Economic Experiments in Recreational Fishing Studies

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Abstract Experimentation in natural science is commonplace, and the reasons and methods for undertaking it are well understood. The use of experimental methods has risen remarkably over the last 25 years in the field of economics and has had a

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tremendous impact on the way economic research is carried out. This raises the question; how can economic experiments advance our understanding of recreational fishing. Here, we argue that many opportunities and benefits exist for the application. There have already been several successful experiments (in the lab or the field) to understand the drivers of recreational fisher behaviour. Despite these efforts, there remains a raft of opportunities to use experiments to help understand cause and effect in terms of recreational fisher behaviour. In this chapter, we provide a taxonomy of economic experiments and highlight their application to recreational fisheries. We sketch how controlled and uncontrolled interventions in recreational fisheries are learning opportunities to better understand fisher behaviour and social-ecological dynamics. Further, we highlight practical aspects of experimentation that will help its implementation and suggest opportunities for future experiments in recreational fisheries.

Keywords Experimental economics · Lab-in-field · Laboratory experiments · Natural experiment · Social science

1 Experimental Approach to Investigation of Recreational Fisher Behaviour

Recreational fisheries can be understood as social–ecological systems with complex feedbacks within and between the ecological system (e.g., fish biomass) and social system (e.g., fisher or manager behaviour) (Arlinghaus et al. 2017). The complex interactions between the social and natural systems mean that emergent outcomes are not easily attributable to any one social–ecological system's component but arise from multiple and simultaneous feedbacks and system properties (Matsumura et al. 2019; Golden et al. 2022). Such emerging complexity makes it difficult to understand causes and effects in the real world (Ferraro et al. 2019). Experimental methods are fruitful approaches to establish causal pathways and corresponding impacts and help unpack some of the complexity inherent in coupled systems (Janssen et al. 2010; Poteete et al. 2010). Experiments are particularly useful to observe and understand behaviour of recreational fishers (Stoop et al. 2012; Ward et al. 2013). However, even though the use of experiments in ecological research of recreational fishing is relatively well developed, many opportunities remain to better understand the social aspects, causal mechanisms, and behavioural responses. For example, it is well known that there are differences in the motivation to participate in the commercial and recreational fishing sectors (Mackay et al. 2020). By using experiments, the difference between recreational and commercial fisher responses to traditional fisheries management interventions might be better understood, and unexpected management outcomes may be averted. This chapter discusses how insights from experiments can help inform a recreational fishing research agenda.

An experiment is a research method used to test hypotheses in a controlled setting. Defining features of an experiment are (i) a manipulation of key variables while controlling possible confounders and (ii) the presence of a control or reference group. Often, the researcher tries to manipulate a variable through an intervention (e.g., the introduction of a licence fee) and asks how that intervention affects an outcome of interest (e.g., days of fishing). The presence of a control group is needed to construct a credible counterfactual that tells the researcher how the outcome variable would have developed in the absence of the intervention. For example, if the introduction of a licence fee coincided with very bad weather conditions, it would not be possible to attribute a drop-in fishing activity to the intervention, as well as the change in weather. Therefore, it is crucial to have a design that creates a credible counterfactual, such as control sites without interventions sampled simultaneously and over time. Especially in complex field settings, counterfactuals are necessary to control for temporal effects unrelated to the intervention.

Depending on the hypothesis, there are different degrees to which a research design resembles reality and the amount of control one has over the setting. In other words, an intervention may be controlled and carefully designed or just "happen" outside the control of the research team studying it. For example, it may be the case that a management agency implements a licence fee without any involvement of a research team and without considering and measuring control sites. The challenge then lies for researchers to establish a credible counterfactual, e.g., a constructed "synthetic" control (Abadie 2021), that helps understand the impact of the intervention over time. If the intervention happens outside the control of the research team, it is known as a "natural experiment." These may be distinct events, brought about by policy change (e.g., access restrictions), natural disasters (e.g., floods), but also wider societal disruptions (e.g., COVID-19) that can inform how such events affected key variables of interest. Including natural experiments in our definition of "experiments" may be confusing to some—however, we believe they provide key opportunities for opportunistically inferring cause and effect in complex fishery systems. Confusion may arise because there is no agreed-upon definition of a natural experiment in social science (Leatherdale 2019), but it is generally accepted that it means that the conditions (i.e., treatment variables) surrounding an intervention are not, or cannot be controlled by the researcher (e.g., natural phenomena like COVID or a tsunami). Natural experiments also include, e.g., management interventions that are not controlled by the researcher, but where the impacts are observed and analysed (Fujitani et al. 2018). Because the treatment variable in natural experiments cannot be manipulated by the researcher, they can also be described as observational studies (Dunning 2008). However, a degree of experimental "control" remains possible through, e.g., site selection (Diamond 1983), and the term natural experiment applies when something fundamental changes that affects key variables of interest. Because this type of analysis of natural experiments is not uncommon in behavioural fisheries science (Pita et al. 2021; Ryan et al. 2021; Sawynok and Sawynok 2021), we include natural experiments in our definition of experiments.

In this chapter, we discuss the full range, from controlled experiments in a laboratory where people's choices (which can be students or the group or people of interest) are tested and observed under specific, often abstract, conditions (Smith 1982) to natural experiments (see Sect. 2) without any controlled manipulation of the research subjects, but which can capture more realistic decision making. This chapter provides an overview of the use of experiments in recreational fisheries and provides a taxonomy of experiments. Each of the experimental settings and the approaches has advantages and disadvantages. The chapter further provides best practices and mentions potential caveats for researchers. For example, when designing an experiment, it is crucial to ensure that the experiment itself is designed and communicated in a way that it is unambiguous to participants. The information provided to participants should be clear, and the context and problem should be understood by all in the same way. Also, experiments involving human subjects require special preparation. Of particular importance are ethical issues that can arise within the research.

1.1 What Is an Experiment?

Even though we have indicated above that we discuss the full spectrum of experiments from laboratory-based (controlled) to natural experiments, the boundary between what is an experiment and what is a “non-experiment” remains blurry. What distinguishes an experiment from other quantitative observational methods that are also testing hypotheses? Crucially, observational studies tend to use naturally occurring variation of key variables to infer relationships. For example, temperature or precipitation may have an impact on fisher participation at a given site. In contrast, experiments tend to measure the impacts of specific interventions on a certain group and compare the results with a control group. These specific interventions may also be naturally occurring events (a natural experiment), but often involve treatments that are applied to randomly selected treatment and control groups to measure the response. For example, in a field experiment where the effects of access fees on fishing effort are tested, different access fees may be randomly assigned to otherwise-similar waterbodies to test the hypothesis that higher access fees will reduce fishing effort. Fishing effort before and after the imposition of access fees can then be compared with those control sites using statistical tools to evaluate support for the hypothesis.

At the foundation of experiments lies the scientific method (Bacon 1818), which goes through several steps to progress from formulating one or more hypotheses to accepting or rejecting them. The hypotheses that are tested in the experiment must be solid (i.e., quantifiable and rejectable) because the experiment cannot make up for flawed assumptions or faulty premises (Webster and Sell 2014). If a hypothesis is tested using an experiment, this will lead to support for the hypothesis (i.e., rejection of the null hypothesis of no effect) or rejection of the hypothesis (i.e., support for the null hypothesis of no effect).

