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Non-parametric tests of behavior in the commons

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ABSTRACT

Commons problems present behavioral dilemmas, with tensions between individual and collective rationality. When users of a common-pool resource are not effectively excluded, the collective behavior of individuals pursuing their self-interests dissipates economic surplus. We derive a non-parametric test of whether individuals' collective behavior in resource extraction is consistent with the canonical commons model, namely Nash tragedy-of-the-commons behavior. Our approach allows for an arbitrarily concave, differentiable production function of total inputs and for heterogeneous agents with arbitrarily convex, differentiable costs of supplying inputs. We extend the test to allow for unobserved total output. We also define distance from the data to the model and develop statistical tests using the distance metric. Applying our approach to panel data of Norwegian commercial fishing vessels, we find the results of our test are consistent with the economic intuition that, in the absence of property rights, tragedy-of-the-commons behavior dissipates surplus. Significantly, we find property rights reforms move firms away from Nash tragedy-of-the-commons behavior.

1. Introduction

Common-pool resources are characterized by rivalry and some degree of non-excludability. Classic examples include sending cattle to a common pasture (Huffaker and Wilen 1991), extracting oil from a common pool (Libecap and Wiggins 1984; Baltrop and Schnier 2016), extracting groundwater (Brazović et al. 2010; Ayres et al. 2021), and fishing from the sea (Gordon 1954; Scott 1955; Huang and Smith 2014). These natural resources are rival in consumption because a unit extracted by one user is a unit unavailable to all others.

Like the prisoner's dilemma, commons problems create a behavioral dilemma highlighting the tension between individual and group rationality. In the canonical commons model (CCM), individuals receive a prorated share of collective output proportionate to their inputs, so by increasing inputs they can obtain a larger share of the pie at the expense of others (Gordon 1954; Weitzman 1974; Dasgupta and Heal 1979; Cornes and Sandler 1983). Consequently, when unchecked by property rights or other institutional arrangements, they have an incentive to overuse the resource, undermining its potential value. In the extreme case of open access (perfect non-excludability), users drive resource rents (i.e. economic rents associated with the fixed natural resource input) to zero (Gordon 1954; Smith 1968).

Surprisingly, there have been few empirical tests of the CCM. Of course, many studies have considered the aggregate effects of

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different property rights regimes. For example, Kirkley et al. (2002) and Felthoven et al. (2009) outline approaches for measuring excess capacity in an industry exploiting a common-pool resource, with resources "wasted" on capital in the effort to exploit. This approach requires estimating a production function for firms. Others consider the effects of establishing property rights on resource stocks and exploitation effort. Birkenbach et al. (2017) and Hsueh (2017) find that individual fishing quotas (IFQs) reduce the pressure to over-fish. Balthrop and Schnier (2016) find that unitizing oil and gas reserves decreases the race to pump. These studies evaluate the effect of policies on socially relevant outcomes, but they do not test whether individual *behavior* is consistent with the CCM.

Highlighting non-excludability as a source of a behavioral dilemma, Huang and Smith (2014) conducted the first micro-level empirical investigation of strategic behavior in a common pool. Using strong functional form assumptions, they develop a dynamic structural model of the microeconomic behavior of fishers. Although policies restrict the total number of fishers, each fisher chooses effort to maximize expected utility given all other fishers' actions without any limits on their individual catches. With estimates from their model, one can quantify the potential efficiency gains of property rights reforms. However, their approach *presupposes* Nash behavior in a commons game as a maintained assumption. *Testing* the validity of such models under minimal assumptions remains an unexplored area.

In this paper, we introduce a non-parametric revealed preference test for behavior in the commons, to test whether behavior is consistent with the CCM. Our test has the advantage of requiring no assumptions about production functions or cost functions (beyond convexity and differentiability). The test is derived from the key characteristics of the CCM that each agent maximizes its objective function independently and from a proportionate sharing rule. It is built on results from Carvajal et al. (2013), who developed a revealed preference test for Cournot equilibrium, deriving properties that hold when firms are strategically interacting as delineated by that model. Using their logic, we derive similar properties that hold under the strategic interactions of the CCM in which individuals compete for a prorated share of the common-pool resource. The test can be implemented with panel data of individual inputs and total output. In particular, given panel data on each agent's input and the total output from exploitation, we show that a data set is consistent with the CCM with convex cost functions if and only if there is a solution to a linear program that we construct from the data.

Further extending the logic of Carvajal et al., we adjust the test to allow for situations in which total input and/or output is unobserved and must be estimated. We introduce a latent parameter, which can be inferred from our linear program under the null hypothesis of behavior consistent with the CCM. The latent parameter relaxes the testable properties, increasing the domain of the linear program, making the test less stringent.

We also derive a new test to gauge the minimum distance from the observed behavior to the CCM. Building on a basic approach first proposed by Afriat (1972) and Varian (1982, 1985), we impose an adjustment factor in the model to guarantee that data would always pass the behavioral test. We apply a nonlinear program to quantify the minimal magnitude of the adjustments, which can be interpreted as a quantitative measure of distance from the model. Using these distance measures, we customize and apply statistical tests for consistency of behavior to the model. Specifically, we use a non-parametric Kolmogorov–Smirnov test and a parametric difference-in-differences Z test. These extensions could be applied to tests of the Cournot model (as in Carvajal et al.) as well. Using simulated data generated from specified behavioral patterns, we confirm that these tests and metrics can detect departures from the CCM.

We apply our tests empirically to the Norwegian coastal fishery for cod and other whitefish (the largest fishery in Norway and a major contributor to the global market for whitefish). Before 2003, this fishery was managed with top-down industry-wide quotas that did not place binding limits on individual vessels. In 2003 individual property rights were introduced for large but not small vessels, suggesting a difference-in-differences research design. Using our test, the CCM is rejected to a greater degree after these reforms, especially for large vessels. This pattern highlights the intuitiveness of the test and its relevance to policy questions, namely whether changing property rights (and thus incentives) can change behavior.

The paper is organized as follows. Section 2 derives the theoretical results for the classic model of the average return game in which agents select their inputs and each unit of input receives the average return (rather than marginal return). We then extend the model to account for the possibility that a full census of resource extraction is not observed. Section 3 offers additional extensions, by quantifying distance from the observed data to the theoretical model and deriving formal statistical tests for the consistency of a data set with the model. Section 4 then runs the tests from Sections 2 and 3 on simulated data for which the true model is known. Section 5 discusses the empirical application, and Section 6 presents the results. Section 7 concludes.

2. Principle result: a nonparametric test of behavior in the commons

2.1. The average return game

Consider an industry consisting of I profit-maximizing firms, indexed by $i = 1, 2, \ldots, I$, each having free access to an exogenously fixed common-pool resource. There are T decision periods indexed by $t = 1, 2, \ldots, T$. Firm i's profit-maximization problem at time t is:

$$\max_{\mathbf{x}_{i,t}} \frac{f_i(\mathbf{x}_{i,t})}{f_i(\mathbf{x}_{i,t}) + \sum_{j \neq i} f_j(\mathbf{x}_{j,t})} * p_t F_t\left(f_i(\mathbf{x}_{i,t}) + \sum_{j \neq i} f_j(\mathbf{x}_{j,t})\right) - \mathbf{w}'\mathbf{x}_{i,t}. \tag{1}$$

Here, $\mathbf{x}_{i,t}$ is a vector of market inputs (e.g., for our fisheries application, gear, crew, fuel, etc.) purchased by firm i at time t, and $\mathbf{w}$ is its corresponding vector of prices. The fraction at the beginning of the expression gives output share for firm i. This output share is a nonlinear transformation $f_i()$ of the $\mathbf{x}$, which differs for each i and is completely nonparametric, although we restrict it to be increasing, differentiable, and time invariant. $F_t(\,)$ is a non-parametric time-varying physical production function for the group collectively, with

$F(0) = 0$, its derivative $F'( ) > 0$, and F' non-increasing for all t. Finally, p_t is the output price, so p_tF_t is group revenue. Thus, the entire first term is firm i's share of total revenue. It depends on its inputs, its transformation function, and similarly those of other firms.

To simplify the notation, let

$$q_{i,t} = f_i(\mathbf{x}_{i,t}),$$

$$Q_t = q_{i,t} + \sum_{j\neq i} q_{j,t},$$

and

$$C_i(q_{i,t}) = \min_{\{\mathbf{x}\}} \mathbf{w}'\mathbf{x}_{i,t} \ s.t. \ f_i(\mathbf{x}_{i,t}) \geq q_{i,t}.$$

With these substitutions, we can rewrite the profit function equivalently as:

$$\max_{q_{i,t}} \frac{q_{i,t}}{Q_t} * p_tF_t(Q_t) - C_i(q_{i,t}). \tag{2}$$

Conceptually, we can think of $q_{i,t}$ as some intermediate input which determines output shares, such as days at sea (for harvesting fish), or, more generically, "resource extraction effort." These intermediate inputs are "produced" from the underlying market goods $\mathbf{x}$ according to the non-parametric function $f_i( )$. For example, a fishing day is produced from labor and fuel inputs and so forth. Using the duality of cost minimization, this functional relationship now appears in the non-parametric cost function $C_i(q_{i,t})$. It is the cost of supplying a unit of effort, not of obtaining a unit of output. Finally, note that the control variable $q_{i,t}$ appears in four places, as it is a component of Q_t. Expressed in this way, this is the canonical commons model (CCM) (Gordon 1954, Weitzman 1974, Dasgupta and Heal 1979, Cornes and Sandler 1996).

