

KNO-013 · Models, Maps and Reality

KNO · Ways of Knowing

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Article Abstract

A scientific model is not a replica. It stands in for a selected part or aspect of the world, and the selecting is the work. This article treats omission and deliberate distortion as working parts rather than defects, and separates an error, which a modeller would remove, from an idealisation, which a modeller would defend. It records one published position holding that the laws which unify best describe least, without adopting it. It states the constraint a population biologist formulated in 1966 and the objection that denies the constraint is necessary. It shows that the most quoted sentence about models is not in the paper usually cited. It closes on the realism question two earlier articles handed forward, states both standards and adopts neither.

In 60 Seconds

A model represents a chosen piece of the world, not the world, so asking whether it is good means asking what it was built for. Leaving something out is not the same as getting it wrong: an abstraction omits, an idealisation distorts on purpose, and only the second asserts a falsehood deliberately. The test is intent, because what a modeller would fix on noticing is an error and what they would defend has to earn its place. One developed position holds that the most unifying laws are the least descriptive, recorded here and not adopted. A constraint from 1966 says generality, retained detail and precision cannot be maximised together in a manageable model; an objection denies that the trade is necessary, not that trades occur. The famous sentence about models being wrong sits in a different document from the one usually cited. Whether a model's parts are real is left open, but fit never accredits itself.

Why This Question Matters

A model in use is not a small copy of anything, and the useful question about one is not whether it is true but what it was built to do and what it gave up doing it. Read without that, a model is either dismissed for the parts it never claimed or trusted for reach it never had. The distinctions here are what let a reader ask which falsehood was chosen, what it bought, and whether a memorable formulation is carrying weight its source does not.

1. A model is not a small copy

The word model suggests a scale replica, and the suggestion is misleading in a specific way. A replica aims to preserve everything at reduced size. A scientific model aims at something narrower: it stands in for a chosen part of the world, and the choosing is the work.

The standard formulation puts the target first. Many scientific models are representational models, and they "represent a selected part or aspect of the world, which is the model's target system" [1].

The phrase to hold on to is selected part or aspect. A model does not represent the world; it represents a piece of it, picked out in advance, and the question of whether a model is good is always the question of whether it is good for that piece and that purpose.

A warning about the map analogy, which this article uses sparingly and on purpose. Maps are the standing illustration of selective representation, and an earlier article in this series treats them directly. But the constraint that binds maps most tightly is a geometric theorem about projecting a sphere onto a plane, and **no corresponding theorem about models in general is established here or transported from KNO-006.** Borrowing the map's force without borrowing its mathematics would produce an impressive sentence and an unsupported one. This article therefore uses the map as an illustration of selection and never as evidence about what models can or cannot do.

Where That Leaves Us

The word model suggests a replica, and that is the misleading part. A scientific model stands in for a chosen piece of the world, and choosing it is the work, so whether it is good is whether it is good for that piece and that purpose. Maps illustrate the choosing and are never evidence here, because their own constraint is a theorem this article does not carry over.

2. Leaving things out is not the same as getting them wrong

Models are not one kind of thing, and the distinctions matter for what follows. A **representational** model stands in for a target system. An **abstraction** leaves features out without asserting that they are absent. An **idealisation** deliberately simplifies or distorts what it keeps. Only the third asserts something false on purpose, and if a model only omitted, it would be an abridgement rather than a distortion.

The distinction that matters is between an error and an idealisation. **An error is something the modeller would remove on discovering it. An idealisation is something the modeller put there and would defend.** The reference formulation is explicit about the intent: idealised models "involve a deliberate simplification or distortion of something complicated with the objective of making it more tractable or understandable" [2].

That definition contains a criterion that can be applied. **An idealisation is answerable to a purpose.** A frictionless plane, a well mixed population, a perfectly rigid body: each is false, each is chosen, and each can be asked whether it earns its falsehood in the use to which the model is put. A distortion that no longer earns it has become an error without anyone changing the equations.

Where That Leaves Us

Three things separate here. A representational model stands in for a target; an abstraction omits without claiming absence; an idealisation distorts knowingly, and only it asserts a falsehood on purpose. The test is about intent rather than accuracy: what a modeller would strike out on noticing is an error, and what they would defend has to earn its falsehood against the use the model is put to.

3. A published argument that goes further

There is a stronger position in the literature, and it should be stated as one position rather than as the consensus.

Cartwright argues that fundamental explanatory laws, the deepest and most admired successes of modern physics, "do not in fact describe regularities that exist in nature" [3]. The argument is not that physics fails. It is that explanatory power and descriptive accuracy are obtained by different means and are traded against each other, so the laws that unify most are the ones that describe least.

The same argument carries a distinction that keeps it from collapsing into general scepticism: on this account "theoretical entities, and the complex and localized laws that describe them, can be interpreted realistically, but the simple unifying laws of basic theory cannot" [4]. **The scepticism is aimed at the top of the hierarchy, not at the bottom.**

This article does not adopt that position. It records it because it is a developed argument in the literature and because it sharpens the question section 6 has to answer. A reader should note that it is contested, and that the sentences quoted here come from the publisher's description of the book rather than from a reading of its argument, a limitation stated in the uncertainty record.

