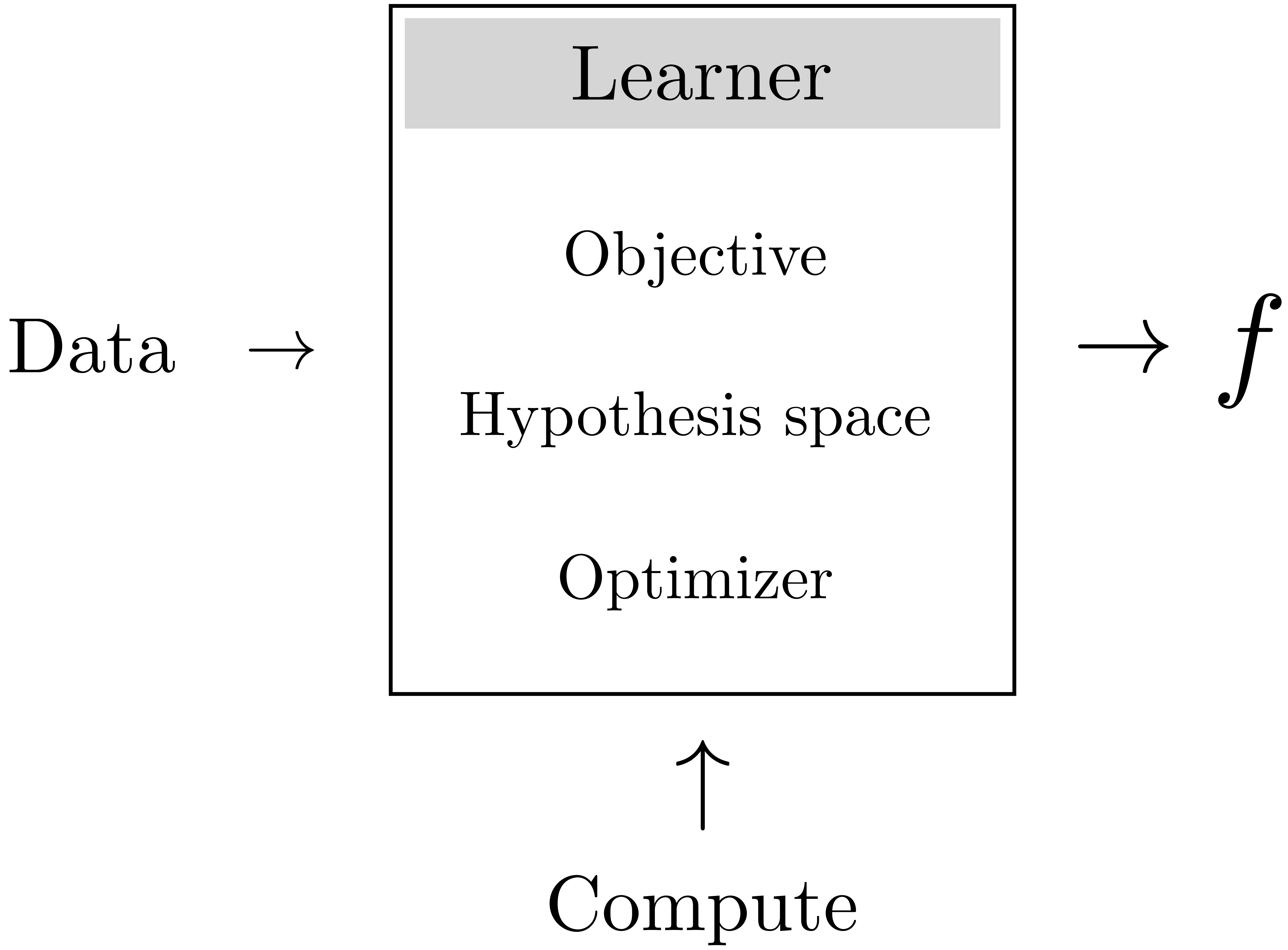
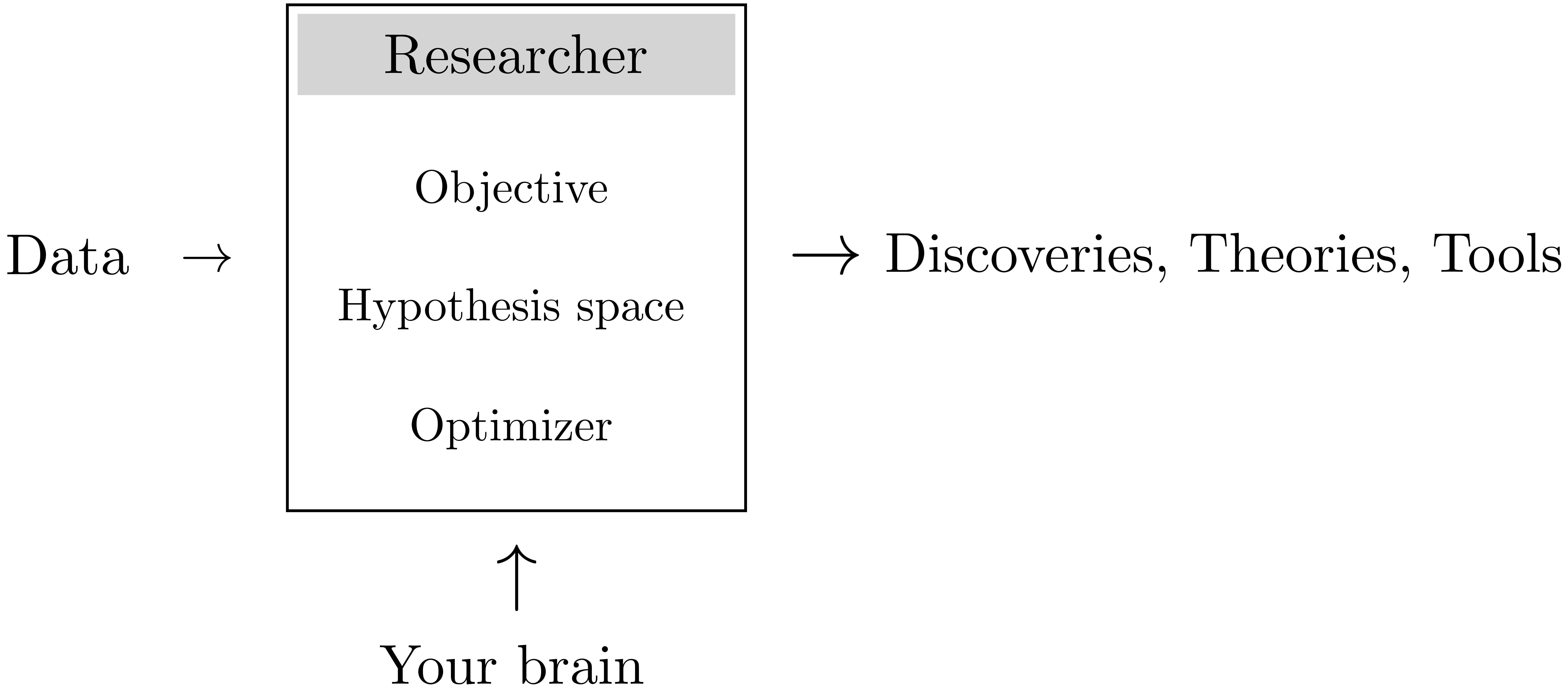
How to do research

Phillip Isola, 2022


Data
Learner
Objective
Hypothesis space
Optimizer
f
Compute

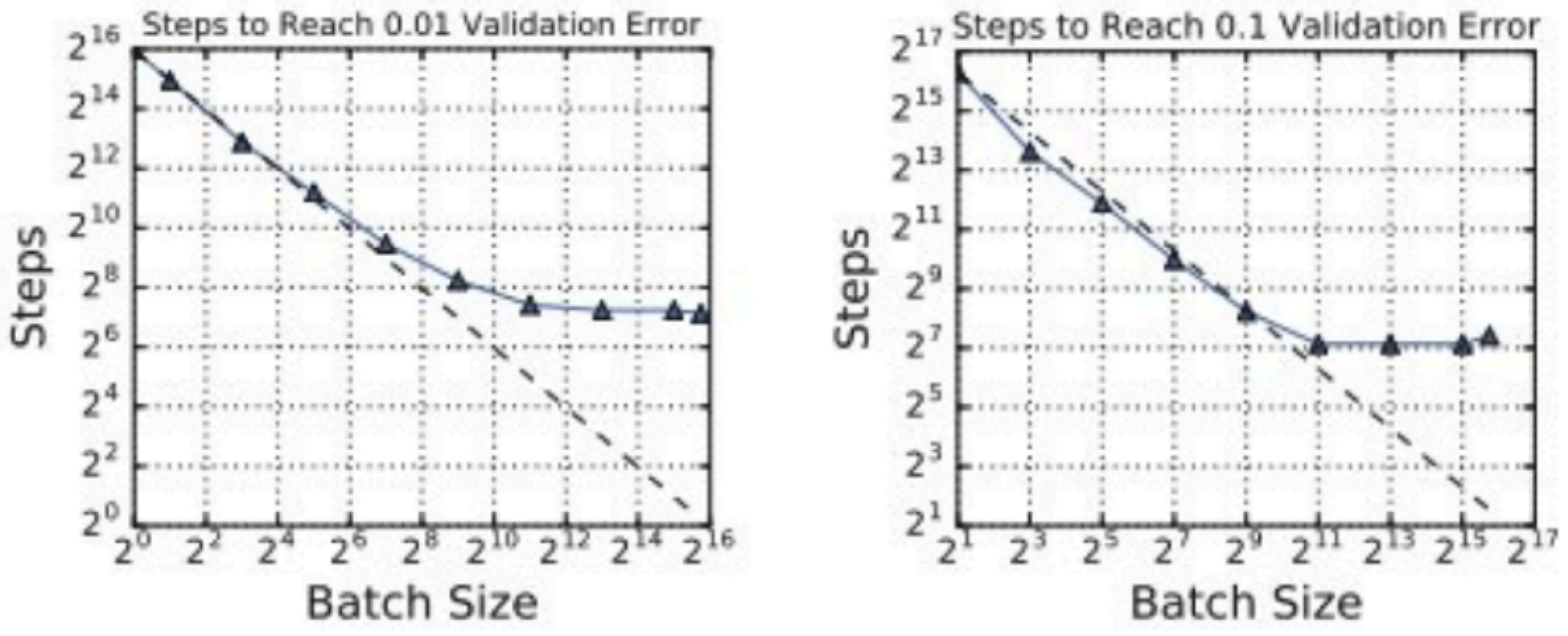

Data →
Researcher
Objective
Hypothesis space
Optimizer
→ Discoveries, Theories, Tools
↑
Your brain