We assume we observe revenue p_tF_t and intermediate inputs $q_{i,t}$ but not necessarily the mapping from $\mathbf{x}_{i,t}$ to $q_{i,t}$. We will say this observed panel data set $\mathscr{O} = \left\{p_tF_t, \ q_{i,t_{i\in 1\cdots N}}\right\}_{t\in 1\cdots T}$ is consistent with the CCM if there exist cost functions $\overline{C}_i(q_{i,t})$ for each firm i, and concave production functions $\overline{F}_t(Q_t)$ for each observation t that jointly satisfy the following two conditions:

(i) $\overline{p_tF_t}(Q_t) = p_tF_t$

(ii) $q_{i,t} \in argmax_{\tilde{q}_{i,t}\geq 0}\left\{\frac{\tilde{q}_{i,t}}{Q_t} * p_tF_t(Q_t) - \overline{C}_i(\tilde{q}_{i,t})\right\}$.

Condition (i) says the modelled revenue (price times the production function) must be consistent with observed revenue at time t. Condition (ii) says firm i's input at time t maximizes its profit given the inputs of all other firms (a standard Nash assumption), when its behavior is consistent with CCM.

Note that we do not need to estimate the production function. We allow the analysis to explain the data using any arbitrary concave production function, as long as, once multiplied by price, it passes through the observed total revenue and inputs, $p_tF_t(Q_t)$ and Q_t, at each decision period. Similarly, no restrictions are placed on firms' cost functions except that they are increasing, convex, and differentiable. Thus, we avoid functional form assumptions.

Taking other firms' actions as given, the first-order condition from (2) is:

$$\frac{q_{i,t}}{Q_t} * p_tF_t'(Q_t) + \left(1 - \frac{q_{i,t}}{Q_t}\right) * \frac{p_tF_t(Q_t)}{Q_t} = C_{i,t}'. \tag{3}$$

where $C_{i,t}'$ is marginal cost. This is the standard result that firms equate marginal cost to a weighted average of marginal returns and average returns (Weitzman 1974; Dasgupta and Heal 1979). In the case of a monopolist, $q_{i,t} = Q_t$ and the entire weight is on the efficient condition to equate marginal cost to marginal return. In the limit, as the firm's share grows small, $q_{i,t}/Q_t$ goes to zero and each firm equates marginal cost to average revenue, thus depleting all resource rents (as in Gordon 1954).

Rearranging terms, we obtain:

$$\frac{p_tF_t(Q_t) - Q_tC_{i,t}'}{q_{i,t}} = \frac{p_tF_t(Q_t)}{Q_t} - p_tF_t'(Q_t). \tag{4}$$

Notice in Eq. (4) that the left-hand side involves firm-specific terms (inputs $q_{i,t}$ and marginal costs $C_{i,t}'$) while the right-hand side involves only market-wide data (total revenue $p_tF_t(Q_t)$, marginal revenue product p_tF_t', and total input Q_t). Consequently, from the first-order condition, we obtain a *common ratio property* analogous to Carvajal et al.'s Cournot model:

$$\frac{p_t\mathrm{F}_t(Q_t) - Q_tC_{i,t}'}{q_{i,t}} = \frac{p_t\mathrm{F}_t(Q_t) - Q_tC_{j,t}'}{q_{j,t}} = \cdots = \frac{p_t\mathrm{F}_t(Q_t) - Q_tC_{I,t}'}{q_{I,t}} \geq 0 \ \forall \ t \in T. \tag{5}$$

In other words, in each period, functions of the total extraction effort, firm-specific effort, and firm-specific marginal cost should all be equal. The expressions are nonnegative given the concavity of the production function.

Moreover, because each firm's cost function is convex, the array $\left\{C'_{i,t}\right\}$ displays nondecreasing marginal costs for each firm i. Thus, with the cost function time-invariant, we also have the *co-monotone property:*

$$q_{i,t} > q_{i,t'} \rightarrow C'_{i,t} \geq C'_{i,t'} \ \forall \ i \in I. \tag{6}$$

Consequently, a set of observations is consistent with the CCM with convex cost functions if and only if there exist nonnegative numbers $\left\{C'_{i,t}\right\}$ for all i,t that satisfy the common ratio and co-monotone properties. In Section 2.2, we derive a formal test of behavioral consistency with CCM based on these two sets of properties.

In the following example, we show that certain data sets are inconsistent with the CCM given the interplay of the two properties. Consider the following observations of two firms i and j sharing a common-pool resource:

(i) At observation t, $p_t F_t(Q_t) = 50$, $q_{i,t} = 50$, $q_{j,t} = 100$.
(ii) At observation t', $p_{t'} F_{t'}(Q_{t'}) = 350$, $q_{i,t'} = 70$, $q_{j,t'} = 60$.

Re-arranging the common-ratio property at t' to isolate $C'_{j,t'}$ and using the fact that $\frac{q_{j,t'}}{q_{i,t'}} C'_{i,t'} \geq 0$, we have:

$$C'_{j,t'} = \frac{p_{t'} F_{t'}(Q_{t'})}{Q_{t'}} - \frac{q_{j,t'}}{q_{i,t'}} \frac{p_{t'} F_{t'}(Q_{t'})}{Q_{t'}} + \frac{q_{j,t'}}{q_{i,t'}} C'_{i,t'} \geq \frac{p_{t'} F_{t'}(Q_{t'})}{Q_{t'}} - \frac{q_{j,t'}}{q_{i,t'}} \frac{p_{t'} F_{t'}(Q_{t'})}{Q_{t'}} = 0.385.$$

Now, we know from the first-order condition (3) that $C'_{i,t} < \frac{p_t F_t(Q_t)}{Q_t}$, at each time t for all i, because $C'_{i,t} = \frac{q_{i,t}}{Q_t}\left(p_t F'_t(Q_t) - \frac{p_t F_t(Q_t)}{Q_t}\right) + \frac{p_t F_t(Q_t)}{Q_t}$ and $F'_t(Q_t) - \frac{F_t(Q_t)}{Q_t} < 0$ given the concavity of production function. Thus, $C'_{j,t} < \frac{p_t F_t(Q_t)}{Q_t} = 0.33$. In addition, from the co-monotone property, we have $C'_{j,t'} \leq C'_{j,t}$ because $q_{j,t'} < q_{j,t}$. Thus, in sum, $0.385 \leq C'_{j,t'} < C'_{j,t} < 0.33$, which is clearly a contradiction. Thus, there are no nonnegative marginal costs that satisfy the common-ratio property and the co-monotone properties, so the data are inconsistent with the CCM.

2.2. Implementation: a linear program for the test

Our approach to testing if behavior in the commons is consistent with the CCM can be reformulated as a linear program. Specifically, when behavior is consistent with CCM, this linear program yields solutions for nonnegative marginal costs that satisfy the common-ratio property (5) and the co-monotone property (6). This linear program is analogous to the conditions specified in Afriat's Theorem for testing whether consumers' choices are consistent with utility-maximizing behavior or, equivalently, the Generalized Axiom of Revealed Preference (GARP) (Afriat 1967; Varian 1982). This overall approach encompasses a diversity of research programs and has been extended to a wide array of settings (Chambers and Echenique 2016; Hands 2014), including firms' costs (Varian 1984) and Cournot competition (Carvajal et al. 2013).

In our context, a set of observations is consistent with the CCM with convex cost functions if and only if, given the observed $p_t \mathrm{F}_t$, $q_{i,t}$, and Q_t there are numbers $C'_{i,t}$ satisfying:

(i) $\frac{p_t \mathrm{F}_t(Q_t) - Q_t C'_{i,t}}{q_{i,t}} = \frac{p_t \mathrm{F}_t(Q_t) - Q_t C'_{j,t}}{q_{j,t}} \geq 0 \ \forall \ i, \ j \in I, \ \forall \ t \in T;$
(ii) $\left(q_{i,t} - q_{i,t'}\right)\left(C'_{i,t} - C'_{i,t'}\right) \geq 0 \ \forall \ i \in I, \ \forall \ t, t' \in T;$
(iii) $C'_{i,t} \geq 0 \ \forall \ i \in I, \ \forall \ t \in T.$

See Appendix A for proof.

Condition (i) is the common-ratio property which follows from the first-order condition; condition (ii) is the co-monotone property which follows from the convexity of the cost function; and condition (iii) is a non-negativity constraint which follows from the fact that the cost function is increasing. For a panel data set, failure to obtain a solution to any element in the marginal cost set $\{C'_{i,t}\}_{\forall \ i \ \in \ I, \ \forall \ t \ \in \ T}$, will result in a rejection of the model.

To understand the implications of this test, we emphasize three features. First, the test is on collective behavior, i.e. an entire *data set*, not individual observations or individual firms. In this respect, it is similar to tests of consumers' choices, in which a consumer's entire data sets is or is not consistent with GARP, not a specific choice. However, one can always throw out particular observations from the data set and consider the effect of doing so. Thus, taking random subsets of the data, one can generate rejection *rates*, as a quantitative measure of "how much" the data are inconsistent with the CCM. Further, one can isolate data from particular firms or periods to see if the data set is more likely to be rejected with or without them. We leverage this possibility in our empirical application to test the effect on rejection rates of including data generated under differing property rights regimes.

