When Something is to be Done: Proof of Environmental Harm and the Philosophical Tradition

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ABSTRACT: This paper is centred around a debate taking place among environmental scientists. One camp argues that proof of a causal connection between a chemical and a biological anomaly must be demonstrated in the laboratory. The other contends that actual damage is underestimated in the lab, and that it is therefore necessary to conduct supplemental ecoepidemiological research in order to determine the full impact of toxic chemicals. Members of the former contingent – claiming to be defending scientific rigour – sometimes accuse their peers of practising an inferior science. This paper argues that this contention is supported by a philosophical tradition tending to favour abstract and formal analysis over the close examination of material detail. To the extent that this preference has been adopted by the media, industry, policy analysts, and regulatory bodies, more is at stake than an intellectual squabble. The paper provides a brief overview of the history of the formalist tendency in philosophy, followed by an illustration of the ways in which advocates of a strict laboratory methodology implicitly rely on this foundation. The work and ideas of contemporary ecoepidemiologists are then compared to this imposing edifice of traditional science.

KEYWORDS: Philosophy of science, epidemiology, environment, pollution, mathematics

A scientific battle is taking place regarding both the extent of toxic contamination of the environment and the best methods of determining the causes of that contamination.[1] The debate becomes particularly heated with regard to the issue of chemically induced diseases and abnormalities in humans and animals. One camp essentially argues that laboratory or experimental results are the only valid demonstration of a relationship between a chemical and an observed effect. The other counters that actual damage is underestimated in the lab, and that it is therefore necessary to conduct supplemental epidemiological or 'ecoepidemiological'[2] research in order to determine the full impact of toxic

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chemicals. An insider clarifies that the advocates of experiment, 'start with the detection of a chemical in an environmental sample', whereas field-oriented scientists, 'start with the observation of a biological anomaly' (Gilbertson 1993: 409). Members of the former group – claiming to be defending scientific rigour – sometimes accuse their peers of practicing an inferior craft. This contention, it will be argued, is supported by a philosophical tradition tending to favour abstract and formal analysis over the close examination of material detail. To the extent that this preference is echoed by the media, industry, policy analysts, and regulatory bodies, more is at stake than an intellectual squabble. Strict reliance on formal methods may be hampering society's ability to make sound collective decisions about the environment.

Three tasks are made necessary by this project. First, a brief overview of the philosophical tradition will be provided, highlighting the noted proclivity. The ways in which advocates of a strict laboratory methodology of science implicitly rely on this foundation will then be discussed. Finally, the work and ideas of contemporary ecoepidemiologists will be contrasted to this imposing edifice of traditional science and philosophy.

I. THE FORMALIST TENDENCY

The philosophical tradition, while resisting simple summaries, is clearly marked by the search for foundational truths. Culminating in twentieth century positivism, philosophers have naturally been attracted to mathematics, attempting to replicate its certainty and simplicity in other areas of inquiry.[3] Two consequences have followed. Firstly, formal properties like quantity and structure have been abstracted from empirical evidence in an effort to perfect the study of nature. This methodological strategy has undoubtedly played an enormous role in many important scientific discoveries, and is for that reason to be commended. Secondly, however, empirical knowledge not susceptible to such an analysis has been diminished through both subtle and overt criticism. The following overview will provide examples of this formalist bias in philosophy and the philosophy of science. While by no means intended as a dismissal of the individuals highlighted, this summary should demonstrate the tenacity with which the formal, mathematical conception of the world has been held.

The Pythagoreans were among the earliest advocates of a mathematical approach to the study of nature. If Aristotle's accounts are accurate, they hypothesised that the nature or essence of being was number (*Metaphysics*: §985b23; §1090a20). The physical body was thought to be inferior to the soul, and the study of mathematics was advocated as an appropriate method of spiritual purification and transcendence (Philip 1986: 167). Aristotle further contends that the Pythagoreans examined the empirical world for analogies to number, with ten planets hypothesised in accordance with the base ten system (*Metaphysics*: 986a8-13). Later Pythagoreans supposedly associated unity and limit with good, and plurality and limitlessness with evil (986a22). Although this is indirect and inconclusive evidence, the Pythagoreans equated numerical certainty with truth and the good, and are responsible for embryonic efforts to evaluate the material realm according to this conjoint standard.

Heraclitus' argument that the material realm was one of flux and transience, and his rejection of the evidence of the senses, had a similarly enormous impact on Greek thought (Robinson 1987: 17). For example, because night inevitably changes into day, Parmenides believed it impossible to make any logically non-contradictory statements about the physical world (Gallop 1984: 7-12). There was, according to Parmenides, but one true object of knowledge: ungenerated, imperishable, and changeless Being. Because this Being is apprehensible only in thought, knowledge is limited to that which is logically certain. In other words, knowledge is divorced from a material world deemed unworthy of study (Gallop 1984: 12-18).

Plato attempted to reclaim the possibility of empirical knowledge by uniting form and matter. Nonetheless, his philosophical system revolved around a fundamental hierarchy between the two realms. According to Plato, abstract qualities were capable of a perfection eluding the fluctuating sensual world. True knowledge therefore pertained only to the rational thought of unchanging and eternal forms or ideas – entities resembling Pythagorean number and Parmenidean Being – existing apart from the material sphere. The analysis of geometry in *The Republic* illustrates this argument. No actually existing triangle can be used to demonstrate proofs such as the Pythagorean Theorem, Plato writes, because it will always fall short of the perfect qualities of the abstract triangle. Plato's solution was to demand that geometry restrict itself to nonsensible forms. Deductive methods could then be applied, and certain knowledge was attainable (*Republic*, §510c-511d). Finally, even though Aristotle rebelled against his teacher and advocated close attention to the empirical realm, he still concluded that arithmetic was the science 'most suited to rule the subordinate [sciences]' (*Metaphysics*: §982b5). The ancients thus established a hierarchy of knowledge in which mathematics and the exact sciences were more highly esteemed than the empirical sciences.

