

Demographic Precision and Causal Narrowness:
A Critical Review of Bennington et al. 2026 on Hector's
Dolphin Fecundity and Bycatch Management

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Abstract

Bennington et al. 2026 provide a technically strong estimate of fecundity in Hector's dolphin (*Cephalorhynchus hectori*) using four decades of photo-identification data from Banks Peninsula. Their central result—that mature females reproduce less frequently than previously assumed—is important for demographic modelling and conservation management. The paper estimates fecundity at approximately 0.29 offspring per mature female per year, corresponding to an average calving interval of about 3.4 years. This is a valuable result. However, the article's framing around “current management of bycatch” raises a serious methodological concern. The model estimates reproductive output. It does not investigate the causes of low fecundity, impaired recovery, long-term decline, or population crisis. It does not include chemical contaminants, infectious disease, toxoplasmosis, immune dysfunction, reproductive toxicity, prey limitation, vessel disturbance, habitat degradation, or cumulative stressors as explanatory variables. This review is written from the standpoint of researchers who regard the chemical pollution hypothesis as serious and insufficiently examined. We do not claim that chemical pollution has been proven as the principal cause of the Hector's and Māui dolphin crisis. Nor do we deny that fisheries bycatch is a real and important threat. Our argument is narrower and methodologically stronger: Bennington et al. 2026 should be read as a contribution to demographic parameter estimation within an already stabilised bycatch-management framework, not as a causal assessment of the population crisis. The paper supports a precautionary reduction of allowable additional mortality. It does not demonstrate that fisheries bycatch is the principal cause of the crisis. The deeper question is whether conservation science has become too accustomed to refining parameters within an established explanatory framework while leaving the unresolved causal structure of the problem insufficiently discussed. This review is therefore an attempt to return to a literal understanding of science: there is a problem; the problem has not been solved; and the competing explanations must be openly examined.

Keywords

Hector's dolphin; Māui dolphin; *Cephalorhynchus hectori*; fecundity; bycatch; set nets; gillnets; trawl fisheries; population recovery; causal attribution; chemical pollution hypothesis; toxoplasmosis; cumulative stressors; reproductive toxicity; immune dysfunction; conservation modelling; structural falsification; scientific silence; stabilised science; accompaniment science; evidentiary asymmetry; research framing; New Zealand conservation policy.

Main Points

- Bennington et al. 2026 is a technically serious and useful demographic paper. Its main contribution is an updated estimate of Hector's dolphin fecundity, not a causal explanation of the Hector's and Māui dolphin crisis.
- The reported fecundity estimate of approximately 0.29 offspring per mature female per year implies a slower reproductive rate than previously assumed and therefore a lower capacity for population growth.
- The paper's mathematical result is important for any model that estimates resilience, population growth, or allowable additional mortality. The review does not challenge the value of this demographic correction.

- The central limitation is conceptual: the article estimates how often mature females reproduce, but it does not explain why fecundity is low or why recovery is impaired.
- The model does not include chemical contaminants, toxoplasmosis, infectious disease, immune dysfunction, reproductive toxicity, prey limitation, vessel disturbance, habitat degradation, climate-related change, or cumulative stressors as explanatory variables.
- A lower fecundity estimate makes any additional mortality more dangerous. This is true for bycatch, but it is also true for any other preventable source of mortality or reproductive impairment.
- The conclusion that lower fecundity reduces resilience is formally correct, but it is not an explanatory conclusion. Without a causal diagnosis, it remains close to a demographic truism: if reproduction is lower, recovery is harder.
- The main scientific question should therefore be: what is preventing recovery? A fecundity estimate contributes to this question, but it does not answer it.
- Bycatch enters the article not as a tested hypothesis, but as an already established management framework. The article refines the demographic basis of that framework rather than reopening the causal question.
- The title and framing direct the reader toward “current management of bycatch”. This is not a neutral scientific signal. It is a policy-rhetorical framing that may lead readers to treat a demographic correction as implicit support for the bycatch paradigm.
- The article should have explicitly stated that it does not attribute historical or current population decline to a single cause and that bycatch implications are precautionary management implications, not causal attribution.
- The problem may be non-stationary. The factors driving population decline may change over time, interact, intensify, or be joined by new factors. A model that uses simplified constant parameters may be useful for estimation, but it cannot by itself explain a complex historical process.
- The possibility of a new or insufficiently recognised factor X should remain open. Conservation science should not prefer simple cognitive solutions merely because complex causal reconstruction is difficult.
- If new documents or data strengthen source and pathway components of the chemical pollution hypothesis, then scientific silence after their publication becomes itself a legitimate object of analysis.
- Structural falsification does not require direct fabrication of data. It may occur when a scientific field repeatedly gives full modelling status to one hypothesis while treating other hypotheses as peripheral, speculative, or methodologically inconvenient.
- An important feature of structural falsification is sincere expert conviction. A research community may believe that its framework simply is “the science”, and this conviction can make omissions appear natural rather than problematic.