Second, our approach tests the minimum *necessary* conditions for the above behavioral model. Under the model's behavioral assumptions, the test eliminates *any* type I error. On the other hand, it is weak in the sense of potentially allowing a great deal of type II error. That is, rejection of the model gives one confidence that the data indeed are not consistent with the CCM, but—as always with the scientific method—failure to reject a hypothesis is not logically equivalent to accepting it as uniquely true. Other hypotheses could also be consistent with the data.

Third, even with the very weak assumptions we bring to the model, we can learn a great deal from the tests we derive from it. Data sets that are consistent with the CCM are inconsistent with at least some rival models. Consider, for example, the case of non-tradable quotas (known as IFQs in the fishing context), which restrict each firm to extract only up to its quota. Although non-tradability prevents cost minimization subject to total extraction by the group (as firms with high costs at the margin may be allocated quota that cannot be traded to low-cost firms), non-tradable quotas do have some advantages.

Importantly, non-tradable quotas do not lead to a common ratio property like Eq. (5). To see this, note that the objective function would now be written as a constrained optimization problem:

$$\max_{q_{i,t}} \frac{q_{i,t}}{Q_t} * p_t F_t(Q_t) - C_i(q_{i,t}) + \lambda_{i,t}\left(L_{i,t} - \frac{q_{i,t}}{Q_t} * F_t(Q_t)\right), \tag{2a}$$

where $L_{i,t}$ is the quota limit and $\lambda_{i,t}$ is the shadow cost of that limit. Note output prices appear in the revenue term but not the constraint. The revised first-order condition is:

$$(p_t - \lambda_{i,t})\left[\frac{q_{i,t}}{Q_t} * F_t'(Q_t) + \left(1 - \frac{q_{i,t}}{Q_t}\right) * \frac{F_t(Q_t)}{Q_t}\right] = C_{i,t}'. \tag{3a}$$

The quota is associated with a firm-specific shadow price on extraction, so it is equivalent to the original problem with an adjusted output price. Finally, rearranging terms, we obtain:

$$\frac{F_t(Q_t) - Q_t C_{i,t}' \Big/ (p_t - \lambda_{i,t})}{q_{i,t}} = \frac{F_t(Q_t)}{Q_t} - F_t'(Q_t). \tag{4a}$$

Taking this equation in isolation, it might appear that instead of solving the linear program by finding numbers $C_{i,t}'$, we could instead simply solve for numbers $C_{i,t}'/(p_t - \lambda_{i,t})$. However, the latter numbers would not be expected to satisfy the co-monotone property, which is based on the convexity of $C_{i,t}'$ alone. For example, ceteris paribus, higher effort one year might come with a higher quota, but this would tend to lower $\lambda_{i,t}$ (as the quota is less binding), and hence decrease the expression $C_{i,t}'/(p_t - \lambda_{i,t})$, perhaps violating the co-monotone property.

Thus, we would expect a non-tradeable individual quota regime to lead to higher rejection rates of CCM. We leverage this insight in our empirical application.

2.3. Testing when aggregate output is unobserved

The test we derived in Section 2 assumes that the data come from a complete *census* (not just sample) of users, so that total effort $Q = \sum_i q_i$ and total revenue $pF(Q)$ are observed. In this section, we consider the case where only a *sample* of users are observed by the analysts, who thus must estimate Q and pF based on a sample mean times N. This extension can be applied to other settings as well, such as the case of Cournot competition considered by Carvajal et al. (2013), who did not consider these issues.

With sampling, even if the sample of individual effort and revenue is measured without error, total effort and total revenue are observed with error because they are based on the sample averages. Let α_t and β_t be the respective proportionate errors in the sample averages, so we observe $p_t\widehat{F}_t = p_t F_t * \alpha_t$ and $\widehat{Q}_t = Q_t * \beta_t$, for $\alpha_t > 0$ and $\beta_t > 0$ (i.e., the actual revenue and total effort may be higher or lower than observed). Then the common ratio property becomes $\frac{\alpha_t p_t F_t(Q_t) - \beta_t Q_t(C_{i,t}')}{q_{i,t}} = \frac{\alpha_t p_t F_t(Q_t) - \beta_t Q_t(C_{j,t}')}{q_{j,t}}$. Dividing both sides by β_t and letting $\gamma_t = \alpha_t/\beta_t$, we can write the linear program with unobserved output as:

(i) $\frac{\gamma_t p_t F_t(Q_t) - Q_t(C_{i,t}')}{q_{i,t}} = \frac{\gamma_t p_t F_t(Q_t) - Q_t(C_{j,t}')}{q_{j,t}} \geq 0, \ \forall\, i, j \in I, \ \forall\, t \in T;$

(ii) $\left(q_{i,t} - q_{i,t'}\right)\left(C_{i,t}' - C_{i,t'}'\right) \geq 0, \ \forall\, i \in I, \ \forall\, t, t' \in T;$

(iii) $C_{i,t}' \geq 0, \ \forall\, i \in I, \ \forall\, t \in T,$

(iv) $\gamma_t > 0, \ \forall\, t \in T.$

This test is still based on the same underlying model, with firms playing the same CCM game with all others. From the analysts' perspective, it also is still based on the same micro data for the observed sample, but it now allows for an adjustment in the aggregate data to account for observing a sample rather than a census.

When the analyst has a census, the base model looks for marginal costs that satisfy the above properties, with the restriction that $\gamma_t = 1$. In contrast, here we treat γ_t as unknown and let the linear program look for the set of $\left\{\gamma_t, C_{i,t}'\right\}_{\forall\, i \in I, \forall\, t \in T}$ that makes the data consistent with the model. The idea is to ask if there is plausible error in the estimated aggregate $\widehat{Q}_t$ and $p_t\widehat{F}_t$ that, when corrected, would make the micro data consistent with the model. Furthermore, when more information (or modeler-defined judgement) of direction or range of the errors is available, we can easily add bounds on the latent parameter in the constraints.[1] Note that, although we can still reject this model as we may not be able to find marginal costs to meet the conditions, we would expect lower rejection rates by relaxing the restriction that $\gamma_t = 1$.

3. Formal statistical testing

3.1. Distance to the model

Starting again from the base model of Section 2, consider an extension in another direction. The basic test is completely binary: either behavior is consistent with the CCM or it is not. However, building on the broad foundation of the marginal-cost-consistency methods originally proposed by Afriat (1972) and Varian (1985), we can gauge the distance of the revealed marginal costs in our tests to those that are consistent with the CCM. In particular, we can find a minimal adjustment to marginal costs needed to turn a rejection of the model to an acceptance. The minimal adjustment represents the distance of observed behavior to the behavior characterized by the model. When firms are playing the CCM (i.e., when their observed behavior passes the test of Section 2), this distance is precisely zero. When their behavior departs from the CCM, the distance represents a measure of "how much."

We implement this method in an innovative way, by adding adjustment factors to marginal costs in the common ratio property, but not the co-monotone property. The intuition is that the marginal costs in the co-monotone property describe the true convexity of the cost function, which should remain unchanged, and the common-ratio property depicts the collective behavior of individual in the group, which may or may not conform to CCM. That is, the choices vessels make are “off” for some reason. Mathematically, by adding adjustment factors to the common-ratio property, we relax the constraints enforced by maximizing behavior governed by CCM but leave the co-monotone property intact to maintain the constraints of convex costs. In this way, solutions of marginal costs together with adjustments are guaranteed to make the observed data consistent with the model. We use a quadratic program to find the minimal magnitude of the adjustments, which is the minimized distance from the revealed marginal costs to those that would be consistent with the model. We denote them as revealed marginal costs and model-consistent marginal costs respectively. The minimum magnitude of the adjustments represents the distance of actual behavior from the CCM. Moreover, based on these solutions, we show that we can then derive Kolmogorov–Smirnov and difference-in-differences Z tests to inform statistical rejection of the model, a connection that to our knowledge has not previously been made in the literature.[2]

We use the following convex quadratic program:

$$\min_{C'_{i,t},\delta_{i,t}} \sum_t \sum_i \delta_{i,t}^2$$

Subject to:

$$\text{(i)} \quad \frac{p_t F_t(Q_t) - Q_t\left(C'_{i,t} + \delta_{i,t}\right)}{q_{i,t}} = \frac{p_t F_t(Q_t) - Q_t\left(C'_{j,t} + \delta_{j,t}\right)}{q_{j,t}} \geq 0, \ \forall\, i, j \in I, \ \forall\, t \in T;$$

$$\text{(ii)} \quad \left(q_{i,t} - q_{i,t'}\right)\left(C'_{i,t} - C'_{i,t'}\right) \geq 0 \ \forall\, i \in I, \ \forall\, t, t' \in T;$$

$$\text{(iii)} \quad C'_{i,t} \geq 0 \ \forall\, i \in I, \ \forall\, t \in T.$$

$\delta_{i,t}$ is the minimum adjustment factor on marginal cost $C'_{i,t}$. Note that the $\delta_{i,t}$ appear only in condition (i), not (ii). Again, the intuition here is that the cost functions are convex (ii), but firms' behavior may deviate from CCM that would show up in their first-order conditions (i). By construction, solutions $\left\{\delta_{i,t}, \ C'_{i,t}\right\}$ satisfying (i)-(iii) always exist.[3] Hence, we can identify and quantify the minimal squared adjustment factors $\{\delta_{i,t}\}$, which are the minimal distances between the revealed marginal costs to the model-consistent marginal costs.