The key to experimental research is the principle of replication, which means that the same experiment should give the same results if conducted under identical conditions. In practice, this is often a question of internal and external validity. Internal validity implies that the results obtained in the experiment can be correctly

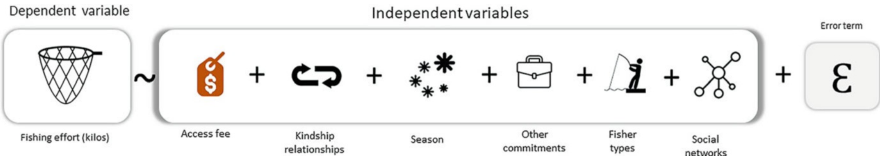


Fig. 1 Hypothetical example of a relationship that might be explored in an experiment (example shown is between fishing effort [the independent variable on the left-hand side of the ~ sign], and access fee and some social and demographic variables [the explanatory variables on the right-hand side of the ~ sign], plus an error term; in this example one of the independent variables [access fee] may be one that is manipulated; icons from https://www.flaticon.com)

attributed to the intervention, rather than a confounding factor. If a setting is very realistic, it may very well be that something happening outside the experiment (e.g., a public policy implemented at the same time) may affect behaviour in the experiment (e.g., the tendency to cooperate). External validity pertains to the question of how generalizable the results are. What do results obtained from an experiment conducted at a small lake in Germany say about recreational fishing in Germany more generally? And can we say anything about recreational fishing in Europe or globally?

Experiments are used to explore causal relationships and to see how manipulation in independent variables influences the dependent variable of interest. There can be more than one different independent variable (see hypothetical example in Fig. 1). Some of the independent variables (like access fee in Fig. 1) may be manipulated whilst the others are not. To test more than one independent variable, it is necessary to test the effect of each of the independent variables individually (e.g., the effect of a licence fee on fishing effort and the effect of season on fishing effort) before testing combinations of two independent variables (Kimmel et al. 2021). To establish causation, changes in the independent variable or variables of interest must be independent of confounding effects. Confounding effects are those other uncontrolled variables that influence the relationship between the dependent and independent variables. For example, comparisons of recreational fisher behaviour between stocked and unstocked lakes (Silwal et al. 2023) may be confounded by previously poor fishing conditions that led to stocking as a management intervention or by a particular fisher type heavily drawn to one lake type being more responsive than others to the intervention.

The gold standard for establishing this independence is through random assignment of subjects (e.g., recreational fishers or their communities) to treatment groups, ensuring all groups are comparable. In this experimental context, there is at a minimum one treatment and one control group, but ideally replication is necessary for each of the two. To avoid confounding effects, the subjects are (ideally) assigned randomly to the different treatment groups to avoid systematic differences between the groups. The treatment group receives a manipulated independent variable (e.g., a higher licence fee) than the control group. The more manipulations one wants to test, the more treatments are necessary. Only then is it possible to claim that the change in the independent variable causes the change in the dependent variable (e.g., increased fishing effort). Of course, multiplicative or interactive effects are possible among the independent variables (e.g., licence fees only affecting effort at otherwise low budget conditions), necessitating even more treatments that combine independent variables.

An example of a study from recreational fisheries that uses treatment groups is by (Fujitani et al. 2017) who randomly assigned recreational fishers organized in angling communities (clubs) to two groups: (i) the control group receives a passive lecture about the ecological effects of stocking, whereas (ii) the treatment group joined a participatory adaptive management program evaluating the effects of stocking in their fishery. A third placebo group received passive lectures, but not on the topic (stocking) of interest, being the control group. The use of this experimental setup allowed the authors to conclude that the treatment group (those who joined the participatory adaptive management program) retained more knowledge about ecological risks involved in stocking than the control group. Another example of the use of treatment groups can be found in the testing of enhanced enforcement actions for northern pike (*Esox lucius*) protected sizes in Alberta (Walker et al. 2007). In partnership with enforcement officers, the study compared deterrence of noncompliance at treatment lakes, which received enhanced enforcement actions, with control lakes subject to normal enforcement patrols. The crux of these experimental approaches is that assignment to treatment groups should be random for experiments to be carried out as randomized control trials (Banerjee 2020). Using the example from Fujitani et al. (2017), if participants can choose whether they want to be part of an adaptive management program, it would be natural to expect that the more motivated individuals will do so. In such cases, participants "self-selecting" into treatments would then drive most of the experimental results, rather than the intervention itself. Therefore, it is crucial that the selection of participants into treatments is carried out at random. It should be noted that such randomization, which is often very clear to researchers, is typically not intuitive for practitioners and at odds with how fisheries are usually managed. For example, organizers of a workshop are more inclined to invite committed users, rather than random ones. Also, management authorities hardly implement regulations at random but think carefully about which locations an intervention may be successful or most needed. The non-random placement of management regulations or area-based management tools (e.g., marine protected area) renders many of these actions quasi-experimental and non-random, with a high possibility for bias. Matching techniques or synthetic control methods may still be used to identify credible counterfactuals and allow causal inference of policy impacts (Ferraro et al. 2019; Medoff et al. 2022; Nowakowski et al. 2023).

In an experiment, researchers often manipulate one or more independent variables and measure the effect on one or more dependent variables. For example, the use of the catch-and-release method (the dependent variable) would be left free to vary, whereas the degree of information given to individual recreational fishers about the biological outcomes of catch-and-release is varied by the researcher (the independent variable). Though researchers may wish to study the effect of several variables at the same time, this reduces the ability to identify effects due to a loss of statistical power, as it is usually infeasible to increase treatment groups to a large degree. For experiments, the number of treatments increases with the number of variables one wants to vary. A full factorial design that can identify all interaction effects for three independent variables requires at least eight treatments ($2 \times 2 \times 2 = 8$), with multiple individual subjects (i.e., recreational fishers) in each treatment or control group. However, to identify the main effects would require varying each independent variable once from a common baseline, which would result in three additional treatments (see also discussion on Balanced Incomplete Block Design in choice experimentation, Young (2019)). The strength of experiments is to identify singular mechanisms, rather than simulating all the complexities of particular settings (Webster and Sell 2014). The topics analysed in experiments can therefore appear to be quite narrow by nature, but still insightful in terms of cause-and-effect inference.

The recreational fishery systems are often highly regulated and constantly exposed to various forms of interventions. Interventions provide learning opportunities, especially if experiments are carried out in a controlled way. Field-based experiments have taken advantage of new fish stocking initiatives to learn about the behavioural response of fisher effort to fish abundance (Lathrop et al. 2002; Mee et al. 2016) and whether fishers cooperate for the common good or not (Stoop et al. 2012). Also, experiments have helped understand the impacts of environmental factors (Golden et al. 2021) and fish population size (Askey et al. 2013; Dassow et al. 2020) on recreational fishers catch rates. Generally, experimentation provides excellent learning opportunities for adaptive management (Lester et al. 2003; Hansen et al. 2015).

