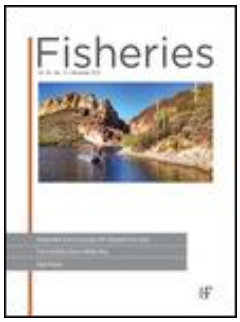

Fisheries

ISSN: 0363-2415 (Print) 1548-8446 (Online) Journal homepage: http://www.tandfonline.com/loi/ufsh20

Correlation and Causation in Fisheries and Watershed Management

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To cite this article: Ray Hilborn (2016) Correlation and Causation in Fisheries and Watershed Management, Fisheries, 41:1, 18-25, DOI: 10.1080/03632415.2016.1119600

To link to this article: http://dx.doi.org/10.1080/03632415.2016.1119600

Published online: 29 Dec 2015.

FEATURE

Correlation and Causation in Fisheries and Watershed Management

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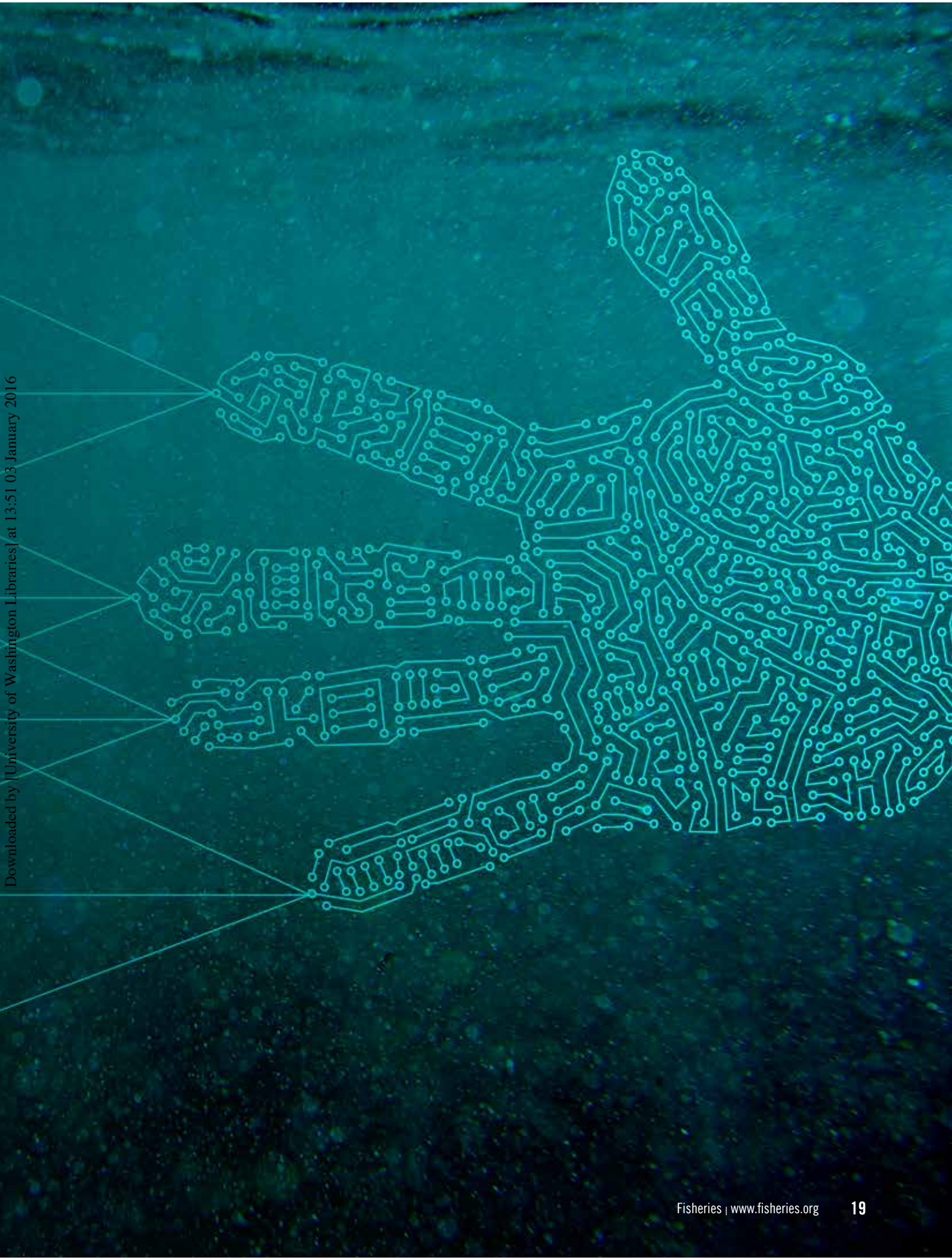