[1] For example, if the modeler suspects $\beta > 1$, concavity of F implies $\alpha < \beta$, so $\gamma < 1$; the opposite would follow if $\beta < 1$.

[2] Our approach considers distance to the model in the space of marginal costs as they show up in Condition (i), marginal cost consistency. An alternative would be to consider distance to the model in the space of inputs. If we allow inputs to be measured with error, then we could frame this approach as asking, how large would measurement error in inputs have to be for it to explain any rejections of the model? This approach would connect our modeling with recent econometric work on revealed preference models with measurement error (Aguiar and Kashaaev 2021). However, this approach would involve a different interpretation of our data. It would ask whether departures from the CCM, when viewed as measurement error, are large enough to reject the CCM, whereas our approach asks whether actual behaviors are far enough from the theoretical CCM behavior to reject the model. As such, customizing the Aguiar and Kashaaev (2021) approach would complement our modeling and is potentially fruitful for future work on revealed preference models of the commons.

[3] The adjustment factors expand the domain of marginal costs to the reals. As they do not have to satisfy the co-monotone constraint, adjustment factors can always be found to satisfy the common-ratio property. Note that it would not do to incorporate the adjustment into all equations, which would be identical to the original model. If there are no numbers $C'_{i,t}$ satisfying (i)-(iii), there are no numbers $(C'_{i,t} + \delta_{i,t})$ either.

3.2. Two statistical tests

Denote the set of marginal costs that are consistent with the model as $\{\widetilde{mc}_{i,t}\}_{\forall i \in I,\ \forall t \in T}$ (model-consistent marginal costs) obtained from the quadratic program in Section 2. Denote the revealed marginal costs obtained from the quadratic program in Section 3.1 as $\{\widehat{\mathrm{mc}}_{i,t}\}_{\forall i \in I,\ \forall t \in T}$.

3.2.1. A Kolmogorov–Smirnov test

Suppose $\widetilde{mc}_{i,t}$ and $\widehat{\mathrm{mc}}_{i,t}$ are random variables. Under the null hypothesis that the data are consistent with the model, the revealed marginal costs and the model-consistent marginal costs are drawn from the same distribution. We can thus apply the Kolmogorov–Smirnov (KS) statistical test to the following null hypothesis against the alternative.

H0. *The two data sets, model-consistent marginal costs* $\{\widetilde{\mathrm{mc}}_{i,t}\}_{\forall i \in I,\ \forall\ t \in T}$ *and revealed marginal costs* $\{\widehat{\mathrm{mc}}_{i,t}\}_{\forall i \in I,\ \forall\ t \in T}$, *are from the same (but unknown) probability distribution.*

HA. *The two data sets are from different probability distributions.*

The KS test assumes that the cumulative distribution function (CDF) of the revealed marginal cost is continuous. Furthermore, under the null hypothesis, the KS test assumes that nT is sufficiently large that the distribution of revealed marginal costs converges to the distribution of the model-consistent marginal costs. The KS statistic is:

$$D_{n,m} = sup_x \left| F_{1,n}(x) - F_{2,m}(x) \right|$$

in which $F_{1,n}(x)$ and $F_{2,m}(x)$ are the empirical CDFs of the two data sets and $D_{n,m}$ is the supremum of the distance between them, where n and m are the sizes of the two samples. In our case, sample 1 consists of the model-consistent marginal costs, and sample 2 the revealed marginal costs. The sample size for both samples is $I * T$. We can analytically approximate the two CDFs and find the maximum distance of the two CDFs in the domain of marginal costs. Note that the KS statistic quantifies a distance between the CDFs of the two sets of marginal costs recovered from data. This distance is a function of the variable $\widehat{\delta}_{i,t}$ that we obtained in Section 3.1. For large samples, the KS statistic follows an asymptotic Kolmogorov distribution. We calculate the KS test statistic defined above. We then report p-values corresponding to our test statistics and the degrees of freedom.

3.2.2. Parametric difference-in-differences test for behavioral change based on asymptotic normality

As a complement to the KS test, we can directly use the distance variable $\widehat{\delta}_{i,t}$ to derive a parametric test of behavioral change when there is a policy intervention. To that end, we combine the logic of Varian (1985), who imposed a strong distributional assumption on a revealed preference model to arrive at an exact distributional form for a test statistic, with an experimental design based on difference-in-differences. We arrive at a test statistic that is asymptotically normal and that is based on differencing unknown quantities that otherwise would be problematic for testing (suppress the subscripts for simplicity in below).

Suppose that the difference, δ, between the revealed marginal cost, $\widehat{mc}$, and the model-consistent marginal cost, $\widetilde{mc}$, is an i.i.d. random variable:

$$\delta = \widetilde{mc} - \widehat{mc}$$

Under the null hypothesis,

$$E(\delta) = \ 0.$$

This equation alone does not provide a path to a test statistic, because an empirical distribution of distances could be centered at zero but involve large discrepancies between the revealed and model-consistent marginal costs. However, we can use the first moment condition as a step toward construction of a test. Note:

$$Var(\delta) \ = \ E\left(\delta^2\right) - (E(\delta))^2 \ = \ E\left(\delta^2\right).$$

Thus, the sample mean of δ^2can be used to estimate the variance of δ:

$$\overline{\widehat{\delta}^2} = \frac{1}{NT} \sum_{i=1}^{n} \sum_{t=1}^{T} \widehat{\delta}^2_{i,t}\ .$$

By the Central Limit Theorem,

$$\overline{\widehat{\delta}^2} \sim N\left(E\left(\delta^2\right), \sigma^2_{\delta^2}\right).$$

Thus,

$$\frac{\overline{\widehat{\delta}^2}\sqrt{nT}}{\widehat{\sigma}_{\widehat{\delta}^2_{i,t}}} \sim N\left(E\left(\delta^2\right), 1\right),$$

where $\hat{\sigma}_{\hat{\delta}^2_{i,t}}$ is the sample standard deviation of δ^2. The problem at this juncture is that we do not know $E(\delta^2)$ under the null hypothesis for each data set that we might consider testing. This makes it difficult to conduct a test within a given sample (unlike the KS test where we do not need to know the distribution). However, note that under the null hypothesis, introducing a policy treatment to one group would not induce a behavioral change. In our setting, this implies that the difference-in-differences of the average squared distance to the model is zero. In computing difference-in-differences, the true means of δ^2 conveniently cancel out. Thus, we can construct a test for the DiD of the distributions. We need only assume independence across the groups.

Let the four group-specific true values for $E(\delta^2)$ be $\mu_{B,Tr}$, $\mu_{A,Tr}$, $\mu_{B,C}$ and $\mu_{A,C}$, where B is before, A is after, Tr is treated, and C is control. We consider the following null hypothesis against the alternative:

H0. $DiD = 0$ (the policy produces no behavioral change)

HA. $DiD \neq 0$.

The difference-in-difference test statistic is:

$$DiD = \left[\left(\overline{\hat{\delta}^2}_{A,Tr} - \mu_{A,Tr}\right) - \left(\overline{\hat{\delta}^2}_{B,Tr} - \mu_{B,Tr}\right)\right] - \left[\left(\overline{\hat{\delta}^2}_{A,C} - \mu_{A,C}\right) - \left(\overline{\hat{\delta}^2}_{B,C} - \mu_{B,C}\right)\right],$$

Because under the null hypothesis the treatment intervention produces no true behavioral change,

$$\left(\mu_{A,Tr} - \mu_{B,Tr}\right) - \left(\mu_{A,C} - \mu_{B,C}\right) = 0.$$

In other words, under the null, if there was a change in the treated group, it corresponds to a change in the control group of the same magnitude over the same period.

Under the null hypothesis, the difference-in-differences reduces to:

$$DiD = \left[\left(\overline{\hat{\delta}^2}_{A,Tr} - \overline{\hat{\delta}^2}_{B,Tr}\right) - \left(\overline{\hat{\delta}^2}_{A,C} - \overline{\hat{\delta}^2}_{B,Tr}\right)\right].$$

Assuming independence, the sums and differences are normally distributed, and we can estimate the variance using sample variances of the group-specific δ^2 estimates. We can then write the test statistic as:

$$Z = \frac{\sqrt{nT}}{\sqrt{\hat{\sigma}^2_{\hat{\delta}^2_{A,Tr}} + \hat{\sigma}^2_{\hat{\delta}^2_{B,Tr}} + \hat{\sigma}^2_{\hat{\delta}^2_{A,C}} + \hat{\sigma}^2_{\hat{\delta}^2_{B,C}}}}\left[\left(\overline{\hat{\delta}^2}_{A,Tr} - \overline{\hat{\delta}^2}_{B,Tr}\right) - \left(\overline{\hat{\delta}^2}_{A,C} - \overline{\hat{\delta}^2}_{B,Tr}\right)\right].$$

The null hypothesis will be rejected when Z is larger than the critical value from a standard normal distribution.