There is evidence that the equation of knowledge with numerical certainty carried over from ancient times into the early days of experimental science. There was now a renewed effort to render empirical knowledge as certain as mathematics. Roger Bacon, the medieval scientist, therefore declared that 'Of [the great sciences] the gate and key is mathematics' (1962: 116). Bacon believed that all of nature operated according to similarly demonstrable and infallible laws. '[I]f in other sciences we should arrive at certainty without doubt and truth without error', he maintained, 'it behooves us to place the foundations of knowledge in mathematics…' (124). Although Galileo rejected the teleological methods of his medieval and ancient predecessors, he echoed Bacon's sentiments almost exactly:

> The book [of the universe] cannot be understood unless one first learns to comprehend the alphabet in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it (1960: 184).

The operations of the physical world could thus be deduced from first principles such as Galileo's law of inertia. 'The deliberations of nature are perfect', Galileo further asserted, 'arguments concerning [it] are either correct and true or else incorrect and false' (Clavelin 1974: 454). Although Galileo and Bacon conducted experiments to determine whether their mathematical formulations accurately reflected reality, the equation of knowledge with mathematical certainty pervades the foundations of natural science.

Descartes independently arrived at a position similar to that of his contemporary Galileo. He expressed dismay that no 'loftier superstructure' had as yet been erected on the solid foundation of mathematics, since,

> The long chains of simple and easy reasonings, by means of which geometers are accustomed to reach the conclusions of their most difficult demonstrations, had led me to imagine that all things, to the knowledge of which man is competent, are mutually connected in the same way. (1994: 15)

Descartes made the erection of this superstructure his goal, setting out to explain the phenomena of nature solely by reference to qualities like motion, size and shape. These formal properties were to be discovered independently of experience and, as such, to be immune to error. Perceptual knowledge was, on the other hand, 'frequently fallacious' (1961: 7–8). Descartes admonished his readers, in their shared 'search for the direct road towards truth', to ignore fields less certain than arithmetic and geometry (7–8).

This historical proclivity becomes most pronounced in the late nineteenth and early twentieth centuries. Auguste Comte, the father of sociology, was also the inventor of the term 'positivism'. Like Galileo and Descartes, he did not want to limit the application of mathematics. 'It is only through mathematics', he declared, 'that we can thoroughly understand what true science is' (Lenzer 1975: 106). In his opus *Philosophie Positive*, Comte studied the 'generalities' of the various sciences and subjected them to a single method (xlviii). This method was the pure logic of abstract mathematics; its first principle the belief that all phenomena were controlled by immutable laws (75). Comte ultimately planned to create an algebra that would replace the alphabet and serve as the language of his new philosophy (1968: 182-83). In the future society utilising this currency,

> [M]athematics might enable us to dispense with all direct observation, by empowering us to deduce from the smallest possible number of immediate data the largest possible amount of results (Lenzer 1975: 105-6).

Since knowledge was far from complete, Comte cautioned that these deductions could not yet be concluded. This status was reflective of a human shortcoming, however. The problem did not lie with the method of mathematics, nor in its application to empirical matters (Comte 1875: 35-36). In the interim, Comte settled for suggesting ways in which the less-than-perfect sciences could make their methods more closely approximate those of physics, 'the ultimate destination in the art of experiment' (Lenzer 1975: 168). He accordingly defined the ideal experiment as a procedure in which the properties of substances were characterised in 'well-defined circumstances', and 'determinate' fashion (154).

The Vienna Circle (also known as the logical positivists) was the name attached to a group of Austrian intellectuals active collectively in the 1920s and 1930s, and individually long afterwards. This movement is perhaps most directly linked to the contention that empirical knowledge must be expressible in abstract and formal terms. The most famous founding members were Moritz Schlick, Rudolf Carnap and Otto Neurath. Karl Hempel and Ernest Nagel maintained sympathetic unofficial links. Karl Popper, although vehemently resisting the label 'positivist', worked with several of the group's members and shared some of their key ideas.

Although not unanimous in their views, two central principles of the logical positivists were set out in a 1929 manifesto. One was the essentially empiricist, 'there is knowledge only from experience, which rests on what is immediately given'. The other reflected a focus on the logical analysis of science:

> The aim of scientific effort is to reach the goal, unified science, by applying logical analysis to the empirical material.… [T]he meaning of any concept, whatever branch of science it may belong to, must be statable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the given (Hahn, Neurath and Carnap 1973: 309).

It was concluded that the concepts of mathematical physics most closely reflected the empirical 'given' (Neurath 1959b: 287; Carnap 1959b: 172; Hempel and Oppenheim 1965: 263-64). This doctrine has come to be known as physicalism. A Comtean algebra, a 'neutral system of formulae, … a symbolism freed from the slag of historical languages', was accordingly developed to refer to this physical reality (Hahn, Neurath and Carnap 1973: 306). For example, the empirical observation, 'It is raining' becomes Ou(r) (Carnap 1959a: 70). Through the logical operations of negation and conjunction with other variables, this symbol could be manipulated into various physical predictions, explanations and hypotheses (Carnap 1959c: 143-44; Hahn 1959: 157). A scientific law would represent the logical summation of hundreds or thousands of these simple statements (Hahn, Neurath and Carnap 1973: 312).

But what does it actually mean to subject a scientific theory to logical testing? Karl Hempel described the goal:

> it ought to be possible … to set up purely formal criteria of confirmation in a manner similar to that in which deductive logic provides purely formal criteria for the validity of inductive inference (Hempel 1965b: 10).