There are many ways to contribute

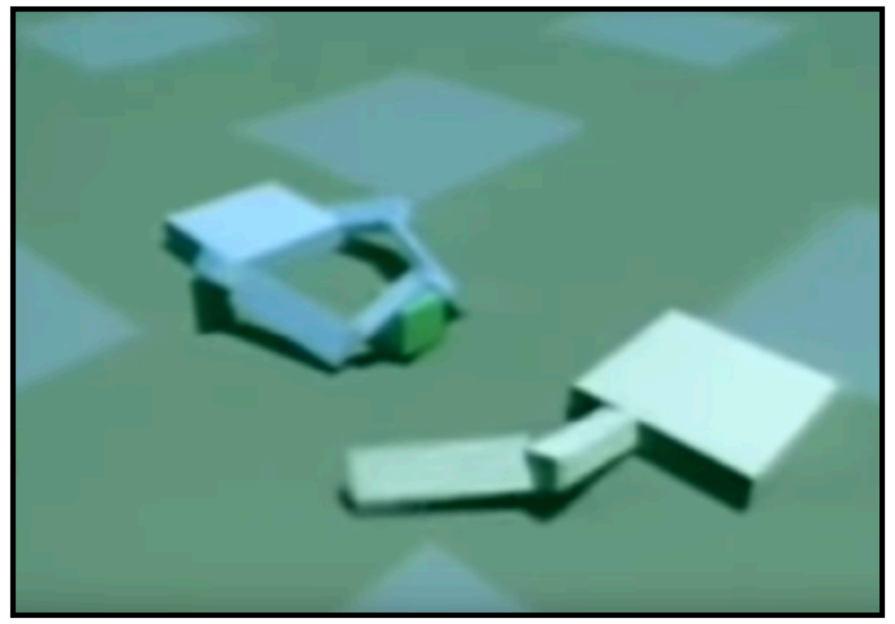
Mathematical theory

Theorem 4. *(weak* topology) Let* $\{\mathbb{P}_n\}$ *be a sequence of distributions. Considering* $n \to \infty$, *under mild Assumption,* $\max_\phi M_{f_\phi}(\mathbb{P}_\mathcal{X}, \mathbb{P}_n) \to 0 \iff \mathbb{P}_n \xrightarrow{D} \mathbb{P}_\mathcal{X}$, *where* $\xrightarrow{D}$ *means converging in* distribution *[3]*.

Empirical theory (aka science)



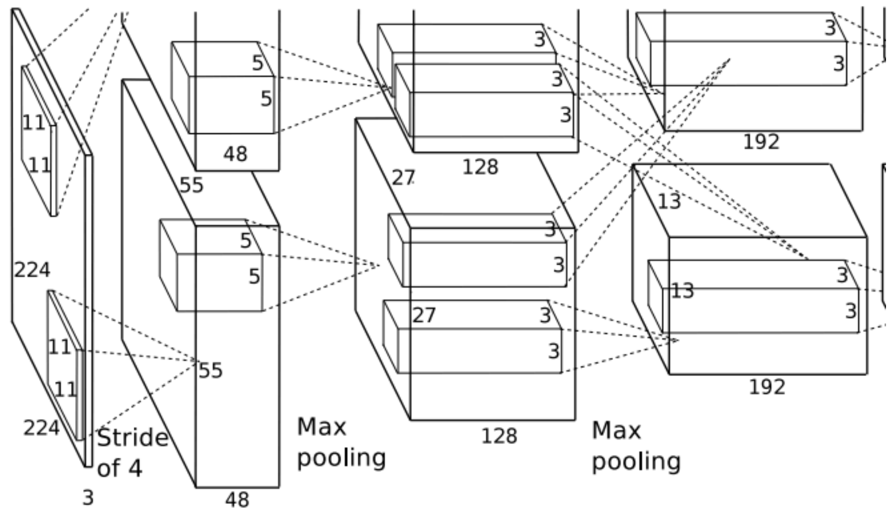
Proposing new problems



Fairness Through Awareness

Cynthia Dwork* Moritz Hardt† Toniann Pitassi‡ Omer Reingold§ Richard Zemel¶

Solving old problems



Tools

BVLC / caffe

Watch 2,236 | Unstar 26,339

<> Code | Issues 629 | Pull requests 263 | Projects 0 | Wiki | Insights

Caffe: a fast open framework for deep learning. http://caffe.berkeleyvision.org/

deep-learning machine-learning vision

Communication

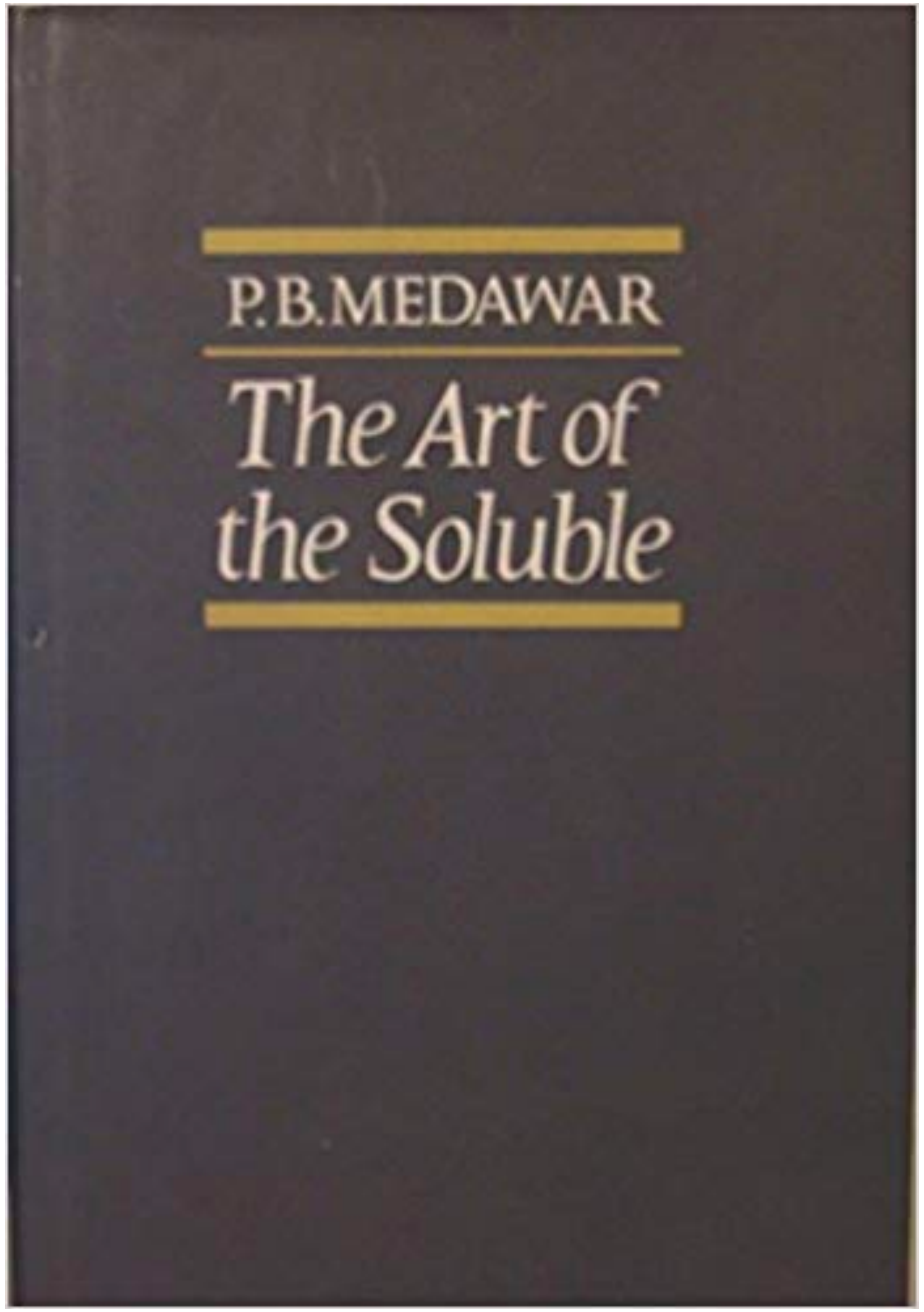
Andrej Karpathy blog — About — Hacker's guide to Neural Networks

The Unreasonable Effectiveness of Recurrent Neural Networks

May 21, 2015

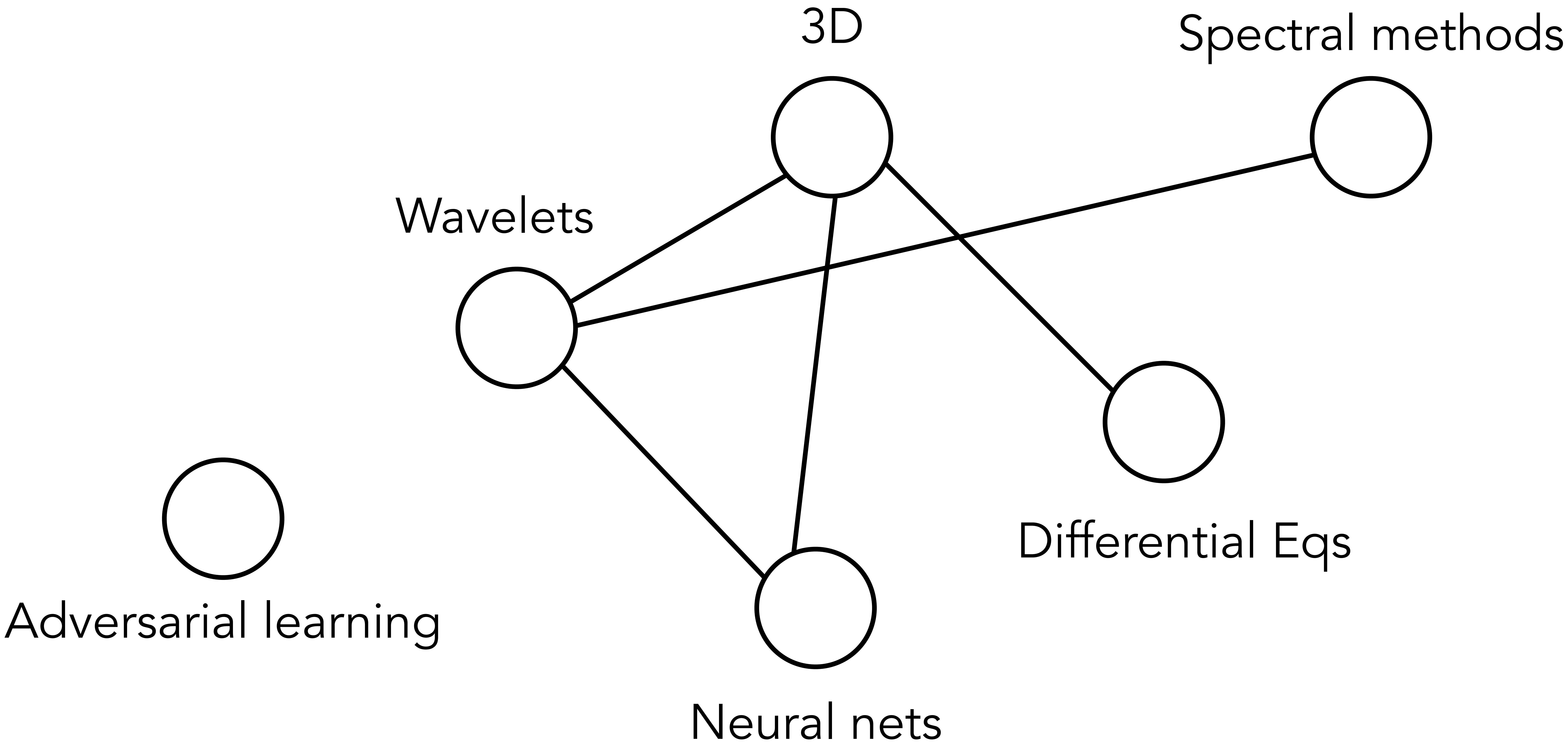
Picking a topic

Science is the "Art of the Soluble"



