Comparativist Rationality and Epidemiological Epistemology: Theory Choice in Cases of Nuclear-Weapons Risk

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ABSTRACT: US testing of nuclear weapons has resulted in about 800,000 premature fatal cancers throughout the globe, and the nuclear tests of China, France, India, Russia, and the UK have added to this total. Surprisingly, however, these avoidable deaths have not received much attention, as compared, for example, to the smaller number of US fatalities on 9-11-01. This essay (1) surveys the methods and models used to assess effects of low-dose ionizing radiation from above-ground nuclear weapons tests and (2) explains some of the epistemological and logical problems (with these methods and models) that have caused scientists to decide against health screening of the most likely test victims. It also (3) argues that, once the faulty presuppositions and question-begging frames about testing and screening are recognized, there are compelling arguments in favor of nuclear-test nations' screening fallout victims, at least among their citizens. Finally, it (4) suggests that logically and epistemically flawed fallout studies/recommendations against screening are more like to occur when scientists adopt a Laudan-style comparativist rationality, rather than when they adopt a metascience more like that of Kuhn and others.

Nuclear weapons have not been used in war since the bombing of Nagasaki in 1945. Yet US above-ground *testing* of nuclear weapons has put billions of innocent civilians at higher risk of cancer and caused more than half a million premature cancer fatalities throughout the globe; the Soviet testing program caused a comparable number of avoidable, premature fatalities (Makhijani and Schwartz, 1998, p. 395; Hu and Makhijani, 1995, p. 586). Because fallout has circled the globe, approximately half of these million-plus deaths, of innocent non-combatants, occurred outside the nations actually engaging in nuclear-weapons tests. Why are these deaths significant, since most nations no longer engage in above-ground nuclear weapons tests?

1. Introduction

For one thing, because even low-dose radiation causes germline mutations and cancers, casualties from the mid-20th-century weapons development will continue for centuries to come. Calling to mind these nuclear risks also is important for at least two additional reasons. One is that some nations, like the US, are attempting to resurrect their commercial nuclear-fission programs, even though no new nuclear plants have been ordered in the US, for example, since 1974. Another reason is that nuclear-generated electricity is more expensive than any other, except for solar photovoltaic.

Considering the hazards of low-dose ionizing radiation is necessary thirdly because, facing high costs from decontamination/closure of existing nuclear plants, cleanup of weapons-testing facilities, and nuclear waste storage, some scientists and regulators have called for weakening radiation-protection standards, primarily as a cost-cutting measure. The head of the main international body making radiation-protection recommendations (the International Commission on Radiological Protection or ICRP), Dr. Roger Clarke (1999), has called for weakening radiation-protection standards, particularly for low-dose exposures. After all, cleaning up the last one percent of pollution is typically more expensive than cleaning up the first 99%. Clarke's proposals would save government and industry billions of dollars, but they have been challenged on ethical, logical, scientific grounds (Shrader-Frechette and Person, 2002). Thus an important reason for examining the epistemological and epidemiological reasoning behind health assessments, of above-ground weapons tests, is that the same epistemological flaws – which encouraged scientists to dismiss risks of nuclear testing – are now occurring again, in the analyses of those who are attempting to dismiss risks of low-dose radiation from commercial and cleanup activities.

Dismissals are occurring even though the database of 70,000 Japanese survivors of Hiroshima and Nagasaki confirms that statistically significant, pre-

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mature, excess cancer deaths have been observed in the exposed Japanese population among those receiving fallout doses equivalent only to 2–3 years of normal background radiation (one cSv or rem), excess cancers among nuclear workers have been three times greater, from such low-dose exposures, than among the Japanese (Nussbaum and Koehnlein, 1996; Nussbaum et al., 1990; Koehnlein and Nussbaum, 1990). And ever since at least a classic article in *Nature* in 1996 (Dubrova et al., 1996; see Wedemeyer, 2001), scientists have known that even low doses of ionizing radiation cause germline mutations, stillbirths, low birth weight, and neonatal mortality. Yet, ignoring good logical, epistemological, and scientific methods and models (and facing industry pressure), the main standard-setting bodies for radiation (the UN Scientific Committee on Effects of Ionizing Radiation, UNSCEAR, and the US Committee on Biological Effects of Ionizing Radiation, BEIR) have not made radiation standards stricter (Nussbaum and Koehnlein, 1994). On the contrary, as just mentioned, ignoring the best empirical data, even prominent ICRP leaders are arguing for weakening the standards.