- The review does not claim that Bennington et al. intentionally misrepresented evidence or acted on behalf of corporate interests. Such claims would require a much higher standard of proof.
- The defensible institutional question is structural: have funding priorities, policy usability, advisory networks, and citation practices favoured the bycatch framework over chemical and cumulative-stressor hypotheses?
- The chemical pollution hypothesis should not be accepted without evidence. But neither should it remain marginal merely because it is more difficult to model than fisheries bycatch.
- The fisheries hypothesis should not be rejected. Bycatch is real and important. But it should not be treated as the default causal explanation in papers that do not causally test it.
- A conservation science that repeatedly improves the precision of one explanatory framework while neglecting the causal complexity of the crisis may become technically impressive and practically inadequate.
- The article is not a bad paper. Its strength is precisely why it matters: strong papers reveal what a field can do well and what it has learned not to ask.
- The proper conclusion is not denunciation but open scientific discussion. The problem remains unresolved. The dolphins have not recovered. The causal question must be reopened with methodological seriousness.

Introduction

The Problem Has Not Been Solved

The Hector's and Māui dolphin crisis has been studied for decades. Scientific work has been extensive. Management plans have been developed. Fisheries restrictions have been introduced. Population models, spatial risk assessments, mortality limits, and conservation measures have all been produced. Yet the problem has not disappeared (1).

This fact should generate scientific unease.

If a conservation problem remains unresolved after decades of research and management, the appropriate response is not merely to refine existing parameters within the dominant framework. It is also necessary to ask whether the dominant framework itself has become too narrow. Science, understood literally, begins with an unresolved problem. It does not begin with the assumption that the problem has already been adequately framed.

Bennington et al. 2026 is important because it represents a mature form of contemporary conservation science (2). It is technically sophisticated, based on long-term data, and directly connected to management. Precisely for that reason, it is a valuable object of critical analysis. Weak papers reveal little about the structure of a scientific field. Strong papers reveal much more. They show not only what a field can do well, but also what it has learned not to ask.

The article estimates fecundity in Hector's dolphins and argues that lower fecundity has implications for bycatch management. This conclusion is understandable within the existing New Zealand management regime. Official management documents identify set nets as the most significant fishing-related threat, while also recognising other human-induced and non-fishing threats, including toxoplasmosis and other pressures [3–6].

The problem is not that bycatch is irrelevant. The problem is that bycatch often appears not as one hypothesis among others, but as the default explanatory and management horizon. Bennington et al. do not prove that bycatch is the principal cause of the Hector's and Māui dolphin crisis. Their model is not designed to answer that question. Yet the article's title, framing, and conservation implications may reinforce the impression that the central causal problem has already been settled.

It has not.

Definitions and Scope

Because this review uses several terms in a precise sense, definitions are necessary.

Fecundity means the number of offspring produced per mature female per unit time. In the reviewed article, fecundity is estimated as offspring per mature female per year.

Bycatch means unintended capture or death of non-target species in fishing activity, including gillnets, set nets, and trawls.

Causal attribution means the identification and weighting of the processes that have produced decline, impaired recovery, or low population growth. It is different from estimating a demographic parameter.

A management implication is a recommendation or consequence for policy. A management implication may be precautionary even when causal attribution remains incomplete.

Stabilised science refers here to a research situation in which the central explanatory and management framework has become normalised. Scientists no longer ask why that framework is dominant; they refine parameters inside it.

Accompaniment science refers to research that accompanies an existing policy regime by improving its parameters, thresholds, monitoring tools, or models without reopening the foundational causal question.

Structural falsification does not mean direct fabrication of data. It refers to the production of a systematically incomplete scientific picture through the long-term exclusion, marginalisation, or non-recognition of relevant information.

Evidentiary asymmetry means a situation in which one hypothesis becomes highly modelled, fundable, and policy-operational, while another remains fragmented, interdisciplinary, and weakly integrated into the recognised body of knowledge.

This review uses these terms analytically. It does not assert individual dishonesty. It examines the structure of scientific framing.

Declared Standpoint of This Review

This review is written by authors sympathetic to the chemical pollution hypothesis (7-9). That standpoint should be stated openly. We consider chemical contamination, including persistent organochlorines, phenoxy-herbicide residues, chlorophenols, solvents, pesticide mixtures, contaminated groundwater pathways, and cumulative toxicological stress, to be a serious and insufficiently integrated line of investigation.

However, this review does not attempt to replace the fisheries hypothesis with the chemical hypothesis by assertion. We do not claim that chemical pollution has been demonstrated as the principal cause of the decline. We do not deny that gillnets, set nets, trawls, and other fishing-related mechanisms can kill dolphins. We do not suggest that bycatch management should be abandoned.

Our position is different.

Competing hypotheses should be evaluated symmetrically. A demographic study should not be allowed to function rhetorically as a causal confirmation when it does not perform causal attribution. If the problem is complex, then scientific writing should preserve that complexity rather than repeatedly compressing it into a single dominant management frame.

This is not advocacy against bycatch research. It is an argument against explanatory closure.