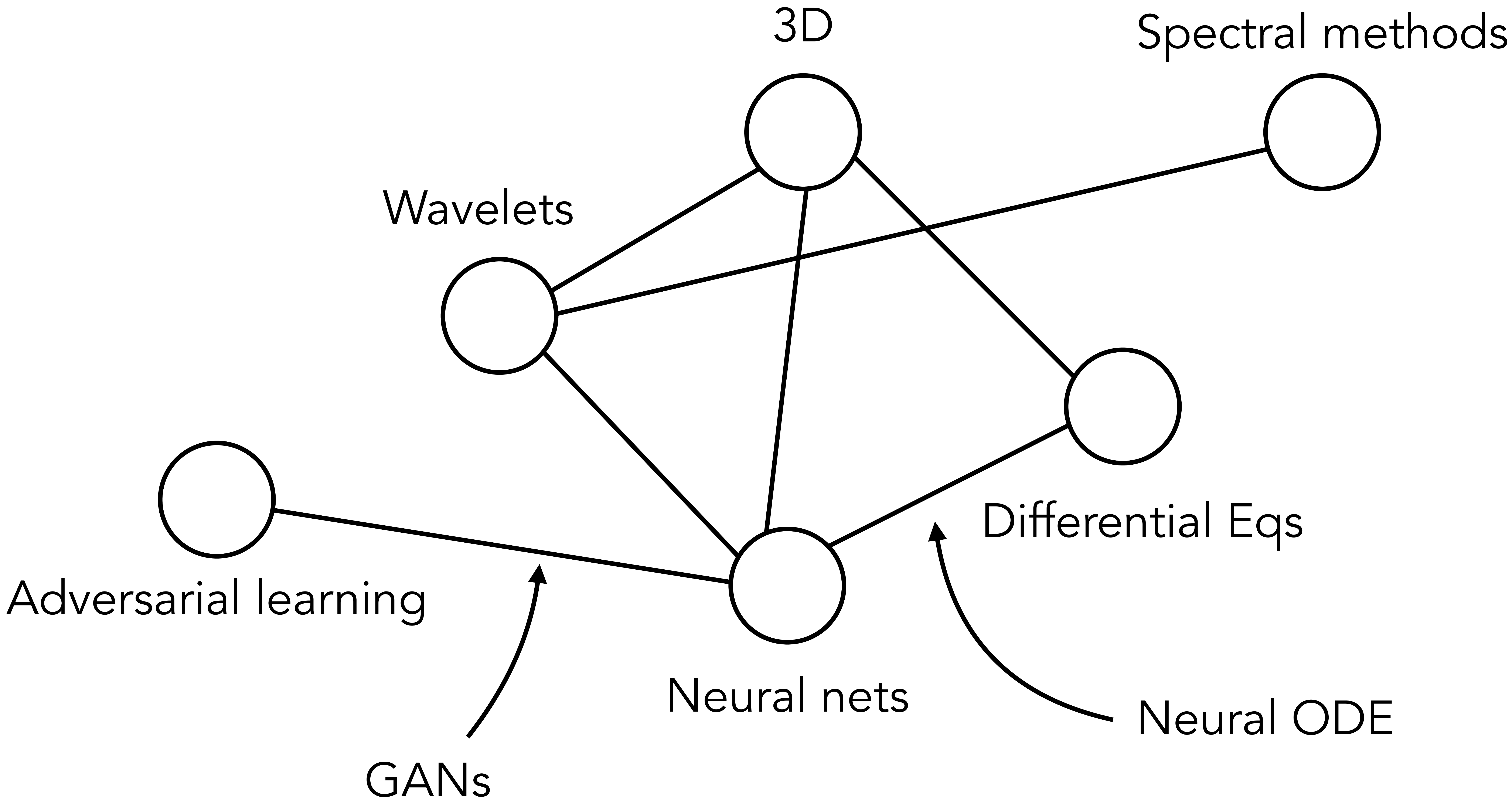
"'Good scientists study the most important problems they think they can solve. It is, after all, their professional business to solve problems not to grapple with them.' —Peter Medawar"
— Jitendra Malik

