasting may be reliable. But if history shows anything, it is surely that human behavior is not law-like. Human behavior does change, and often we would have to say that this is a good thing. But from an epistemic perspective, one would have to argue that the more models include human behavior, the more likely they may be to break down, particularly if the models assume some degree of persistence forecasting.

Increasingly, scientists recognize that terrestrial systems are never completely closed to human effects, and the systems we are most interested in are often ones in which human effects – or effects on humans – loom large. When a model is built in aid of decision-making, this almost by definition means there is a human interest, and likely a human component, in that system. Climate change is a problem for us largely because it will impact (and potentially overwhelm) our infrastructure. With rare exceptions, earthquakes do not kill people, buildings and bridges that collapse and fall on people (because of an earthquake) do. Earthquake hazard arises *because* of the interplay between the earthquake and human activities – the same, to a large extent, is true of climate change. We are building models because there is some kind of problem, and therefore there is an implication that changes in human behavior might be needed (or at least useful). The very fact that the model is being built might be an indication that human activity in the future will change, in response to the problem that is being modeled. This means that in many cases accurate description of human components may be required for reliable operation of the model. In other words, the social scientific component matters, and if it is handled poorly, the knowledge claims that emerge are not likely to be robust.

Focusing specifically on the question of climate change, sociologist Steven Yearley has noted that, despite the fact that social dimensions are built into the IPCC analysis, the overall orientation of the IPCC has been focused primarily on the natural scientific aspects, even though arguably the "social science side of the equation […] has outweighed in its implications the natural science side." This matters, he notes, because "this orientation has led to a neglect of the importance of the ways that economic and social scientific aspects of global warming have entered into the business of forecasting, understanding, and trying to manage the changing climate" (Yearley 2009, 390). While the IPCC has clearly made progress on this question since 2009, the basic point remains legitimate.

8. Epilogue

In a follow-up paper, recently published in Osiris, Paul Forman (2012) extended his argument to insist that postmodernity is also characterized by the breakdown of disciplinarity. Disciplines, he suggests, with their "proceduralist" attention to means and methods, have of late given way to various forms of anti-disciplinarity. The analysis presented here strongly supports this claim, as earth scientists have increasingly acknowledged – albeit mostly implicitly – that the problems that society supports them to address – water supply, climate change, seismic and volcanic risk – are hybrid problems, borne of the interaction between human activity and natural systems. Earthquakes have always existed, but they became a problem for human societies when we began to build rigid infrastructures. Water supply has famously challenged human cultures since ancient times, and the failure of some of ancient civilizations to manage their water supplies was a significant cause of difficulty for, if not actual collapse of, those civilizations. Climate change, as a natural phenomenon, has always existed, but climate change understood as “dangerous anthropogenic interference in the [natural] climate system” is a problem borne of our capacity to recover and exploit the energy stored in fossil fuels, coupled with our seeming incapacity to reform an economic system that fails to account for environmental damage as an accumulating cost.

The incorporation of social perspectives into the earth sciencea is thus almost certainly a good idea, and how it is being done surely a topic for further examination. Yet at the same time, it would be difficult to describe these trends as “discipline-disregarding,” as most of the practitioners involved still consider what they do to be earth science. The IPCC, as I have stressed, maintains an organizational structure that presupposes a clear separation between the physical and social sciences; seismologists still call themselves seismologists; hydrologists are still hydrologists. One sees only modest attempts on the part of earth scientific journals or societies to change their names or compass to reflect a broader concern with topics that fall outside traditional definitions of earth science.[14] Rather, it seems more accurate to suggest that at least some earth scientists are doing social science without quite acknowledging that this is what they are doing, without adequate training and understanding of social phenomena, and, in the worst cases, without respect for colleagues who have greater experience and insights into the workings of social systems. In the process, they may do it poorly, to the detriment of the society that they believe themselves to be serving.

References

Bolin, Bert. 2008. *A History of the Science and Policy of Climate Change: The Role of the Intergovernmental Panel on Climate change*. Cambridge: Cambridge University Press.

Brysse, Keynyn, Naomi Oreskes, Jessica O’Reilly, and Michael Oppenheimer. 2013. Climate Change Prediction: Erring on the Side of Least Drama? *Global Environmental Change* 23 (1): 327-37.

Chakrabarty, Dipesh. 2009. The Climate of History: Four Theses. *Critical Inquiry* 35: 197-222.