Many examples exist of observational studies where fisher behaviour is observed across space and time, and site choice studies are an example of such natural experiments. Many fisheries are made up of multiple discrete patches across landscapes (i.e., lakes, streams, marine reefs, etc.), thus fishers must make site choice decisions involving travel time and travel costs (Post et al. 2008; Hunt et al. 2011; Camp et al. 2018; Wilson et al. 2020). Time series with a perturbation of site characteristics have identified site choice behaviour with aggregate measures of total fishing effort in experimentally opened fisheries (van Poorten and Post 2005) and regulation changes from harvest to catch-and-release fisheries (Johnston et al. 2011). In their review of where people fish, travel-related times and costs, as well as catch-related measures of fishing quality, were consistently demonstrated to be important in fisher site choice behaviour (Hunt et al. 2019). Other aspects of fisher behaviour have also been investigated through observational data. For example, harvest or release decisions, trip context (i.e., day or multi-day trips), and compliance with regulations was found to vary with travel time or costs and fisher typologies (Parkinson et al. 2004; Hunt et al. 2011; Ward et al. 2013; Kaemingk et al. 2019).

1.2 A Taxonomy of Experiments

When we consider the role of experiments in understanding the behaviour of recreational fishers, there are many different settings in which this can take place (i.e., experimentation in a laboratory setting, in the field, and the hybrid lab-in-the-field experiment, or a natural experiment) (Fig. 2 and Table 1). Experimental settings occur along opposite gradients of control and realism (Fig. 2). Therefore, research takes place along a gradient of controlled experimental methods situated in a laboratory to natural experiments (observational studies) undertaken in the "wild." Laboratory experiments allow the researcher complete control of the independent variable and are therefore internally valid (i.e., causation) can be supported. It can sometimes be challenging to generalize the results of lab experiments to the real world—even thoughtfully designed ones (e.g., Stoop et al. 2012). This is especially pertinent after finding that lab experiments involving human participants heavily rely on participants from "Western, Educated, Industrialized, Rich, and Democratic (WEIRD)" societies (Henrich et al. 2010). External validity, or generalizability to "real world" systems, is a strength of natural experiments. Real-world data sources

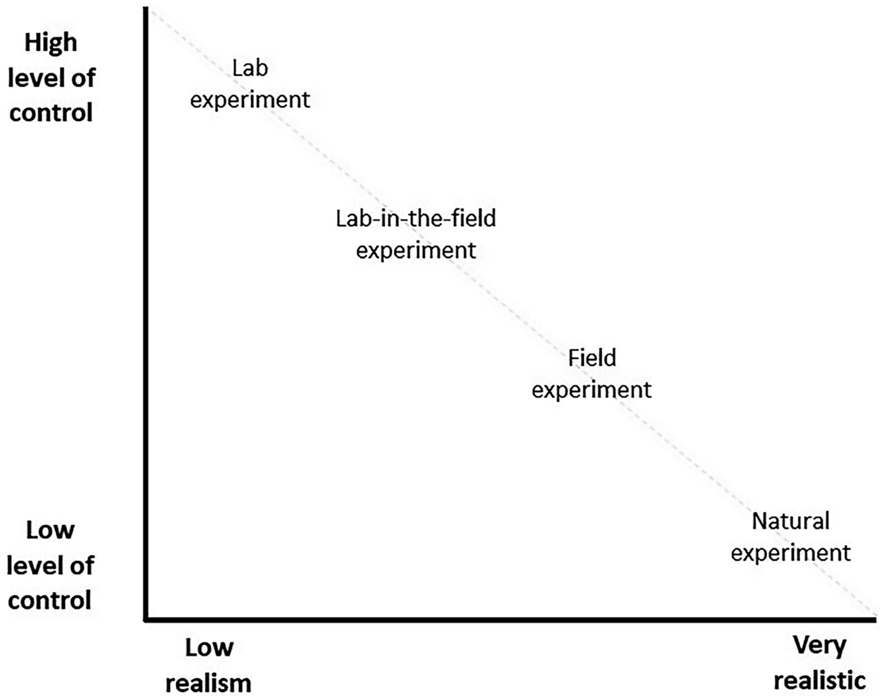


Fig. 2 Schematic illustrating the gradients of control and realism between different experimental settings

Table 1 Different types of experimental settings. (Based on Harrison and List 2004, Viceisza 2016, Gneezy and Imas 2017)

	Lab experiment[a]	Lab-in-the-field experiment[a]	Field experiment[a]	Natural experiment
Management (control)	Experiment conducted under highly controlled conditions	Control over conditions and ability to make direct comparisons between different populations and contexts	Experiment cannot control extraneous variables (different gradients of control)	No control over the independent variable as it occurs naturally in real life
Environment (realism)	Conducted in laboratories (i.e., environments that are abstract from the natural setting) Imposed	Conducted in the natural environment of the participants Imposed	Conducted in the everyday (i.e., real life) environment of the participants Imposed	Conducted in the everyday (i.e., real life) environment of the participants Natural
Participants (population)	Narrow set of participants, typically university students Artefactual = with a nonstandard subject pool Framed = field context in the information set	Targeting the theoretically relevant population but using a standardized, validated lab paradigm to perform tasks that are not part of their day-to-day environment	Typically use a non-student theoretically relevant population that is not aware that their decisions are being studied	Same as field experiments participants do not know that they are in an experiment
Comments	Results obtained for the respondent population may not be representative of behaviour in wider population	More confident that the results are applicable to the relevant context	Confidence that the results are applicable to the relevant context but extraneous variable influence	Natural experiments occur opportunistically and require random or "near-random" sorting into treatment groups.
Resources (design & implementation)	(Janssen et al. 2010) (Guala 2005) (van Soest 2013) (Poteete et al. 2010)	(Gangadharan et al. 2022) (Viceisza 2012, 2016)	(Glennerster and Takavarasha 2014) (Banerjee 2020)	(Dunning 2008, 2012) (Huntington-Klein 2021) (Cunningham 2021)

Note: Variation in control and realism is continuous. For instance, it is not that all field experiments are highly realistic and unable to control extraneous variables; rather, there is variation in realism and control

[a] Referred to elsewhere as a controlled experiment

are messy in that they are prone to multicollinearity, and causation, or internal validity, can only be established if a credible counterfactual can be constructed (e.g., Fujitani et al. 2012).

To test hypotheses in recreational fisheries and to make inferences that may be generally applicable to fisheries in many contexts, experiments and observational studies along the full spectrum of gradient of control and realism (Fig. 2) are necessary. Gold standard experiments with replication, controls, and randomization of treatments in a fisheries context have high internal validity, but these standards are frequently unachievable (Hilborn 2016). Experimental results, however, must demonstrate their external validity through effective choices of study participants and of explanatory variables that correspond with drivers of change in multiple fishery systems (Hilborn 2016). Controlled experiments benefit from being completed in conjunction with monitoring of "real world" systems (Lynch et al. 2021; Lennox et al. 2022). Other methods of inference, such as theoretical models or comparative studies, can also help to validate and provide context for experimental studies.