Still another reason for examining the epidemiological epistemology used to assess risks from low-dose ionizing radiation is the coverup of consequences of catastrophic nuclear accidents like Chernobyl. At a 90% confidence level, biological effects of Chernobyl are known to have caused, world-wide, 430,000 premature, fatal cancers by the year 2000, only 14 years after the accident. Yet the Russians claim the accident caused only 28 casualties, because they counted only immediate, acute fatalities (Maclachlan, 1994, pp. 11ff.). And the pro-nuclear UN Agency, the International Atomic Energy Agency (IAEA), claimed only 31 fatalities occurred because of the accident; the IAEA (1991, p. 4) used reasoning like that of the Russians and examined no heavily contaminated areas and did no epidemiological or genetic tests. The pro-nuclear US Department of Energy (DOE) claimed Chernobyl has caused 32,000 premature fatal cancers, but DOE assumed excess cancers would occur only in this generation, rather than be carried on through germline mutations; yet nuclear workers and Japanese survivors already proved that, because radiation can cause cancer through germline mutations, excess fatalities will occur long beyond the current generation (Shcherbak, 1996, p. 46). Virtually all non-industry medical scientists agree that the Chernobyl premature-fatality figure is close to half a million, once one recognizes the latency period for low-dose cancers and counts the "statistical casualties" appearing years after the accident. Physicians for Social Responsibility warned that low-dose pollution from ionizing radiation is a "public health and safety emergency," a "creeping Chernobyl" (Lown, 1995, p. xiv).

2. Fallout victims and their risks

If nuclear pollution is indeed an "emergency," one would expect the nations (like the US) most responsible for it to be doing something about it, at least to provide medical care for most civilian victims of its weapons tests. This has not happened, and the prognosis for help for non-US fallout victims is even worse. For the last half century, US atomic veterans and nuclear-weapons facility workers, experiencing statistically significant increases in radiation-induced cancers, have lobbied the US government for medical compensation. In the year 2000 President Clinton signed a bill to compensate nuclear workers, and in 1990 the Congress passed the Radiation Exposure Compensation Act (RECA), a limited monetary compensation effort that provided 50,000 payments to people who showed they lived in "designated affected areas" of Nevada, Utah, and Arizona during high weapons-fallout periods of the 1950s and 1960s. To obtain the 50,000 compensation, however, citizens had to have been diagnosed with one of 13 radiation-related cancers and to have filed claims within 6 years after enactment of the 1990 RECA legislation. To date, however, the US government has not agreed to compensate, to provide medical care for, or even to screen, the hundreds of thousands of citizens, spread all across the US and the world, who received dangerous, nuclear-weapons doses of ionizing radiation far in excess of 100 rads (IOM, 1998, p. 42). As the National Cancer Institute (NCI, 1997, pp. 8.5–8.31) dose data reveal, virtually all US citizens, living east of California, are downwinders, and some people living in New York or Europe, for example, likely received higher radiation doses than many citizens living near the Nevada test site. (One reason for such distant high exposures is that fallout dose is a function of factors such as rainfall and atmospheric conditions.) Nothing – not even education or public-

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health followup – has been done to help most downwinders, civilian fallout victims of more than 200 above-ground nuclear weapons tests in the US during the 1950s and 1960s. Most live outside Nevada, Utah, and Arizona.

Should the governments responsible screen, provide medical care for, or compensate these downwinders? The National Cancer Institute (NCI) and the Centers for Disease Control and Prevention (CDC) have sponsored at least two meetings, on January 19–21, 2000 and June 8, 2000, at which they considered one part of this question, risk from Iodine-131 exposure from US weapons testing, but so far they have been reluctant to make a recommendation. This analysis argues that, to the degree that recommendations against free health screening (for fallout victims) rely on faulty epistemological methods and models, they are questionable. But if so, it may be time to support free screening of those most at risk.

Who is most at risk? To begin to determine effects of the fallout, in 1982 (under P.L. 97–144), the US Congress mandated a study (1) to develop assessment methods, (2) to estimate dose, and (3) to determine risk to the public from Iodine-131 (I-131) exposures from US atmospheric weapons testing. Fifteen years later, in 1997 the NCI reported the results of the first two of these three tasks. It estimated that just the I-131 exposures, from US testing, most likely would cause 11,000–214,000 cases of premature thyroid cancer among Americans, or approximately 75,000 such cancers, not counting cancers abroad. The NCI report also said citizens most at risk for fallout-induced thyroid problems (such as cancer, hypothyroidism, and hyperthyroidism) included those who were children in the 1950s, particularly females who drank significant quantities of milk (NCI, 1997).