Efforts to understand how to manage aquatic ecosystems often rely on correlations between human actions and impacts in the ecosystem. We are often warned that correlation does not imply causation and that the gold standard for identifying cause and effect relationships is manipulative experiments. History shows us that correlations are often not causal and that managers should not design policies based on the assumption of causality. However, in the absence of manipulation, correlative evidence may be all that is available. Correlative evidence is strongest when (1) correlation is high, (2) it is found consistently across multiple situations, (3) there are not competing explanations, and (4) the correlation is consistent with mechanistic explanations that can be supported by experimental evidence. Where possible, manipulative experiments and formal adaptive management should be employed, but in large-scale aquatic ecosystems these opportunities are limited. More commonly, we should emphasize identifying the range of possible causal mechanisms and identify policies that are robust to the alternative mechanisms.

Correlación y causalidad en pesquerías y manejo de cuencas hidrográficas

Los esfuerzos que se realizan para administrar los ecosistemas acuáticos, a veces se basan en correlaciones entre las acciones humanas y los impactos de éstas sobre los ecosistemas. Suele insistirse en que la correlación no implica causalidad y que el estándar crítico para identificar relaciones causa-efecto son los experimentos controlados. La historia muestra que las correlaciones no siempre son causales y que los manejadores no debieran diseñar políticas basadas en la suposición de existencia de causalidad. No obstante, en ausencia de manipulación, la evidencia correlativa puede ser lo único que hay disponible. La evidencia correlativa es más fuerte cuando (1) la correlación es alta, (2) se obtiene de forma consistente a través de múltiples situaciones, (3) no existen explicaciones alternativas, y (4) la correlación es consistente con explicaciones mecánicas que pueden ser apoyadas con evidencia experimental. Cuando sea posible, se deben utilizar los experimentos controlados y el manejo adaptativo formal, sin embargo en el caso de ecosistemas acuáticos de gran escala, estas oportunidades son limitadas. Más comúnmente, se debe hacer énfasis en identificar un rango de posibles mecanismos causales e implementar políticas de manejo que sean robustas a los mecanismos alternativos.

Corrélation et Causalité entre la Pêche et la Gestion des Bassins-versants

Les efforts pour comprendre comment gérer les écosystèmes aquatiques se basent souvent sur les corrélations entre les actions humaines et les impacts sur l'écosystème. Nous sommes souvent mis en garde sur le fait que la corrélation n'implique pas la causalité et que le critère de référence pour identifier des liens de cause à effet sont les expériences de manipulation. L'histoire nous enseigne que les corrélations sont souvent sans causalité et que les responsables ne devraient pas concevoir les politiques en se basant sur la présomption de causalité. Néanmoins, en absence de manipulation, la preuve de la corrélation est peut-être la seule chose disponible. La preuve de la corrélation est plus forte quand (1) la corrélation est importante, (2) elle est retrouvée systématiquement à travers de nombreuses situations, (3) il n'y a pas d'explications antagonistes, et (4) la corrélation est cohérente avec des explications mécanistes qui peuvent s'appuyer sur la preuve expérimentale. Si possible, des expériences de manipulation et la gestion formelle adaptative devraient être utilisées, mais dans les écosystèmes aquatiques à grande échelle ces opportunités sont limitées. Généralement, nous devons mettre l'accent sur l'identification de l'habitat des possibles mécanismes de causalité et identifier les politiques qui sont robustes pour les mécanismes alternatifs.