Add a node



3D
Spectral methods
Wavelets
Differential Eqs
Adversarial learning
Neural nets

Add an edge

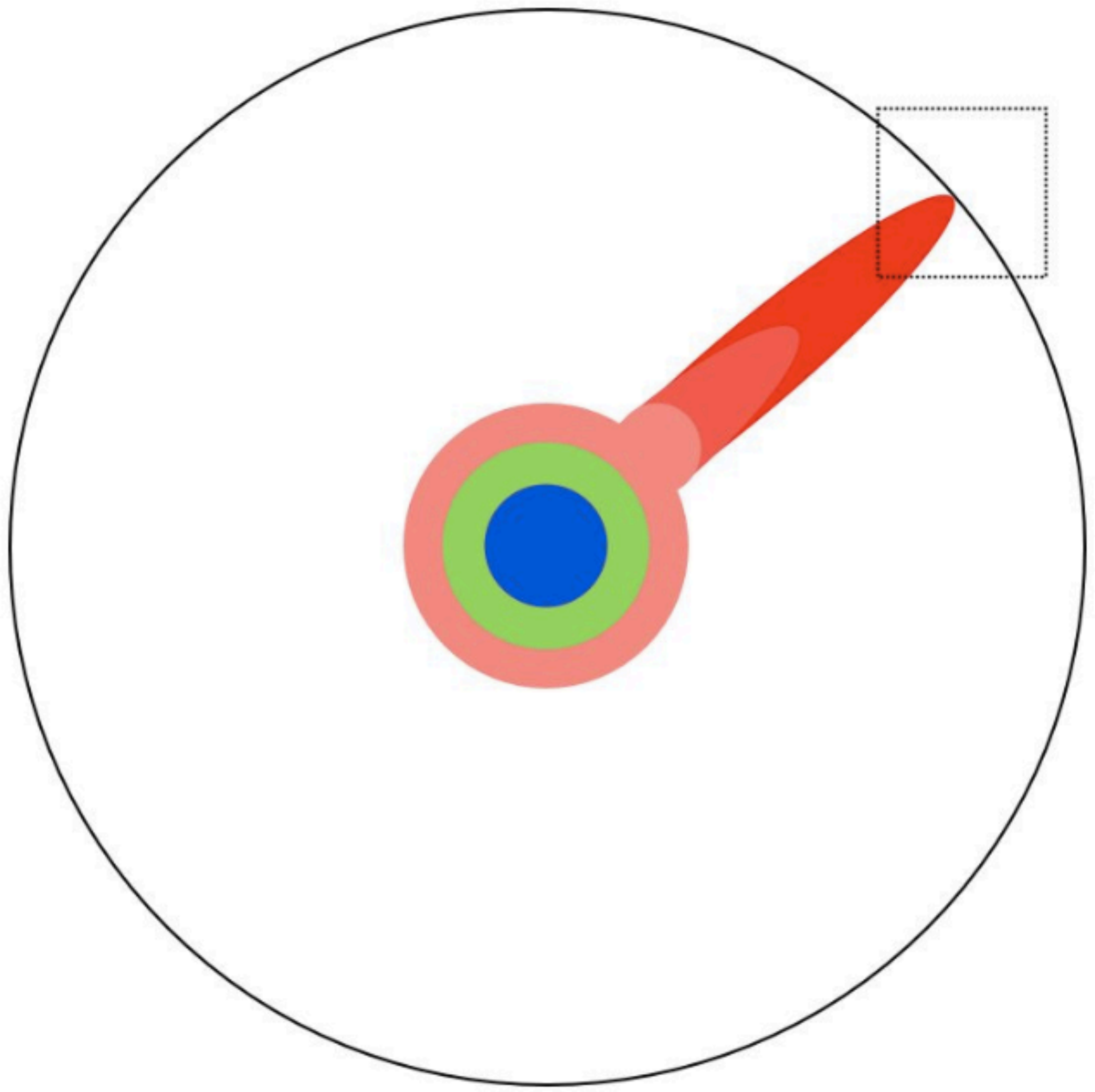


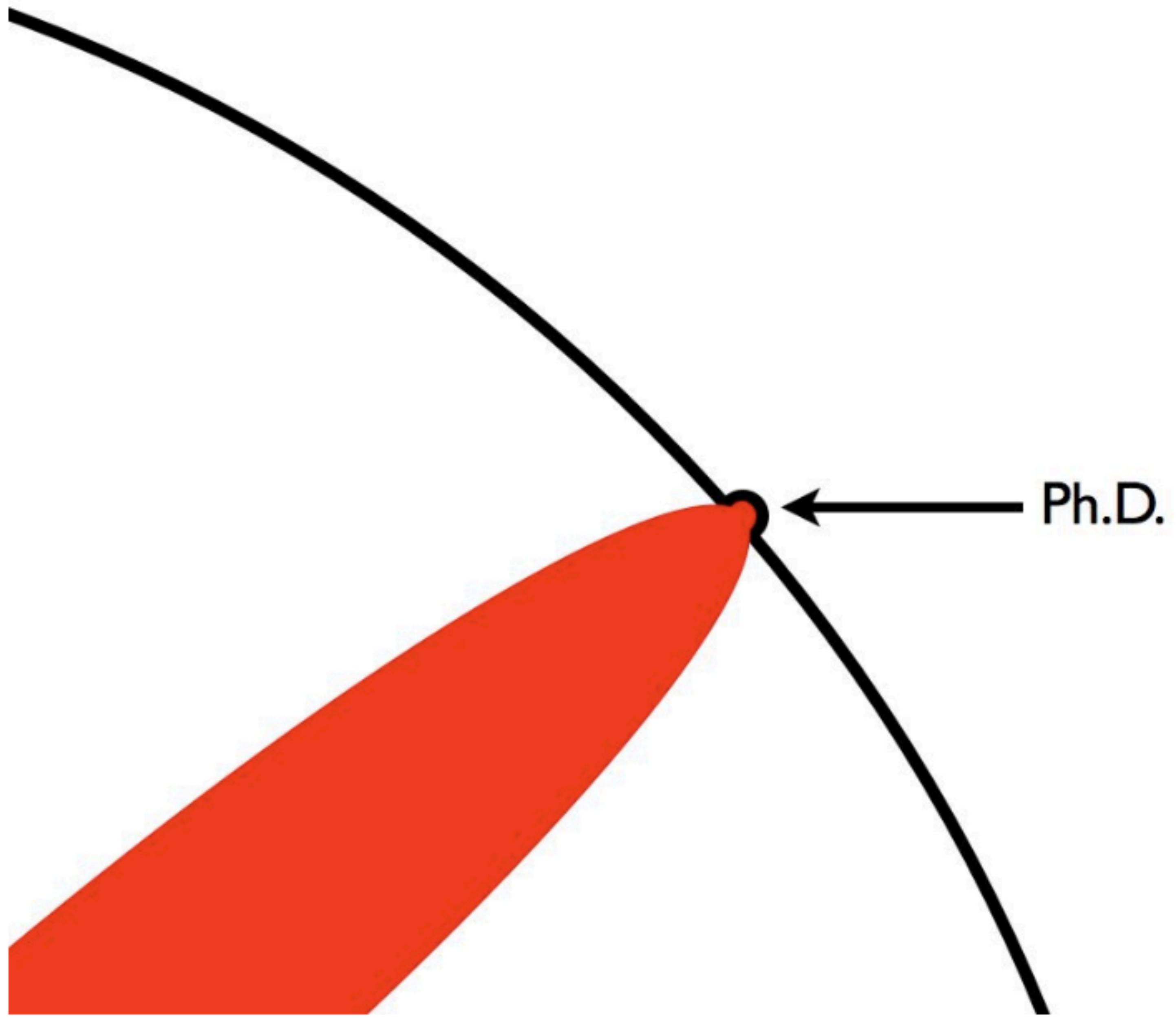
3D
Spectral methods
Wavelets
Differential Eqs
Adversarial learning
Neural nets
GANs
Neural ODE

Novelty

What matters is novelty w.r.t. humanity.

Very hard to achieve without knowing what has already been done.





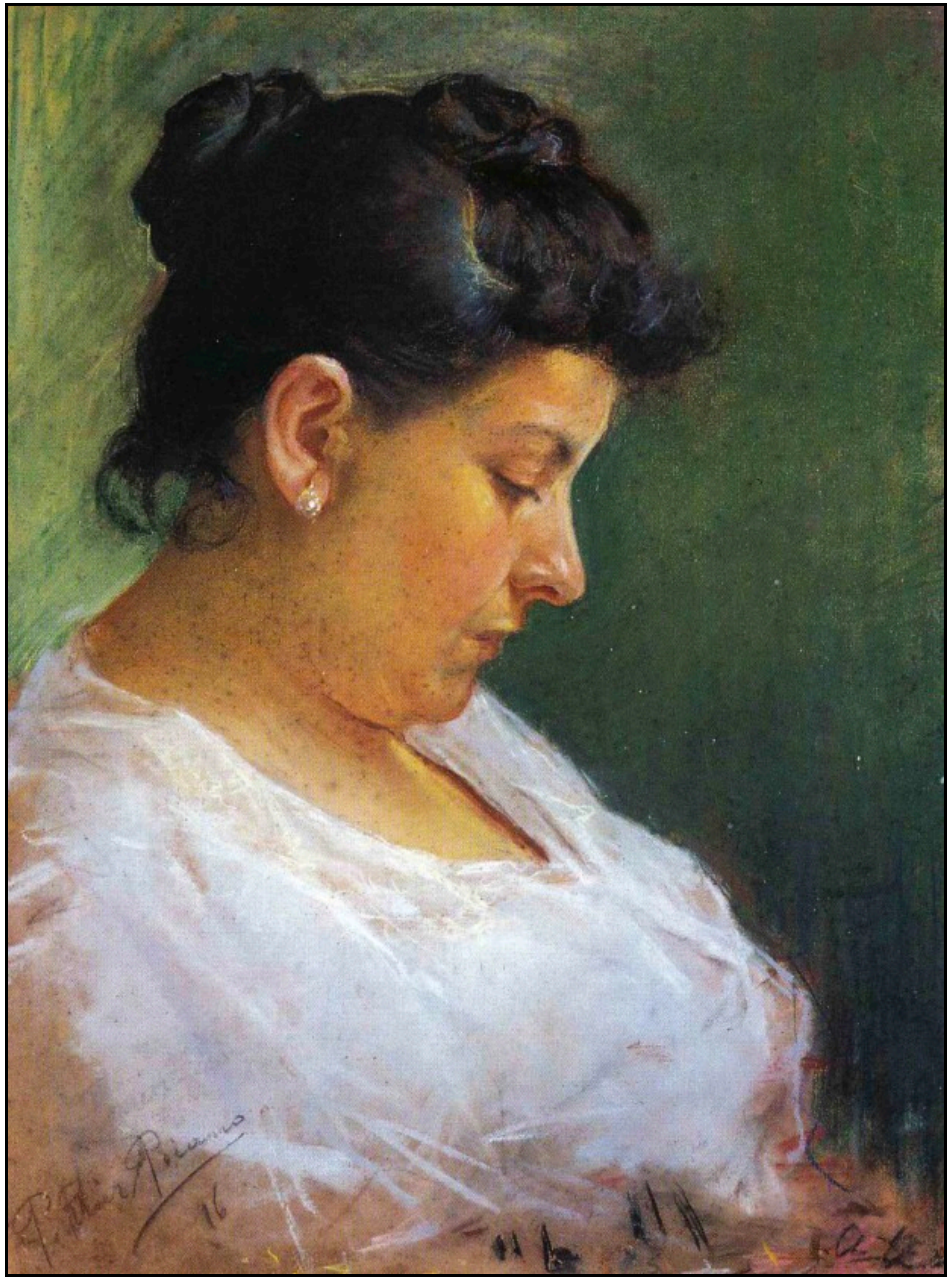
http://matt.might.net/articles/phd-school-in-pictures/

Novelty

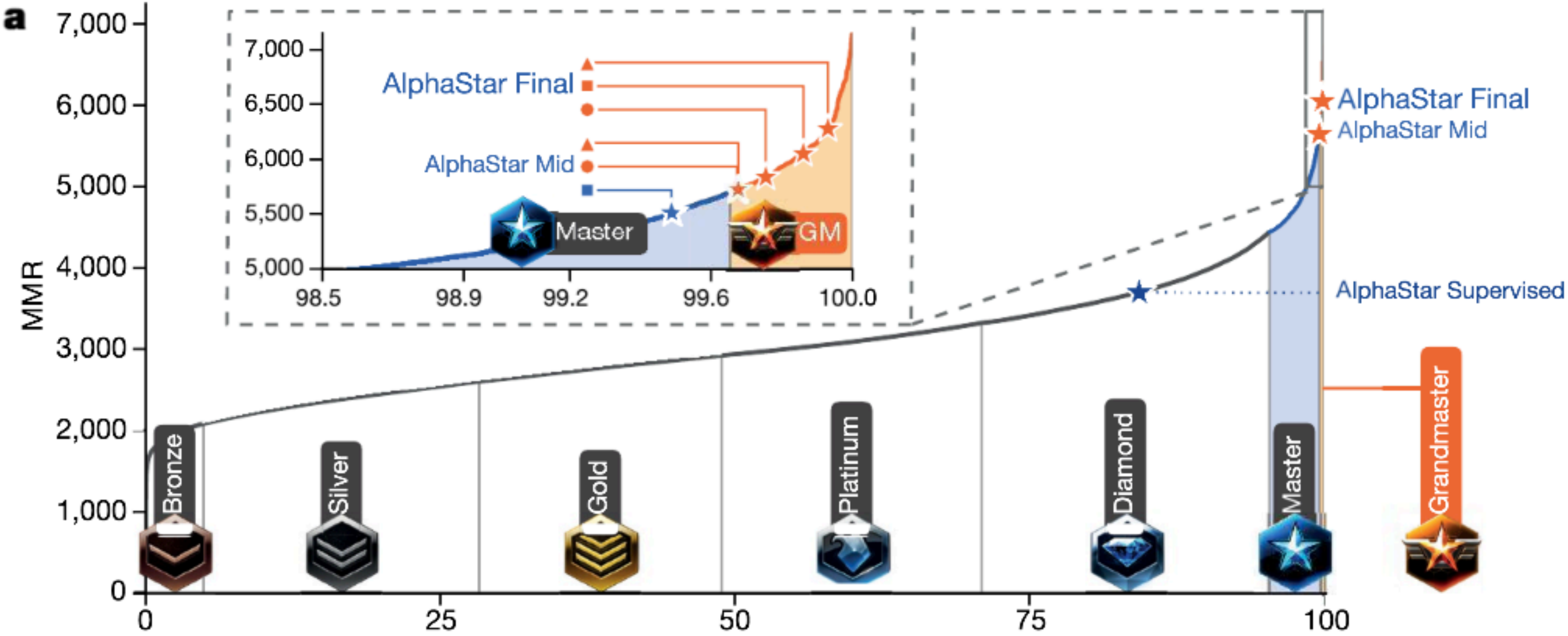
My personal favorite heuristic of research quality is **surprisal**.
Aim to do work that is surprising.

What makes something surprising?