Natural experiments leverage uncontrolled changes that "naturally" assign random or pseudo-random treatment groups (Dunning 2008). Not all interventions can be studied through field experiments; e.g., local authorities may be reluctant to vary user fees or start charging users in the first place to enable different treatment groups. Also, some behaviours such as noncompliance are notoriously difficult to study in the field (Arlidge et al. 2023), because any attempts to measure non-compliance lead to behavioural change (Boonstra et al. 2017, Diekert et al. 2021, but see Walker et al. 2007). In such a case, a lab experiment may offer valuable insights. Laboratory experiments allow researchers full control of the study environment, the selection of treatment groups, and the application of treatments or independent variables. Many lab experiments in fisheries use experimental economics methods in which participants are presented with a task where they are incentivized to maximize a financial reward (often corresponding to a modest payout after the experiment) within specific rules and a highly controlled experimental setting (Guala 2005). The goal of this strict control is to avoid unknown confounding effects (omitted variables) introducing bias to the experimental results. In these experiments, the greatest unknown factor is the participants themselves. Participants who are distracted, confused, or even over-cooperative (i.e., by "helping" run the experiment) may bias the results. Therefore, in addition to careful recruitment of (to the extent possible) a random selection of participants, effectively introducing and explaining the rules of the experiment to participants is of critical importance. Mackay et al. (2020) present a laboratory experiment utilizing a common-pool resource game. Participants (who in this case were students—not fishers) were incentivized to harvest fish in the context of a social dilemma, where higher individual catches would increase their payment, but higher total group catches would decrease their payment. This experimental approach allowed the testing efficacy of "nudges" leveraging social norms versus the effects of low- and high-probability enforcement of regulations, which is difficult and expensive to test in the field (but see Walker et al. 2007). However, trade-offs of experimental approaches include a reliance on student participants in some studies (as in Mackay et al. 2020) and the unavoidable artificiality of a laboratory environment, potentially compromising the study's external validity.

One way to increase external validity is to combine a laboratory approach with a field approach (Stoop et al. 2012) or adopt a lab-in-the-field approach (Table 1). Though lab experiments sometimes rely on students as a study population, lab-in-the-field approaches take, as the name suggests, the lab to the field. This connects the controlled setting of the lab (or the less-controlled setting of a field site) with the study population of interest. One example is the multi-method lab-in-the-field approach to experiments conducted by Stoop et al. (2012) to observe whether classical economics could predict the cooperative behaviour of participants engaged in a social dilemma. For one lab experiment, participants were recreational fishers rather than students (unlike a typical laboratory experiment), and they were aware that they were taking part in an experiment (unlike a typical field experiment). The lab experiment consisted of a social dilemma game where, on pen and paper, participants could choose to cooperate by “harvesting” fewer fish to increase the group’s payout (see Chapter on Behavioural Economics). In addition to economic experimental methods, lab-in-the-field approaches can also include (and study) recreational fishing behaviour in practice. For example, Stoop et al. (2012) additionally investigated cooperation behaviour when decisions were followed up by actual fishing behaviour at a pond with a known fish population, and they found no evidence of cooperative behaviour as was found in the lab experiment. By increasing the realism of the experiment, the researchers enhanced the study’s external validity and detected decision patterns that are predicted by classical economics (Noussair et al. 2015) further expanded the experiment’s realism by making the game dynamic and including feedback between overharvesting and reduced resource abundance. (Monk and Arlinghaus 2018) examined the relationship between fisher skill, their ability to locate fish, and their catch rates as recruited participants fished an experimental lake for perch (*Perca fluviatilis*), tracked by an acoustic telemetry system, and fisher behaviour using GPS on boats. Each of these experiments investigated recreational fishery dynamics that were relevant to a broad range of systems by implementing internally valid experimental methods within a naturalistic setting.

Field studies, in contrast to lab-based and lab-in-the-field experiments, can entirely avoid the trouble of recruiting a representative group of participants because they observe the response of fishers to experimental changes applied to their own fishery system. These fishers are unaware that they are part of an experiment and therefore present the greatest degree of realism in their responses. In field experiments, researchers apply interventions (Fig. 3) such as regulations, access fees, information, or stocking to selected waterbodies. Particularly critical in these studies is the appropriate selection of sites and the assignment of their treatments. Mee et al. (2016) estimated the responses of catch rate and fishing effort to a range of stocking densities in 34 lakes. As a result of this experimental design and the use of statistical models to account for temporal trends, regional effects, and differences in lake accessibility, this study could independently evaluate the effects of stocking density on fishing effort to identify optimal stocking densities. These sorts of experimental studies to evaluate the effects of management actions have been identified as a key research gap in recreational fisheries (Holder et al. 2020).

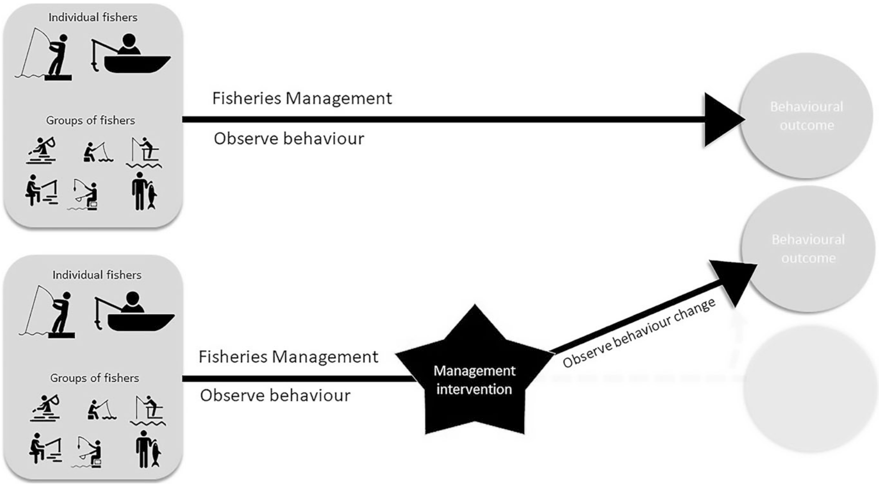


Fig. 3 Schematic diagram of fisher behaviour observed under "Normal" fisheries management (base case or counterfactual; panel a) and a management intervention is implemented, and fisher behaviour change is observed (panel b; icons from https://www.falticon.com)

Not all experiments are explicitly designed. In some cases, naturally occurring "interventions" can be opportunistically leveraged to investigate the effects of the naturally occurring change. Natural experiments can have different "treatments"—but unlike lab-in-the-field experiments, they do not have an active control and need a lot of care to statistically control for possibly confounding variables (Cunningham 2021; Huntington-Klein 2021). Correlation of independent variables may obscure mechanisms of causation. Ineffective statistical design can lead to confounding of the treatment with other site characteristics (Ferraro et al. 2019). Natural experiments are a common approach to learning about recreational fisher behaviour, and they can provide unique opportunities for deductive reasoning in natural, field settings. A controlled experiment faces very different challenges from a natural experiment. Design is key in controlled experiments; the experiment must be carefully set up to allow the desired causal claims, whereas the analysis is relatively simple. In natural experiments, there is no (apparent) design, but the researchers must find a convincing causal identification strategy. Definitions of natural experiments vary by author and field, spanning from studies leveraging naturally occurring change that is applied to participants or sites "as if" randomly (Dunning 2008), to observational studies utilizing careful selection of sites to maintain controls (Diamond 1983), to observing sites or participants before and after the effects of interventions that are not controlled by the researcher (Leatherdale 2019). For example, Klatt et al. (2014) investigated fishing participation before and after the introduction of viral hemorrhagic septicemia (VHS) in different management regions with different regulations. Similarly, studies of global events such as the COVID-19 pandemic have detected changes in harvest (Bunt and Jacobson 2022) and fishing effort (Ryan et al. 2021; Trudeau et al. 2022; Britton et al. 2023) before and during the height of the pandemic. Other studies have successfully investigated fisher behaviour by investigating how fishing effort responds to a variety of drivers, including regulations (Whitehead et al. 2011), facilities, travel time or cost (Post et al. 2008), stocking rate (Fayram et al. 2006), or regulatory changes (Beard et al. 2003).