INTRODUCTION

When natural resources, such as a fishery or an ecosystem, are managed in order to achieve a desired societal goal, it is necessary to change or manipulate certain elements of the resource. Managers change laws, regulations, expenditures, and actions, and each one of these changes will have a distinct effect. Fisheries and watershed management is no different from any other form of management, whether management of a national economy, a manufacturing plant, or a patient in a hospital. To predict the likely outcome of different actions, managers rely on a mixture of historical experience, theory, and intuition. Of these three elements, historical experience is considered the most reliable because the more often alternative actions have been tested in multiple applications, the more confident managers will be in the likely outcomes.

Historical experience can take two basic forms: observational and manipulative. Observational studies are far more common and summarize the historical experience of the relationship between two variables of interest. A classic example is the analysis of spawning stock and subsequent recruitment. Given a history of the size of a spawning stock and the subsequent recruitment, we can estimate how any manipulation of the size of a spawning stock will affect subsequent recruitment. In a summary of historical knowledge of the relationship between spawning stock and recruitment, Myers et al. (1994) concluded that there was strong evidence that reducing spawning stock size below certain levels will lead to reduced recruitment. This relationship between low spawning stock size and low recruitments was observational evidence that supported the move in the United States and other countries to prevent low spawning stock size and increase the abundance of stocks that were at low density.

In this case, the correlation between low spawning stock size and low recruitment has generally been interpreted as being causal, and throughout much of fisheries and watershed management we often assume that correlation means causation. Yet it is also widely accepted that correlation does not necessarily mean causation. Should we then find the strongest correlations and assume causation?

Learning from manipulation is the gold standard in learning from experience, and in fisheries management much has been learned from manipulation. Hatchery managers deliberately experiment with time or size of release, feeds, and rearing densities. Freshwater fisheries managers experiment with stocking times and densities. In both cases, these manipulations are often controlled; that is, alongside the experiment there is another population that is thought to be largely similar but not given the treatment of interest. Any manipulative experiments should ideally have three elements—controls, replication, and randomization of treatments—and the more units available to carry out experiments, the more likely these three elements will be achieved.

As we move to larger spatial scales, particularly in marine fisheries and watershed management, the opportunities for controls and replication are fewer. There are hundreds of marine fisheries and watersheds around the United States, and though they may contain the same species, each one is geographically and environmentally different enough to weaken any comparison or potential for manipulation. Even large-scale meta-analyses involving dozens or hundreds of data sets as in Myers et al. (1994) have their flaws. In response to the Myers et al. (1994) argument that low spawning stocks lead to low recruitment, Gilbert (1997) argued that the causal relationship lies elsewhere. Poor environmental conditions actually cause sustained low recruitment that leads to lower spawning stocks, and increasing spawning stock size by dramatically reducing fishing pressure will not result in higher recruitment. This has recently received support from Szuwalski et al. (2015), who analyzed over 200 data sets and found that environmental regime changes provide a better explanation for changes in recruitment than changes in spawning stock size.

In many cases, fishery managers have only the experience in a single fishery or watershed of interest to guide them as they seek to determine what factors lead to better outcomes. Examining the historical evidence will often point toward a correlation between environmental conditions and the productivity of a fish stock. Then we must ask, if such an environmental condition could be affected by management, will manipulating it lead to better stock size or production?

This is a question of causality. Does the environmental condition really cause a change in stock productivity? Though everyone is familiar with "correlation does not imply causation," correlative studies have often been the foundation for management actions in fisheries and other fields and, in the absence of other information, managers are certainly tempted to assume that correlation at least suggests causation.

The nature of causation is one of the oldest questions in philosophy, and though much of the theory is either abstract or highly mathematical, it is quite relevant to fisheries. In this article, we will review the theory of causation, examine the relationship between correlation and causation, and evaluate the current state of how to approach fisheries management when the major evidence is correlative. To do this, I will review some examples from outside fisheries where correlation proved to also be causation (cigarette smoking and cancer) and where correlation was not causation (hormone replacement therapy and heart disease) and relate the lessons from these studies to fisheries and watershed management.