RESULT

What the Article Actually Does

The article estimates fecundity. It uses long-term photo-identification data from Banks Peninsula and applies a Bayesian open-population multi-event capture-recapture model. The model addresses a genuine methodological difficulty: field observations of dolphins are incomplete. A female may not be seen in a given year. A calf may be missed. The reproductive status of an observed female may be uncertain. Directly observed reproductive histories can therefore underestimate or distort true reproductive output.

The modelling strategy is appropriate for this limited demographic task. At a simplified conceptual level, the model distinguishes hidden biological states from observed field states. For female i in year t , let the true, unobserved state be represented as:

$$Z_{i,t} \in \{B, NB, D\}$$

where B means a breeding female, NB means a non-breeding female, and D means dead.

The observed field state may be represented as:

$$Y_{i,t} \in \{B_{obs}, NB_{obs}, U, NO\}$$

where B_obs means observed with a calf, NB_obs means observed with a juvenile, U means observed but reproductive status uncertain, and NO means not observed.

The model therefore has two linked components: a biological process model for transitions among hidden states and an observation model for imperfect field detection and state assignment.

$$\text{Biological process: } P(Z_{i,t+1} | Z_{i,t})$$

$$\text{Observation process: } P(Y_{i,t} | Z_{i,t})$$

The main parameters include re-sighting probability p , survival probabilities ϕ_B and ϕ_{NB} , state-assignment probabilities δ_B and δ_{NB} , and transition probabilities between breeding and non-breeding states. The key transition parameters for fecundity are:

$$\psi(B, NB) = P(Z_{i,t+1} = NB | Z_{i,t} = B, \text{ alive})$$

$$\psi(NB, B) = P(Z_{i,t+1} = B | Z_{i,t} = NB, \text{ alive})$$

This is technically appropriate for the demographic question. It is not, however, a model of the causes of low fecundity or population decline. The variables that might explain why a female breeds or does not breed in a given year are not included as causal covariates.

The Core Mathematics and Its Correct Interpretation

The core mathematical result can be reconstructed from the transition probabilities between breeding and non-breeding states. Conditional on being alive, the simplified two-state reproductive transition structure is:

$$\text{from B to NB: } \psi(B, NB)$$

$$\text{from NB to B: } \psi(NB, B)$$

$$\text{from B to B: } 1 - \psi(B, NB)$$

$$\text{from NB to NB: } 1 - \psi(NB, B)$$

Let F denote the long-run proportion of years in which a mature female is in the breeding state. In a two-state stationary approximation, F satisfies:

$$F = F[1 - \psi(B, NB)] + (1 - F)\psi(NB, B)$$

Solving this equation gives:

$$F[\psi(B, NB) + \psi(NB, B)] = \psi(NB, B)$$

$$F = \psi(NB, B) / [\psi(B, NB) + \psi(NB, B)]$$

Using the reported transition values discussed in the article:

$$\psi(NB, B) \approx 0.374$$

$$\psi(B, NB) \approx 0.883$$

therefore:

$$F \approx 0.374 / (0.374 + 0.883)$$

$$F \approx 0.374 / 1.257$$

$$F \approx 0.2975 \approx 0.298$$

This simple reconstruction is close to the published fecundity estimate of approximately 0.291. The difference is expected because the article obtains its final result through posterior simulation, not by direct substitution of posterior mean transition values into a single deterministic equation [2].

The average calving interval can then be approximated as the reciprocal of fecundity:

$$ICI \approx 1 / F$$

Using the article's estimate:

$$ICI \approx 1 / 0.291 \approx 3.44 \text{ years}$$

Thus, the mathematical result is straightforward: mature females are estimated to be in a breeding state in roughly 29% of years, or to produce one calf approximately every 3.4 years. This is an important demographic correction.

The interpretive boundary is equally clear. The mathematics estimates a reproductive rate. It does not identify the causes of that rate. Formally, the model estimates F as a function of observation histories and demographic transition parameters:

$$F = g(p, \varphi_B, \varphi_{NB}, \delta_B, \delta_{NB}, \psi(B, NB), \psi(NB, B), \text{observation histories})$$

It does not estimate F as a function of possible causal drivers:

$$F_t = f(\text{contaminants}_t, \text{disease}_t, \text{toxoplasmosis}_t, \text{prey}_t, \text{maternal condition}_t, \text{habitat}_t, \text{vessel disturbance}_t, \text{cumulative stress}_t)$$

This distinction is the central mathematical limitation of the paper. A low fecundity estimate is compatible with multiple causal explanations. It may reflect life history, age structure, environmental pressure, disease, contaminant exposure, prey limitation, calf loss, maternal condition, or interacting stressors. The model can estimate the average reproductive outcome, but it cannot explain the causal process that produced it.

The Strength of the Paper

The strengths of the paper should be recognised clearly.

First, the study uses a long-term dataset. Four decades of monitoring are rare and valuable in marine mammal conservation.

Second, the paper does not rely on naïve counts of calves. It recognises that field observation is imperfect and that reproductive events may be missed.

Third, the multi-event framework is well suited to the problem. It allows the authors to distinguish biological states from observation states and to estimate reproductive dynamics under uncertainty.