4. Proof of concept with simulated data

To demonstrate that our method can distinguish between data coming from the CCM and data from a common-pool resource managed with individual quotas, we develop a simulated data experiment based on a hypothetical fishery, with and without IFQs. In particular, we assume that aggregate output F is generated from a Cobb-Douglas production function, with equal weight on inputs of total fishing effort (Q) and the stock of fish. There are 100 heterogeneous firms, which each take the price of fish as exogenous. Costs are quadratic in effort, and each firm has a unique cost parameter. Following the standard Gordon-Schaefer bioeconomic model, fish stocks grow logistically. See Appendix B for additional details, including all functional forms and parameter values.

To simulate data from the CCM, we solve the optimization problem for each firm, following the model of Section 2 and the first-order condition given by Eq. (2). To simulate the IFQ regime, using the same parameters, we solve for the fishery's maximum sustainable yield, invert the production function to obtain the corresponding aggregate effort level in each period, and allocate time-specific effort quotas across the 100 firms in proportion to their CCM effort shares. This general approach to simulated bio-economic experiments, in which firms' individual behavior is a function of the resource base while their collective behavior feeds back onto the resource, follows Ferraro, Sanchirico, and Smith (2019).

We generate four simulated data sets: the two property rights regimes (no rights under CCM and IFQs), each with low- and high-cost scenarios. In both cost scenarios, our linear programming results fail to reject the CCM for data coming from the CCM, as expected, whereas the linear program does reject the CCM when simulated data come from the IFQ regime. Details of these results can be found in Appendix B. When sub-sampling from the data and computing rejection rates, as described in Section 2.2, we always fail to reject the CCM with data generated from the CCM, whereas we reject the model under the simulated IFQ regime at least 20% of the time with subsamples containing as little as 3 years and 5 vessels, and 100% of the time with 50 or more vessels (see appendix Table B1). Finally, computing distance to the CCM, as described in Section 3.1, we find a distance of zero or near-zero for the simulated CCM, whereas distance to the CCM is much higher in the simulated IFQ regime (Table B2), as expected. Running the non-parametric KS test, we find consistent results. Namely, we fail to reject the null that the empirical and model-consistent marginal costs are from the same distribution when the true model is CCM, and we reject the null that the empirical and model-consistent marginal costs are from the same distribution when the true model is the IFQ. These results illustrate the ability, in principle, of our approach to detect departures from the CCM.

5. Empirical application

We apply our test to the Norwegian whitefish fishery using data for the period 1998 to 2007. The setting is fitting for two reasons. First, fisheries are a classic example of a common-pool resource. Moreover, even when regulators partly restrict access through industry-wide (but not individual) quotas, open-access incentives to obtain a larger share of that shared quota persist (Homans and Wilen 1997; Smith et al. 2008; Abbott and Wilen 2011; Birkenbach et al. 2017). Second, this particular fishery experienced a management change during the sample period that strengthened individual property rights for a portion of the fleet, thereby reducing open-access incentives. As such, we expect the CCM to be more consistent with the data before the policy change than after. Dividing the sample into treatment and control groups before and after the policy change allows for a comparative test of two policy regimes.

In the remainder of this section, we further describe the Norwegian fishery and the data.

5.1. The Norwegian whitefish fishery

Norway has the largest fishing industry in Europe. Its most valuable fishery is whitefish (also known as groundfish). The Norwegian whitefish fishery includes many species, but cod, haddock and saithe (Atlantic Pollock) are the most important in terms of total volume and value. Norway's whitefish fishery is biologically separate from other major fisheries, so output from the fishery $F(Q)$ can be modeled in isolation as a single resource. The fleet targeting whitefish includes various vessel groups of different sizes and gear. Trawlers are relatively large vessels, with lengths ranging from 28 to 76 m, and they fish in deeper off-shore waters. The coastal fleet comprises smaller vessels under 28 m using a variety of gear such as long lines and troll nets. Our sample contains only the coastal fleet, as individual vessel quotas were introduced for the trawlers at an earlier time. The management system requires that each fishing vessel is separately owned by an operator, so vessels can be taken as firms in our model.

In 1989 a total allowable catch (TAC) quota was set for the whole whitefish fishery, with the TAC divided between the trawler fleet and the coastal fleet. In 1990, a non-tradable individual vessel quota system was introduced for the Norwegian coastal fleet, at least theoretically. However, to ensure that the allocated quotas were fished within the coastal vessel group, an "overbooking system" was introduced, in which the sum of the individual vessels' quotas were substantially higher than the TAC for the vessel group. Consequently, the individual vessel quota system was non-binding, making the management more like a regulated restricted access system (RRA) than a true IFQ system with security of individual harvest rights. From the perspective of our theoretical model, we view this early period as preserving the open-access CCM regime, with some restrictions on technological inputs and total catch, but with no individual limits on catch (or effort) and with incentives promoting excessive effort to claim shares. Our data (described below) begin in 1998, during this regime.

In 2003, the quota for the coastal fleet was divided into four groups by vessel length (<11 m, 11–15 m, 15–21 m, 21–28 m). Thus, groups no longer needed to compete across vessel length categories. This appears to have helped the small vessels as a group, as they no longer had to compete with larger vessels for their share of the quota. However, initially, the sum of the individual quotas still exceeded the TAC (group quota). Hence, although theoretically they could catch all of their individual quota, vessels still had to race other vessels of the same size class to reach their limit. Moreover, there was no guarantee they would get any quota. Effectively, the individual quotas remained upper-bound constraints, but they could not be binding for everybody.

Finally, in 2004, overbooking ended for vessels above 15 m. Additionally, these larger coastal vessels were allowed to combine quotas from several vessels onto one, thereby introducing a form of transferability into the system. Thus, the regime for larger coastal vessels transformed to a truly binding IFQ system in 2004, while it remained an RRA system for smaller vessels.[4] Our analysis ends in 2007 when an IFQ system was also introduced for vessels between 11 and 15 m.

To summarize, from 1998 to 2002, all vessels in our data set were under an RRA regime that we expect preserved CCM incentives. After 2003, larger coastal vessels transitioned into an IFQ regime while the small vessels were still under an RRA regime. In between, 2003 was something of a transition year. Small vessels and large vessels were given separate group quotas, but still competed within group, a problem that may have been especially severe for small vessels.

This change in property rights regimes affords an opportunity to apply our test of the tragedy of the commons using a difference-in-differences (DiD) design. We expect higher rejection rates for large coastal vessels for the 2003–7 period, relative to the 1998–2002 period, and relative to the corresponding difference for small vessels. In sensitivity analyses, we also omit 2003.

5.2. Description of data

The data for the Norway coastal fleet covers the period 1998 to 2007 and come from an annual random survey conducted by the Norwegian Directorate of Fisheries of vessels with only a sample of the registered active vessels being surveyed each year. Table 1 summarizes the data. The first row shows the sample size. The second row shows the total number of vessels registered in each year (population). The total sample comprises 1127 individual vessels from 1998 to 2007. Each vessel is identified with a unique ID. We have information on the length and weight of each vessel as well as intermediate and basic inputs, including days at sea, operating days (days at sea plus days working at port), fuel expenditure, labor compensation, and the average number of crew members operating the vessel.

[4] (Hannesson, 2013), Standal et al. (2016) and (Cojocaru et al., 2019) provide further information about the fishery and the development of the management system. Liu et al. (2024) analyze the policy's effects on productivity.

Table 1
Summary statistics for selected output variables.

Variable		1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
Obs.		307	321	328	323	316	279	321	306	317	359
Population		1193	1143	1081	1063	1230	1441	1342	1131	1165	1290
Sampled annual value (100 mil. NOK)		3.61	3.67	3.67	3.91	3.98	4.54	4.61	4.68	6.58	7.40
Total annual value (100 mil. NOK)		17.64	14.91	13.83	15.60	14.33	12.58	13.55	14.65	19.62	19.30
Sampled annual harvest (10 million kg)		4.17	4.62	4.94	5.31	5.81	6.64	7.84	8.23	8.43	9.25
Cod	Mean	77.7	55.2	45.0	48.3	52.2	51.5	59.4	72.0	85.4	73.7
(thousand kg)	SD	87.2	60.3	53.6	51.2	38.5	38.3	45.4	63.2	72.3	66.6
	Min	0.1	0.9	0.6	0.2	0.1	0.2	0.1	0.2	0.3	0.0
	Max	471.4	411.1	581.8	334.6	332.6	299.3	294.6	452.0	444.4	451.3
Haddock	Mean	19.8	10.7	9.0	11.4	12.7	12.6	11.4	16.7	17.7	21.4
(thousand kg)	SD	38.3	21.9	19.7	14.3	26.9	32.7	21.3	30.4	28.2	38.7
	Min	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	Max	204.3	188.1	211.3	92.4	251.3	416.2	158.5	260.5	185.0	310.8
Saithe	Mean	29.9	26.3	22.8	24.7	19.7	23.2	22.8	31.9	50.1	47.3
(thousand kg)	SD	68.9	49.5	32.9	42.6	37.8	33.3	38.0	68.5	101.6	101.6
	Min	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	Max	574.1	418.7	251.7	420.0	321.1	197.3	199.2	716.4	873.8	943.7
Other	Mean	70.4	58.6	91.3	51.9	40.5	41.1	32.8	45.3	61.9	71.7
(thousand kg)	SD	248.2	212.3	302.6	178.1	131.9	94.7	77.7	110.4	162.4	263.9
	Min	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	Max	1807.2	1859.2	2203.4	1864.4	1409.4	644.3	673.4	899.4	2014.3	2482.1

With respect to outputs, we have vessel-year data on the total quantity landed and revenues received by species (cod, haddock, saithe and other whitefish), in tons and Norwegian Krone (NOK), respectively. However, our test only requires knowing the aggregate revenue. Thus, we first create an index by summing over fish species, then sum over vessels to obtain the total sample revenue for each year, $p_t\widehat{\mathrm{F}}_t$. Then, we multiply the average sample revenue by number of total vessels in the population to obtain the aggregate revenue. Row 3 of Table 1 shows the total sample revenue. Row 4 converts this sample to an estimate of total revenue, multiplying the sample mean by the number of vessels operating. This is the value of output $p_t\widehat{\mathrm{F}}_t$ used in our test. It shows some ups and downs followed by an upward trend after 2003. The next row similarly shows the trend in sampled catch in tons.