THEORY

Philosophical discussions of the nature of causation date back at least to Aristotle, who distinguished between different kinds of causes (Holland 1986). Locke (1690; cited in Holland 1986:950) proposed: "That which produces any simple or complex idea, we denote by the general name 'cause', and that which is produced, 'effect.'" This philosophy seems to provide little guidance for managers who must make decisions. Of much more practical use is the statistical literature. Rubin (1974) suggested that randomized trials are by far the most powerful form of evidence and, despite opposition from many of his contemporaries, argued that nonrandomized trials can also be informative. A most important point in using any historical experience is that "trials in the study are representative of other future trials" (Rubin 1974:697). There are two key parts to this assertion: one is often called stationarity, in which relationships from the past will be the same in the future. The second is that the manipulated experimental units, such as fish stocks or watersheds, represent the kind of units that will be managed in the future. However, Rubin (1974) also pointed out that observational studies have advantages over randomized trials because they generally observe systems in a broader set of states than randomized experiments. Experimental units should be as similar as possible, thus representing only a small fraction of conditions in the real world. Finally, Rubin (1974) argued that nonrandomized studies can be very powerful if the analyst can successfully assert that there is only one variable—the one of interest—that may causally affect the outcome.

Though everyone is familiar with "correlation does not imply causation," correlative studies have often been the foundation for management actions in fisheries and other fields and, in the absence of other information, managers are certainly tempted to assume that correlation at least suggests causation.

Hill (1965) evaluated the relationship between correlation and causation in public health and medicine and identified three primary characteristics that provided increased evidence for a causal correlation. The first is strength of association: the higher the correlation, the greater the belief that the factor is causal. The second is consistency: when looking across populations or units of observation, the more the factor is consistently associated, the greater the belief. Finally, there is specificity, which, in the biomedical world, is a lack of other correlations or plausible explanations. The same is true in fisheries. If any other variables also correlate with the effect of interest, there is less specificity and thus weaker evidence for causation.

Holland (1986) developed a model of causal inference almost exclusively for randomized trials, but he also reviewed the literature on nonrandomized trials and observational studies and emphasized that we should look more for the effects of causes, rather than the causes of effects, which fits nicely into the framework of management and decision theory. We want to know what will happen if we manipulate the system (change the causes) more than we want to know what causes the effect.

> *As we know more about the biology of a system, the actual cause may be refined. For example, do bacteria cause disease? Well, yes … until we dig deeper and find that it is the toxins the bacteria produce that really cause the disease; and this is really not it either. Certain chemical reactions are the real causes … and so on, ad infinitum. (Holland 1986:959)*

What the manager wants to know is, if he prevents contact with the bacteria, will there be no disease—not the actual cause of the disease.

The essence of Rubin and Holland for fisheries and watershed managers is summarized in the phrase, "No causation without manipulation" (Holland 1986:959).

CLASSIC EXAMPLES OF CORRELATION

Two examples that highlight the potential and perils of using correlation for public policy are found in the medical literature. That smoking causes lung cancer is now widely accepted, whereas that hormone replacement therapy reduces heart disease turned out to be a false correlation. Worse yet, randomized clinical trials showed that hormone replacement therapy actually increased heart disease.

Smoking and Lung Cancer

During the first half of the 20th century, the incidences of lung cancer increased steadily and various explanations were put forward (White 1990), including cigarette smoking, industrial pollution, smoke from domestic fires, and tars used in road construction. The least persuasive evidence was that the frequency of smoking increased at the same time that lung cancer increased. Stronger evidence was that smokers were much more likely to develop lung cancer than nonsmokers (Doll and Hill 1950). But the strength of the evidence was much criticized. Most interesting, from a historical perspective, were the many papers by Sir R. A. Fisher, perhaps the best known statistician of the 20th century. Fisher (a heavy smoker) argued quite correctly that there were two reasons why the correlation may not be causal.