Fourth, the paper has direct management relevance. If fecundity is lower than previously assumed, then the population's capacity to compensate for additional mortality is also lower. This matters for any management regime that sets allowable mortality limits.

The critique offered here does not depend on rejecting the model. The model may be technically appropriate for estimating fecundity. The problem lies in the movement from demographic estimation to causal and management framing.

What the Article Does Not Do

The article does not determine why fecundity is low.

It does not determine why recovery is slow.

It does not determine the relative contribution of bycatch, disease, toxins, prey limitation, climate change, vessel disturbance, habitat degradation, or cumulative stressors.

It does not model chemical exposure.

It does not model toxoplasmosis.

It does not model immunological impairment.

It does not model reproductive toxicity.

It does not model historical pollution sources, contaminant transport pathways, marine exposure, tissue burden, or population-level toxicological consequences.

This matters because fecundity is not merely a demographic number. It may also be an ecological signal. Low fecundity may reflect life history, age structure, prey conditions, chronic stress, disease, endocrine disruption, immune dysfunction, contaminant exposure, or other interacting factors. A model can estimate fecundity without explaining it.

Therefore, the logical conclusion of the article is limited: fecundity is lower than previously estimated; lower fecundity reduces the population's capacity to compensate for additional mortality. The article does not justify the stronger conclusion that the principal cause of the crisis is fisheries bycatch.

This distinction is central.

Recovery Without Causal Diagnosis

The weakest conceptual point in the article concerns the language of recovery. In the simplest demographic form, population change can be written as:

$$N(t+1) = N(t) + B(t) - D(t)$$

where $B(t)$ represents births or recruited calves and $D(t)$ represents deaths. The article improves the estimate of the reproductive component. If $M(t)$ is the number of mature females, then a simplified birth term can be written as:

$$B(t) \approx F(t) \cdot M(t)$$

The article's contribution is to estimate F , or more precisely an average fecundity parameter derived from the long-term observation histories. That is useful. But the causes of $F(t)$, the causes of $D(t)$, and the causes of variation in both terms remain largely outside the model.

Deaths can be decomposed as:

$$D(t) = D_{\text{bycatch}}(t) + D_{\text{disease}}(t) + D_{\text{toxoplasmosis}}(t) + D_{\text{pollution}}(t) + D_{\text{vessel}}(t) + D_{\text{unknown}}(t)$$

Births and calf recruitment may also depend on multiple variables:

$$B(t) = f(\text{age structure, nutrition, disease, contaminants, maternal condition, calf survival, cumulative stress})$$

A recovery claim requires more than a fecundity estimate. It requires showing that the population growth rate exceeds replacement under a specified causal scenario:

$$\lambda(t) = N(t+1) / N(t)$$

Recovery requires, at minimum, $\lambda(t) > 1$ over a relevant period.

Bennington et al. do not estimate the causal relationships required to explain $\lambda(t)$. Consequently, they cannot show that recovery will be achieved by bycatch reduction alone. They can show only that, under lower fecundity, any additional mortality becomes more dangerous.

That statement is formally correct, but it is not a causal diagnosis. It is close to a demographic truism: if reproduction is lower, recovery is harder. The scientific question begins at the next step: why is reproduction lower, and what processes are preventing recovery?

A study that estimates fecundity contributes to the recovery question, but it does not answer it. If the goal is recovery, the central scientific question must be: what is preventing recovery?

Non-Stationary Processes, Factor X, and the Limits of Simple Models

The process leading to dolphin decline may not be constant through time. It may have phases. It may involve changing mixtures of causes. It may include interactions among stressors that intensify or weaken. It may include a new or insufficiently recognised factor X that appears only at a certain historical moment or becomes visible only when older data are re-examined.

In mathematical terms, the problem may be non-stationary. Fecundity, mortality, exposure, disease pressure, prey availability, and fishing risk may all vary through time:

$$F(t) \neq \text{constant}$$

$$D_k(t) \neq \text{constant for } k \in \{\text{bycatch, disease, pollution, vessel, unknown}\}$$

$$X(t) = \{\text{contaminants}(t), \text{infection}(t), \text{prey}(t), \text{habitat}(t), \text{fishing effort}(t), \text{climate}(t), \text{unknown factors}(t)\}$$

A constant-parameter model may estimate an average over a historical period:

$$\bar{F} \approx (1/T) \sum_t F(t)$$

but an average can conceal phases, thresholds, delayed effects, and changing causal mixtures. A model may be technically elegant and still be causally inadequate if the real process is time-varying and multi-factorial.

The point is not that complex models are always better. The point is that the object itself may be complex. If the decline of Hector's and Māui dolphins is driven by interacting or time-varying processes, then the research programme must develop methods capable of examining such processes. Scientific difficulty is not an argument for excluding difficult hypotheses.

In this field, the correct response is not to look for simple cognitive comfort. It is to bring new methods to the problem: integrated exposure reconstruction, toxicological pathway analysis, necropsy synthesis, disease surveillance, food-web analysis, time-varying population models, spatial contamination mapping, and systematic comparison of competing hypotheses.