2 Choosing the Appropriate Approach to Measure Preferences

Often, experimental research in recreational fishing tries to understand recreational fishers' behavioural choices and preferences. Such information may be obtained through stated or revealed preference methods (see Melstrom et al. 2026). Stated preference methods ask participants to make hypothetical choices between options that vary experimentally and systematically in their attributes. They are generally survey-based experiments.

Behaviour of recreational fishers can also be observed (visually, by site visits, or using digital apps or wildlife cameras). Observational methods "reveal" the preferences or behaviours of fishers without asking them directly. These revealed preference methods are not experimental in the true sense of the term and use the real-life decisions of fishers as dependent variables. For example, in a study by Bennear et al. (2005), the demand for state fishing licenses was used to infer the benefits of recreational fishing. Revealed preference approaches are more akin to natural experiments or observational studies and, by definition, use the most realistic and externally valid data of actual fishing behaviour. Revealed preference methods include surrogate market approaches such as travel cost and hedonic pricing. For example, Alvarez et al. (2014) use site choice by saltwater fishers in the Southeast US to estimate monetary compensation for recreational losses due to the Deepwater Horizon oil spill. Prayaga et al. (2010) used the travel cost methods to estimate the value of recreational fishing in a region in Queensland, Australia. Navarro et al. (2021) used phone diary surveys to develop Random Utility Models (RUMs) of site choice to predict the effect of management interventions (e.g., closed areas) on fisher behaviour.

There is a range of novel technologies that can measure recreational fisher preferences. These approaches include cameras, satellite imagery, radar, and hydrophones. Hartill et al. (2020) suggest that robustly designed digital camera monitoring systems are a cost-effective means of monitoring recreational fisher behaviour and that they can provide insight into fishery dynamics. The use of digital camera systems has been trialled in New Zealand (Hartill et al. 2016), Australia (Keller et al. 2016; Steffe et al. 2017), and Germany (Kaiser 2016). Satellite data has also been used to monitor recreational fisher behaviour in, e.g., Greece (Keramidas et al. 2018), where it helped estimate the number of recreational vessels in the Aegean and Ionian Seas. Satellite data has been used to evaluate the role of recreational fishing activities in transporting pest and disease vectors (Bacela-Spychalska et al. 2013).

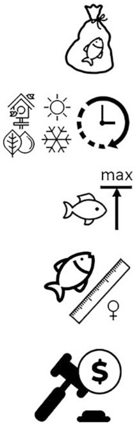
Hydrophone, or acoustic data, has been used successfully in Australia to monitor fishing activity in MPAs (Kline et al. 2020). Other approaches to gain insight into the way fishers behave are using apps, providing an opportunity to exchange information that could change the way fishers fish and engage with management (Venturelli et al. 2017). Smartphone apps are now used to collect data on aspects of fisher behaviour in many different countries, but experts agree that they might not be a stand-alone method now (Skov et al. 2021). It is worth noting that even though the predictors in these revealed preference approaches on their own are unable to establish causation, they have external validity, in that the behavioural decisions of the fishers are real.

Some internal validity can be gained through combining different approaches. For example, combining autonomous cameras' information with creel surveys (both revealing fisher behaviour through observation) (Dutterer et al. 2020; Lai et al. 2021) to estimate, e.g., fishing effort. This dual approach was also used to test hypotheses of optimal management policy for rainbow trout (*Oncorhynchus mykiss*) fisheries in British Columbia (Askey et al. 2013; Mee et al. 2016; Carruthers et al. 2019).

Internal validity can be gained through additional surveys or questionnaires, where respondents are asked for their views, opinions, or preferences (Hoyos 2010; Hess and Daly 2014). For example, via a questionnaire, Pouso et al. (2018) found a relationship between ecological recovery and a change in recreational fishing behaviour. The potential impact and effect size of a particular type of regulation inconsistently impacting fishers can be picked up with the use of survey methods (Thomas et al. 2016). Surveys can be implemented on site (e.g., access point or boat ramp surveys) or off site (e.g., diary panels). Recreational diary panels are a common approach to gain insight into fisher behaviour, but a carefully designed recruitment process is key to ensuring the estimates of catch, harvest, release, and effort are not biased (Lewin et al. 2023) (see Chapter "Ecology for Understanding Recreational Fishers and Fisheries" in Sect. 3).

Many studies have investigated the implications of differences between fishers through parameters that are allowed to vary by individual (McFadden and Train 2000) or by including covariates that describe characteristics of the fishers themselves (Kamakura and Russell 1989). The goals of these studies vary widely. They include testing the effects of demographic variables or recreational fisher typology (such as specialized and committed fishers vs. casual fishers) on fishing preferences and site choice behaviour (Beardmore et al. 2013; Goldsmith et al. 2018), estimating the economic value of fishery resources (Lew and Larson 2012; Alvarez et al. 2014), and predicting spatial patterns in fishing effort (Post et al. 2008; Wilson et al. 2020). Some studies combine stated preference and revealed preference data, as in Hindsley et al. (2023), who combine data from a creel survey with a discrete choice experiment. They find that there may be hypothetical bias in the stated preference data and that the willingness to pay estimates for improved catch is higher than the revealed preference estimate. A review of revealed preference and stated preference studies in recreational fishing is found in Hunt et al. (2019).

Discrete choice experiments, a type of stated preference method, are quite popular in understanding recreational fishing behaviour (see Fig. 4 for an example). Choice experiments, e.g., may evaluate fisher response to regulations such as bag or size



Management attribute	Status quo	Levels A	Levels B	Levels C
Daily bag limit	1/day	2/day	3/day	5/day
Season length	15 weeks	12 weeks	52 weeks	24 weeks
Maximum seasonal catch	15	25	10	30
Maximum size limit for females	105mm	115mm	110mm	95mm
Penalties for noncompliant act	$100	$150	$50	$250
Your choice	☐	☑	☐	☐

Fig. 4 Hypothetical example of question in a discrete choice experiment (loosely based on Mackay et al. 2020; icons from https://www.flaticon.com)

limits (Morey et al. 1991; Aas et al. 2000; Oh et al. 2005; Beardmore et al. 2011; Carr-Harris and Steinback 2020; Carter et al. 2020), catch outcomes (Koemle et al. 2022; Bronnmann et al. 2023), gear restrictions (Knoche and Lupi 2016), or the value of stocked versus wild fish (Arlinghaus et al. 2014). Alternatively, management attributes may describe particular management outcomes such as reductions in crowding (Lee et al. 2014), economic benefits to particular user groups (Wattage et al. 2005), or improvements in water quality (Kosenius 2010; Zhang and Sohngen 2017). Holzer and McConnell (2017) use a choice experiment for the Gulf of Mexico flounder (*Cyclopsetta chittendeni*) fishery to show that fishers exhibit disutility from randomness in the catch, and Koemle et al. (2022) found that fishers preferred ambiguity in expected catch over known probabilities of low catch rates. Risk aversion leads to different predictions on fisher participation and welfare from those assuming risk neutrality.