In simpler language, an observation such as Ou(r) would confirm a law if it could be logically deduced from this law in the same way that mathematical proofs are formulated. For example, general laws are hypothesised indicating that water freezes at 0^o Celsius and that its pressure, if enclosed, increases as temperatures fall. On a night when the temperature dropped below freezing, a car with a tightly sealed radiator was left on the street. From the combined effect of the laws and these background facts, it can be concluded that the radiator will be cracked by morning (Hempel 1965c: 232). The implication drawn by advocates of the deductive method is that this cracking is logically determined by properly established laws. 'Whenever and wherever conditions of a specified kind … occur', Hempel generalises, 'then so will, always and without exception, certain conditions of another kind.…' (1966: 54).

The adoption of the deductive method of hypothesis testing suggests a specific protocol for science. Theories must be expressed in precise, formulaic terms, ideally using logical symbols like Ou(r) (Hempel 1965c: 247; Popper 1982: 32-3). Hempel further stipulated that scientific explanation necessitates the determination of general laws. In fact, he argued that anything falling short of this standard was but a 'pseudo-explanation', or a 'vague claim' (1965c: 233-34; 1965d: 348). Classical mechanics, with its 'physical theories of deterministic character', therefore provided the paradigm of the deductive model (1965d: 351). Hempel also proclaimed that explanation and prediction were symmetrical exercises (364-70). Any worthy explanation, be it physical, biological or historical, must be able to predict a future event given similar initial conditions, and vice versa. Darwin's theory should consequently predict future species development and extinction, if the initial conditions and laws were fully disclosed (369-70). Several of the positivists endorsed this view; hence Otto Neurath's bold statement, 'there will be no more talk of "different kinds of causality"' (1959b: 294).

Karl Popper disagreed with the leveling of the distinction between the natural and social sciences (1969: 100-102). He also argued that while scientific theories can be falsified, they could never be conclusively verified (1969: 27-70). Popper nonetheless maintained that a 'scientifically significant physical effect may be defined as that which can be regularly reproduced by anyone who carries out the appropriate experiment.…' (45). Even though the existence of these laws cannot be decisively demonstrated, Popper agreed with the positivists that the goal of science was to deduce the mathematically certain laws of nature to the best of its present ability.[4] Although philosophy does not always receive an extra-disciplinary audience, Popper is widely read and admired by contemporary scientists. His pronouncements could possibly lead to the endorsement of the methods of the laboratory, at the expense of less-controlled forms of research.

II. FORMALISM AND LABORATORY METHODOLOGY

This paper has so far discussed the philosophical treatment of the sciences, suggesting that a formal, mathematical approach has prevailed. It now turns to the question of whether this methodology similarly influences actual scientific practice. Noted biologist Ernst Mayr submits that this is indeed the case. He contends that the controlled experiment of mathematical physics is venerated, while 'observation and comparison' are viewed with some suspicion (1988: 16). The language and research tools employed by certain individuals engaged in environmental science suggests that this hierarchy is intact in that field. There appears to be a widespread belief that interpretations based on field observations are to be viewed with some suspicion because of the possible influence of unknown variables. While this stance is unproblematic in itself, criticism of epidemiology is frequently expressed in stronger terms. One group of scientists contends that biological plausibility, or the proof that a chemical agent can feasibly have a hypothesised effect, *must* be demonstrated in the laboratory. In other words, if an abnormality is observed in the environment, its relationship to a causal agent cannot be proven until it has been confirmed in a controlled experiment (Willes et al. 1993: 314). Another scientist frankly declares, 'We have to put a stop to the increasing tendency to use epidemiological techniques to demonstrate causality' (Munkittrick 1993: 1568). Even John Ziman, a physicist critical of his specialty's domination of the sciences, writes,

> Epidemiology has immense practical value in alerting society to hidden dangers, or making evident some unsuspected association that might have possible causal origins, but it is a crude technique of research, and there is no 'instrument' of statistical analysis that will filter the desired signal from all the noisy data with which it has been scrambled (1978: 70).

Similar sentiments are found elsewhere (Lotti 1991: 435; Henschler 1990: 528). The gist of the criticism is that correlation cannot be adequately distinguished from causation in epidemiological research.

This concern bears resemblance to the philosophical drive for abstraction and certainty expressed in the first part of this paper. The extent to which these initial comments bespeak a further indebtedness to formalism in environmental science will now be explored. An investigation of the work of laboratory scientists has uncovered two related principles that tacitly endorse the abstract approach and justify a mathematics-based methodology. Ecoepidemiology, resting on a foundation questioning the premise that mathematics is an apt model for all knowledge, will be shown to represent a challenge to the scientific and philosophical tradition.

A. Physicalism

The first principle operating as a general law for the laboratory paradigm is physicalism. This physicalism bears some resemblance to the philosophies expressed by Galileo, Descartes, Comte and the Vienna Circle. Recall Descartes' emphasis on the abstract properties of motion, size, and shape, or Comte's dream of a mathematically-determined science. Advocates of the deductive method similarly boasted of the generality of their laws. Predicted results, stripped down into the spatio-temporal language of physics, 'always' and 'inevitably' followed deductively from these laws. Replicability of results marked a standard of science.

This philosophy is echoed quite closely by one team of environmental researchers, as they perhaps unwittingly endorse the deductive, mathematical method. 'The fate and biological activity of a compound', it is declared, 'are *determined* by the chemical properties of a compound' (Willes et al. 1993: 314, emphasis added). Other laboratory scientists are less emphatic, yet still convey the same message: 'Biodegradation rates of synthetic chemicals are … very dependent on chemical structure. …' (Klopman, Balthasar and Rosencranz 1993: 231).[5] In both of these examples, biological activity is reduced to chemical processes. There is no doubt considerable truth to this assumption. However, its rigid acceptance can foster the belief that the release of chemicals into the environment will only result in effects already observed in the laboratory. A longer passage reveals this conviction in action:

> In applying the scientific principles that dictate the relationship between physical/chemical properties and biological effects and environmental fate characteristics, it is possible to make judgments as to the likely behavior and potential for adverse effects of various groups of chlorinated chemicals (Willes et al., 1993: 320).