The problem is not that Bennington et al. use a simplified model for fecundity estimation. That is acceptable for the limited task. The problem is that the article's framing may allow a limited model to reinforce a broader causal picture that the model does not test.

Why Bycatch Becomes Central

Bycatch appears in the article not as a hypothesis being tested, but as an already established management framework. This is understandable. New Zealand conservation management already uses fisheries-related mortality limits, spatial closures, and bycatch reduction measures. Bycatch is visible, countable, regulatable, and compatible with demographic modelling.

The management logic is approximately:

$$\text{lower fecundity} \rightarrow \text{lower intrinsic growth potential}$$

$$\text{lower intrinsic growth potential} \rightarrow \text{lower sustainable additional mortality}$$

$$\text{lower sustainable additional mortality} \rightarrow \text{stricter bycatch limits}$$

Where the Population Sustainability Threshold or a comparable mortality limit is used, the logic is often represented in the general form:

$$PST = N \cdot 0.5R_{\max} \cdot F_R$$

where N is population size, $R_{\max}$ is the maximum population growth rate, and F_R is a recovery or tuning factor. If $R_{\max}$ is reduced, the threshold for allowable additional mortality is reduced as well. This is a valid management argument.

But the implication is conditional and precautionary, not diagnostic. The correct inference is:

$$F \downarrow \Rightarrow \text{tolerance for additional mortality} \downarrow$$

The incorrect inference is:

$$F \downarrow \Rightarrow \text{bycatch is the principal cause of the crisis}$$

The article supports the first statement. It does not support the second.

The same demographic logic would apply to any preventable source of mortality or reproductive impairment. If chemical contaminants reduce fertility, suppress immune function, increase calf mortality, or increase susceptibility to disease, then lower fecundity would also strengthen the case for investigating those mechanisms. If toxoplasmosis contributes significantly to mortality, then lower fecundity would strengthen the case for toxoplasmosis control. If prey limitation affects maternal condition, then lower fecundity would strengthen the case for food-web analysis.

Thus, the demographic result should widen the causal discussion, not narrow it.

Stabilised Science and Accompaniment Science

This article is a useful example of what may be called stabilised science. By this term we mean a research situation in which the central explanatory and management framework has already become normalised. Scientists no longer ask why that framework is dominant. They refine parameters inside it. The original framework appears “natural”, and this apparent “naturalness” is precisely what allows other scientific viewpoints to be ignored.

Stabilised science is not necessarily bad science. It can be technically excellent. It can produce precise estimates. It can improve models. It can help management. But it may also lose contact with the unresolved structure of the original problem.

This is precisely what appears to be happening in the present case. The dolphin population crisis has not been resolved, yet the relevant expert community continues to operate within a closed explanatory space and appears to treat other hypotheses as if they were naturally unworthy of serious attention. Within this institutional setting, a judgment seems already to have been made: competing explanations do not require sustained examination.

Such situations are often characteristic of privileged universities and research clusters that receive regular funding from multiple sources and are treated, almost by default, as the “frontier” of science. Under such conditions, external experts may too easily be dismissed as amateurs, eccentrics, or marginal figures whose questions do not deserve serious engagement.

It appears that elements of this situation may be present in relation to the University of Otago and the group of experts working on Hector’s and Māui dolphins. The high ranking

and general academic reputation of the University of Otago are not the issue (10). A high university ranking, however, is not a licence to ignore researchers who do not work at Otago and who do not belong to an expert circle committed to one dominant explanatory hypothesis.

If a university is recognised as a high-ranking institution, then it has a particular responsibility to ensure the quality, transparency, and scientific integrity of the work carried out under its name. There should be no reasonable doubt about the professional competence, methodological openness, and ethical standards of its researchers.

We did not create the standards of scientific inquiry, and it is not for us to abolish them. But those standards exist. However highly ranked the University of Otago may be as an institution, it has no right to revise the standards of scientific knowledge in practice by allowing a closed expert group to treat a complex ecological crisis through a narrow and habitual vocabulary.

A highly ranked university should embody the standards of science. It should not become an institutional shelter for expert circles that repeatedly use familiar explanatory formulas to manage the appearance of understanding, while the underlying ecological crisis remains unresolved.

Bennington et al. can also be read as an example of accompaniment science. By this term we mean research that accompanies an existing policy regime by improving its parameters, thresholds, models, or monitoring tools, without reopening the foundational causal question. Accompaniment science is not automatically invalid. But it becomes problematic when the environmental crisis remains unresolved and competing hypotheses remain underdeveloped.

In this case, the article refines the demographic basis of bycatch management. It does not ask whether the bycatch-management framework is sufficient to explain the crisis. It does not ask whether the low fecundity it estimates may itself be an effect of unmodelled stressors. It does not ask whether the causal architecture of the problem should be revised in light of competing evidence.

This is the central criticism.

The issue is not that the authors should have written an entirely different paper. The issue is that a technically strong paper should clearly mark the limits of its inference. When it does not, it risks strengthening a causal paradigm it has not tested.

It is possible that the expert community has long considered the principal questions resolved and therefore no longer returns to them. If so, that is itself the problem. A crisis that has not been solved should not be treated as cognitively closed.