Cherry, John, Michael Ben-Eli, Lalita Bharadwaj, Richard Chalaturnyk, Maurice B. Dusseault, Bernard Goldstein, Jean-Paul Lacoursière, Ralph Matthews, Bernhard Mayer, John Molson, Kelly Munkittrick, Naomi Oreskes, Beth Parker, and Paul Young. 2014. *Environmental Impacts of Shale Gas Extraction in Canada. The Expert Panel on Harnessing Science and Technology to Understand the Envi-*

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[14] One notable exception is the journal *Climatic Change*, founded by Steven Schneider, who worked assiduously to understand the social as well as the physical aspects of climate change and encouraged his colleagues to do the same.

ronmental Impacts of Shale Gas Extraction. Ottawa: Council of Canadian Academies.

Clancey, Gregory. 2006. *Earthquake Nation. The Cultural Politics of Japanese Seismicity, 1868-1930*. Berkeley, CA: University of California Press.

Dasgupta, Partha Sarathi, Veerabhadran Ramanathan, and Roland Minnerath. 2014. *Sustainable Humanity, Sustainable Nature: Our Responsibility* <http://www.casinapioiv.va/content/accademia/en/events/2014/sustainable.html> (Accessed June 6, 2014).

European Environment Agency. 1998. Environmental Risk Assessment: Approaches, Experiences and Information Sources. *Environmental Issue Report* 4. Luxembourg: Office for Official Publications of the European Communities.

Forman, Paul. 2007. The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology. *History and Technology* 23: 1-152.

Forman, Paul. 2012. On the Historical Forms of Knowledge Production and Curation: Modernity Entailed Disciplinarity, Postmodernity Entails Antidisciplinarity. *Osiris* 27: 56-97.

Geschwind, Carl-Henry. 2001. *California Earthquakes: Science, Risk, and the Politics of Hazard Mitigation*. Baltimore and London: Johns Hopkins University Press.

Gould, Stephen Jay. 1987. *Time's Arrow, Time's Cycle*. Cambridge, MA: Harvard University Press.

Hall, Stephen S. 2011. Scientists on Trial: At Fault? *Nature* 477 (14 September 2011): 264-9 <http://www.nature.com/news/2011/110914/full/477264a.html> (Accessed April 18, 2014).

IPCC WG III. 2000. *Special Report on Emission Scenarios*. Cambridge et al.: Cambridge University Press.

IPCC WG I. 2007. *Climate Change 2007 – The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the IPCC*. Cambridge et al.: Cambridge University Press.

IPCC WG II. 2007. *Climate Change 2007 – Impacts, Adaptation and Vulnerability. Contribution of Working Group II to the Fourth Assessment Report of the IPCC*. Cambridge et al.: Cambridge University Press.

IPCC WG III. 2007. *Climate Change 2007 – Mitigation of Climate Change. Contribution of Working Group III to the Fourth Assessment Report of the IPCC*. Cambridge et al.: Cambridge University Press.

IPCC SREX. 2012. *Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation. A Special Report of the Intergovernmental Panel on Climate Change*. Cambridge et al.: Cambridge University Press.

IPCC WG I. 2013. *Climate Change 2013 – The Physical Science Basis. Working Group I Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge et al.: Cambridge University Press.

IPCC WG II. 2014. *Climate Change 2014 – Impacts, Adaptation and Vulnerability. Working Group II Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*, 2 vols. Cambridge et al.: Cambridge University Press.

IPCC WG III. 2014. *Climate Change 2014 – Mitigation of Climate Change. Working Group III Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge et al.: Cambridge University Press.

ISSO (International Seismic Safety Organization). 2012a. *Position Statement on Earthquake Hazard Assessment and Design Load for Public Safety*, 6 August 2012 <http://www.assisi-antiseismicsystems.org/Territorial/GLIS/Glisnews/PositionStatementFirstEdition-ISSO_6Aug2012.pdf> (Accessed April 18, 2014).

ISSO (International Seismic Safety Organization). 2012b. *Cover Letter to President Giorgio Napolitano Concerning the Position Statement on Earthquake Hazard Assessment and Design Load for Public Safety*, 8 November 2012 <http://cngeologi.it/wp-content/uploads/2012/10/CoverletterandStatementISSO1.pdf> (Accessed April 18, 2014).

Kluger, Jeffrey. 2012. Scientific Illiteracy: Why The Italian Earthquake Verdict is Even Worse Than it Seems. *Time*, 24 October 2012 <http://science.time.com/2012/10/24/scientific-illiteracy-why-the-italian-earthquake-verdict-is-even-worse-than-it-seems> (Accessed April 18, 2014).