As in the philosophies influenced by mathematics, the mechanistic terms of the physical sciences function as general laws from which specific biological results are deduced. It is believed that there is nothing inherently unique about biological or environmental systems that would require observations outside of the laboratory, or a departure from the predictive model. Physicalism therefore acts as the first law of laboratory research and can operate to reinforce the notion that epidemiology is unnecessary.

B. The Dose-Response Relationship

A specific application of physicalism is encountered in the deployment of the 'dose-response' relationship, which functions as the second law of laboratory analysis. Neatly summed up in the phrase, 'the dose makes the poison', it essentially signifies that below a certain exposure, all substances are harmless (Doull and Bruce 1986: 3). It is frequently noted to this end that many industrially-produced chemicals are naturally present in the environment, to no apparent ill effect (Willes et al. 1993: 315, 331-33; Mackay 1992: 49; Ames, Magaw and Gold 1987: 271-77). The task for science is thus to determine the safe dosage, or 'threshold', of various chemicals (Klaassen 1986: 19-20). Field researchers and epidemiologists also make frequent use of the dose-response relationship. In accordance with physicalism, however, laboratory scientists demand four attributes of their dose-response relationships: linearity, replicability, statistical significance, and specificity. These features can be connected to the philosophical tendency outlined above, and operate to further condemn epidemiology.

1. *Linearity.* A mathematical, formal, interpretation of the dose-response relationship specifies that toxicity to chemicals increases with increased exposure in a linear, monotone fashion after the threshold point for a particular substance. A small dose can only be reflected by a small, or perhaps no effect. Larger doses of genuinely harmful chemicals should result in catastrophic events like cancer or death, endpoints on which laboratory research places almost exclusive emphasis.[6] One research team summarises the environmental ramifications of a linear dose-response principle:

> [the dose response relationship] dictates that the magnitude of the response of biological systems to chemicals depends on their concentrations in organisms, which in turn depend on the magnitude and duration of exposure (Willes et al. 1993: 314).

Just as biological properties in general are deduced from physical/chemical properties, the response of a specific organism to a chemical is 'dictated' by the size of the dose. The function of regulation, is, accordingly, 'to reduce risk [dosage] to accepted levels' (314). Although exceptions to linearity are occasionally noted, advocates of laboratory science nonetheless adhere to this principle quite religiously.[7] Conservative policy analyst Aaron Wildavsky has similarly endorsed a rigidly linear dose-response relationship, asking, 'would it not be better if we talked about carcinogenic or toxic 'doses' of chemicals, rather than calling the chemicals themselves 'carcinogenic' or 'toxic'?' (1995: 271). He concludes that all chemicals are safe below their threshold dose.

Furthermore, if the size of the chemical dose is unknown, experimentalists contend that there is no way to prove whether a specific chemical has actually inflicted the observed anomalies. Epidemiology's 'lack of exposure and dose data' leads to its further rejection by advocates of the laboratory method for this very reason (Klaassen 1986: 18; Owens 1994: 17; Willes et al. 1993: 344). This criticism of field research has filtered down to the media, where it is expressed in somewhat less explicit terms. The *Economist*, for example, notes that epidemiological studies regarding the possible decline in men's sperm counts are fraught with problems, 'because it is hard to estimate people's exposure to particular substances over periods of decades' (1996: 67). The fact that most research has been naturally confined to laboratory animals is then used to support the contention that harm to humans has yet to be demonstrated. For those doubting the merits of field research, there would seem to be no way out of this double bind.

2. *Replicability*. Earlier it was observed that the Vienna Circle demanded that scientific results be as replicable as mathematical proofs. This position translated into the requirement that all theories should be capable of yielding accurate predictions. Popper went as far as to call this the gatekeeping mechanism of science. Certain environmental scientists similarly argue that proof of a dose-response relationship must be replicable, either by different scientists, in different surroundings, or in a different species.[8] Potential variability affecting this duplication is believed to be best controlled in the laboratory, as was advised by the logical positivists. A standard contemporary toxicology text admonishes that a suspect chemical, 'must be capable of producing an adverse response in the test species under defined exposure conditions …' (Doull and Bruce 1986: 9). This assertion virtually echoes Comte's above declaration that the properties of substances are to be studied in 'determinate' and 'well-defined' circumstances.

Such an assumption has serious implications for the practice of environmental science. 'I am limited in the types of field studies I will undertake', one individual confesses, 'by our attempts to limit and simplify a field situation to control variability' (Munkittrick 1993: 1569-70). Further criticism of epidemiology is the correlate:

> Inconsistency or lack of replication in observations between studies has been identified as a major factor in detracting from cause-effect relationships based on epidemiological … information (Willes et al. 1993: 340).[9]

Studies regarding the chemical contamination of deep waters are believed to involve too many uncontrollable factors (Munkittrick 1993: 1569). Research pertaining to the human health effects of chemicals is similarly uncertain. Variables possibly connected to breast cancer, for example, include the following: smoking habits, age, diet, family history, hormonal imbalance, age of first pregnancy, evidence of miscarriage, length of nursing, age of menarche, and age at menopause. This list was provided by scientists skeptical of the epidemiological correlation between PCBs and the disease. It was concluded that laboratory tests have provided the only predictive, and hence valid, data about the disease to date (Willes et al. 1993: 340-41). Again, there is evidence that these scientific arguments have made some headway in more mainstream circles. Aaron Wildavsky writes, ‘A result is not meaningful unless it fits within a prior expectation generated by a theory and comes out the same way on another trial’ (1995: 404-5). In other words, predictable, replicable results are the hallmark of real science.