Where That Leaves Us

A stronger position exists in the literature and this article records rather than adopts it. On that account the deepest explanatory laws do not describe regularities, because reach and accuracy are bought by different means, and its own distinction keeps it from general scepticism: localised laws and the entities they describe may be read realistically, the unifying ones may not. The quoted sentences come from a publisher's description.

4. The trade you cannot design around

One of the most useful results about modelling is a constraint, and it was stated plainly in 1966. **It is a position in a live debate rather than a settled theorem**, and the objection to it is stated below rather than left implicit.

Levins wrote that "it is of course desirable to work with manageable models which maximize generality, realism, and precision toward the overlapping but not identical goals of understanding, predicting, and modifying nature. But this cannot be done" [5].

Read the sentence carefully, because its structure is often lost. **It names three properties, attaches them to three different goals, and says the three cannot be maximised together in a manageable model.** It does not say that any one of them is unattainable, and it does not rank them. **The word realism here is not the realism of section 6:** in Levins's triad it means how much of a system's detail a model keeps, while in section 6 it is a position about whether the unobservable content of a theory should be believed. The two are unrelated and this article does not let one carry the other. What it says is that within a manageable model the three cannot be maximised together, so that where one is pushed the others are under pressure, and that the trade is a decision whether or not anyone notices making it. **It is Levins's claim about model building as he framed it, and this article does not extend it into a law that every model must obey.**

The objection is direct, and it is not a quibble about wording. Orzack and Sober reject the claim that generality, realism and precision must necessarily trade off against one another, so that the conflict Levins describes is not a universal constraint on model building [12]. Read it as narrowly as it is written: it denies that the trade is necessary, not that a trade ever occurs. A modeller who finds one of the three under pressure has not been refuted, and one who finds all three improving together has not broken a law. **What the objection removes is the entitlement to treat the constraint as settled**, which is what this section was already refusing to claim.

This is the practical content of everything above. Selection, idealisation and the gap between explaining and describing all show up here as a single budget. A model that is very general is unlikely to be very precise about a particular case. A model tuned to one case in detail is unlikely to travel. **Asking which of the three a model gave up is usually more informative than asking whether the model is right.**

Where That Leaves Us

The constraint is that generality, retained detail and precision cannot be maximised together in a manageable model, and detail here is not the doctrine section 6 discusses. The objection recorded beside it denies that the trade is necessary, not that trades happen, so neither pressure on one property nor improvement in all three settles anything. What survives is which was spent, and on what.

5. Wrong, and useful, in two different documents

One sentence about models is quoted far more than the rest of the literature on it, and it is often attributed to the wrong document. **How often is not established here:** no bibliometric study was consulted, and the point of the section does not need one.

The 1976 paper is the one usually cited. What it actually contains is this: "Since all models are wrong the scientist cannot obtain a 'correct' one by excessive elaboration. On the contrary following William of Occam he should seek an economical description of natural phenomena" [6]. That is a complete argument on its own, and it is not the famous sentence. **A direct check of that paper confirms that the phrase but some are useful does not appear anywhere in it [7].**

The famous wording is in a technical report from 1979, where the literal string "ALL MODELS ARE WRONG BUT SOME ARE USEFUL" appears [8], accompanied by the observation that "it would be very remarkable if any system existing in the real world could be exactly represented by any simple model" [9].

The two texts say compatible things and they are not the same text. The 1976 passage is an argument against over elaboration: since no model is correct, adding parameters cannot make one correct, so economy is the right response. The 1979 wording is an argument about the question to ask: not whether the model is true, but whether it is useful. Citing the second sentence to the first paper is a small error with a general moral, and this article states it because the moral is the article's own: **a formulation that circulates on its own is the kind least likely to be checked against its source.**

Where That Leaves Us

One sentence circulates more widely than the literature around it and is commonly credited to the wrong document. The 1976 paper argues that elaboration cannot rescue a model, so economy is the answer, and a direct check finds the famous phrase nowhere in it; the wording belongs to a report from 1979. The moral is this article's own: what travels alone is what nobody checks.

6. The question two earlier articles handed forward

Two earlier articles in this series stopped at the same doorway. One asked whether a classification scheme corresponds to a real division of the world and declined to answer. The other asked whether a measured number corresponds to something real and declined likewise. Both handed the question here, and it is the same question in different clothes.

The two positions should be attributed before any claim of neutrality is made.

Scientific realism, as the reference formulation states it, "is a positive epistemic attitude toward the content of our best theories and models, recommending belief in both observable and unobservable aspects of the world described by the sciences" [10]. On this view the unobservable content of a successful theory is a candidate for belief, and a model that works is evidence that its posits are approximately right.