Konikow, Leonard F. 1986. Predictive Accuracy of a Ground-Water Model: Lessons from a Post-Audit. *Groundwater* 24: 173-84.

Lane, Stuart N. 2014. Acting, Predicting, and Intervening in a Socio-Hydrological World. *Hydrology and Earth System Sciences* 18: 927-52.

Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, MA: Harvard University Press.

Latour, Bruno. 1993. *We Have Never Been Modern*. Cambridge, MA: Harvard University Press.

Mahoney, Martin, and Michael Hulme. 2012. The Colour of Risk: An Exploration of the IPCC's "Burning Embers" Diagram. *Spontaneous Generations: A Journal for the History and Philosophy of Science* 6 (1): 75-89.

Manning, Martin R, James A. Edmonds, Seita Emori, Arnulf Grubler, Kathy A. Hibbard, Joos Fortunat, Mikiko Kainuma, Ralph F. Keeling, Tom Kram, Andrew C. Manning, Malte Meinshausen, Richard Moss, Nabosja Nakicenovic, Keywan Riahi, Steven K. Rose, Steven J. Smith, Rob Swart, and Detlef P. van Vuuren. 2010. Misrepresentation of the IPCC CO_2 Emission Scenarios. *Nature Geoscience* 3: 376-7.

Mooney, Chris. 2005. *The Republican War on Science*. New York: Basic Books.

Moss, Richard H., Mustafa Babiker, Sander Brinkman, Eduardo Calvo, Timothy R. Carter, Jae A. Edmonds, Ismail Elgizouli, Seita Emori, Lin Erda, Kathy A. Hibbard, Roger Jones, Mikiko Kainuma, Jessica Kelleher, Jean François Lamarque, Martin R. Manning, Ben Matthews, Jerry Meehl, Leo Meyer, John F. B. Mitchell, Nebojsa Nakicenovic, Brian O'Neill, Ramon Pichs, Keywan Riahi, Steven K. Rose, Paul Runci, Ronald J. Stouffer, Detlef P. van Vuuren, John P. Weyant, Thomas J. Wilbanks, Jean-Pascal van Ypersele, and Monika Zurek. 2007. Towards New Scenarios for Analysis of Emissions, Climate Change, Impacts and Response Strategies. *IPCC Expert Meeting Report, 19-21 September, 2007, Noordwijkerhoot, The Netherlands*. Geneva: Intergovernmental Panel on Climate Change <http://www.ipcc.ch/pdf/supporting-material/expert-meeting-report-scenarios.pdf> (Accessed June 6, 2014).

Moss, Richard H., Jae A. Edmonds, Kathy A. Hibbard, Martin R. Manning, Steven K. Rose, Detlef P. van Vuuren, Timothy R. Carter, Seita Emori, Mikiko Kainuma, Tom Kram, Gerald A. Meehl, John F. B. Mitchell, Nebosja Nakicenovic, Keywan Riahi, Steven J. Smith, Ronald J. Stouffer, Allison M. Thomson, John P. Weyant, and Thomas J. Wilbanks. 2010. The Next Generation of Scenarios for Climate Change Research and Assessment. *Nature* 463: 747-56.

National Research Council. 1994. *Science and Judgment in Risk Assessment*. Washington, DC: The National Academies Press.

National Research Council. 1996. *Understanding Risk: Informing Decisions in a Democratic Society*. Washington, DC: The National Academies Press.

Nature. 2012. Shock and Law. The Italian System's Contempt for its Scientists Is Made Plain by the Guilty Verdict in L'Aquila. *Nature* 490 (25 October 2012): 446 <http://www.nature.com/news/shock-and-law-1.11643> (Accessed April 18, 2014).

Oreskes, Naomi. 2007. The scientific consensus on climate change: How do we know we're not wrong? In *Climate Change: What It Means for Us, Our Children, and Our Grandchildren*, ed. Joseph F. C. DiMento and Pamela Doughman, 65-99. Cambridge, MA and London: MIT Press.

Oreskes, Naomi, and Kenneth Belitz. 2001. Philosophical Issues in Model Assessment. In *Model Validation: Perspectives in Hydrological Science,* ed. Malcom G. Anderson and Paul D. Bates, 23-41. London: John Wiley and Sons.

Oreskes, Naomi, and Ronald E. Doel. 2002. Physics and Chemistry of the Earth. In *The Cambridge History of Science, Vol. V: Modern Physical and Mathematical Sciences,* ed. Mary Jo Nye, 538-52. Cambridge: Cambridge University Press.