In the case of I-131, the bulk of exposures came from its deposition on pasture grasses and its transfer to cows' and goats' milk. Most of the I-131 arose from 90 (of more than 200 total) nuclear tests conducted mainly in 1952, 1953, 1955, and 1957 that released about 150 million curies of I-131. "Some radioiodine was deposited everywhere in the United States... In the Eastern part of the country, most of the deposited Iodine-131 was associated with rain... [Because of the half-life of I-131, most exposure] occurred primarily during the first 2 months following a test" (NCI, 1997, p. ES.1).

3. Problems with the NCI methods and models

Although the NCI tended to minimize health problems caused by I-131 exposure, this belated NCI report employed flawed risk methods and was made public only because of citizen pressure. There also are grounds far believing that the 75,000 figure grossly underestimated I-131 fallout-induced cancers. Instead international physicians' groups say the premature I-131 cancers are likely about 500,000 (Rush and Geiger, 1997–1998, pp. 1–5), a number that the NCI (1998) considers too high.

What are some of the flawed epistemological assumptions leading to the apparent underestimate of the risk? One problem is inappropriate comparisons between indiscriminate (e.g., whole body or whole population) versus selective (e.g., single organ or population subset) exposures. A second flaw is that the NCI reported only county-average doses, which means it missed the high-dose tails of the exposure distribution. Third, the NCI ignored the significance of children's exposures. Because of their drinking milk, children's doses were seven times higher than average doses and as great as 160 rads – enough to kill sensitive members of the population. The NCI also erred in failing to explain how it arrived at estimates of excess US cancer cases, in not explaining why their thyroid-cancer rates were only one-tenth as high as those that already have been observed on the basis of Chernobyl data; in producing results that no one checked through appropriate oversight, and in failing to use open scientific competition to select researchers to do the NCI work (IOM, 1998; ACERER, 1998).

The US Department of Health and Human Services' (DHHS) Advisory Committee for Energy-Related Epidemiologic Research (ACERER) created pressure on NCI to release the final report about fallout-related cancers. ACERER conducted a number of reviews of the report, disputed the screening recommendations of NCI, and suggested that screening be conducted for non-cancer thyroid and parathyroid diseases for high-risk groups, such as US females who were children I the 1950s–1960s (ACERER, 1998, p. 6). ACERER also unanimously adopted a resolution that US efforts to address public-health consequences of weapons fallout are inadequate; that difficulties in identifying fallout injuries do not absolve the US government from

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making a public-health response; that research, alone, is not an adequate public-health response; and that delays in sharing public-health information about fallout have reinforced public cynicism about the government (ACERER, 1998, pp. 1–3). ACERER recommended that DHHS complete a comprehensive fallout-dose reconstruction project; notify Americans of their possible fallout risks; create a public and health-care-provider information service; and support archival projects to document experiences of exposed peoples (ACERER, 1998, pp. 2–4). To date, none of these recommendations has been implemented.

4. Problems with the US National Academy of Sciences methods and models

The controversy over risks of weapons testing accelerated when the Institute of Medicine (IOM) of the US National Academy of Sciences (NAS) released its 1998 evaluation of the 1997 NCI report. The academy committee said its estimates of the collective radiation dose to the American people were "consistent" with those of the NCI committee, but it highlighted numerous flaws in the NCI methodology and conclusions (IOM, 1998). Indeed, even the IOM (1998, p. 42) noted that many US children, on a diet of goats' milk, received thyroid doses from I-131 higher than 160 rads – a lethal doser for some of them. Despite such flaws, the academy committee nevertheless recommended that, instead of a government-financed thyroid screening program, the Department of Health and Human Services should provide a program of public information and education about the consequences of the US weapons tests (IOM, 1998).

Frustrated by the NCI and IOM studies, Ohio Senator John Glenn initiated hearings before a US Senate subcommittee. The hearings emphasized, as the NCI had noted, that many citizens in the Midwest and Northeast, such as in Albany, New York – indeed throughout the country – received radiation doses from weapons that far exceeded exposures to citizens living near the test site.

Highlighted in testimony by Dr. F.O. Hoffman (US Congress, 1998, pp. 421–439), the hearings also revealed that 3.5 million US children – in addition to those abroad – received average cumulative doses of I-131 that were 50 times greater than normal annual background radiation; that NCI "delayed the release" of the report, even though it was "substantially done" 10 years earlier; and that because of this delay, millions of radiation victims received no timely notification of their exposures. As a result of this delay, citizens in Utah, Arizona, and Nevada (the only states covered by the 1990 RECA 50,000 compensation) had no data enabling them to sue for compensation within the 6 years allowed. The Congressional hearings also revealed that the same NCI problems of poor methods, poor management, and lack of openness are compromising the success of the current Chernobyl studies. They said that NCI, as a result, is losing the ability both to locate and screen the millions of Chernobyl victims. Indeed, there is no public oversight of the HHS/ACERER investigations (US Congress, 1998).