* People should *gain a lot of information from it*
  * It should be new
  * It should be understandable
  * It should be just a bit outside of current paradigms



[Picasso]



First imitates humans, then innovates

["AlphaStar", Vinyals et al., Nature 2019]

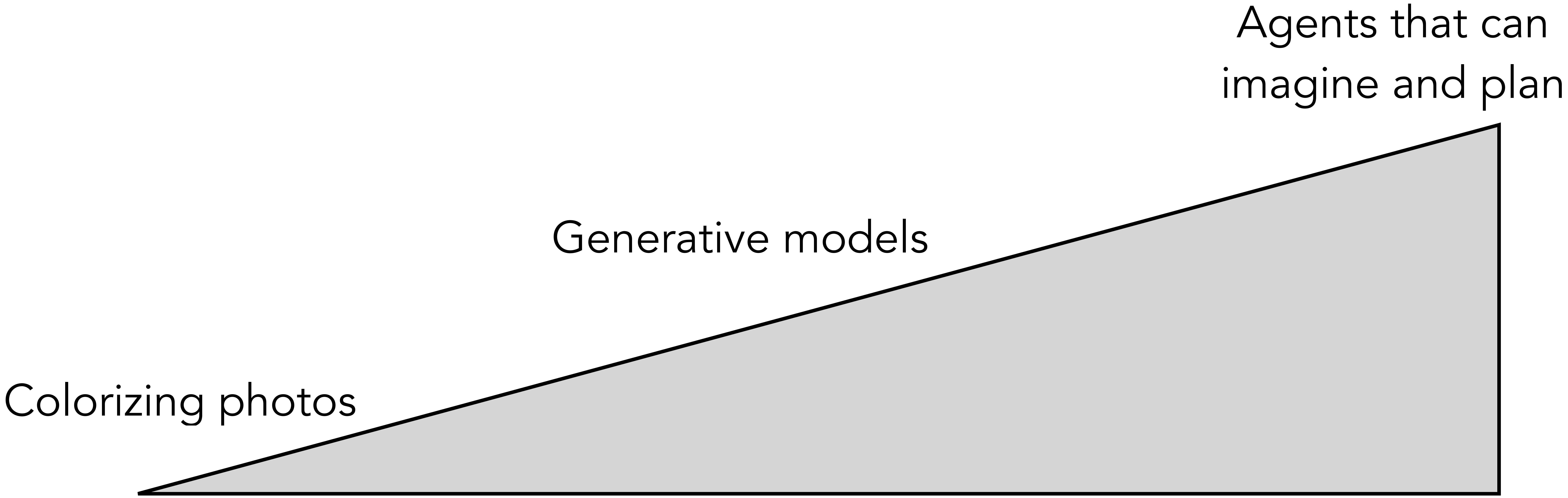
Know something no one else knows

- It’s necessary but not sufficient to master the core knowledge of your field

- Acquire a unique skillset
  - Take difficult or unusual classes
  - Read old papers
  - Take on a complementary hobby
  - Talk to people in other fields

“My answer to "Now What" is "here is a research problem which is unusual, perhaps significant, novel, that I can pose and probably solve because of my background in physics". The situation would not be readily identified as a problem at all by those whose background seems much more relevant than my own.”
— “Now What”, John Hopfield

Build a ramp



"When you are famous it is hard to work on small problems. This is what did Shannon in. After information theory, what do you do for an encore? The great scientists often make this error. They fail to continue to plant the little acorns from which the mighty oak trees grow."
— Richard Hamming, "You and Your Research"

Think about the consequences

Why are you working on the problem you are working on?

What would happen if you were successful? Is that what you want to happen? Consider impact on science. Consider impact on society.

Get comfortable being confused

Michael Black @Michael_J_Black · 21h

Replying to @Michael_J_Black

There is an essential stage of confusion necessary for the formation of really new ideas. I call this the "high-temperature" state, where exploration happens. It can be uncomfortable for students and advisors but I encourage it.

1 2 24

Michael Black @Michael_J_Black · 21h

My role as an advisor is sometimes to raise the temperature, apparently increasing confusion by suggesting new directions. Then, at a critical point, I quench the system, dropping the temperature, and helping the student "close the deal."

1 1 25

Picking a topic

Don't worry too much if a topic is unpopular

If it is important, and you do good work on it, you can *make it popular*

Doing good work on that topic

Data $\rightarrow$

Researcher

Objective

Hypothesis space

Optimizer

$\longrightarrow$ Discoveries, Theories, Tools

$\uparrow$

Your brain

Our goal is to produce *knowledge that generalizes*

"One kind of result which probably won't generalize is: "algorithm A works better than algorithm B." Different application areas have their own requirements… The kind of result I believe generalizes to new situations is the nature of the tradeoffs between different approaches."
— "Which Research Results Will Generalize?" Roger Grosse [https://lips.cs.princeton.edu/which-research-results-will-generalize/]

Fit the data

"Now I'm going to discuss how we would look for a new law. In general, we look for a new law by the following process. First, we guess it (audience laughter), no, don't laugh, that's the truth. Then we compute the consequences of the guess, to see what, if this is right, if this law we guess is right, to see what it would imply and then we compare the computation results to nature or we say compare to experiment or experience, compare it directly with observations to see if it works.

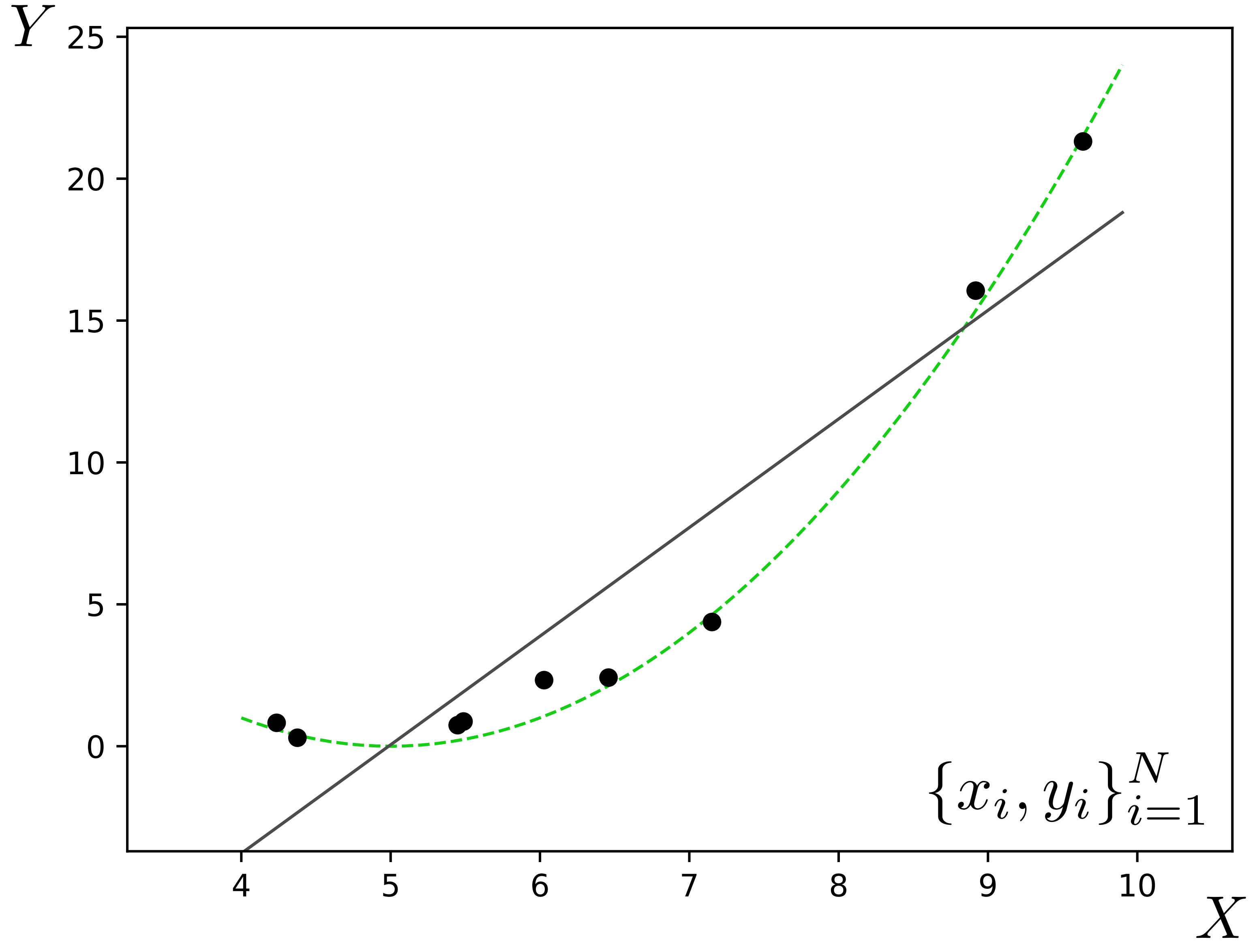
If it disagrees with experiment, it's wrong. In that simple statement is the key to science. It doesn't make any difference how beautiful your guess is, it doesn't matter how smart you are who made the guess, or what his name is … If it disagrees with experiment, it's wrong. That's all there is to it."
— Richard Feynman

Omit needless bits

“Perfection is finally attained not when there is no longer anything to add, but when there is no longer anything to take away”
— Antoine de Saint Exupéry