3. *Statistical Significance*. Laboratory defenders call for other mathematical specifications of their dose-response relationships. If universal physical-chemical laws determine biological outcomes, there is an automatic presumption that the effects of such laws take place with high frequency. Note the use of the words ‘dictate’ or ‘determine’ in reference to laws and their effects in three instances cited above. Rare outcomes must, analogously, occur often enough to rule out the possibility that they are the result of measurement error. This requirement was a signature feature of the deductive method favoured by positivism, and it can also be linked to the formal approach of Galileo and Descartes.

The deductive method is interestingly reinforced by standard statistical practices in the sciences. It is taken for granted that the more serious type of scientific error is the acceptance of a hypothesis that there is a relationship between two variables, when there is in fact no such connection.[10] This attitude is the result not of negligence, but is an artifact of statistical technique. Firstly, the hypothesis that there is no relationship between variables is given the benefit of the doubt as the null hypothesis, H_0. Standards are typically set so that a test will have only a one to five percent chance (the level of significance) of rejecting the null hypothesis when it is actually true (Mendenhall, Wackerly and Scheaffer 1990: 447). Secondly, as the stated purpose of statistics is to ‘make an inference about a population’, (3) the emphasis is not on single individuals or even a few individuals. Statistics is concerned with measurable trends; outcomes seldom observed may be reflective of rare individual variability as opposed to population features. The presuppositions of these seemingly neutral customs could form the basis of another paper; for the present it will simply be noted that it is assumed that a lack of connection between natural events is the default state of affairs. This principle fits in well with the presuppositions of the Vienna Circle, as laws were defined as the summation of atomistic events, and were given greater weight according to the number of tests they had passed.

In environmental science as practiced by laboratory adherents, these principles translate into the assumption that the graver error is to contend that a chemical causes a particular biological anomaly when it actually does not do so. The burden of proof accordingly falls on scientists attempting to demonstrate the existence of a previously unacknowledged relationship. If the effect being measured is slight or rare, as is frequently the case in epidemiology, it becomes possible to attribute it to measurement errors or chance. Hence, lab scientists occasionally dismiss the results of studies that have sparked serious concern in epidemiologists. Regarding the possible connection between chlorine-based compounds and breast cancer, laboratory adherents write:

> The differences in tissue concentrations of PCBs and DDE between women with breast cancer compared to those with benign breast disease were small, within the analytical error of the method, and within the range of concentrations observed in the general population (Willes et al. 1993: 341).

Because the differences in PCB concentration were 'small', there is a chance that they were not indicative of a genuine causal relationship. The well-publicised epidemiological link between maternal PCB consumption and reduced cognitive performance in their children is similarly questioned (341).

Wildavsky has once again adapted this tenet for his policy recommendations, as he bluntly advocates that regulators ignore evidence pointing to 'weak causes and weaker effects' (1995: 272). The unusual deaths that are the occasional province of epidemiology are hence defined as outside the focus of legitimate research.

4. *Specificity*. Finally, laboratory adherents advocate extricating the activity of *individual* chemicals in dose-response relationships. This is formally called the 'specificity' requirement, of which there are two varieties (Hill 1965: 297; Susser 1986a: 120-1; Fox 1991: 367-8). Specificity of effect ideally means that a certain cause produces one effect and one effect only. Since other factors could still produce the same effect, this form of specificity does not necessarily provide evidence of a causal relationship. For example, although smoking frequently leads to cancer, many other agents are believed to play a role in causing the disease (Fox 1991: 367; Susser 1986a: 120). Specificity of cause, on the other hand, means that a specific event has only one likely antecedent. The birth defects typical of thalidomide babies were so unusual that it was fairly likely that a specific cause was to blame. The latter specificity is in general thought to have greater evidential weight than the former.

All scientists endorse some standards of specificity when investigating a causal claim. Laboratory researchers aspiring to mathematical certainty, however, are particularly exacting. Having already demanded linearity, it is now claimed that the environmental effect of multiple chemical exposures will be *additive*. If chemicals interact, in other words, the resulting combined outcome will simply be the sum of the effects of the individual agents. This principle is stated with a high level of confidence, despite acknowledged evidence to the contrary. '[T]he potential combined toxicity of complex mixtures', writes one group of lab scientists, 'can be predicted from analytical data by assuming simple additivity of the toxicity of individual components' (Kovacs et al. 1993: 281). It follows that the effects of each particular chemical can be isolated in the lab without changing the nature of the remaining interactions. The broader implication is that when a chemical is withdrawn from circulation in the environment, all that will happen is that the portion of environmental anomalies attributable to it will be subtracted. It is thought unnecessary to look for non-linear, non-additive, relationships between chemicals and the environment. Mathematical models further intrude into the methodology of science.

The next step is the demand that chemical agents *should* be tested individually for their potential environmental effects, as expressed in the following assertion:

> scientifically valid evaluations … must be based on specific data related to chemical and physical properties of individual compounds (Willes et al. 1993: 321).