Although it requires only annual aggregate revenue on the output side, our test requires micro-level data on the input side. Vessels are not necessarily sampled in each year and do not necessarily fish in all years anyway, so we have an unbalanced panel of vessel-level inputs. Also, some fields were left blank in the survey. Accordingly, we exclude vessels that reported no operating days or days at sea but positive labor, fuel or other operating expenses. Table 2A shows raw data on inputs, including operating days, days at sea, person-years, labor compensation, and fuel expenditure.

5.3. Quantifying effort

In taking the theoretical model to the data, a central modeling question is how to measure effort (or intermediate input) $q_{i,t}$, which appears as a scalar in the theoretical model. As measures of effort, we consider the following four proxies: operating days, imputed days at sea, imputed days at sea times vessel length (Length* Days), and an estimated scalar-valued function of effort based on multiple inputs. Table 2B shows summary statistics for these four input proxies. Of these, operating days, which includes days at sea as well as days processing and offloading in port, is the most straightforward proxy.

Our second measure is days at sea. Averaging over time, days at sea contains 81.3 fewer days fleet-wide than operating days, and there are 748 observations with positive operating days but zero reported days at sea. Since it is impossible to have zero days at sea when operating days and catch are positive, we treat these zeros as missing and replace them with imputed values when the associated operating days are positive. Details of this imputation are provided in Appendix C. Our third measure of input uses these imputed days at sea times vessel length. Rescaling fishing time by measures of vessel size is a common practice when estimating fisheries production functions, as a better measure of overall inputs (Squires 1987; Huang and Smith 2014). Our fourth and final measure of input aggregates multiple input variables into a scalar-valued function, using additional structure. This too is a common practice in the fisheries literature (see McCluskey and Lewison 2008 for review and discussion). This approach essentially imposes a functional form on *f*() from Eq. (1), so this version of the CCM is semi-parametric. Specifically, suppose the production function for vessel *i* in year *t* is

$$\ln\left(Catch_{i,t}\right) = b * lnq_{i,t} + \lambda_t + e_{i,t}, \tag{6a}$$

where λ_t is a dummy which captures year effects, such as different stock levels, and $q_{i,t}$ denotes the overall effort level for vessel *i* at year *t*, and is a sub-function of other inputs. In particular, let

$$\ln\left(q_{i,t}\right) = \alpha_2\ln\left(person-years_{i,t}\right) + \alpha_3\ln\left(fuel\ expenditure_{i,t}\right) + \alpha_4\ln\left(labor\ compensation_{i,t}\right) + vesselid_i, \tag{7}$$

in which person-years denotes the labor input (measured at the day level) and labor compensation is the total payment to workers on the vessel and $vesselid_i$ is vessel level fixed effect that captures vessel length, tonnage, and unobserved heterogeneity in fishing skill.

Table 2A
Summary statistics for selected input variables (raw data).

Variable		1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
Obs.		69	72	80	76	71	279	321	306	317	359
Operating days	Mean	268.2	262.0	268.5	253.8	244.2	213.3	193.8	220.9	227.4	210.1
	SD	32.6	41.1	41.1	45.2	44.0	54.7	51.7	56.2	57.0	53.6
	Min	204.0	176.0	190	107	146	99.0	83.0	90.0	93.0	90.0
	Max	338.0	364.0	348	338	342.0	354.0	342.0	345.0	355.0	338.0
Days at sea	Mean	219.4	211.4	198.3	175.5	178.2	168.7	168.8	178.3	189.5	168.9
	SD	33.2	40.0	50.1	42.8	46.6	46.9	46.0	58.7	56.2	53.9
	Min	152.0	117.0	60.0	50.2	95.0	72.0	77.0	55.0	72.0	68.2
	Max	295.0	322.0	343.0	335.0	287.0	336.0	324.0	330.0	345.0	325.0
Person years	Mean	2.3	2.2	2.1	2.2	2.1	2.2	2.1	2.3	2.4	2.4
	SD	1.8	1.8	1.8	1.6	1.6	1.4	1.3	1.5	1.5	1.5
	Min	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0
	Max	12.0	12.0	12.7	11.0	12.6	10.7	8.1	10.0	8.1	9.0
Labor compensation (thousand NOK)	Mean	637.3	607.6	574.8	652.3	593.7	511.2	607.4	772.3	1025.8	1015.9
	SD	799.9	808.9	791.9	821.6	592.5	480.4	562.8	721.6	937.6	979.2
	Min	65.5	81.5	65.8	63.1	109.3	104.1	108.0	149.1	141.5	158.2
	Max	5161.4	6658.9	5930.7	6151.7	4918.5	3906.7	4606.4	4973.9	6920.2	7184.6
Fuel expenditure (thousand NOK)	Mean	47.9	52.3	80.6	70.6	59.8	59.7	72.6	108.0	135.5	121.6
	SD	73.0	91.9	161.3	127.3	108.1	92.6	97.9	163.7	177.8	194.1
	Min	3.0	3.4	1.5	4.6	3.2	1.3	3.1	6.9	10.2	9.6
	Max	539.5	745.7	1405.7	1458.6	1066.7	1113.5	937.7	1610.0	1605.5	1623.6

Table 2B
Summary statistics for selected input variables (as used in analysis).

Variable		1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
Obs.		69	72	80	76	71	279	321	306	317	359
Operating days	Mean	258.2	262.0	268.5	253.8	244.2	213.3	193.8	220.9	227.4	210.1
	SD	32.6	41.1	41.1	45.2	44.0	54.7	51.7	56.2	57.0	53.6
	Min	204.0	176.0	190.0	107.0	146.0	99.0	83.0	90.0	93.0	90.0
	Max	338.0	364.0	348.0	338.0	342.0	354.0	342.0	345.0	355.0	338.0
Imputed days at sea	Mean	217.4	211.4	198.3	175.5	178.2	168.7	168.8	178.3	189.5	169.0
	SD	33.2	40.0	50.1	42.8	46.6	46.9	46.0	58.7	56.2	53.9
	Min	152.0	117.0	60.0	50.2	95.0	72.0	77.1	55.0	72.0	68.2
	Max	295.0	322.0	343.0	335.0	287.0	336.0	324.6	330.0	345.0	325.0
Length times Imputed days at sea	Mean	4169.2	4067.3	3748.1	3248.1	3197.8	2200.5	2237.6	2434.3	2605.8	2377.7
	SD	1261.4	1449.2	1713.7	1312.7	1377.6	1090.4	1146.4	1349.6	1260.3	1247.1
	Min	2133.6	1772.6	877.8	707.8	1459.2	696.0	672.0	581.9	816.4	606.6
	Max	7707.8	8826.0	9415.4	9195.8	7720.7	8564.4	8898.0	9058.5	8771.2	8908.3
Estimated effort	Mean	9.66	9.41	8.86	9.61	7.66	3.23	3.72	4.72	5.98	5.82
	SD	6.01	6.71	7.21	6.77	5.74	2.95	3.32	4.31	5.23	5.43
	Min	0.83	1.79	1.35	1.38	2.15	0.83	0.94	0.96	1.11	1.07
	Max	29.18	36.55	36.11	35.33	31.54	25.54	25.03	28.99	39.45	41.24

Substituting Eq. (7) into (6), we estimate the combined model, recovering the $\hat{\boldsymbol{\alpha}}$. Once these are estimated, we construct a scalar $\hat{q}_{i,t}$ using (7). Note, however, that we cannot separately identify b in Eq. (6) from the alphas in Eq. (7). Thus, we do not identify effort to scale. This is not problematic, however, because our test treats the cost of effort as a latent function, so any arbitrary change of scale in effort can be reconciled by an offsetting change in the scale of the cost function. The results of estimating this model are shown in Appendix Table C2.