In advising against government screening for thyroid problems induced by weapons testing, the IOM (1998) arguably made at least two major errors. First, it reduced the ethical question, of whether the government ought to bear responsibility for the risk it imposed, to the scientific question, of the radiation-exposure level and its medical consequences. Second, in alleging only minor medical consequences of the fallout the IOM thereby sanctioned a number of flawed epistemological presuppositions. These are apparent if one considers some pro-screening arguments that IOM ignored.

5. Three arguments for government screening: getting the epistemology right

At least three compelling arguments, suggest that, given flawed NCI and IOM methods and models, the US ought to screen its highest-risk fallout victims, given that it imposed this radiological risk on its citizens without their consent. These six arguments focus, respectively, on ignoring stakeholder assessment, suppressing evidence of harm, and relying on average exposures.

The first argument for screening is that if stakeholders were properly recognized and participating in the assessment of radiation risks, as the National Academy of Sciences says is necessary for sound risk assessment (NRC, 1996), then screening would likely be required. Important practical information about the relevant risks cannot be secured by assessors that

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include only scientists. One reason is that scientists are likely to make unrealistic assumptions about the circumstances of exposure or to be pressured by those who fear liability for harm. Thus, a recent National Academy risk study emphasized that stakeholder deliberation is just as important as expert analysis (NRC, 1996). Because the NCI and IOM studies failed to incorporate any significant stakeholder representation and deliberation, its recommendations about risk methods and screening are based on incomplete information and are at odds with earlier National Academy recommendations (NRC, 1996).

A second argument for screening is that, because government officials "actually suppressed" all evidence of this harm (ACERER, 1998, p. 10; see Ball, 1986), it is impossible to know the real risk without screening potential victims most at risk. To allege the risk is minimal, given evidence of coverup, is to beg the question of the level of fallout-induced harm. In the face of government coverup of harm, sound epistemology requires information, in part from victims.

The third argument, in favor of government screening (at least) of fallout victims most at risk for thyroid disease such as hypothyroidism, is that failure to do so would amount to sanctioning the erroneous belief that averages represent information about accurate levels of harm. Yet average harm, as reported in NCI and IOM studies, underestimates risks to at least four groups: the 25% of the population that is more medically sensitive to radiation; those who are more sensitive because they were children in the 1950s or 1960s; those who are too poor to pay for the screening themselves, such as the 46 million Americans without health insurance; and the Native Americans and Latinos who comprise a disproportionate segment of the downwinders. If the government does not provide free screening (at least for hypothyroidism, and at least for the four groups most at risk),it will have no reliable information about high-consequence effects of testing. But if there are epistemic grounds for arguing that screening is necessary, to determine the real effects of low-dose exposures, on what faulty epistemic presuppositions do opponents of screening rely?

6. Five arguments against government screening

In response to the preceding arguments in favor of screening, what are opponents likely to say? At least five arguments against screening arose in the NCI-CDC debate. They focus, respectively, on cost-effectiveness, average harm, risk magnitude, greater good, and epidemiology. Because of faulty presuppositions, none appear to succeed.

The cost-effectiveness argument, as endorsed by the IOM (1998), is that it would be prohibitively expensive to screen all downwinders in order to detect a relatively small number of thyroid cancers. Yet although screening for thyroid cancer may not be cost-effective, as the IOM (1998) claims, nevertheless screening for I-131-induced hypothyroidism is far more cost-effective, as ACERER (1998, p. 5) recognizes, because it involves only a simple blood test, because undiagnosed hypothyroidism can be a seriously debilitating or lethal condition, and "because the number of diagnoses and referrals for treatment could be substantial," given I-131, weapons-related exposure. Thus this first anti-screening argument errs in attacking a straw man: complicated and more costly cancer screening. It also begs the question of whether the fallout risk is minimal or whether it may have caused many problems, including half a million premature cancers, as PSR claims (Rush and Geiger, 1997–1998). The argument likewise errs because it ignores the fact that rights violations typically make cost-effectiveness claims irrelevant, as in the case of prosecuting a murderer. To the degree that most murderers are one-time offenders who commit crimes of passion, it is not cost effective to try them, in part because it is unlikely that they pose threats to other citizens. Yet justice requires that these murderers be brought to trial, that they be held accountable, regardless of whether it is cost effective to do so. The same is arguably true in this anti-screening argument. It errs in presupposing cost-effectiveness always trumps justice.