> *Two classes of alternative theories which any statistical association, observed without the precautions of a definite experiment, always allows—namely, (1) that the supposed effect is really the cause, or in this case, that incipient cancer, or a pre-cancerous condition with chronic inflammation, is a factor in inducing the smoking of cigarettes, or (2) that cigarette-smoking and lung cancer, though not mutually causative, are both influenced by a common cause, in this case the individual genotype. (Fisher 1957:297).*

However, throughout the 1950s and 1960s, evidence continued to increase, including data showing that the incidence of lung cancer was indeed related to the frequency of smoking, that those who quit smoking showed a reduced incidence of lung cancer compared to those who continued, and, finally, the demonstration in laboratory studies that elements of tobacco smoke could induce cancer in laboratory animals. By 1959, the U.S. Surgeon General concluded that "(t)he weight of evidence at present implicates smoking as the principal etiological factor in the increased incidence of lung cancer" (Burney 1959:141). Yet the matter was not altogether laid to rest. In an editorial, the editor of the *Journal of the American Medical Association* said, "A number of authorities … do not agree with his conclusion" (Talbot 1959:162), but by 1964, both the Surgeon General and the *Journal of the American Medical Association* had made public statements that smoking was a health hazard.

Throughout the entire discussions of the 1950s, it was always understood that, unless a causal mechanism was identified, any correlation would be questionable. "The definitive investigations," Berkson (1960:369) wrote, "must come from the biologic sciences, pathology, pharmacology, chemistry, and so forth. … We will not really know whether smoking causes cancer, till we know at least something in a precise way about how it causes cancer." Throughout the 1960s and 1970s, the carcinogenic effects of elements of tobacco smoke were more and more demonstrated in vitro and in vivo (Miller and Miller 1981). Of course, the definitive evidence would have had to come from randomly assigning individual humans to smoking and nonsmoking groups, an ethically indefensible experiment. In experiments with laboratory animals, though, the evidence is strong.

Hormone Replacement Therapy

In his 1966 bestseller *Feminine Forever*, Robert Wilson argued that menopause was a treatable phenomenon and women could take hormones to replace the ones their bodies were no longer producing. Most women would certainly love to avoid consequences of menopause such as hot flushes, weight gain, decreased sexual desire, and masculinization, and when Wilson told them that this could all be avoided with simple hormone replacement therapy (HRT), the news was greeted with great joy. Then the news got even better. Comparisons of women who took HRT with those who did not indicated that HRT reduced heart disease and hip fractures (Grady et al. 1992). Though this evidence was correlative, the lure of a technical solution to an undesirable condition was such that HRT was widely prescribed and used in developed countries.

However, unlike with smoking, randomized trials with HRT were possible and were conducted in the 1980s. Women who sought treatment were randomly assigned to either HRT or a placebo in double-blind trials, and their medical history was followed for a number of years. The not-so-joyous results were published in 1998 (Hulley et al. 1998): the frequency of heart disease was in fact higher in the HRT treatment group, as was the risk of breast cancer, stroke, and blood clots. Moreover, the women on HRT in the earlier, correlative studies had been richer and better educated, had better diets, exercised more, and had better access to medical care than those not on HRT (Lawlor et al. 2004a, 2004b), which accounted for their general better health and counteracted the rather modest negative effects of HRT. The HRT did not reduce heart disease, but it appeared to do so in correlative analysis because those who received HRT were healthier than those who did not receive HRT. Correlation was not causation, but the only way it was identified was by manipulation.