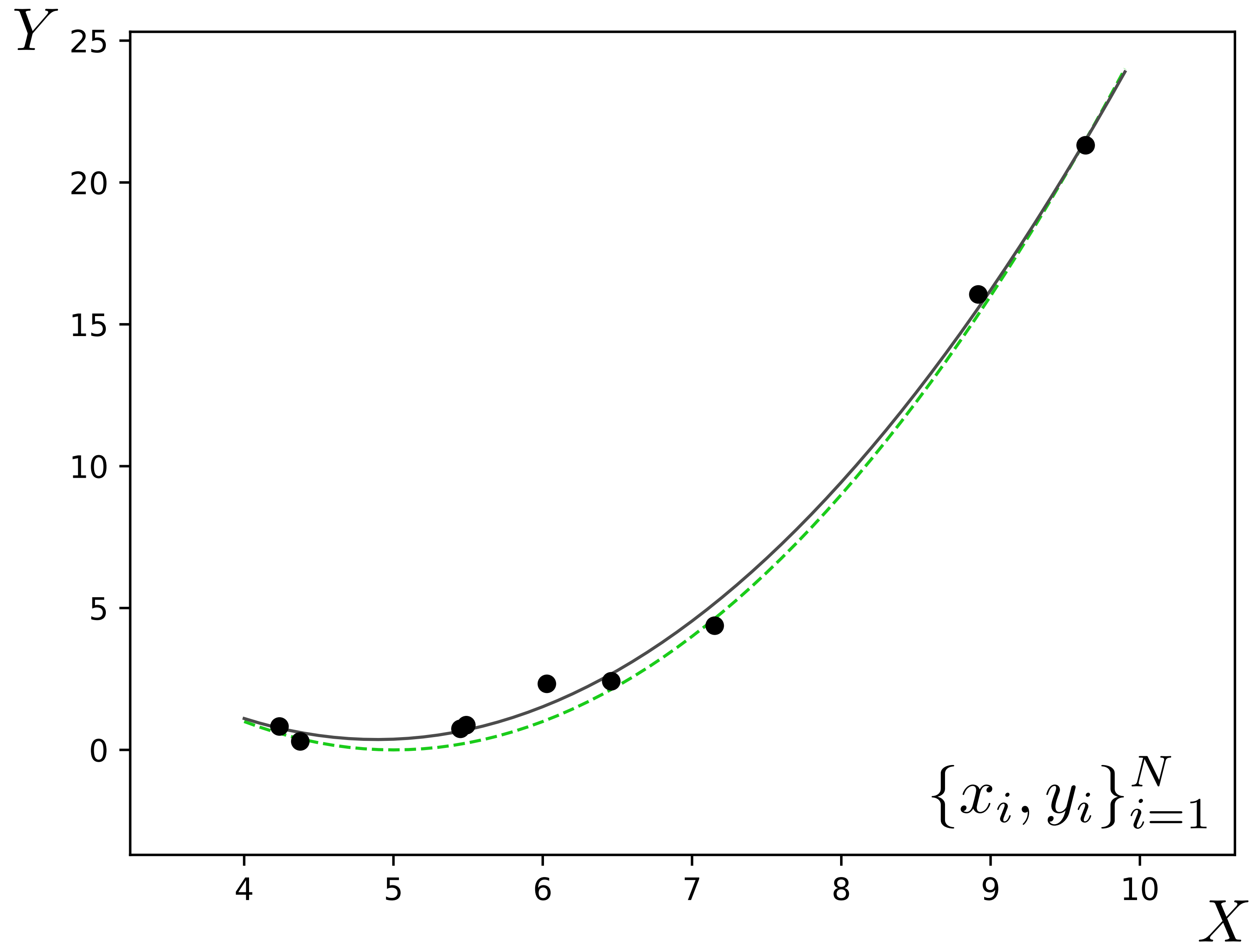
Underfitting

$\boxed{K = 1}$



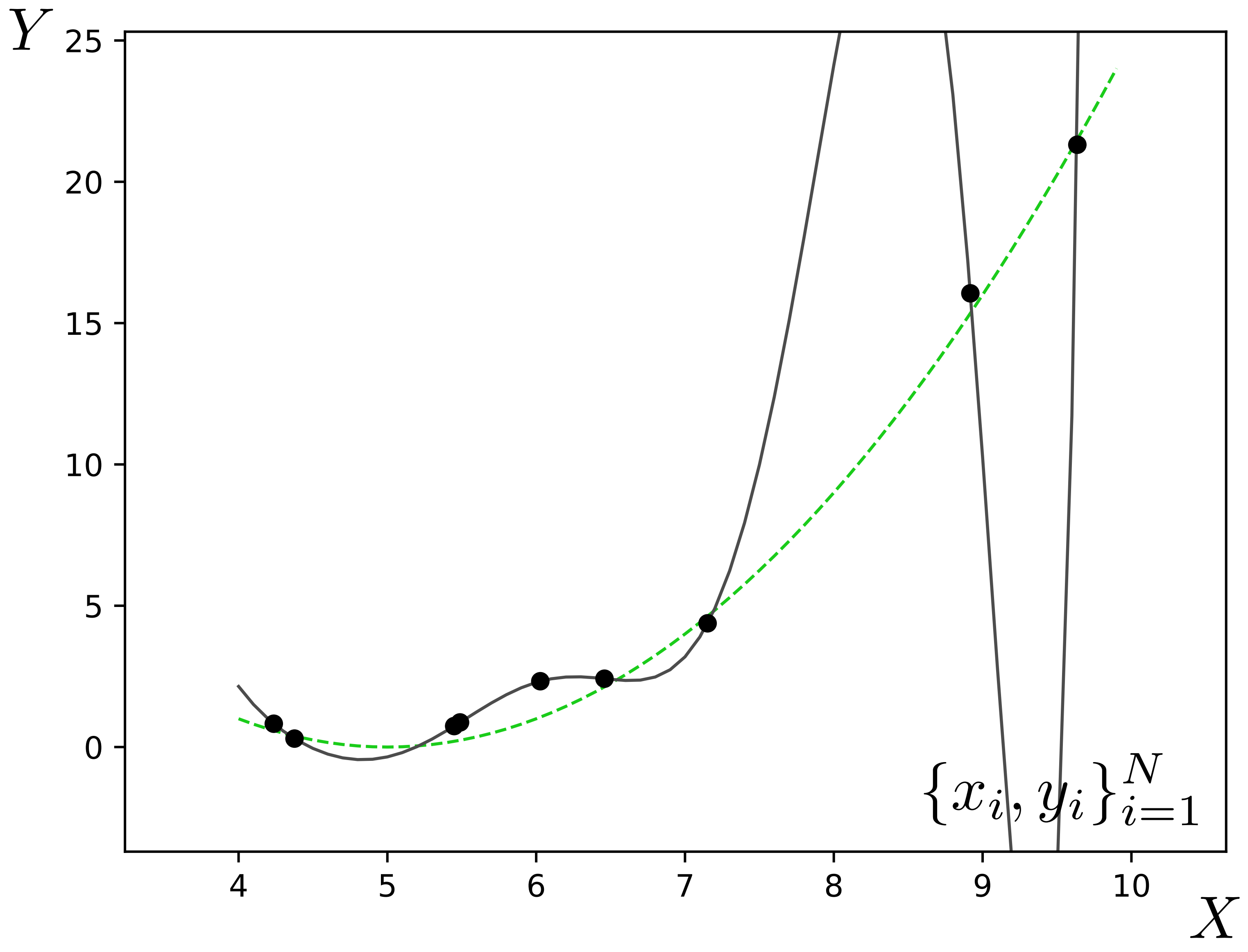
Appropriate model

$\boxed{K = 2}$

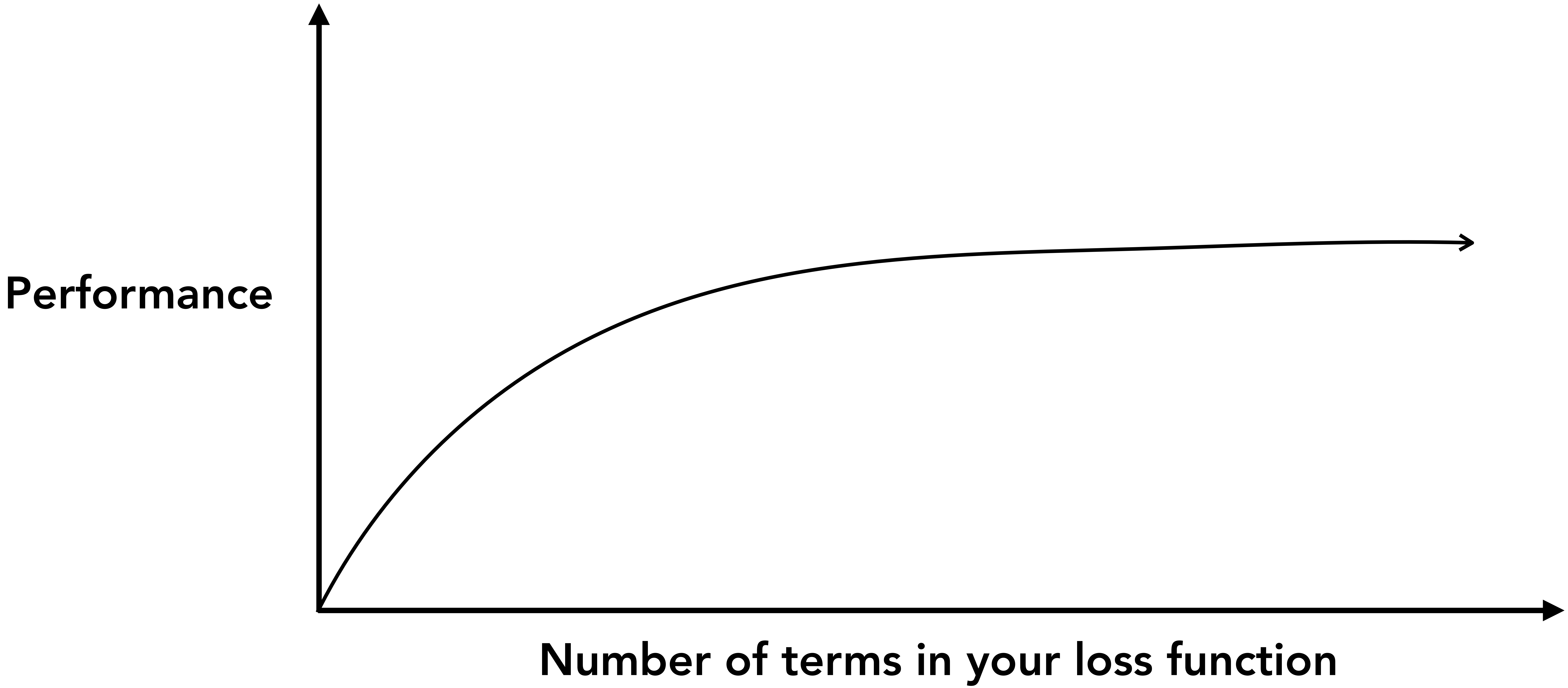


Overfitting