A second argument often given against screening is that, on average, weapons fallout has done little harm. While possibly correct, this argument misses the point that although the average harm from weapons-induced thyroid *cancer* may be small, nevertheless the average harm from weapons-induced *hypothyroidism* likely is not small, as ACERER notes (previous paragraphs). Moreover, even if the *average* harm were small, the individual harm may be great, especially for those in the four high risk groups mentioned earlier. That is, there would be what John Stuart Mill (1910) called "the tyranny of the major-

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ity," the tyranny of protecting only those who are easiest to protect, such as adult males. If someone releases x level of radiation and induces cancer in a child, that risk imposition ought not be ignored because x level of exposure is harmless for the average adult. The average-harm argument thus also errs because it ignores the 25% of the population with sensitivities far above average, such as children, for whom the same radiation exposure is seven times deadlier than for adults. The average-harm argument likewise errs because it focuses merely on average harm from only about 90 weapons tests, less than half of those done in the US. And once one considers the cumulative fallout from US tests in the Marshall Islands, from tests of other nations, from 2500 US nuclear facilities such as Oak Ridge Laboratories and Hanford Laboratories, as well as fallout not merely from I-131, but also from radionuclides such as Strontium 90 and Cesium-137, even the average radiation risk from all these exposures could be quite high.

A third argument often given against screening, the magnitude argument, is that the weapons fallout (at least from I-131) has caused minimal harm because thyroid problems rarely cause death, and people have thyroid check-ups (IOM, 1998). This argument errs for at least four reasons. For one thing, just because a harm (hypothyroidism, thyroid cancer) may not cause death does not mean it is minimal, as the argument presupposes. Someone who induces hypothyroidism and therefore seriously debilitating conditions, like depression, arguably causes a serious harm. Also there is uncertainty as to whether the harm is indeed minimal, not only because the PSR (1997–1998) and other groups say fallout-induced cancers are 6–7 times higher than the NCI and IOM claim, but also because there is controversy over the slope of the radiation dose-response curve (Jones and Southwood, 1987), because Chernobyl has caused many more thyroid cancers than expected (Abbott and Barker, 1996; Campbell, 1996), and because quantitative risk assessments sometimes err by 4–6 orders of magnitude (Shrader-Frechette, 1991). Moreover, the IOM (1998, ES-2) admitted that its dose estimates were "too uncertain to be used in estimating individual exposure," in part because direct fallout measures were made for only about 100 places nationwide, and exposure depends on a variety of critical factors, such as food consumption patterns, that are unknown. Given all these uncertainties, it begs the question to avoid screening and to claim the magnitude of the fallout risk is small. After all, virtually all counties of the US, except for a few in California, West of the testing, received doses in excess of 20 rads, an amount sufficient to induce hypothyroidism (NCI, 1997, pp. B-8 through B-29). An alternative to accepting this argument against screening would be to use screening to empirically determine the real risk of fallout-induced diseases. Moreover, for people on welfare, or with no regular medical insurance, or with highly restrictive health-maintenance organization (HMO) insurance coverage, the probability of blood tests or thyroid examinations may be low. For them the fallout-induced harm may not be small in magnitude.

A fourth objection to screening (at least of the thyroid) is the greater-good argument. This stance is that, if the government did thyroid-cancer screening for high-risk fallout victims, the patients would worry about false positive or benign thyroid nodules, would have unnecessary thyroid surgeries, and would be needlessly fearful, in part because there is controversy over whether thyroid-cancer screening reduces thyroid-cancer mortality (IOM, 1998). In other words, the argument is that great harm might be induced by thyroid-cancer screening because 20–30% of indeterminate thyroid samples could lead to unnecessary thyroid surgeries (IOM, 1998, pp. E5-6, 127). Like the average-harm argument, the greater-good argument misses the point and attacks a straw man, cancer screening. Although screening might bring about public anxiety and unnecessary surgeries, the argument ignores the fact that a simple blood test for hypothyroidism likely would not induce much anxiety. It also ignores the fact that not screening has induced anxiety, a distrust of government, and a dangerous tendency to underestimate fallout-related disease. It also ignores the ethical principle that those who cause harm ought to make amends for it and ought not use paternalistic arguments (against public ignorance and anxiety), so as to avoid bearing responsibility for that harm. Victims have the right to help decide about screening.