Fish Examples

One holy grail for fisheries scientists has been understanding the fluctuations in the recruitment of fish stocks. We know that survival of eggs and larvae through the first year of life depends greatly on the availability of food, shelter from predators, and for many species favorable currents to either transport larvae to the appropriate habitat or to retain them where they were born. Throughout the 20th century, fisheries scientists looked for measureable environmental conditions that correlate with recruitment success by searching data sets of environmental variables. Shepherd et al. (1984) reviewed 47 studies that had found correlations between environmental variables and recruitment success. Myers (1998:298) looked at each of these stocks again and found that by the mid-1990s only one of the 47 relationships was used by assessment scientists because the correlations that existed in the data up to 1984 had failed to reliably predict recruitment in later years. With more data, the correlations fell apart. The exceptions generally were stocks at the limit of their geographic range, where some correlations with temperature were consistent. Myers concluded that, on the whole, using environmental correlates should be avoided when choosing management actions: "The rarity of the use of environment/recruitment correlations is clear evidence against their general usefulness in assessments. Even if an

environmental variable is important, it does not mean that it is key to the management of the fishery."

A different way of looking at what determines fish recruitment has been analysis of the relationship (i.e., correlation) between spawning stock and recruitment. Ricker's classic 1954 "Stock and Recruitment" is one of the most cited papers in the fisheries literature, and for many agencies the spawner–recruit relationship is one of the foundations for setting management policies. Though conceptually one can think of a spawning stock as being manipulated, in fact, almost all stock recruitment analyses look for a repeatable relationship (again, a correlation) between number of spawners and number of recruits. Myers et al. (1994) presented a meta-analysis of spawner–recruit data of 72 finfish stocks. They concluded that there was indeed evidence that lower spawning stocks produced lower recruitment, and one could identify spawning stock thresholds below which lower recruitment could be expected. They said that their analysis "should help dispel the widely held notion that observed recruitment is usually independent of spawning biomass. … The mean recruitment is generally greater above the threshold than below" (Myers et al. 1994:203). Their argument was that higher spawning stock size on average was associated with higher recruitment; therefore, higher spawning stock size *caused* higher recruitment. This would be a logical assumption. However, should the opposite be true, rather strong density-dependent processes must be operating.

On the other hand, we do know that there are lots of mechanisms leading to strong density dependence, and Gilbert (1997) proposed an alternative hypothesis. He suggested that recruitment is largely driven by periodic shifts in environmental factors and that recruitment will be high when conditions are good and poor when conditions are bad. A sustained period of good recruitment will result in a sequence of high recruitments, which in turn leads to high spawning stock. Conversely, a period of poor environmental conditions will result in a series of poor recruitments that will lead to low spawning stocks. Thus, Gilbert (1997) proposed that low recruitment causes low spawning stock and high recruitment causes high spawning stock and that when spawning stocks are low, increasing them is unlikely to increase recruitment.

This interpretation has not been generally accepted in fisheries management agencies, although a greater number of assessments now include periodic shifts in stock productivity when calculating management reference points. However, in support of Gilbert's perspective, Szuwalski et al. (2015) looked at several hundred data sets both of spawner–recruit relationships and biomass-to-surplus-production relationships and concluded that the behavior of far more stocks is consistent with period regimes of good and bad conditions rather than biomass leading to stock productivity.

There have been at least two deliberate manipulative experiments in Pacific salmon. Based on analysis of spawner–recruit data, the number of Sockeye Salmon *Oncorhynchus nerka* allowed to spawn in Rivers Inlet, British Columbia (Walters et al. 1993), and in the Kvichak River, Alaska (Eggers and Rogers 1987), were increased. In both cases, recruitment did not increase and the manipulative experiments failed to demonstrate causality.

Moving from single species to ecosystems, there is increasing use of mechanistic models as a way to evaluate ecosystem impacts—Ecopath with Ecosim (Christensen and Walters 2004) and ATLANTIS (Fulton et al. 2005). These models do not rely on correlation in their construction but do rely on correlation of their predicted values to observed data as informal validation of the mechanisms. Thus, though they represent highly complex hypotheses, they have the same characteristics of simple (A causes B) hypotheses with respect to validation of predicted impacts of management policies. When correlative evidence is weak, manipulation is the strongest test.