Otherwise, innocent chemicals will be blamed for the activity of other chemicals. When chemicals are not tested separately, as is frequently the case in epidemiology, it becomes possible to argue that causation has not been precisely determined (Willes et al. 1993: 337-42; Henschler 1990: 528; Klaassen 1986: 18). There is a further implication: A well-hedged causal argument about a particular chemical must be specified before environmental research can be proposed or funded. Observational evidence of slight abnormalities in humans or animals, leading to the hypothesis that these require exploration for possible connections to chemicals in general, is apparently an inadequately specified research design. 'Environmental surveys without well-defined hypotheses', it has been declared, 'are very wasteful' (Hodson 1985: 1227). There is some evidence that this narrowing of research horizons is echoed by a reluctance on the part of government agencies to fund field research (Gilbertson and Schneider 1991: viii; Swain 1989: 18). The majority of regulatory decisions appears to be made on the basis of anomalies and their connection to specific, individual chemicals as determined in laboratory studies (Colborn, Dumanoski and Myers 1996: 220; International Joint Commission 1996: 15-17).

In conclusion, there is some evidence that a formalist, mathematically-driven philosophy influences laboratory scientists. The paper now turns to its final component, an analysis of the work of epidemiologists and field researchers involved in environmental science.

III. THE CHALLENGE TO MATHEMATICAL METHODS

A. Organicism

The experimentalist's reliance on a laboratory methodology is rooted in a strict physicalism reminiscent of vital components of the philosophical tradition. Epidemiology and ecoepidemiology are, in contrast, governed by different assumptions. It will be remembered from the introduction that field researchers, 'start with the description of an anomaly'. The first principle for the epidemiological paradigm is hence a direct challenge to physicalism, as these scientists contend that biological systems are fundamentally different from physical ones. This belief, sometimes called 'organicism', stipulates that the information stored in the genetic code of an organism has no parallel in the physical world. New and unpredictable properties accordingly develop as biological entities emerge from physical matter and interact with their environment. Ernst Mayr, an advocate of this paradigm, summarises the traits of living things as follows:

> Organisms are unique at the molecular level because they have a mechanism for the storage of historically acquired information [DNA or RNA], while inanimate matter does not. … The presence of this program gives organisms a peculiar duality, consisting of a genotype and a phenotype. The genotype (unchanged in its components except for occasional mutations) is handed on from generation to generation, but, owing to recombination, in ever new variations. In interaction with the environment, the genotype controls the production of the phenotype, that is, the visible organism which we encounter and study (1988: 16).

Because of genetic recombination, random mutations, and the organism's openness to its environment, individual variability is a rule in biology to an extent that it is not in physics (Mayr 1988: 15-16; Ziman 1978: 68-69).

This does not mean that there are no relatively constant biological features, nor that variability is not ultimately reliant on physico-chemical processes (Mayr 1988: 1, 16, 40). It is also not a defence of a chaotic theory of nature. '[I]ndeterminacy does not mean lack of cause', clarifies Mayr, 'but merely unpredictability' (Mayr 1961: 1505). The implication is simply that biologists can have less recourse to the concept of law, and that exceptions to laws will not merely be the result of experimental error or incomplete knowledge, as those influenced by a mathematical methodology contend (Mayr 1988: vi, 12). To a certain extent, every organism is unique, rendering exact predictions difficult (15-16). The logically certain deduction of real-world results from laboratory-derived formulae accordingly yields some ground to *post hoc* observation and explanation.

Organicism provides the intellectual defence of field work just as the physicalism of the laboratory method justifies that methodological choice. Because laboratory science is limited in the number of unique individuals it can analyse, it is argued that the field can provide *more* data. For example, epidemiologists know that a virus may be associated with several ailments, none of which it will necessarily bring about in a particular individual on a particular exposure occasion (Evans 1976: 175-195). Accordingly, epidemiology has relied on the reporting of anomalies by individuals, even non-scientists (Fox 1991: 366). Ecoepidemiologists contend that chemical agents and biological entities will have similarly variable relationships, and that the neglect of field research can result in the underestimation of biological abnormalities caused by chemicals. One researcher goes as far as to advise that scientists should 'listen to fishermen' (Gilbertson 1984: 1537). This demand is far from facetious. It is built on the belief that those close to an environment are exposed to a large variety of individual organisms, and often know when something is amiss. Lab analysts, on the other hand, can only test for anomalies that they have prior reason to suspect exist.

Field research should not only be undertaken to provide new grist for the mill of experimental research, however. Because novel properties are brought to light as entities interact with their environments and mutate over generations, the field provides *different* data than the laboratory. PCB toxicity in the field, for example, is approximately five times higher than it is in the lab, owing perhaps to processing it undergoes in the food chain (Gilbertson 1993: 417). Similarly, it appears that some chemicals can trick the body into thinking that they are hormones. Such is the case even though the molecular structure of these chemicals is nothing like that of hormones. Lab researchers adhering to the physicalist belief that chemical properties dictate biological responses had never imagined this development (Colborn, Dumanoski and Myer 1996: 71; Davis and Bortone 1992: 113). Organicism demands intimate contact with the field, and hence operates as the philosophical foundation and first law of ecoepidemiology.

B. Dose-Response Relationship

1. *Non-linearity*. With the adoption of organicism, the dose-response relationship is freed from its formal strictures, and operates as a set of guidelines as opposed to absolute laws. The linearity of the connection is first questioned. Glen Fox observes that causal relationships, 'need not be linear or monotonic; in some, there is a marked threshold, others are sigmoid, and yet others are parabolic' (Fox 1991: 369-70). There is at least one known instance in which the dose-response relationship displays no trend: the connection between the anti-miscarriage drug DES (diethylstilbesterol) and rare cancers in offspring (370). It has also been known for some time that certain chemicals that bind to DNA have the potential to be carcinogenic in doses of a single molecule.[11] Ecoepidemiologists raise the possibility that microscopic doses of other chemicals might cause harms less obvious than cancer. It appears that a single, small, precisely timed exposure to the hormone mimics just mentioned can be enough to spark a disproportionately large response in a developing fetus (Colborn, Dumanoski and Myer 205-207; International Joint Commission 1996: 11). Theo Colborn indicates that it is even feasible that after a certain dose level, the embryo will show little or no further response than it did at a tiny dose (Colborn, Dumanoski and Myer 1996: 206). This finding forces a rethinking of the view that only catastrophically large doses of chemicals can lead to serious harms, and introduces the possibility that some chemicals might be virtually intolerable, particularly at certain points in the life cycle. Finally, biological variability implies that some individuals will display resistance to chemicals, while others will be hypersusceptible (Klaassen 1986: 19). It thus becomes impossible to say that the dose-response relationship

dictates biological outcomes in any physicalist, mathematically precise sense of the word.