The rival standard is empirical adequacy, associated with constructive empiricism and given in the same reference through van Fraassen's formulation: "a theory is empirically adequate exactly if what it says about the observable things and events in the world, is true" [11]. On this view a theory that saves the observable phenomena has done all that is required, and belief in its unobservable machinery is optional. **The criterion is not the same as fitting a finite body of data.** As formulated it ranges over what a theory says about the observable, including the observable it has not been tested against, so a good fit to the data in hand is evidence bearing on empirical adequacy rather than the thing itself.

Having stated both, this article adopts neither, and the reason is not diplomatic. The disagreement is about what success licenses, and nothing in the practice of modelling settles it: the same model, performing identically, is a reason for belief on one account and a well functioning instrument on the other. **What the disagreement does settle is a procedural point that both sides accept.** Whichever standard is in force, that a model fits the observable record is itself a finding that has to be established, and it depends on the data used, on how those data were treated, on the rules of comparison, on the assumptions carried into the fitting and on the uncertainty attached to the result. **Fit does not accredit itself.** Any further claim about what the model's parts correspond to is then a separate step again, and has to be argued separately. **The articles that handed this question forward can therefore proceed, and what they get is narrower than an answer.** The realism question is not resolved here and this article does not resolve it. What is established is that a classification and a measurement can each be assessed **operationally**, by what it does and what it was built for, without that assessment waiting on the realism question. The question itself stays open and stays handed forward.

Where That Leaves Us

Two standards are set out and neither is adopted, because the disagreement is about what success licenses and no amount of modelling practice decides it. What both sides grant is procedural. That a model matches the observable record is itself a result to be established, resting on the data, their handling, the comparison rules, the assumptions and the uncertainty. Correspondence is a further step, argued separately.

7. Reading a model well

What is the target? A model represents a selected part or aspect, and criticising it for the parts it never claimed is a category error.

Which distortions are deliberate? Ask which falsehoods the modeller would defend and which they would fix. The second kind are errors; the first kind have to earn their place.

What was traded away? On Levins's account the three cannot be maximised together in a manageable model. Identify which was spent and on what, and treat the constraint as the position it is.

Is the claim about explaining or about describing? At least one developed position holds that the laws that unify best are not the ones that describe best. Whether or not that is accepted, the two virtues are worth distinguishing.

Does the argument need the unobservables to be real? Sometimes it does and sometimes it does not, and an argument that works either way is stronger than one that needs the question settled.

Is a memorable formulation doing load bearing work? If so, check it in the source rather than in the citation, because the most repeated sentences are the least examined.

A model is a deliberate misdescription with a job. Judging it means asking what job, what was left out, what was distorted on purpose, and what was given up to make it usable.

Where That Leaves Us

The article ends in questions to carry to a model: what it targets, which distortions are deliberate, what was given up, whether the claim concerns explaining or describing, whether the argument needs its unobservables to be real, and whether a memorable formulation is bearing weight. A deliberate misdescription with a job to do. What is established, and how, follows.

What We Know, and How

The reference formulation states that many scientific models are representational and stand in for a selected part or aspect of the world, and that idealised models involve a deliberate simplification or distortion aimed at tractability. Cartwright is reported, from the publisher's description of the book rather than from its argument, as holding that fundamental explanatory laws do not describe regularities in nature while localised laws and theoretical entities may still be read realistically. Levins wrote in 1966 that generality, realism and precision cannot be maximised together in a manageable model, with realism there meaning retained detail. Orzack and Sober reject the necessity of that trade-off. The 1976 paper argues against elaboration and a direct check confirms the famous phrase is absent from it; the literal wording appears in a 1979 technical report. The reference entry gives scientific realism as belief in observable and unobservable content, and van Fraassen's empirical adequacy, quoted within that same entry, as truth about the observable.

What We Still Don't Know

Whether the unobservable parts of a working model correspond to anything real, which this article states both standards for and settles neither. Whether Cartwright's position survives a reading of the argument itself, since what is reported here comes from a publisher's description. How often the famous sentence is misattributed, which no bibliometric study was consulted to establish. And whether, in any particular case, generality, retained detail and precision do trade against one another, which the objection leaves open by denying only that they must.

What Could Change Our Minds

A reading of Cartwright's argument rather than its publisher's description would settle whether the position is as stated. A bibliometric study would turn the misattribution from an observation into a measured frequency. On the constraint, a case in which all three properties improve together would not refute Levins as he framed it, but a demonstration that they must trade in some

specified class of models would narrow the objection. On realism, an argument that success licenses belief without assuming what it sets out to show would move a question this article leaves open.

If You Remember Only One Thing

A model is a deliberate misdescription with a job, so ask what job, what was left out, what was distorted on purpose, and what that cost.

Related Articles

- KNO-006 · *Maps and Spatial Representation*

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The bracketed numbers in the body are claim numbers, not reference numbers. Each resolves to a row of the claims ledger, which names the source below that supports it.

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