3 A Practical Guide to Doing an Experiment

In this section, we discuss the practical side of doing experiments to understand recreational fisher behaviour. There are many aspects that need to be considered, including hypothesis generation, selection of appropriate statistical models, recruitment of study participants, and ethical considerations of experiments involving human subjects.

3.1 Hypothesis Generation and Selection of Appropriate Statistical Models

The study of human behaviour within recreational fisheries draws from many disciplines. This means that researchers have many tools and theoretical frameworks available to them when formulating hypotheses and designing experiments. Although the interdisciplinary nature of recreational fisheries research generates a rich variety of perspectives on the relationships between humans and nature, the sheer variety of theoretical frameworks available in the literature means that researchers may have difficulty explicitly basing their recreational fishing experiments on theory. Before beginning the process of designing an experiment, a first key step is to identify the theoretical framework that is most appropriate for your study system and research question. The choice of theoretical framework will inform the scope of your hypothesis testing as well as your choice of statistical models. For example, random utility theory (McFadden 1974) is an effective framework for investigating and quantifying welfare changes for a population of fishers, but it would not be effective for understanding how, e.g., social norms influence fisher choice behaviour, which is more rooted in fields outside economics (Bicchieri 2005, 2017) (see Chapter "Social Psychology for Understanding Recreational Fishers and Fisheries" Social Psychology and Chapter "Behavioural Economics for Understand ing Recreational Fishers and Fisheries" Behavioural Economics). Explicitly rooting your hypothesis in theory should improve your experiments' potential applicability to other systems and avoid producing findings that cannot be replicated. If possible, collaborating with an expert in the discipline is an opportunity to identify unmet assumptions or "unknown unknowns" that may weaken or invalidate your results if unaddressed.

Before beginning any experiment, one should therefore identify an applicable theoretical framework, carefully review previous work to narrow the research gap, and develop a hypothesis describing the expected relationship between the predictor and response variables. The statistical model describes the relationship that the researcher is hypothesizing, i.e. that X change in a predictor, all else being equal, is expected to produce Y change in the response variable (e.g., Fig. 1). The fit of the final model and the effect size and uncertainty of the estimated coefficients are then used to evaluate the likelihood that the null hypothesis (that there is no effect or no relation among variables) should be rejected, given the data that were observed. The choice of appropriate statistical models will depend on whether the response variable is continuous or discrete, as well as its statistical distribution (e.g., Gaussian, binomial, Poisson, negative binomial). In addition, the model must account for any non-independence among observations. Non-independent observations may occur, e.g., when observations of individual fishers are collected across multiple waterbodies. Two observations of catch rates within a single waterbody will be more similar than two observations of catch rates from different waterbodies. Similarly, observations that are closer to each other in time or space may exhibit temporal or spatial autocorrelation. Both the experimental design and the statistical model must be formulated to either avoid or account for cases when observations are not independent. A rich set of statistical tools now exists to accommodate almost any data structure (Stevens 2012; Cunningham 2021; Huntington-Klein 2021).

Pre-testing the statistical model on simulated data is an effective strategy to (i) practice with the chosen statistical modelling tool (e.g., R packages, Python libraries, proprietary statistical software) before working with the "real" data and (ii) choose an appropriate sample size and experimental design. Simulated data may best be produced with a known relationship between the predictor and response variables. Different versions of the simulation can include smaller or larger effect sizes and narrower or wider variance of the error term to test the model's power to detect the known effect across a range of sample sizes and experimental designs (i.e., varying the number of treatment groups and replicates). These power analyses can be completed "manually" as described here or by using specific tools developed for that purpose. Pre-testing the experiment, e.g., by conducting an initial version of an economic experiment with a reduced sample size, provides another opportunity to practice fitting the statistical model and identify potential errors (e.g., non-convergence, suspicious residuals, or unexpected distribution of response variables) before beginning the energy- and resource-intensive process of conducting the experiment.

Finally, experiments that are not carefully linked to theories and hypotheses bear the risk that the research itself becomes a fishing trip, where researchers search for interesting and perhaps surprising results and try to come up with ex-post explanations for those. Therefore, it has become good practice to pre-register any experimental designs with hypotheses and expected results. This limits flexibility for researchers but takes away the pressure to find something interesting and makes it easier to publish null results.

3.2 Recruitment Processes

Recruiting representative recreational fishers or recreational managers for experimental research can be notoriously difficult, although in some jurisdictions, licence records provide a sample frame for probability-based sampling design. The fishers can be hard to reach simply because they are geographically dispersed (Moore et al. 2015). Often, fishers are hard to reach because there is no central way to contact them as a group. Increasingly, though, there are apps and social media sites that recreational fishers use, and these can provide a way for researchers to contact them and connect with them. Contacting fishers through fishing-specific media, however, may cause the recruitment of only highly engaged fishers (e.g., Venturelli et al. 2017). The risks of obtaining a biased sample of the overall population of fishers apply to almost any method of contacting, and researchers should try to understand and minimize or account for such biases and incentivize participation (e.g., using payments). Ideally, participants are a random sample of fishers, whereas in practice, the most motivated and committed fishers are those who are most likely to be responsive towards a request to participate in a study.

Participants may be recruited in person via or in different ways in a recreational fishing context. Access point surveys (where fishers are approached at boat launching ramps or popular fishing spots) are one way in which this has traditionally happened. A study by Griffiths and Fay (2015) addresses the challenge of reaching "hard-to-reach" longtail tuna (*Thunnus tonggol*) recreational fishers in Australian waters by trialling different methods. They compare the effectiveness (and other benefits) of time-location sampling (at e.g., tackle shops and trade shows) and online diaries to traditional access point surveys. Online diaries were inexpensive but showed signs of recruitment bias. Time-location sampling at key fisher infrastructure (such as tackle shops) was cost-effective and reached a broader set of fishers. Access point surveys were expensive and, perhaps surprisingly, were not able to reach the same breadth of fishers as time-location sampling. Regardless of how participation in the experiment is solicited, the researcher should be aware of any ethical issues around this.

In relation to recruiting individuals to the research, researchers must make sure that there is no undue pressure for people to participate. Even though this may seem unsurprising—undue pressure can be exerted in unexpected ways. To get participation in a survey, a researcher may send an email to the president of the local recreational fisher club and ask them to forward the email to their club members. Might it be that members feel pressured to do the survey because otherwise, there may be consequences from the president? Maybe—and maybe not—but this needs to be a thought that is considered when thinking about how to recruit participants. It also needs to be clear in the recruitment process that no false promises are made (e.g., if you do this survey, your fishing experience will certainly improve). In blinded experiments where individual people are allocated to specific treatments, e.g., in (Fujitani et al. 2017), it is important to be clear upfront that an experiment is being conducted and that all individuals are debriefed later about which treatment they belonged to, and which results were gathered. When interventions include sensitive issues, e.g., providing wrong messaging or lies, even greater standards apply for ethical approval. Selected interventions (e.g., providing information on catches) in real fisheries may substantially change fisher behaviour "in the real world," which can cause another set of ethical issues that must be clearly disclosed or avoided in the process.