2. *Replicability*. The requirement that dose-response relationships be replicable across different situations is also subjected to qualifications by epidemiologists. Glen Fox notes that 'there are many situations in which repetition is impossible', and cites Love Canal as an example (Fox 1991: 368). Epidemiologist Austin Bradford Hill observes that arsenic causes skin cancer in human beings, while this has never been demonstrated in laboratory animals (Susser 1986a: 298). The control provided by the experimental method was in this instance a shortcoming. In general, it is argued that there will be events occurring in the environment that in all likelihood could never be precisely reproduced in the lab or even in a different natural environment:

> The opportunities for exact or nearly exact replication by means of which the physical scientist demonstrates consistency and rules out chance are not available to the epidemiologist (Susser 1986a: 122).

Ernst Mayr and Mervyn Susser conclude that the deductive method favoured by laboratory scientists, resting as it does on the belief that valid results should be replicable and predictable, cannot simply be transplanted into biology (Mayr 1988: 18; Susser 1986b: 715-16). But it is important to acknowledge once more that this is not simply because epidemiology is a less exact science. Epidemiology entails acceptance of the organicist principle that living entities are unique at a certain level. Therefore, only a real and perhaps unique environment can provide all of the many variables with which a chemical will ultimately interact. An inability to replicate an experimental result is therefore not automatically grounds for its rejection.

3. *Standards of Significance*. Epidemiology proposes a similar rethinking of the statistical standards to which scientific studies have been traditionally held. While field research is obviously incapable of exploring the impact of every single chemical on every single individual, the appreciation of individual variability stemming from organicism leads to the closer examination of rare or subtle outcomes. Furthermore, ecoepidemiologists believe that weak relationships are to be expected when many different chemicals are contributing to an observed anomaly, or when a single chemical interacts with an unknown number of biological variables. '[F]ar too often we deduce 'no difference' from 'no significant difference'', warns Austin Bradford Hill (1965: 300).[12] It is also cautioned that statistically insignificant yet nonetheless real effects – like the loss of intelligence associated with PCB exposure – could accumulate across generations to yield a more dramatic impact (Bertell 1994: 29). The suitability of the traditional one to five percent level of significance is thus doubly questioned. It is also suggested that more attention be paid to the second measure of a statistical test, its 'power' (Bertell 1994: 29). Power is a gauge of a test's ability to detect

a false hypothesis that there is no relationship between the relevant variables.

The ‘precautionary principle’ advocated by many environmentalists also challenges the formalist presumptions of accepted statistical practice. Given the existing evidence of a broad connection between chemicals and environmental degradation, this principle declares that the default scientific hypothesis can no longer be ‘no relationship’. The organicist belief that events and entities are connected to one another also plays a role. The null hypothesis H_0 is thus reconceptualised to ask, ‘what evidence must I ignore to conclude that a causal relationship does not exist?’ (Fox 1991: 370). In other words, it is argued that the graver statistical error is the conclusion that there is no causal connection between two or more variables when there in fact is such a connection. This reversal of onus is believed to be justified, given the potentially lethal repercussions of environmental degradation (Fox 1991: 372; Bertell 1994: 28). It is an unscientific proposition only when the drive for mathematical certainty is allowed to dictate the practice of science.

4. *Specificity*. Finally, ecoepidemiologists challenge the experimentalists’ contention that adequate specification entails the isolation of individual chemical causes and precise effects. The example of the initial connection between typhus and a particular contaminated water source is offered by way of analogy. It was not water itself that caused the outbreak, yet public safety necessitated the initial call to avoid the water source in question (Fox 1991: 361). Similarly, not every component of tobacco smoke is linked with cancer, yet the cigarette is certainly the form in which most people receive the relevant carcinogens (363-64). Noted epidemiologists Lilienfeld and Lilienfeld summarise as follows:

> A causal relationship [is] recognized to exist whenever evidence indicates that the factors form part of the complex of circumstances that increases the probability of the occurrence of the disease and that a diminution of one or more of these factors decreases the frequency of that disease (1980: 295).

The specificity requirement cannot, therefore, be used to justify the search for an ultimate single cause, as this quest might simply be too time-consuming, fraught with difficulty, or even futile (if greater than one variable is in operation). The gravity of the risks in question necessitates a weakening of proof standards.

It must be repeated that it is not *merely* a question of relaxing scientific protocol. The rejection of the reliance on strict specificity also stems from the belief that chemicals can act in tangent to produce biological effects they would not have led to individually (Colborn, Dumanoski and Myers 1996: 140). The isolation and testing of a single chemical might therefore fundamentally alter the activity of that chemical and the chemicals remaining in the environment. Once physicalism and its mathematical presuppositions are questioned, the possibility that two or more chemicals can operate together to create unanticipated effects is introduced. The relationship of chemicals is therefore not simply additive; it may be synergistic or even antagonistic (Colborn, Dumanoski and Myer 1996: 140; Klaassen 1986: 16-17; Ludwig et al. 1993: 796-97).