Structural Falsification, Expert Conviction, and Scientific Silence

The concept of structural falsification is useful here, but it must be used carefully. It does not mean that individual scientists fabricated data. It does not mean that the authors of

Bennington et al. intentionally misrepresented evidence. Such claims would require a different and much higher standard of proof.

Structural falsification refers to a different mechanism: the scientific picture may become distorted through the long-term exclusion of significant information from the recognised body of knowledge. In the formulation developed by Gibbs and Nikolaenko, structural falsification is not direct data fabrication but a situation in which the organisation of scientific visibility produces a systematically incomplete representation of reality (9).

A crucial feature of structural falsification is that it can operate through sincere conviction. Members of a specialised expert community may believe that their framework simply represents “the science”. Precisely because this conviction is sincere, omissions may not be experienced as omissions. Competing hypotheses may appear irrelevant before they are examined.

This is relevant because the Hector’s and Māui dolphin case contains competing hypotheses: fisheries-related mortality, demographic vulnerability, infectious disease, habitat change, toxoplasmosis, and chemical pollution. Gibbs and Nikolaenko argue that newly released Groundwater Technology documents from 1995–1996 strengthen the source and pathway components of the chemical hypothesis, while not proving chemical causation by themselves [11–15].

If such data were hidden, neglected, or unavailable for decades, and if after publication they still fail to enter the recognised research programme, then the functioning of the scientific community becomes a legitimate object of inquiry. Silence after publication is not necessarily misconduct. But structured silence around potentially important evidence is not scientifically neutral.

The key point is methodological. If new evidence capable of changing the research programme appears, and the dominant research strategy remains essentially unchanged, then the response of the scientific system itself becomes an object of analysis. The question is not merely “What caused the decline?” but also “Which forms of evidence have been allowed to shape the causal reconstruction?”

This question is especially important in environmental contamination cases. Chemical causation often requires reconstruction across a long chain: industrial source → contaminated site → groundwater → runoff → coastal transport → sediments → food web → tissue accumulation → biological response → population consequence

Bycatch, by contrast, is more directly visible and more easily incorporated into population models. One can count observed captures, estimate fishing effort, model spatial overlap, and evaluate closures. This difference creates evidentiary asymmetry: one hypothesis becomes highly modelled and institutionally mature, while another remains fragmented and methodologically underdeveloped.

The comparison may therefore become one between a mature research programme and a partially developed one, rather than between two hypotheses tested on equal grounds [8,9].

Bennington et al. is not itself a case of direct falsification. It may, however, contribute to structural narrowing if read as another technically strong study that places bycatch at the centre while leaving alternative causal hypotheses outside the analytical frame.

The Problem of Framing

The title of the article is: “Multi-event modeling of Hector’s dolphin fecundity using four decades of monitoring: Implications for current management of bycatch.”

A more neutral title might have been: “Multi-event modeling of Hector’s dolphin fecundity using four decades of monitoring: Implications for population recovery under multiple stressors.” Another possibility would have been: “Implications for population recovery and threat assessment.”

The existing title is not factually false. But it is a framing choice. It connects fecundity directly to bycatch management before the article has performed any causal test of bycatch as the primary explanation. This matters because titles, abstracts, and conservation implications strongly influence how scientific work is received by managers, journalists, NGOs, and the public.

A demographic paper can therefore function socially as a bycatch paper, even if it does not causally test the bycatch hypothesis.

This is not a minor issue. If many papers use similar framing, the cumulative result may be a scientific and public common sense in which bycatch appears not as one serious hypothesis among several, but as the natural explanation of the entire crisis.

That is how stabilised science becomes socially powerful.

In this specific sense, the title has a policy-rhetorical and quasi-propagandistic function. It guides attention toward a preferred management object and away from the unresolved causal question. This does not require assuming bad faith. But it does mean that the title is not merely descriptive. It participates in the construction of the problem.

For a scientific article, such framing should be handled with greater caution. The stronger the policy implication, the clearer the boundary between demographic estimation, precautionary management, and causal attribution must be.

What the Article Should Have Said

The article would have been more balanced if it had included a clear methodological limitation such as the following:

“This study estimates fecundity and does not attribute historical or current population decline to a single cause. Lower fecundity may interact with multiple stressors, including

fisheries bycatch, infectious disease, toxoplasmosis, chemical contaminants, prey limitation, vessel disturbance, and cumulative environmental change. Therefore, implications for bycatch management should be understood as precautionary management implications, not as causal attribution.”

Such a paragraph would not weaken the article. It would strengthen it.

It would preserve the value of the demographic result while preventing its overinterpretation. It would also acknowledge the actual complexity of the problem and invite broader scientific discussion.

The absence of such framing is significant. It leaves the reader with a technically precise result embedded in a causally narrow management frame.

Institutional Context Without Unsupported Accusation

There are legitimate questions about the institutional structure of research in this field: funding priorities, policy usefulness, citation networks, advisory roles, and the repeated centrality of certain explanatory frameworks. These questions should be investigated. However, this review does not claim that the authors of Bennington et al. acted on behalf of any corporate interest. The article itself does not provide evidence for such a claim.