5.4. *Sampling subsets of data*

As specified in Section 2.2, the principal test is on collective behavior, so the presence of only one firm behaving out of step with the other firms could result in rejecting the entire data set. Furthermore, if cost functions actually shift over time, assuming they are constant over long time periods could lead to false rejections. To sidestep these issues, we follow Carvajal et al. (2013) and repeatedly sample smaller subsets of data. Sampling the data allows us to consider rejection *rates* (percentage of data sets that do not conform to the CCM), rather than one single all-or-nothing conclusion. We divide the entire data set into multiple subsets, with each set consisting of N vessels and T consecutive years, where $N \in \{5, 10, 50, 100, 150\}$ and $T \in \{3, 6, 8, 10\}$. Then we separately test for consistency with the CCM using each set. We randomly sampled 100 subsets from each N-by-T combination, giving us a reasonable estimate of the rejection rates for each combination. To facilitate comparisons, we used the same subsample of data for each cell across models. Note that increasing N and T only increases the number of constraints, so when the samples are drawn from the same data generating process, rejection rates are non-decreasing in N and T in expectation. This is not to say they must converge to one, as they may asymptote. Thus, if the data are generated from the CCM, rejection rates will remain low as N and T increase. In contrast, if data is truly generated from a process inconsistent with the CCM, rejection rates will indeed approach one as N and T increase.

5.5. Weighted sampling and property rights regime comparison

As discussed in Section 5.1, the evolution of property rights in the Norwegian fishery motivates splitting the data into the periods of the RRA regime (1998–2002) and the period of IFQs for the coastal vessels at least 15 m in length (2003–2007). Accordingly, we cut the data into four cells using a 2×2 design; large coastal vessels ($\geq$15 m long) and small vessels ($<$15 m), each before the IFQ regime (1998–2002) and after (2003–2007). The data generated from the IFQ regime is not expected to be consistent with the CCM, especially for large vessels. Looking at the difference in differences, we expect the before-after difference for large vessels will be higher than those for small vessels.

It is worth noting that, though we sub-sample by vessel size in this exercise, in the common-ratio properties for each group of each year, we keep the total input Q_t and revenue $p_t F_t(Q_t)$ fixed across subsamples. That is, behavior of all vessels (regardless of length) still affects the optimal behavior of any one vessel because the aggregate resource stock in coastal areas is a common pool.

In this unbalanced panel for the Norwegian coastal fleet, the administration of the random survey led to fewer surveyed vessels in earlier time periods (before 2003) than later (after 2003). When we sample subsets as described in Section 5.4 with no restrictions (where each vessel has an equal probability to be selected), the sets sampled in later periods will contain more data points than those from earlier periods. Given the nature of our test, more data points create more constraints, which tends to yields higher rejections holding all other things equal. Hence, to make sure the gap in rejection rates per group is attributed to behavioral difference under different management regimes, rather than the difference in the number of observations in the samples, we employ weighted sampling to generate comparable samples for each group.

The weighted sampling is implemented by redistributing sampling probabilities among vessels in later periods (2003–2007). Sampling probabilities for vessels with more observations (3 and 4 data points in periods 2003–2007) are reduced, and the reduced probabilities are added to vessels with fewer observations (1 and 2 data points), with the total probability always summing to one. The largest adjustment of the probability of a vessel is less than 0.0002, while the original probability of a vessel being sampled is around 0.00116, so the adjustment is less than 17%. After weighted sampling, the maximum difference in the number of observations between the groups (before vs. after) is less than 0.2% (difference in observations divided by total observations in subsample sets). In our 2×2 design, our weighted sampling ensures that the large-after and large-before groups have similar numbers of observations, as do the small-after and small-before groups.

6. Results

In this section, we first present results of the principle test as described in Section 2.2. We then present results with the latent parameter to account for not observing total output (Section 2.3) and statistical tests based on distance from revealed marginal costs to model-consistent marginal costs (Section 3.2), including results based on our DiD design.

6.1. Results of test pooling all data

We first implement our tests on all data pooled together. In this case, we expect to reject CCM behavior, as we are including post-reform data in the analysis. Table 3 presents results using the principal test of Section 2.2 using operating days as a proxy for effort. Appendix tables C4-C6 present results with three alternative proxies for effort. Each cell in the tables shows the rejection rate for a sample of 100 data sets for N vessels and T consecutive years, for varying N and T. Note first that the rejection rate is increasing in the sample size. As noted in Section 5.4, this trend follows mechanically in expectations. More substantively, the trend also is consistent with the idea that, as we increase T, we risk including data from the period after the property rights reform, when the CCM is unlikely to apply. Indeed, in this case, when more than 100 vessels are considered for longer than 6 years, the rejection rates approach one. This indicates that the behavior of fishers in our sample is inconsistent with the CCM for the pooled data.

Table 3
Rejection rates — operating days.

Years Number of Vessels	3	6	8	10
5	0.01	0.00	0.04	0.22
10	0.04	0.03	0.30	0.53
50	0.40	0.58	0.96	1.00
100	0.81	0.88	1.00	1.00
150	0.93	1.00	1.00	1.00

Additionally, we test consistency with the model incorporating a latent parameter (as discussed in Section 2.3). The range of error we adopted is [−5%, 5%]. That is, we restrict the multiplier γ_t to be between [0.95, 1.05]. Given that the average revenue in our sample is 1.4 million NOK (around 166,000 USD) per year per vessel, this bandwidth allows for an average adjustment to the revenue of 67,000 NOK (around 8000 USD) per year per vessel.[5] Table 4 and appendix tables C7-C9 present the results. As we would expect with added flexibility, rejection rates of the CCM allowing for error in the total output are slightly lower than those in the basic model (comparing like cells). But the previous patterns remain. This result provides additional support for the conclusion that behavior of fishers in our overall sample is not consistent with the CCM.

6.2. Results comparing property rights regimes

Recall that all vessels operated under RRA before 2003. Throughout the period (1998–2007) in our sample, a TAC for all participants was in place, but in 2003 the quota was distributed to groups based on vessel length. After 2003, small vessels remained operating under a total allowable catch and the RRA regime, while large vessels transitioned to an IFQ regime. Whereas there is competition among vessels under a group quota, competition among large vessels is reduced under the property-rights based management of IFQs because each large vessel is guaranteed its share of the total catch. The effectiveness of the property-rights approach of IFQs over the non-property-rights based approach of RRA motivates a DiD design to test empirically for behavioral change.

Table 5 and appendix tables C10-C12 present rejection rates per group using the weighted sampling described above in Section 5.5. The results indicate that, after the reform, large vessels experience a higher relative increase in rejection rates of the CCM compared to small vessels.[6] As shown in the last column, most DiD effects are positive. This implies the IFQ regime generates fishing behavior that is less consistent with the CCM than the RRA regime. In other words, the IFQ regime nudges fishing behavior away from Nash behavior more effectively than does RRA, as one would expect.

We also replicated these tests omitting 2003, which was a transition year. Our results are qualitatively similar using this approach and are available upon request.

We also tested the CCM by quantifying minimum distance to the model and deriving test statistics (Section 3). The first column of Table 6 presents the results for the combined data. For each of the four measures of effort, the first row shows an adjusted mean squared error $\delta^2_{i,t}$ per cell, per constraint to be satisfied.[7] The second row shows the absolute error as a percentage of marginal cost, similarly adjusted. The errors are small, with the mode being zero. The third row shows the p-values for the KS test of Section 3.2. As shown in this first column, for all four measures of effort, we reject the CCM with the pooled data with p-values < 0.01. Results from these tests confirm our observation from the rejection rates in Tables 3 and 4.

In Columns 2–5 of Table 6, we break down the distance results by segment of the data. Comparing across vessel sizes and property rights regimes for any one measure of effort, we see a notable increase in the distance to the CCM in the "after" period relative to the "before" period, as we would expect (first two rows). Moreover, we see greater increases for the large vessels, as expected. The final column shows the DiD, which is consistently positive. For completeness, we also provide KS for each group (row three) but caution that the sample sizes are different and that p-values general cannot be compared across non-nested models. The fourth row shows the Z-test and associated p-value for the difference-in-differences from Section 3.2. We reject the null (at $p < 0.05$) for three of the four inputs. These results are consistent with our hypothesis that the IFQ regime changed behavior for the treated group and moved behavior away from the CCM. The results are also consistent with parametric difference-in-difference regressions that find the IFQ policy increased productivity for large vessels (Liu et al., 2024).

Finally, we consider one additional sensitivity analysis. In case there are any doubts that the reform altered the game played by small vessels as well as large, it is reasonable to consider a more conservative approach that leverages only the before-after difference around the 2003 reform, pooling small and large vessels in each time period. We thus cut the sample in two rather than four groups and replicate the analysis of Table 6, considering only the inter-temporal difference, while pooling vessel sizes. The results are shown in Appendix Table C3. They continue to show an increase in average distance to the CCM model after the reform. Moreover, the KS tests reject the hypothesis that the distributions are the same in the after period (p-vale < 0.02 in all cases). They also reject the hypothesis that the before and after distributions are the same. These results lend further support to our conclusions and illustrate how our test can signal policy-induced behavioral change.

[5] Because of the large number of missing values in the sampled data, we apply narrow boundaries to the permissible error. Our unbalanced panel data of the Norwegian whitefish fishery has 79.3% of data points missing. The amount of missing data substantially reduces nonempty constraints in our test, which makes it easy to find marginal costs that are consistent with the model. Allowing for a larger adjustment to the total revenue makes the tests even less stringent and reduces the rejection rates towards zero. For instance, all rejection rates are zero when the boundary is 10% in our case.