Still another reason offered for opposing screening is the epidemiology argument. It is premised on the claim that it would be difficult to identify the group of people most likely to have fallout-induced thyroid problems. As the earlier discussion made clear, however, scientists already know that females, born in the

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1950s, who were significant fresh-milk consumers, especially of goats' milk, are most at risk, mainly because the milk was more likely to have been drunk right after collection, a factor that allowed less time for I-131 to disintegrate and become less harmful. Knowing these facts, the screening simply could be offered to those in this highest risk group, and then offered later, to other groups, if the risks for them seemed likely to be statistically significant, given results from the first group. The identification of those at risk could proceed, step by step. There is no need, as the epidemiology argument presupposes, to have completed the full identification (of all victims) before any screening is offered.

7. Comparativist rationality and theory manipulation or bias

If preceding paragraphs are correct, political and economic factors (such as the desire to minimize harms from weapons development, to reduce government liability for fallout damage, and to avoid public outcry against radiological pollution) appear to explain some problematic fallout science and question-begging arguments against government screening. What epistemic and metascientific factors might have encouraged faulty scientific and screening decisions?

One answer might lie with comparativist metascience. To understand it, recall that members of the logical school of epistemology/philosophy of science (LS), like Carnap, Hempel, and Braithwaite, had one approach to metascience, while members of the historical school (HS), like Kuhn, Laudan, and Feyerabend, had another. On one hand, LS emphasized the theory neutrality of experiments and the presence of a common observation language; because they appealed to some apriori criteria to evaluate individual scientific theories, these LS proponents might be called non-comparativists. On the other hand, HS spoke of conflicting paradigms as "competitors" (Kuhn, 1962, pp. 147–150, 154–155) that were scored in terms of progressiveness (Laudan, 1977, 1997). HS tended to deny the theory neutrality of experiments and the presence of a common observation language; because they believed most or all criteria for theory evaluation were comparative, HS proponents might be called "comparativists." Although comparativists differ, especially in the degree to which they say there are epistemic criteria for theory choice, independent of theory comparison, Larry Laudan is perhaps the most extreme comparativist. Laudan (1977, 1997) says the rationality of theory choice is explicable only in terms of problem solving ability, relative to another theory, and not vice versa: Comparative problem solving defines rationality, and not vice versa. While Feyerabend claimed one could approach closer to truth, and Kuhn admitted there were rational values, like predictability and heuristic power, independent of theory comparison, Laudan (1997, p. 306; 1977) says "what does (and what should) principally matter to scientists is not so much whether those hypotheses are true or probable. What matters, rather, is the ability of theories to solve empirical problems."

In addition to the comparativist problems noted by Gunderson (1994) and Mayo (1997), the remainder of this paper argues for two theses: (1) LC errs in overestimating the potential of procedures, independent of truth and probability, to warrant relative choice among theories. (2) Comparativists' preferring "problem solving ability" to truth allows for more scientific manipulation/misinterpretation, as occurred in the fallout case.

LC appear to fall into problems (1) and (2) because of three main claims, (A), (B), and (C). (A) They ignore truth, probability, and evidence, and instead focus only on relative problem-solving ability (Laudan, 1997, p. 306; 1977); I call this "the truth claim." (B) LC say a severe test of a theory means simply "that is has survived tests its known rivals have failed to pass," not vice versa, and hence that a theory ought not be rejected until a better one is available (Laudan, 1997, p. 314; 1977); I call this "the test claim." (C) LC claim that theory competition should be restricted only to those that are "extant rivals." Laudan (1997, p. 314) explicitly says that choosing one theory over another "requires no herculean enumeration of all the possible hypotheses for explaining the events in a domain. The fact that there are in principle ... theories which could pass all the same tests ... is, from a comparativist perspective, neither here nor there – until such time as these in – principle theories are given flesh-and-blood in the form of a clearly articulated formulation." I call this last tenet "the availability claim."