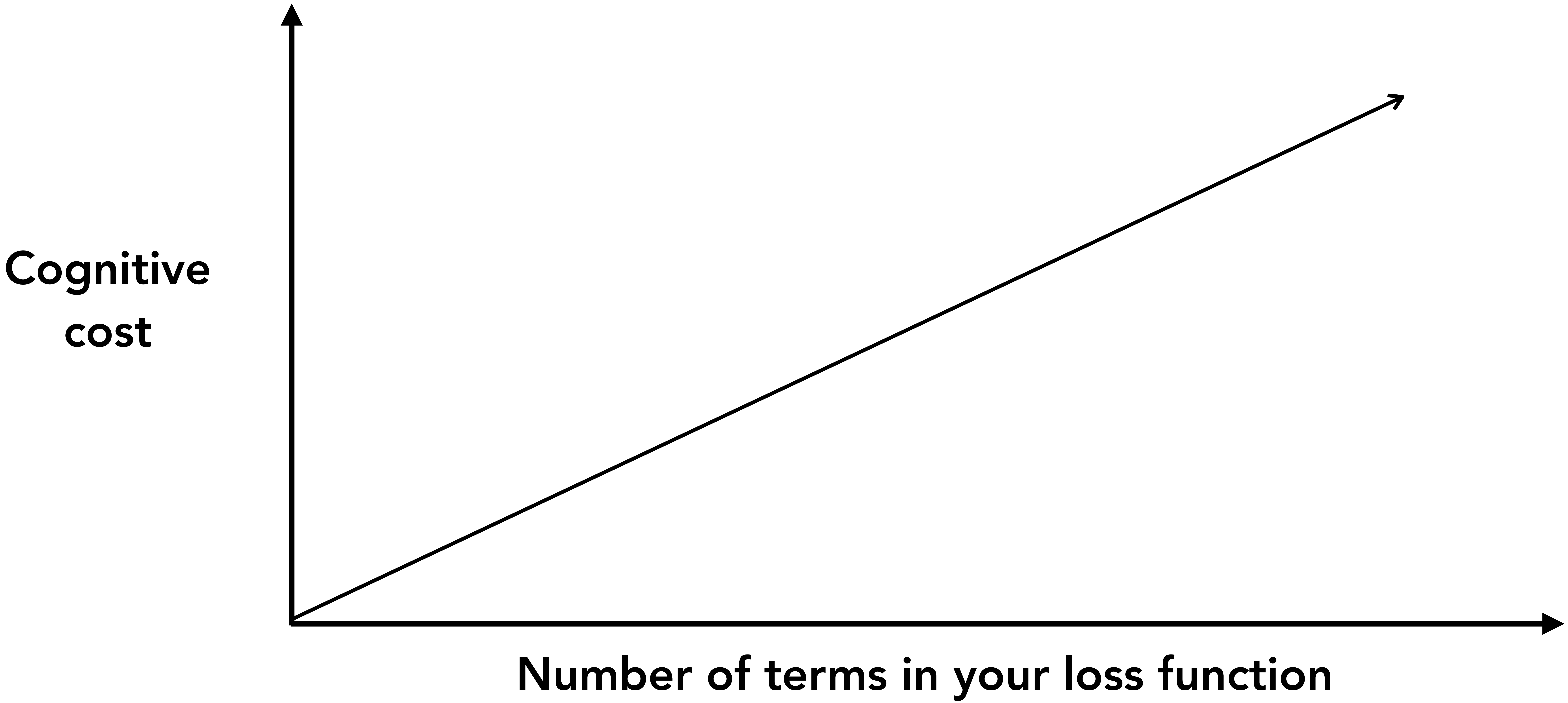
$\boxed{K = 10}$



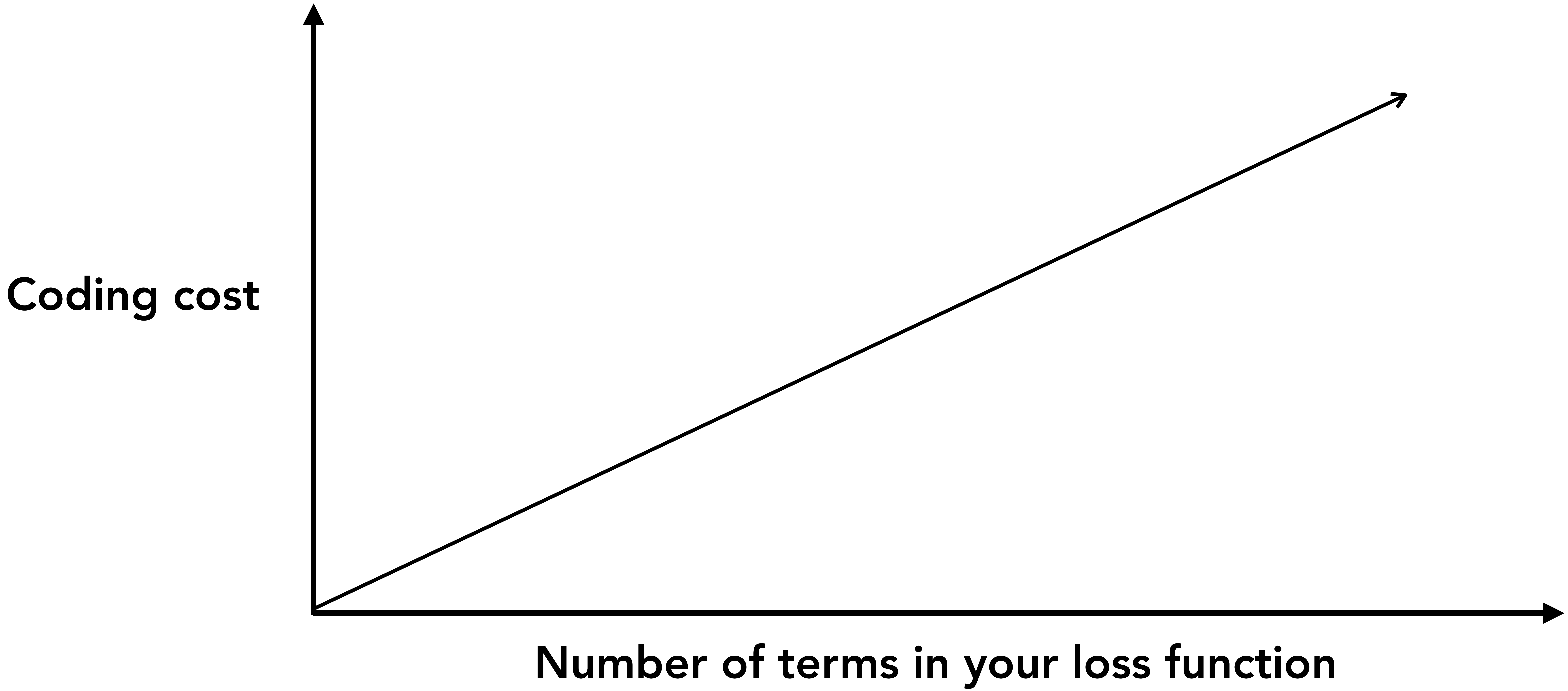
Information creep



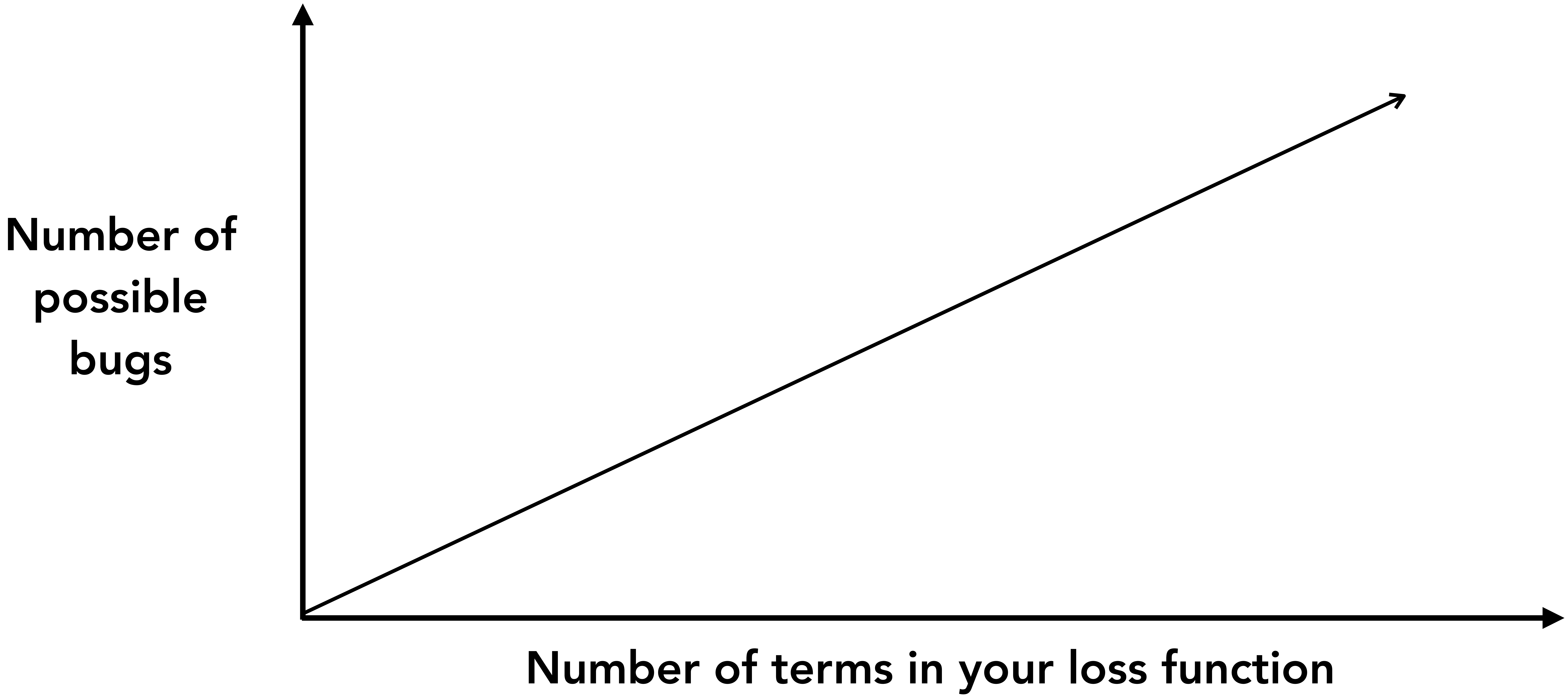
Information creep



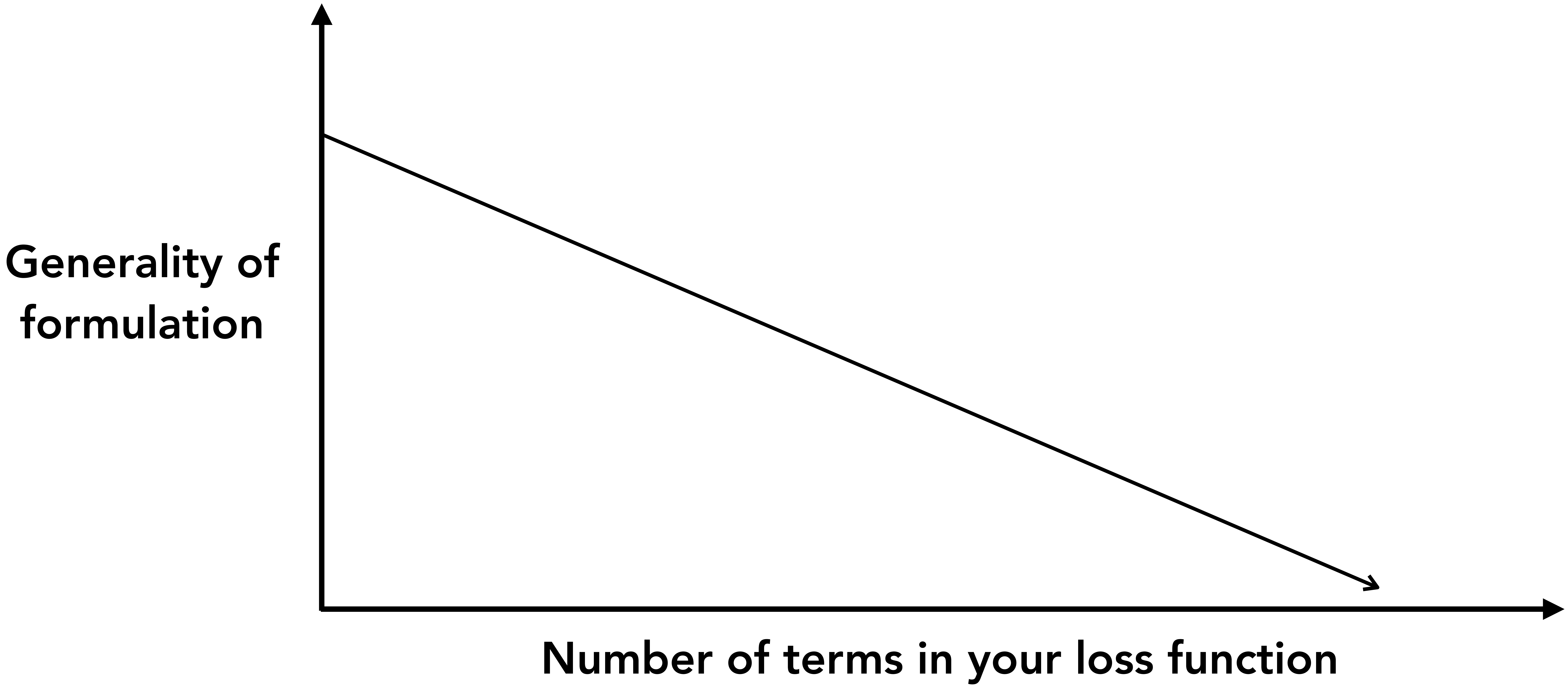
Information creep