[6] Note that after we split the data into four groups, there are fewer observations to sample from per group. Because the weighted sampling only controls for the *difference* in the number of observations of each paired group (before vs. after), but not the *magnitude* of observations in samples, the levels of rejection rates are sensitive to the number of observations in the respective subgroups, but the difference and DiD results reflect the overall change in management regimes and are more stable.

[7] This adjustment is necessary for comparisons across cuts of the data in the next subsection, as it accounts for the changing number of constraints. For example, if there are N vessels and T years of data, and if there were no missing data, there would be NT cells used as the denominator for the simple mean squared error, but $N(T^2\text{-}T)/2 + T(N^2\text{-}N)/2 = NT(N + T\text{-}2)/2$ constraints used as the denominator for the adjusted mean squared error. Our actual calculation accounts for missing values in the formula.

Table 4
Rejection rates — operating days, with error.

Years Number of Vessels	3	6	8	10
5	0.00	0.15	0.26	0.21
10	0.00	0.13	0.25	0.38
50	0.03	0.15	0.31	0.59
100	0.10	0.21	0.36	0.69
150	0.18	0.28	0.40	0.75

Table 5
Rejection rates per group with weighted sampling – operating days.

Years	Vessels	Large-after	Large-before	Small-After	Small-before	Diff-in-Diff
3	5	0.04	0.15	0.01	0.07	−0.05
3	10	0.28	0.30	0.08	0.23	0.13
3	50	0.92	1.00	0.57	0.97	0.32
4	5	0.19	0.16	0.05	0.08	0.06
4	10	0.53	0.40	0.16	0.30	0.27
4	50	1.00	0.99	0.89	1.00	0.12
5	5	0.16	0.10	0.05	0.09	0.10
5	10	0.48	0.46	0.18	0.29	0.13
5	50	1.00	1.00	0.90	0.99	0.09

Table 6
Distance to the model, by vessel size and property rights regime.

Measure of Effort		(1) Combined	(2) Small Before	(3) Small After	(4) Large Before	(5) Large After	(6) DiD
Operating Days	Adjusted MSE	0.00145	0.00020	0.00130	0.00030	0.00382	0.00242
	Adjusted Abs Pct E	0.0589%	0.0300%	0.0559%	0.0357%	0.1851%	0.1236 pp
	KS p-val	<0.001	0.869	0.002	0.626	0.165	–
	Z	–	–	–	–	–	4.042
	p-val						<0.001
Imputed Days at Sea	Adjusted MSE	0.00342	0.00004	0.00227	0.00223	0.00697	0.00251
	Adjusted Abs Pct E	0.0712%	0.0094%	0.0578%	0.0507%	0.2052%	0.1062 pp
	KS p-val	<0.001	0.988	<0.001	0.716	0.415	–
	Z	–	–	–	–	–	1.985
	p-val						0.047
Days x Length	Adjusted MSE	0.00003	0	0.00002	0.00002	0.00007	0.00003
	Adjusted Abs Pct E	0.1271%	0%	0.0605%	0.0765%	0.2608%	0.1238 pp
	KS p-val	<0.001	1	<0.001	0.716	0.352	–
	Z	–	–	–	–	–	2.512
	p-val						0.012
Estimated Total Effort	Adjusted MSE	0.49853	0.10541	0.36138	0.43094	0.90580	0.21889
	Adjusted Abs Pct E	0.0111%	0.0122%	0.0092%	0.0240%	0.0267%	0.0057 pp
	KS p-val	0.005	1	0.018	0.716	0.982	–
	Z	–	–	–	–	–	0.841
	p-val						0.400

This table shows, for each of the four measures of effort, the mean-squared error (i.e., mean of the squared distances between model-consistent marginal costs and the revealed marginal costs) adjusted for the number of constraints in the quadratic program (rather than the number of cells), the mean absolute error as a percent of marginal costs (similarly adjusted, pp stands for percentage point), and p-value for the KS and the difference-in-difference Z tests.

7. Conclusion

Work to date on testing the tragedy of the commons has focused either on policy outcomes involving the state of shared resources or, when using behavioral data, has relied on highly structural models involving numerous maintained assumptions. Drawing on applications of revealed preference theory to behavioral data, including Carvajal et al. (2013) on the Cournot model, we derive non-parametric tests of the CCM using minimal assumptions.

We apply this new test to the Norwegian groundfish fishery. Overall, we find the behavior of individual vessels of the Norwegian Coastal Fishery does not conform to the CCM. More importantly, we find that rejection rates are larger after property rights reforms, especially for the large vessels that received stronger property rights. Additionally, using a distance-based metric, we find that behavior moves further from the pure CCM after the reforms. Our results suggest that Norwegian policy has changed behavior and ameliorated the commons problem for large coastal vessels at least to some extent.

Our methodological innovations have practical significance. By providing methods that account for errors in aggregate output and that can gauge distances to the CCM with associated statistical tests, our approach is broadly applicable to applied problems that are likely to arise in regulated common-pool resource settings. For example, in most settings outputs or inputs are likely to be measured with error, often coming from self-reports or other surveys. Although fishery provides an iconic illustration of the commons, our approach can also be applied to a wide range of other resources. Candidate common-pool resource problems include clearcutting forests or fuelwood collection under different governance structures; grazing livestock on common land; siting offshore aquaculture facilities; pumping groundwater; oil, gas, and other mineral extraction; collective management of infectious diseases; pesticide resistance; and controlling invasive species.

Just as our specific model can be applied to other common resources, our general approach can be extended and applied to other behavioral rules and settings beyond common-pool resources. While our test of the CCM pertains to the average-return game with Nash behavior, Banzhaf and Liu (2016) extend the CCM to the case of conjectural variations (rather than Nash behavior) as suggested by Cornes and Sandler (1983). Because our empirical application involves hundreds of players, we expect Nash behavior to be more relevant than conjectural variations, but small numbers of players are more prevalent in other resource settings such as unitization of oil fields or groundwater extraction from a local aquifer. Moreover, the game can be modified to apply to the average cost (rather than average returns) game, where agents choose outputs and pay average costs. Such problems are relevant to many problems involving the division of joint costs, such as telephony. Looking past natural resources, our approach could be applied to collective farms or other enterprises whenever outputs are divided proportionately to inputs (Sen 1966). Alternatively, simply by adapting the objective function, numerous other sharing rules could be considered and the respective behavior tested, including equal per capita sharing, which tends to lead to shirking rather than over exploitation. In this way, our approach potentially extends to many surplus-sharing games.

Whatever the application, policies to regulate the commons are extremely diverse, so it is natural to ask whether some policy configurations move behavior away from the CCM more than others. Our approach can facilitate comparative work on the behavioral consequences of different policy interventions and other approaches to governing common-pool resources. Economists often imagine a stylized first-best policy to ration access to the commons, with perfectly secure and transferable individual property rights. That first-best policy is juxtaposed with a complete lack of policy under pure open access. However, real-world policies are configured in myriad ways that differ from theoretical first-best policies. For example, in fisheries, rights-based systems differ along dimensions of the security of the property right, the length of term, transferability, and a number of other restrictions that often come about as political compromises to address community or industry concerns (Asche et al. 2018). Moreover, property rights-based policies tend to build on existing institutions, which already ration access to the commons to some degree and create incentives differing from those of pure open access (Birkenbach et al. 2017). In general, our model and distance-based metric have the potential to examine whether different policies induce more or less commons-like behavior.

One limitation of applying our approach directly in comparative work is the unknown true variance of distance to the CCM. We are able to circumvent this problem in our application using difference-in-differences because we have treated and control units measured before and after a policy change. But in comparative work, analysts may not observe common-pool resource regimes before and after a policy or governance change and may be limited to comparing regimes across applications. This reality reinforces the need to build on revealed preference models for which the data have well-understood econometric properties. For example, models of revealed preference with measurement error, such as Aguiar and Kashaev (2021), potentially could be customized to study the commons and allow for a more general approach than ours to conduct statistical tests in comparative work.

Such comparative work using revealed preference methods has broad interdisciplinary appeal. Although economists have studied the commons problem for more than a century, Hardin's (1968) coining of the term "tragedy of the commons" in the general scientific literature helped to garner attention from ecologists, other environmental scientists, non-economics social scientists, legal scholars, and systems modelers (Banzhaf 2023; Frischman et al. 2019). Subsequently, Ostrom (1990) criticized Hardin for ignoring the potential for self-organized solutions to the commons and ultimately helped to develop a new interdisciplinary field to investigate problems in the commons (Wilson et al. 2013). Within this new field, there are widely disparate normative views on what governing the commons ought to achieve and thus what outcomes should be measured. Economists are often stereotyped as having a singular focus on formal property rights-based solutions to the commons and the generation of resource rents without regard for potential unintended consequences on local communities (Young et al. 2018). Our approach offers a neutral behavioral test of interest across disciplines that does not depend on how different scholars attach normative weight to resource rents, distributional outcomes, employment, or any other indicators of community well-being.

Declaration of competing interest

The authors have no competing interests to declare.

Data availability

The authors do not have permission to share data.

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Supplementary materials

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