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If LC accept the truth, test, and availability claims, why are they more likely to err in believing (1) that comparative theory-evaluation procedures are sufficient to warrant theory choice? One reason is that comparative theory assessment provides justified grounds for theory choice only if the choice is as objective/rigorous as possible, and only if it is able to distinguish between legitimate and illegitimate influences on theory choice. One attempt to distinguish legitimate and illegitimate influences has been to separate cognitive from social values, as Larry Laudan proposed in *Progress and Its Problems* (1977). But if one subscribes to the truth claim and ignores truth/probability, then it is unclear how they can, in practice, separate cognitive and social values and therefore, on their own terms, be as objective/rigorous as possible. Suppose a scientist working for the US DOE, an avid proponent of nuclear power and weapons, was motivated to discourage inquiry into effects of US nuclear-weapons tests. Suppose further that he denied (and gave no behavioral evidence whatsoever) of being motivated in these "social" ways. Suppose, even further, that he instead gave purely cognitive reasons for negligible fallout effects and for rejecting screening. Practically speaking, the only way to show his bias would be either to judge his intentions (and thereby fall into non-empirical accusations) or to show that independent scientific criteria, such as severe statistical testing (see Mayo, 1997), did not support his conclusions. But the first option would be question-begging and unscientific, whereas LC would not allow the second, given the truth claim. Moreover, comparative problem solving, in which one compared (i) the theory that fallout caused few casualties, versus (ii) the theory that it caused at least hundreds of thousands of casualties, would not be possible because of the availability and test claims. It would not be possible because the availability claim requires scientists to compare only fully formulated theories, and the test claim requires a theory not to be rejected until a better one is available. Although Feyerabend and Kuhn urge pluralistic theory development, LC do not. As a result, LC are not required to compare (i) and (ii) because (ii) is not fully formulated. And it is not fully formulated, in part, because the US has tested only for some (not most) effects of I-131 exposure; only for some I-131 (not other, longer-lived radionuclides') effects; and has done no epidemiological fallout-effects tests whatsoever.

By avoiding funding research on theory (ii) the US government has thus caused (ii) not to be developed. Instead it has accepted the most developed/best comparative theory, (i), on grounds that it "explains" the apparent absence of a fallout-induced cancer epidemic. In other words, the US has accepted (ii)'s problem-solving ability because (ii), not (i), better explains the absence of evidence (for hundreds of thousands of fallout casualties). Because the US has not funded epidemiological testing/screening, it has kept (ii) from being fully developed, and it has allowed (I) to remain the dominant theory. Thus it has confused the absence of (epidemiological) evidence (for (ii)) with evidence of absence (of support for (ii)). Thus LC use of the truth and availability claims seem unlikely to support procedurally sound theory comparison. It more likely supports biased comparison precisely because LC believe there are no norms of truth/probability/evidence that a flawed theory must meet (the truth claim); that a flawed theory can be rejected only on grounds of comparative problem-solving ability (the test claim); and that it need be compared only to fully formulated theories (the availability claim). Whenever vested interests keep research from being done, as in the case of weapons testing, and as allowed by LC, who do not promote alternative-theory development, then (2) comparativists' preferring "problem solving ability" to truth allows for more scientific manipulation and misinterpretation of the data because LC have no powerful tools (such as severe testing through probabilistic-statistical studies), enabling them to "call to account" probabilistic biases, begging the question, or making some of the flawed presuppositions mentioned in earlier sections of the paper. LC have no resources, within their account, to criticize one of the most basic forms of manipulation and misinterpretation: failing to fund studies that could prove a dominant (but perhaps politically motivated) theory wrong. And once they accept the test, availability, and truth claims, then given a dearth of research, the default position will be either the null hypothesis (e.g., no serious effects of weapons testing) or the most-developed hypothesis, like (i), having the most (relative) problem-solving ability. To see why (2) appears likely, consider that, in above-ground US weapons testing, people have been able to accept the default hypothesis (that testing had harmless/negligible effects) because vested interests prevented epidemio-

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logical analyses. Even when, 40 years later in the 1980s, the US Congress called for analyzing effects of weapons testing, once the studies were done, they were covered up for 15 years, again apparently for political reasons. They were made public, and thus potentially available for comparativist theory choice about testing effects, only after the National Academy (IOM, 1998; see NCI, 1997) and the Congress (1998) held hearings and tried to undo the cover-up. Thus, even if one could fully separate cognitive and social values, real-world operation of social values (delaying weapons studies) skews the ability of LC to choose rationally and suggests (2) is correct. LC accounts may work theoretically, but practically speaking, it requires a perfect world in which no such values and vested interests can dominate research. The weapons case reveals that, by avoiding research necessary to formulate a theory and discover anomalies in it, the extant theory can retain its hold. But if so, then despite their theoretical appeal, LC accounts appear to have inadequate cognitive resources to handle problems outlined in (1) and (2).