Oreskes, Naomi, and Eric M. Conway. 2014. *The Collapse of Western Civilization*. New York, NY: Columbia University Press.

Pappas, Stephanie. 2012. Scientists face four years in prison for failing to predict earthquake. *Fox News*, 27 September 2012 <http://www.foxnews.com/science/2012/09/26/italian-seismologists-could-get-four-years-in-prison> (Accessed April 18, 2014).

Revkin, Andrew C. 2009a. Climate 'Embers' Burning Brighter. *The New York Times*, 23 February 2009 <http://dotearth.blogs.nytimes.com/2009/02/23/warming-embers-burning-brighter> (Accessed June 6, 2014).

Revkin, Andrew C. 2009b. Why I.P.C.C. Report Lacked 'Embers'. *The New York Times*, 26 February 2009 <http://dotearth.blogs.nytimes.com/2009/02/26/why-2007-ipcc-report-lacked-embers/?_php=true&_type=blogs&_r=0> (Accessed June 6, 2014).

Rosol, Christoph. 2015. Hauling Data. Anthropocene Analogues, Paleoceanography and Missing Paradigm Shifts. *Historical Social Research* 40 (2): 37-66. doi: 10.12759/hsr.40.2015.2.37-66.

Rudwick, Martin J. S. 1992. *Scenes from Deep Time: Pictorial Representations of the Prehistoric World*. Chicago: University of Chicago Press.

Rumsey, Brian. 2015. From Flood Flows to Flood Maps: The Understanding of Flood Probabilities in the United States. *Historical Social Research* 40 (2): 134-50. doi: 10.12759/hsr.40.2015.2.134-150.

Smith, Joel B., Stephen H. Schneider, Michael Oppenheimer, Gary W. Yohe, William Hare, Michael D. Mastrandrea, Anand Patwardhan, Ian Burton, Jan Corfee-Morlot, Chris H. D. Magadza, Hans-Martin Füssel, A. Barrie Pittock, Atiq Rahman, Avelino Suarez, and Jean-Pascal van Ypersele. 2009. Assessing Dangerous Climate Change through an Update of the Intergovernmental Panel on Climate

Change (IPCC) “Reasons for Concern”. *Proceedings of the U.S. National Academy of Sciences* 106 (11): 4133-7.

Smith, Kerry. 2015. Earthquake Prediction in Occupied Japan. *Historical Social Research* 40 (2): 105-33. doi: 10.12759/hsr.40.2015.2.105-133.

Sivapalan, Murugesu, Hubert H. G. Savenije, and Günter Blöschl. 2012. Socio-Hydrology: A New Science of People and Water. *Hydrological Processes* 26 (8): 1270-6.

Steinacher, Marco, Joos Fortunat, and Thomas F. Stocker. 2013. Allowable carbon emissions lowered by multiple climate targets*. Nature* 499: 197-201.

Tribunale di L’Aquila. 2012. *Sentenza nella causa penale contro Barberi Franco, De Bernardinis Bernardo, Boschi Enzo, Selvaggi Giulio, Calvi Gian Michele, Eva Claudio, Dolce Mauro*, 22 October 2012 <http://processoaquila.files.wordpress.com/2013/01/sentenza-grandi-rischi-completa-1.pdf> (Accessed April 18, 2014).

Uhrqvist, Ola. 2015. One Model to Fit All? The Pursuit of Integrated Earth System Models in GAIM and AIMES. *Historical Social Research* 40 (2): 271-97. doi: 10.12759/hsr.40.2015.2.271-297.

U.S. Environmental Protection Agency. 2012. *The History of Risk at EPA, last updated on 31 July 2012* <http://www.epa.gov/risk_assessment/history.htm> (Accessed April 18, 2014).

U.S. National Academy of Sciences. 1983. *Risk Assessment in the Federal Government: Managing the Process*. Washington, DC: The National Academies Press.

Westermann, Andrea. 2015. Geology and World Politics: Mineral Resource Appraisals as Tools of Geopolitical Calculation, 1919-1939. *Historical Social Research* 40 (2): 151-73. doi: 10.12759/hsr.40.2015.2.151-173.

Yearley, Steven. 2009. Sociology and Climate Change after Kyoto: What Roles for Social Science in Understanding Climate Change? *Current Sociology* 57 (3): 389-405 <http://csi.sagepub.com/content/57/3/389> (Accessed April 18, 2014).