3.3 Survey Biases

We recommend that researchers familiarize themselves with survey methods (Chapters "Fundamentals of Sampling and Implementing Human Dimensions of Fishing Surveys" and "Measurement Instruments") to ensure they are constructing clear, quality questions and prompts for participants. A variety of resources exist, including Survey Methodology by Groves et al. (2009). As mentioned earlier, it is important in all experiments that the respondents understand the question or task they are asked to carry out. If the questions are vague or poorly written, or the interviewer is impacting or influencing the respondents' answers, this can easily lead to response bias. There are many reasons the respondent may not answer the questions truthfully, for instance, if they are uncomfortable. Two related reasons they may not answer truthfully are confirmation bias and social desirability bias. The former is where the respondent's answer will conform to the direction of the question (Kunda and Sinclair 1999). For example, if fishers are asked if they are catch-and-release fishers, they may preferentially remember times that they released fish, which may bias their responses towards greater catch-and-release orientation. The latter is where the respondent second-guesses what is socially desirable and over-reports the behaviours that are desirable or answer to influence some fishery management outcome (Maccoby and Maccoby 1954; Fisher 1993). For example, if catch and release angling is perceived by the fisher as virtuous behaviour, they may over-report their catch and release activities. For activities that are socially unacceptable or illegal, such as noncompliance with fishing regulations, alternative approaches may be required, such as anonymous responses and indirect questions (Bova et al. 2022) or a randomized response technique (Lewis 2015; Thomas et al. 2015) where partial randomization of responses in a survey provides individual cover for illegal activity but allows population-level inference about rates of noncompliance (e.g., Blank and Gavin 2009).

Another important bias to be aware of is non-responder bias, where respondents refuse to answer the question or even return or participate in a survey (Tarrant et al. 1993). Non-response bias is essentially a form of selection bias. Selection bias may happen when the people targeted by the study are not included in the experiment. A good example of this is when, for a phone survey, only landlines are called, but many people no longer have landlines. Sampling bias is the same as selection bias and happens when the sampling frame does not represent the true population. This may be due to the sampling technique or the data collection method. It is good practice to report response rates and not to generalize to the population level if non-response or selection bias is present.

In lab and lab-in-the-field experiments, other biases apply that can create confounding effects. Participants may misunderstand the experimental procedures, or they may make incorrect inferences about their own preferences. Where participants interact with each other in an experiment, they may respond based on their subjective beliefs about the others and have false expectations (Cubitt et al. 2001).

When designing surveys, the information that is in the survey should be unambiguous and interpreted in the same way. This means that the researchers need to think carefully about how the participant will perceive and interpret the experiment and how to avoid divergence (Kawagoe and Takizawa 2019). For example, when asking fishers about their preferences for different management approaches (Mackay et al. 2020) using a stated choice experiment (which is a survey-based experiment), the description of the management attributes (that the respondent is weighing up and choosing between) needs to be clear and well understood.

All approaches are prone to participant and survey biases. Asking people for their preferences has an inherent risk that the respondent might not be answering truthfully or be self-aware of their preferences or behaviour. There may also be a lack of incentive to tell the truth. The disincentive may be created because the respondent thinks they should answer something to please the interviewer, or saying something that they think is right, gives them a warm glow, or it conforms with the social norm (Bova et al. 2022). Cognitive anomalies apply in the stated preference approach, e.g., the hypothetical setting may be confusing or difficult to relate to and thus causing problems. Similarly, there are issues with the revealed preference approach because it might be impossible to know which behavioural options were discarded in preference for the revealed behaviour. Neither the stated preference nor the revealed preference approach, when applied cross-sectionally, can account for preference changing over time. There is some consensus that revealed preference estimates are, in general, considered more reliable than stated preferences (List and Gallet 2001; Murphy et al. 2005; de Corte et al. 2021), this is thought to be due to the tendency for people to overstate their preferences, also known as "hypothetical bias" (Loomis 2011; Johansson-Stenman and Svedsäter 2012).

3.4 Counterfactual Thinking

In experimentation, a critical concept is not only to understand the causal relationships and the confounders but also to think in terms of the counterfactual or control (i.e., if X had not happened, then Y would not have occurred). In recreational fisheries, e.g., Dundas and von Haefen (2020) used counterfactuals in a study that identified and quantified the economic value of behavioural adaptation to climate change by fishers. A counterfactual simulation predicts welfare change under future climate scenarios. The behavioural response of fishers to high temperatures is to switch their recreational activities to night-time. This behavioural adaptation can save up to $27 million annually. In the counterfactual simulation, the fishers behavioural change to night-time fishing does not occur. Another example of the use of counterfactuals in recreational fisheries is presented by Fujitani et al. (2012) and their work on fishers' use of marine reserves in the Gulf of California. Authors compared visitation patterns from fishers to an area that was declared a marine reserve in 2022 and visitation patterns to that area if it had not been declared a marine reserve (i.e., counterfactual scenario). Findings showed that the marine reserve influenced fisher behaviour in the short term, as visits declined from the moment the marine reserve was announced. However, the strength of the reserve to hinder recreational fishing trips to that area declined over time as compliance was not adequately enforced, thus showing that the effect did not last without strong management implementation (Fujitani et al. 2018).

For experimental methods that use controlled interventions, the treatment measures the experimental effect, and the control serves as the counterfactual (Cunningham 2021). In choice experiments and welfare studies, the counterfactual is the status quo or base case. For natural experiments with observational data, we do not have a "parallel universe" that tells us what the world looked like, say, if COVID-19 never happened. Hence, it is the task of the researcher to construct a credible counterfactual. For example, in conservation research, the study of counterfactual scenarios can be used to celebrate success stories by evaluating the success of past conservation efforts as well as to predict the potential impact of a management approach (Grace et al. 2021). One key aspect in assessing counterfactual scenarios is to define the variable used to evaluate the impact of the treatment (which will result in a counterfactual outcome, different from the current outcome). For example, the past impact of a treatment (e.g., a conservation action) in a system where we know the outcome (e.g., current conservation status of a species), a counterfactual scenario might be where no conservation efforts were undertaken for that species (Grace et al. 2021).

One option is to compare *changes* in, e.g., a water body before and after treatment to changes in other systems (a diff-in-diff approach also referred to as a before-after-control treatment design). The difference-in-difference approach observes two groups over a certain period. If there were no treatments, the two groups would have similar trends (equal trend assumption), and there would be no time-varying differences. But then one of the groups (the treatment group) has a change in the independent variable over time (whereas the control group does not). For example, the introduction of benchmarking for fish stocks reduced behavioural biases in stock assessments in the treatment group when compared to the control group, where there was no benchmarking (Fujitani et al. 2017; Schuch and Richter 2022). The estimator of the difference in difference approach is literally the difference in outcome in the first period (before treatment) and second period (after treatment) in the treatment group minus the difference in average outcome in the control group before and after treatment (Fox and Swearingen 2021). There have been advances in statistical analysis towards causal analysis of multivariate data using structural causal and structural equation models (Pearl 2009; Pearl et al. 2016).