Another example of LC's problems doing rigorous, non-manipulated theory choice is evident in the work of the best known comparativist, Larry Laudan. He accepts the theory that there are only minimal levels of technological risks. In his volume, *The Book of Risks*, Laudan (1994, pp. 9, 23, 24, 14) repeatedly discounts the "scare stories," "exaggerated reports," and "media obfuscation" of "most of us ... most of the time" regarding societal risks like nuclear power and toxic chemicals. Laudan's (1994, pp. 3–4) chosen theory is that, once one considers "the straight facts" and keeps "editorializing to a minimum," industrially and environmentally induced risks, are seen in reality to be very small, yet they are misperceived and exaggerated by most people. He appears to confirm his hypothesis, consistent with LC beliefs, by using (what I call) the test and availability claims. That is, he assumes his negligible-risk theory is correct because there is no better-developed alternative and because, in the absence of a fully developed alternative, his theory "wins" by default. For example, he says the Chernobyl nuclear accident caused 31 fatalities and that mining causes about four deaths per 10,000 miners (Laudan, 1994, pp. 6–7). In both claims he considers only acute fatalities, immediate deaths, not statistical casualties (which are approximately 10 times greater than acute) resulting from risks such as radiogenic cancers and mining-induced black lung/lung cancer. Yet annually in the US, approximately 7000–11,000 people die from acute workplace fatalities, and 62000–86000 die from occupationally induced diseases like cancer (Leigh, 1995, pp. 3–7, 215). The latter, higher numbers of deaths are arrived at by using an alternative theory based on a dose-response curve derived from effects of epidemiological tests in other cases; because vested interests prevent epidemiological studies in the current cases, like Chernobyl or coal mining, the alternative theory (that casualties from both are actually about 10 times higher than Laudan claims) "loses" out, in the comparative competition, because it is not developed and because, given the availability claim, comparativists need not consider it.

Consider a second example of how LC handle comparative theory choice. When someone has a worry about a possible industrial–environmental risk, using the availability claim, Laudan does not allow it to challenge his chosen theory, under comparativism, unless the challenger is a "clearly articulated formulation" (Laudan, 1997, p. 314). To support his theory that industrial risks are low, Laudan (1994, pp. 9–10) says "unless someone can tell you what level of risk is associated with a given activity, then they have no business telling you that it is risky to begin with." Using the test and availability claims thus forces Laudan to count unknown/unquantified risks as 0 risks, and unknown/unquantified information as 0 information. He says nothing about getting more information, attempting quantification, or assessing when catastrophic consequences trump considerations of probability. Again, the LC stance appears to result in an appeal to ignorance, in confusing absence of evidence with evidence of absence. Laudan's use of the truth, test, and availability claims thus appears to cause cognitive bias in theory choice and to support claims (1) and (2).

Biased theory choice also seems more likely on a LC account because, as Gunderson (1994, pp. 307–308) notes, comparativists presuppose unambiguous theory choice, yet ambiguity is almost always built into alternatives, especially in the real world where social and political values often infect theorizing. Given this "infection," the comparativists' look-and-see attitude hardly suffices for theory choice. As ethicists have been quick to recognize, procedures are rarely sufficient fully to justify choice/action, in part because few

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procedures are ever followed perfectly in the real world. If not, what compromises procedures can compromise objectivity. Care (1978), for example, discussed the way participatory democracy, like trial by jury, warrants its resulting outcomes. He noted that necessary conditions for this warrant include more than 20 items such as all participants' being non-coerced, rational, accepting of the terms of the procedure by which they seek agreement and evaluation, disinterested, committed to community self-interestedness and to joint agreement, willing to accept only universal solutions, and possessed of equal and full information, and so on. Care argues that, because circumstances never permit full satisfaction of these procedural conditions, following procedures alone will not obviously produce a rational choice. If not, one might make an analogous argument: Because circumstances never or rarely permit full satisfaction of necessary conditions for warranting a comparative outcome, following LC competition, alone, will not obviously produce a rational/objective choice. Relying on comparison, in the absence of perfectly followed procedural rules, is like relying on the invisible hand, in the absence of full market information and free market choice.

8. Conclusions

If the arguments of the previous sections are correct, and if LC have problems because of their accepting the truth, test, and availability claims, what might be done to improve the comparativist account? With the truth claim, instead of explanatory power's trumping truth/probability, as Laudan (1997, p. 306) requires, one might accept the more modest claim that explanatory power is "at least as important as questions of truth, probability, or falsity" (Laudan, 1997, p. 308). This claim would give the comparativist resources to use, beyond mere comparison, in theory evaluation. Second, instead of the test claim and "relativizing severity to the class of extant theories" and requiring "clearly articulated formulation" of competing theories (Laudan, 1997, p. 314), comparativists might require as full theory-formulation as possible, before engaging in comparison. Third, with respect to the availability claim, comparativists might admit there are grounds for rejecting a theory (such as bias in its formulation), even in the absence of a compelling or better alternative. If LC metascience and epistemology were amended in these ways, scientists might avoid some cognitive biases, and citizens might have more trust in their governments to protect them from science-related harms.

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