Earlier it was suggested that some chemicals are potentially intolerable even at microscopic doses. Synergy now raises the possibility that the combination of numerous small doses of chemicals might result in new and unpredicted responses. If the endpoint being measured is insidious – for example, the reduced performance on the cognitive tests of children linked to maternal PCB ingestion – measurement of the combined impact of several chemicals becomes important. The following conclusion is reached by ecoepidemiologists:

> In the real world, where humans and animals are exposed to contamination by dozens of chemicals that may be working jointly or sometimes in opposition to each other and where timing may be as important as dose, neat cause-and-effect links will remain elusive (Colborn, Dumanoski and Myer 1996: 196).

As this passage indicates, the need for field research is a permanent feature of environmental science. There will be no future in which everything can be predicted from the safety of the laboratory.

CONCLUSION

If nature were as universally determinate as mathematics and its deductive proofs, humanity would not be confronted by chemical contamination. Despite the obvious failure this environmental degradation represents, scientists advocating the greater use of epidemiological techniques face a considerable battle. But ecoepidemiologists are not in any way suggesting that laboratory research be abandoned. It is instead proposed that a laboratory science informed by an enriched and expanded program of field research would reap the benefits of both worlds (Gilbertson 1993: 417). Insofar as philosophical assumptions privileging a mathematical methodology influence science, the regulatory process, and perhaps even independent policy analysts and the media, these assumptions need to be explicitly challenged. Greater openness to epidemiology is the goal, not the cessation of laboratory research.

It is noteworthy that some of the philosophers cited in the earlier parts of this paper acknowledged the shortcomings of the mathematical model. Auguste Comte conceded that biological phenomena were probably too complex to be rigourously subjected to the experimental method, although this sentiment did not interfere with the development of his positivist system (Lenzer 1975: 167). It is Aristotle, however, the great empiricist sensitive to the beauty and power of mathematics, who provided the most pertinent warning to rigid adherents of a formal methodology:

> Now experience seems in no respect inferior to art in a situation in which something is to be done. The reason is that experience, like action or production, deals with things severally as concrete individuals, whereas art deals with them generally. … If, then, someone lacking experience, but knowing the general principles of the art, sizes up a situation as a whole, he will often, because he is ignorant of the individuals within that whole, miss the mark and fail to cure; for it is the individual that must be cured (*Metaphysics*: §981a13-25).

Aristotle strove to find a balance between abstract analysis and detailed empirical examination. The mathematical model provides a general overview, while the empirical model supplies the particulars in cases 'when something is to be done'.

NOTES

[1] This issue was brought to my attention by Michael Gilbertson of the Canada-US International Joint Commission, and Glen Fox of Environment Canada. Good overviews are accordingly found in Gilbertson (1993) and Fox (1991).

[2] Although epidemiology is not limited to field research, for the purposes of this paper, I will equate the terms 'epidemiology', 'ecoepidemiology', and 'field research'. Epidemiology is defined as follows: 'Epidemiology is concerned with the patterns of disease occurrence in human populations and the factors that influence these patterns… ' (Lilienfeld and Lilienfeld 1980: 3). Ecoepidemiology, on the other hand, is epidemiology conducted with an eye to the effects of ecological hazards. The label was coined by Bro-Rasmussen and Løkke, who elaborate as follows:

> [Ecoepidemiology is] the study of ecological effects which are prevalent in certain areas (localities) or among certain population groups, communities, and ecosystems. Such systems involve not only the ecotoxicological description of disturbances, but also analyses of the possible causes. … Man should be considered part of the environment in ecoepidemiological studies, and damages would include diseases in individuals and species, as well as in populations… (1984: 391-92).

[3] Although mathematics has historically referred to a variety of intellectual pursuits, it is possible to define it in general terms with an approximate relevance across the ages. Mathematics is, for the purposes of this paper, 'the reduction of qualitative phenomena to measurable quantities and structures'. See Fleischhacker (1995: 11).

[4] None of the above comments should be taken to mean that positivism requires the perfect fulfillment of a hypothesis for it to be considered a law. Popper cautioned that 'a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified' (1982: 86). Most of the positivists agreed with this position. Hempel even incorporated probabilistic laws into his method, contending that the events of a coin toss, or the likelihood of curing a strep throat with penicillin, were of a different nature than the bursting of a radiator. Despite this, these events were to occur in a 'high percentage of cases' (1965d: 378-83). Positivistic science was, therefore, still governed by the search for general laws.

[5] Other studies and analyses make implicitly physicalist assumptions;. See Hermens et al. (1985: 220); Kovacs et al. (1993: 285-86); and Mackay (1992: 50).
[6] See Willes et al. (1993: 324-331); Kovacs et al. (1993: 281-89); Hermens et al. (1984: 151-53); and Klaassen (1986: 19).
[7] Research summarised in Klaassen (1986: 29-30), suggests that a single molecule of some chemical carcinogens can bind to DNA, resulting in tumor formation. Willes et al. (1993: 323-24), concede this possibility, yet immediately counter that 'in the case of most if not all chemical compounds, there is a dose below which no adverse effects can be observed'.
[8] Lotti asks, 'How can [a] general statement be derived from a handful of studies performed in one country only?' (1991: 435).
[9] Here Willes refers to the work of Glen Fox to support his argument, implying that Fox unequivocally rejects epidemiology when it fails to provide replicable results. This is a half-truth, as will be shown in a later section of the paper.
[10] There are two types of statistical error, Type I and Type II. A Type I error occurs when H_0, the null hypothesis (there is no observed association between two variables) is rejected and it is in fact true. A Type II error occurs when the alternative hypothesis, H_1 or H_a (there is an association between two variables) is accepted when there is in actuality no such association. The significance level of an experiment, or the probability of making a Type I error, is set before the experiment commences.
[11] See note 7.
[12] This sentiment is echoed by Rosalie Bertell: 'Just because a study shows nothing does not mean there is nothing happening' (1994: 28).

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