Some of the counterintuitive results can be explained by the work of George Sugihara and his students, who emphasized the nonlinearity of ecological systems (Glaser et al. 2011; Sugihara et al. 2011) and used simple nonlinear models to demonstrate both that lack of correlation does not mean lack of causation and that correlation does not mean causation. In these models, it is easily demonstrated that, although species A feeds on species B and fishing down species A should result in an increase in the abundance of species B, observation of time series data will often show no correlation between A and B. In summary, the fisheries literature suggests that in general correlation has not meant causation.

Management Approaches

If we take "no causation without manipulation" literally, most fisheries management reaches a dead end because almost all management systems have enough unique features that make replication and control impossible. However, the theory of adaptive management (Walters 1986) can provide guidance on how to proceed. Only when there is the potential for spatial or temporal replication can true experimental designs be established and experiments performed. But these cases are unusual enough that adaptive management that involves noncontrolled experiments may provide the best approach. Right away, a fishery manager has the key advantage of little concern with any of the details of causation. What matters are the effects of manipulation. A manager wants to know whether a system will respond differently to implementation or nonimplementation of a management action.

Adaptive management should begin with modeling that "is intended to serve three functions: (1) problem clarification and enhanced communication among scientists, managers, and other stakeholders; (2) policy screening to eliminate options that are most likely incapable of doing much good, because of inadequate scale or type of impact; and (3) identification of key knowledge gaps that make model predictions suspect" (Walters 1997:2). The key result of the modeling step is identification of alternative hypotheses about the dynamics of the system and an evaluation of the degree of support for each hypothesis. Guided by Hill (1965), we recognize that the more competing explanations there are, the less support correlation provides for causation. The second step in adaptive management is the design of management manipulations. With the help of modeling, the expected value in terms of both costs and benefits of alternative actions can be calculated for any set of management actions. This is a form of risk analysis, and the product should be an experimental design. The third step is implementation, and the fourth is evaluation. Most well-designed adaptive management plans will use information gained early in the process to guide later management actions.

It is in the modeling and design stages that the correlation issue will need to be confronted. If alternative models that allow for noncausation are not considered, correlation will likely be interpreted as causation. For instance, in the original work on fisheries adaptive management, Walters and Hilborn (1976) explored the uncertainty in spawner–recruit relationships but never considered Gilbert's hypotheses about changing

recruitment causing changed spawning stock. Thus, they never questioned that there was a relationship between spawning stock size and recruitment; they only tried to reduce the uncertainty in what that relationship was. In the light of what we know now, a similar analysis would consider a much broader range of hypotheses.

A widely praised adaptive management program aims to maintain and restore valuable resources in the riparian zone of the Colorado River in the Grand Canyon (Meretsky et al. 2000; Walters et al. 2000). One experimental treatment was "mechanical removal" (i.e., electrofishing) of Rainbow Trout *O. mykiss* and Brown Trout *Salmo trutta* because they are known to eat juveniles of the endangered Humpback Chub *Gila cypha*. A major removal treatment started in 2004 and was followed by substantial increases in chub recruitment. However, in the same year, the Colorado River warmed up considerably due to low reservoir stages at the head of the canyon in Lake Powell, which led to releases of water from in and above the reservoir thermocline. Such warmer water was also expected to benefit chub by reducing vulnerability to predation and increasing juvenile growth rates to sizes where predation is lower.

Warm reservoir releases and low trout abundances persisted until 2012 and only in 2013 did the system start returning to a high trout–cold water state. Only now, when cold water conditions are likely to last for a few years, is it possible to replicate the initial trout treatment to see whether trout reduction alone is sufficient to insure good Humpback Chub recruitment. This is a great example of why replication, even though it can be very slow to achieve, is critical in large-scale experiments. This kind of scenario is likely to be typical of ecological systems in general.

Thus, though adaptive management does hold promise for resolving uncertainty, beware of its many pitfalls. Most of the problems are rooted in the design of replicated experiments, and we have to admit that for large-scale fisheries and watersheds the difficulties of randomization, replication, and controls are often just too overwhelming. The net result is that we will almost always make decisions faced with great uncertainty and have to recognize the range of possible causal mechanisms.