3.5 Ethical Considerations

Ethics are a key consideration when conducting experimental research involving human subjects. The extent of these considerations will depend on the types of data collected and the degree of local community engagement with the research. Although recreational fisheries experiments are unlikely to cause direct physical harm or mental distress to participants, harm to participants is possible through the breach of private data or the release of local, traditional, or Indigenous data to wider audiences without the consent of that community (Hudson 2009; Webster and Sell 2014). In addition, experiments where false information is provided, which is not allowed in experimental economics (see Chapter "Social Psychology for Understanding Recreational Fishers and Fisheries", Social Psychology), or the treatment is to expose illegal behaviour, can also risk harm. Some, but not all, of these potential harms are addressed through institution-specific human subjects research authorization protocols and reviews, which vary in degree from country to country. It is advisable to approach faculties that regularly engage in human research, e.g., psychology, to learn about ethical review needs in each culture or country.

Processes of authorization for human subject research depend on the location of the research and the policies of the research institution, and we will not present a full review of those policies here. It is therefore vital for researchers to verify local, national, and institutional policies before beginning any experiments involving interaction with and recruitment of human subjects, including administering surveys and conducting interviews. In a recreational fisheries context, researchers will likely need to address potential issues regarding participant privacy (i.e., avoiding breaches of private data), facilitating informed consent of participants, and ethical recruitment of study participants, including appropriate uses of incentives (Webster and Sell 2014). This process can be lengthy, even for experiments that pose little risk to participants. It is important to begin this process well in advance of the project's start date. Research carried out by non-governmental groups (NGOs) or independent consultants does not always have access to ethics committees to follow these same formal ethics approval pathways, but regardless, there is a need and opportunity for these organisations to consider the ethical aspects of their work.

Ethical considerations go beyond gaining institutional approval (Heimer and Petty 2010; Wilmer et al. 2021). Fisheries researchers increasingly adopt participatory research or knowledge co-production approaches to bridge the gap between scientific knowledge and policy implementation (Cooke et al. 2021). Regardless of institutional requirements, the integrity of the research design and the goal of maintaining equitable partnerships with fishery stakeholders and rights holders depend on obtaining informed consent and the avoidance of extractive research practices. These unethical research practices benefit academic researchers by extracting data, publications, and prestige without benefiting the community holding that knowledge (e.g., Trisos et al. 2021; Wilmer et al. 2021). Teams conducting research that relies on local, traditional, or Indigenous ecological knowledge should ensure that they are obtaining not just individual consent, but collective consent to broadly share this knowledge. Individual motivations for participating in research vary, so individuals should not be uncritically accepted as community representatives without broader community input (Hudson 2009). Consistent and clear communication between researchers and community members outside of typical academic modes (i.e., journal articles and formal presentations) is essential to ensure continued collective consent and to ensure that research goals are "credible, relevant, actionable, and impactful" (Nyboer et al. 2022). Different communities will have different needs, and a research protocol that is harmless in one community may cause significant harm elsewhere. It is essential to understand the history of the specific community with which you collaborate, and particularly their history of abuse by governmental entities and academics (e.g., Hatch et al. 1993; Smith 1999). However, every community is different, so teams should be prepared to listen to feedback and adjust their behaviour rather than relying exclusively on arbitrary or contextually ill-suited benchmarks or consider the job being done once ethical approval is obtained. Clearly, in an experiment, participating subjects need to be debriefed at the end and results fully disclosed and shared with participants.

4 Future Directions

As outlined in this chapter, experiments offer learning opportunities to study recreational fishing and they are currently underused in recreational fishing research, which is often only observational or cross-sectional (e.g., recreational fisher surveys). The social–ecological context of recreational fishing very much leans itself towards a "living laboratory" of constant learning and experimentation for at least two reasons. First, recreational fisheries are heavily regulated and tightly controlled in some parts of the world. Management interventions are implemented on a regular basis in recreational fisheries and each of those is an opportunity to learn. Second, recreational fishing is often an organized social activity with entrance licences, fishing clubs, and fixed fishing sites, which can serve as sampling units (e.g., Fujitani et al. 2017). Recreational fishers are also often a particularly motivated and engaged group who have a strong interest in their ecological system, which should make it easier to involve them in research activities than random citizens who lack such motivations. Together, this makes the economic experimental toolbox particularly useful for recreational fishing (e.g., Stoop et al. 2012).

One may argue that many of the ideas presented in this chapter are just old wine in new bottles. After all, the notion of "adaptive management" (Walters 1986) very much embraces the idea of experimentation, learning, and looking holistically at a social–ecological system. Yet, we think that recent developments in experimental economics can bring something to our profession and advance our understanding of recreational fishing. First, a rigorous application of "randomized control trials" where entire sites or lakes or recreational fishing groups or communities or clubs are "treated" and compared to control sites or groups holds the potential to answer big questions around recreational fishing. Studies using such an approach are clearly very costly to conduct, but the potential scientific impact is high as well; see, e.g., Radinger et al. (2023) who used a participatory habitat enhancement vs. stocking design applied to different angling clubs and implemented in partnership to learn ecologically how the entire system changed to interventions. Second, methods from economic experiments are increasingly useful to understand social dynamics and social norms (Andreoni et al. 2021), which are highly relevant for recreational fishing. Also, preferences of fishers are often unobserved, leaving practitioners guessing when it comes to the acceptability of, and response to policy interventions. Incentivized games (Diekert et al. 2023) or choice experiments (Rambonilaza and Kerouaz 2023) may offer excellent opportunities to learn about fisher preferences and behaviour. Third, recent developments around "natural experiments" and statistical models of causal inference (Pearl et al. 2016) offer opportunities to learn from the many interventions that are conducted by management authorities as "uncontrolled experiments." In such a case, establishing a credible counterfactual will always remain a challenge, because one needs to compare an observed outcome to one that would have happened in the absence of the intervention and therefore never happened. Yet, sufficiently long time series and the presence of potential control sites can help to construct a credible counterfactual.

Though there are reasons to be enthusiastic about the prospects of using experimental methods from economics for recreational fishing, we would like to end on a cautionary note. Conducting experiments in recreational fishing requires close coordination with stakeholders and time investment to build trust. Although this is ideal for genuine long-term collaborations, a quick and poorly designed experiment can be perceived as intrusive and shows a lack of respect for study participants and stakeholders. Therefore, there are strong ethical implications related to conducting such research. Also, methods from experimental economics are not necessarily easy to master, and those who are trained in those methods are not necessarily trained to work in interdisciplinary teams. So, there are certainly challenges when it comes to applying those methods in a field setting, and when it comes to bringing those field contexts to universities that teach those methods. Finally, the great strength of the approaches sketched in this chapter is the ability to generate quantitative insights, which is by no means the only and probably not even the best framework to study human experience and perceptions. Unlike natural scientists, social scientists hold the privilege of being able to talk to their study objects and ask questions. It is straightforward to recommend that qualitative and quantitative methods should be combined in a complementary way, but doing this in practice is much less straightforward. So here lies another challenge for future researchers to overcome.

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