The stronger and more defensible point is structural.

The article should be analysed as a text operating within a broader institutional field in which some explanatory frameworks become highly modelled, fundable, policy-operational, and repeatedly cited, while others remain fragmented, interdisciplinary, difficult to model, and weakly integrated into management.

The question is not whether individual scientists intentionally served an external interest. The question is whether the structure of research funding, institutional priorities, policy usability, advisory relationships, and scientific recognition has systematically favoured the bycatch framework over chemical and cumulative-stressor hypotheses.

This is a scientific question. It can be studied empirically through citation analysis, funding histories, authorship networks, policy-document reconstruction, conflict-of-interest review, advisory-group mapping, and comparison of how different hypotheses are represented in models, reviews, and management plans.

There may be important questions about the relationship between environmental science, industry exposure, and institutional incentives. But in this review those questions remain research questions, not accusations. This distinction is essential.

Why This Matters

The danger is not only academic. It is practical.

If the real problem is primarily bycatch, then stricter bycatch reduction should lead to recovery, provided other mortality is low enough. If the real problem is multi-causal, then

bycatch reduction may be necessary but insufficient. If low fecundity is partly driven by chemical exposure, disease interaction, prey limitation, immune dysfunction, or cumulative stress, then refining bycatch limits alone will not solve the problem.

A conservation science that repeatedly improves the precision of one framework while neglecting the causal complexity of the crisis may become highly technical and practically inadequate.

One can improve demographic precision until the last dolphin disappears.

Precision is not enough. Explanation is required.

There is an ecological problem. It involves the continued vulnerability of Hector's and Māui dolphins. The task is to understand the causes of this process and to take preventive measures. If a research standard produces increasingly refined parameters but does not clarify the causal architecture of the crisis, then it is not enough. It becomes routine. It becomes accompaniment. It becomes science that manages the appearance of understanding rather than understanding the process.

Summary Assessment

Dimension	Assessment
Quality of demographic modelling	High
Value of updated fecundity estimate	High
Usefulness for precautionary management	High
Clarity about causal limits	Insufficient
Treatment of alternative hypotheses	Weak; alternatives are largely absent from the analytical frame
Evidence that bycatch is the principal cause	Not provided
Role in bycatch-management framework	Strong
Risk of reinforcing a single causal paradigm	High
Relevance to chemical and cumulative-stressor hypotheses	Important but indirect
Overall reading	A strong demographic paper of a limited kind

Bennington et al. 2026 is not a bad paper. It is a strong paper of a limited kind. Its strength makes the limitation more important. It shows how contemporary conservation

science can operate with considerable technical sophistication while remaining inside a narrow causal frame.

The article estimates fecundity. It does not explain the crisis.

Open Invitation to Scientific Discussion

The appropriate conclusion is not denunciation. It is discussion.

We invite the authors of Bennington et al., and other researchers working on Hector's and Māui dolphins, to engage in an open scientific discussion of the causes of population decline, impaired recovery, and low population resilience. Such a discussion should not be restricted to fisheries bycatch. It should include bycatch, toxoplasmosis, infectious disease, chemical pollution, contaminant transport pathways, reproductive toxicity, immune dysfunction, prey limitation, habitat change, and cumulative stressors.

The chemical pollution hypothesis should not be accepted without evidence. But neither should it remain peripheral because it is more difficult to model.

The fisheries hypothesis should not be rejected. But neither should it be treated as the default explanation in studies that do not causally test it.

Materials from such a discussion can be published in different journals, including *Pollution and Diseases* (16). Replies, critical responses, data notes, methodological comments, and longer monographic treatments would all be scientifically useful. We are prepared to make such discussion public and prompt.

The point is not to increase bibliometric indicators, secure another round of funding, or protect an established research routine. The point is to understand why the dolphins have not recovered. Environmental research exists not only for careers, institutions, or management rituals. It also exists to solve real environmental problems.

The problem is unresolved. The dolphins have not recovered. The scientific task is therefore not merely to accompany an existing management framework, but to reopen the causal question with methodological seriousness.

There is a problem.

It needs to be discussed.

Conclusion

Bennington et al. 2026 provides a technically strong and valuable estimate of Hector's dolphin fecundity. Its central mathematical result—that mature females reproduce less frequently than previously assumed—is important for population modelling and for precautionary management.

However, the article's framing around current bycatch management risks converting a demographic correction into an implicit reinforcement of a causal paradigm. The model does not include competing or interacting causes of low population growth, such as chemical

contaminants, disease, toxoplasmosis, immune dysfunction, reproductive toxicity, prey limitation, vessel disturbance, habitat degradation, or cumulative stressors. Consequently, the paper supports a precautionary reduction of allowable additional mortality, but it does not demonstrate that fisheries bycatch is the principal cause of the Hector's and Māui dolphin crisis.

The article should therefore be read as a contribution to demographic parameter estimation within an already stabilised bycatch-management framework, not as a causal assessment of the population crisis.