SUMMARY

Fisheries and watershed management is decision making under uncertainty, and when actions are proposed, their consequences should be evaluated across as many possible hypotheses as possible. The weight of evidence of each causal pathway needs to be assessed, and ideally, historical data on randomized manipulative experiments will be the strongest form of evidence. Unfortunately, we will often only have observational evidence, and the evaluation of the support for alternative causal pathways will almost always be subjective. Though strong correlation is good evidence (Hill's strength of association), it should not be the primary factor in determining strength of evidence for any causal pathway. Equally important are consistency (when factor X changes, output Y changes consistently) and specificity (are there alternative hypotheses?). The plausibility of mechanisms adds support for any causal pathway.

Plowright et al. (2008:424) warned that "in practice, looking for positive evidence for a favored theory can be a useful first strategy, but can harden into a bias that prevents the evaluation of alternate explanations for patterns that surface as more data become available" and emphasized that one should adopt the method of multiple working hypotheses and evaluate the evidence for each of the hypotheses simultaneously—a method also recommended in Hilborn and Mangel (1997). Perhaps the greatest error would be to find the strongest correlation, assume that it is causal, and ignore other possible causal mechanism.

The most useful conclusion from the range of studies in ecology and other fields is that maintaining multiple working hypotheses throughout the analysis is the most important guidance, that managers should identify policies that are robust to the range of alternative hypotheses (Schindler and Hilborn 2015), and that correlative evidence should be regarded a priori as weak support for causation.

ACKNOWLEDGMENT

I thank Carl Walters for his perspective on this issue and the specific adaptive management examples he provided.

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2016 AFS International Fisheries Section Fellow Award

Applications Due March 6, 2016

Attention Students and Young Professionals:

The International Fisheries Section (IFS) of the American Fisheries Society (AFS) is excited to announce the 2016 International Fisheries Section Fellow Award. The Fellow will represent the International Fisheries Section of AFS by presenting a poster or oral presentation at the 2016 Fisheries Society of the British Isles (FSBI) annual symposium, July 18-22, Bangor University, United Kingdom. The theme of the 2016 FSBI annual symposium is "Fish, Genes, and Genomes: Contributions to Ecology, Evolution, and Management." For more information go to: fsbi.org.uk/conference-2016/symposium-theme-2.

Benefits:

- FSBI presentation (a presentation slot has been reserved for the recipient of this award)
- Up to $2,100 (USD) of reimbursable travel expenses associated with attending the FSBI annual symposium
- Meeting registration, dormitory accommodations, and meal stipend while at the symposium
- 1 year of membership to FSBI
- 2 years of leadership in the IFS

Eligibility:

This award is open to all student and young professional members (within three years of graduation) of the AFS, especially those working on topics of international interest or those associated with the symposium theme. However, we encourage all interested student and young professional members to apply. Current membership with AFS is mandatory to apply for and receive this award.

Application:

- To apply, please submit the following information as a single PDF document to the committee chair of the IFS Fellow Program, Zach Penney (zpenney@critfc.org), and the deputy committee chair, Jeremy Higgs (j.higgs@usm.edu), by **March 6, 2016:**
- Letter of interest (please include information about your involvement in AFS)
- Proposed FSBI presentation title and abstract
- Concise resume
- Proposed travel budget (i.e. airfare, ground transportation, etc.); please specify availability of supporting funds you may have from other sources if budget exceeds allowance

Obligations:

The awarded Fellow is expected to fulfill the following obligations:

- Attend the 2016 FSBI annual symposium and present an oral or poster presentation
- Submit to the committee chair a travel report and photographs for print publication by August 14, 2016
- Submit travel receipts to the committee chair by August 14, 2016 for expense reimbursement
- Attend the AFS meeting and IFS committee meeting in Kansas City, Missouri, USA, August 21-25, 2016
- Act as a site host at the Kansas City AFS meeting for the FSBI Fellow
- Serve as the deputy committee chair of the IFS Fellow Award in 2017 and the committee chair in 2018