A literal understanding of science requires more. It requires returning to the unresolved problem itself. The central question is not how precisely existing management models can be adjusted, but whether the explanatory framework guiding those models is adequate to the crisis it claims to address.

References

1. Gibbs, Andrew, and Dmitry Nikolaenko. 2026. "Chemical Pollution and the Population Crisis of Hector's and Māui Dolphins. A Critical Research Bibliography, 1970–2026." *Pollution and Diseases*, June, 51 pages. <https://doi.org/10.66659/ybmw2c82>
2. Bennington, S., Dillingham, P. W., Wickman, L., Webster, T., Brough, T., Slooten, E., Dawson, S. M., and Rayment, W. J. 2026. "Multi-event modeling of Hector's dolphin (*Cephalorhynchus hectori*) fecundity using four decades of monitoring: Implications for current management of bycatch." *Conservation Science and Practice* 8: e70242. <https://doi.org/10.1111/csp2.70242>
3. Currey, R. J. C., Boren, L. J., Sharp, B. R., and Peterson, D. 2012. *A Risk Assessment of Threats to Māui's Dolphins*. Ministry for Primary Industries and Department of Conservation, Wellington, New Zealand.
4. Roberts, J., Webber, D. N., Roe, W. D., Edwards, C. T. T., and Doonan, I. J. 2019. *Spatial Risk Assessment of Threats to Hector's and Māui Dolphins (Cephalorhynchus hectori)*. New Zealand Aquatic Environment and Biodiversity Report No. 214. Fisheries New Zealand.
5. Cooke, J. G., Constantine, R., Hamner, R. M., Steel, D., and Baker, C. S. 2019. *Population Dynamic Modelling of the Māui Dolphin Based on Genotype Capture-Recapture with Projections Involving Bycatch and Disease Risk*. New Zealand Aquatic Environment and Biodiversity Report No. 216. Fisheries New Zealand.
6. Department of Conservation and Fisheries New Zealand. 2020. *Hector's and Māui Dolphin Threat Management Plan*. Department of Conservation Te Papa Atawhai and Fisheries New Zealand, Wellington, New Zealand.
7. Gibbs, Andrew. 2026. "Beyond the TCDD Lens in Paritutu New Plymouth, New Zealand: Invisible Phenoxy-Herbicide Co-Contaminants, Visible Developmental Signals, and Decision-Making under Incomplete Evidence." *Pollution and Diseases*, May. <https://doi.org/10.66659/ar7fqk65>
8. Gibbs, Andrew, and Dmitry Nikolaenko. 2026. "Competing Hypotheses and Scientific Non-Recognition: Investigating the Hector's and Māui Dolphin Decline in the Context of

Wartime Chemical Legacies.” *Pollution and Diseases*, June.

<https://doi.org/10.66659/10csws50>

9. Gibbs, Andrew, and Dmitry Nikolaenko. 2026. “Structural Falsification, Scientific Silence, and a New Research Strategy: A Critical Reconstruction of the Hector’s and Māui Dolphin Case.” *Pollution and Diseases*, June, 23 pages.
<https://doi.org/10.66659/tms16797>
10. University of Otago. <https://www.otago.ac.nz/reputation-for-excellence>
11. Australian Analytical Laboratories (AMDEL). Environmental Assessment Project. Certificates of Analysis and Laboratory Reports. 1995. Laboratory analytical documentation prepared for Groundwater Technology (NZ) Ltd.
12. Groundsearch EES Ltd. Ground Penetrating Radar (GPR) and Resistivity Survey for DowElanco (NZ) Ltd. Prepared for Groundwater Technology (NZ) Ltd. November 1995.
13. Groundwater Technology (NZ) Ltd. 1996. Summary of Soil and Groundwater Evaluations, Paritutu Site, New Plymouth, New Zealand. Confidential environmental investigation prepared for DowElanco (NZ) Ltd., 4 July 1996. Publicly released by the Taranaki Regional Council in 2024 after remaining unavailable to the scientific community for approximately three decades.
14. Groundwater Technology (NZ) Ltd. Summary of Soil and Groundwater Evaluations, Paritutu Site, New Plymouth, New Zealand. Appendices. Part 1. Confidential technical report. Prepared for DowElanco (NZ) Ltd. 4 July 1996. Released 2024.
15. Groundwater Technology (NZ) Ltd. Summary of Soil and Groundwater Evaluations, Paritutu Site, New Plymouth, New Zealand. Appendices. Part 2. Confidential technical report. Prepared for DowElanco (NZ) Ltd. 4 July 1996. Released 2024.
16. *Pollution and Diseases*. Journal. <https://pollution-diseases-ojs.org/index.php/pd>

Bibliographical Note

This review deliberately uses a compact bibliography. It is not a new literature review of the whole Hector’s and Māui dolphin field. That broader task has already been addressed in the critical research bibliography by Gibbs and Nikolaenko (1).

The present bibliography is therefore selective. It includes the reviewed article, key official and management texts needed to establish the bycatch-management context, and the authors’ prior works defining the chemical hypothesis, scientific non-recognition, and structural falsification. Additional references should be added only if they perform a clear argumentative function.