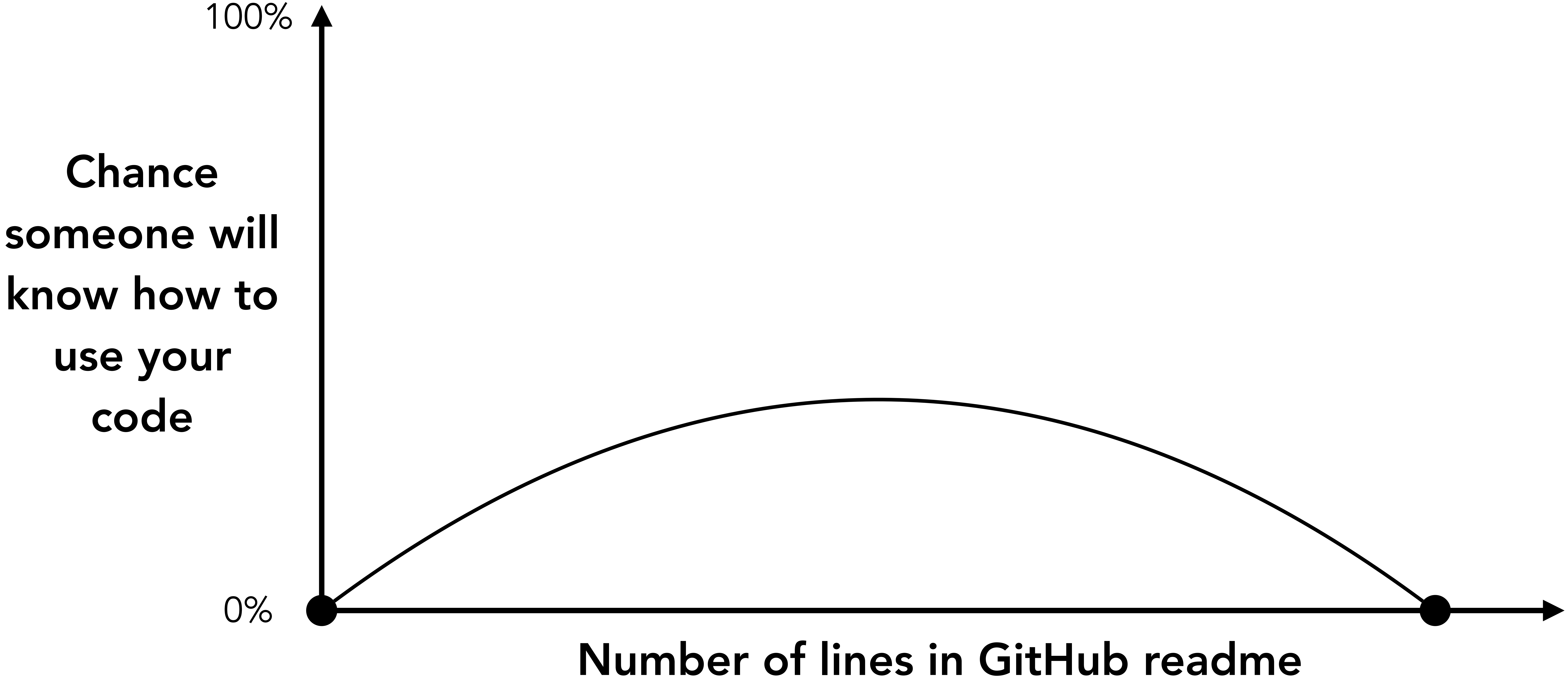
Information creep



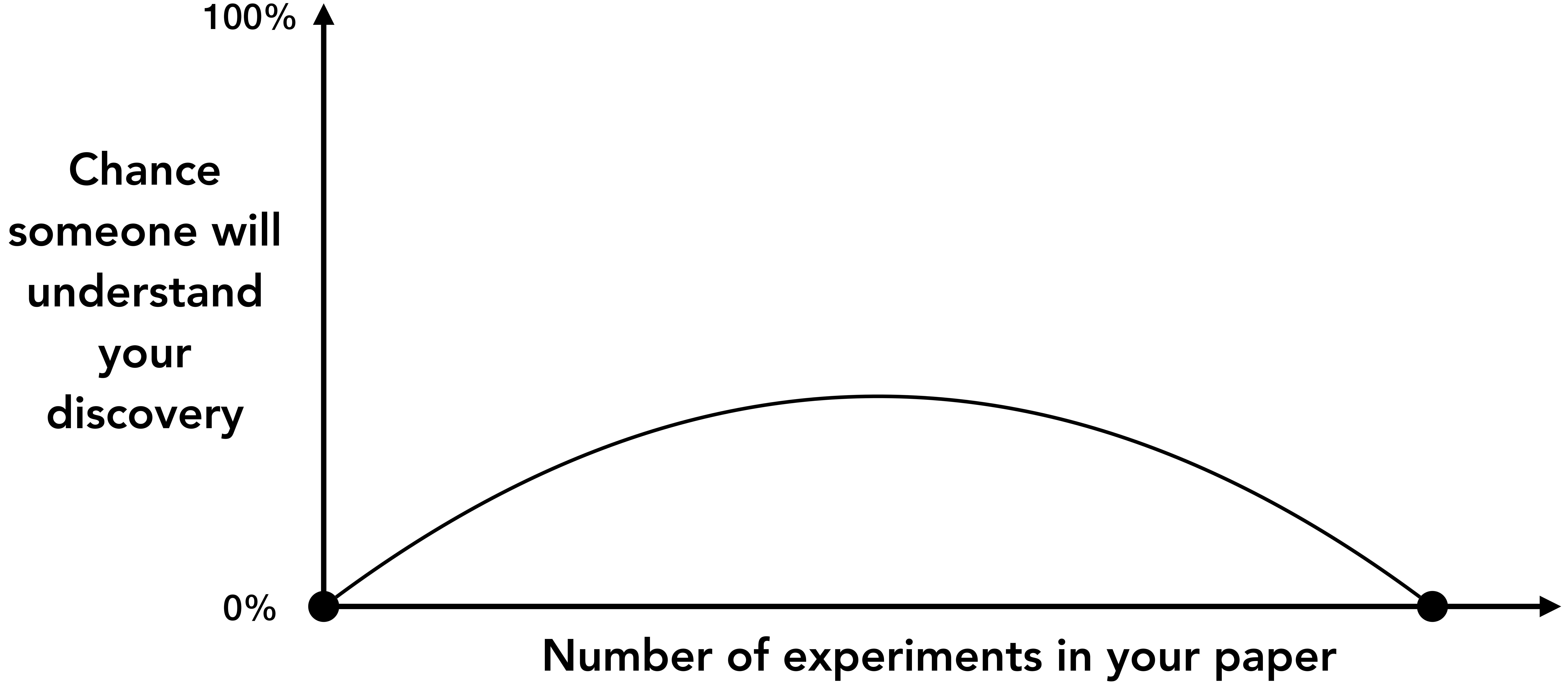
Information creep



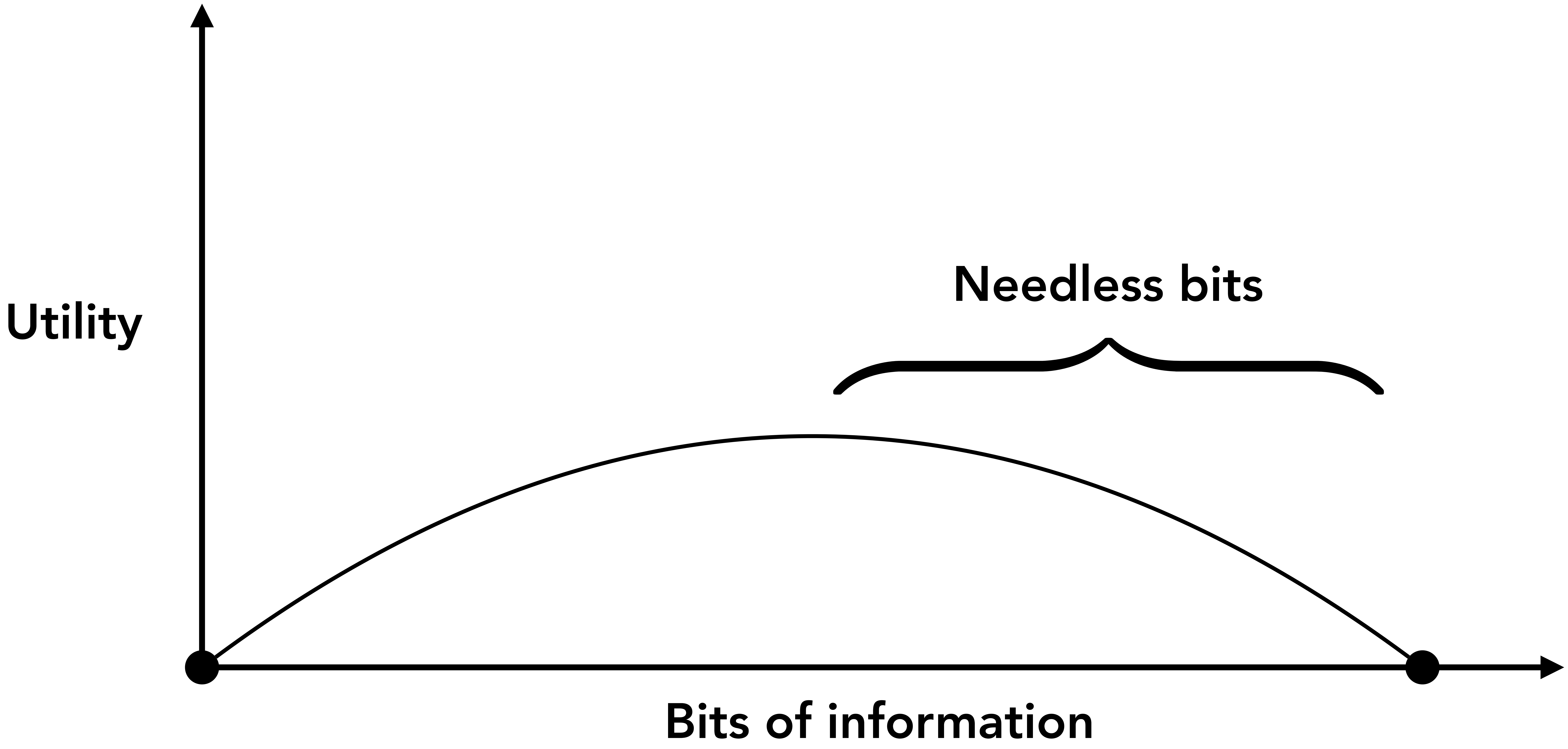
Information creep



Information creep



Information creep



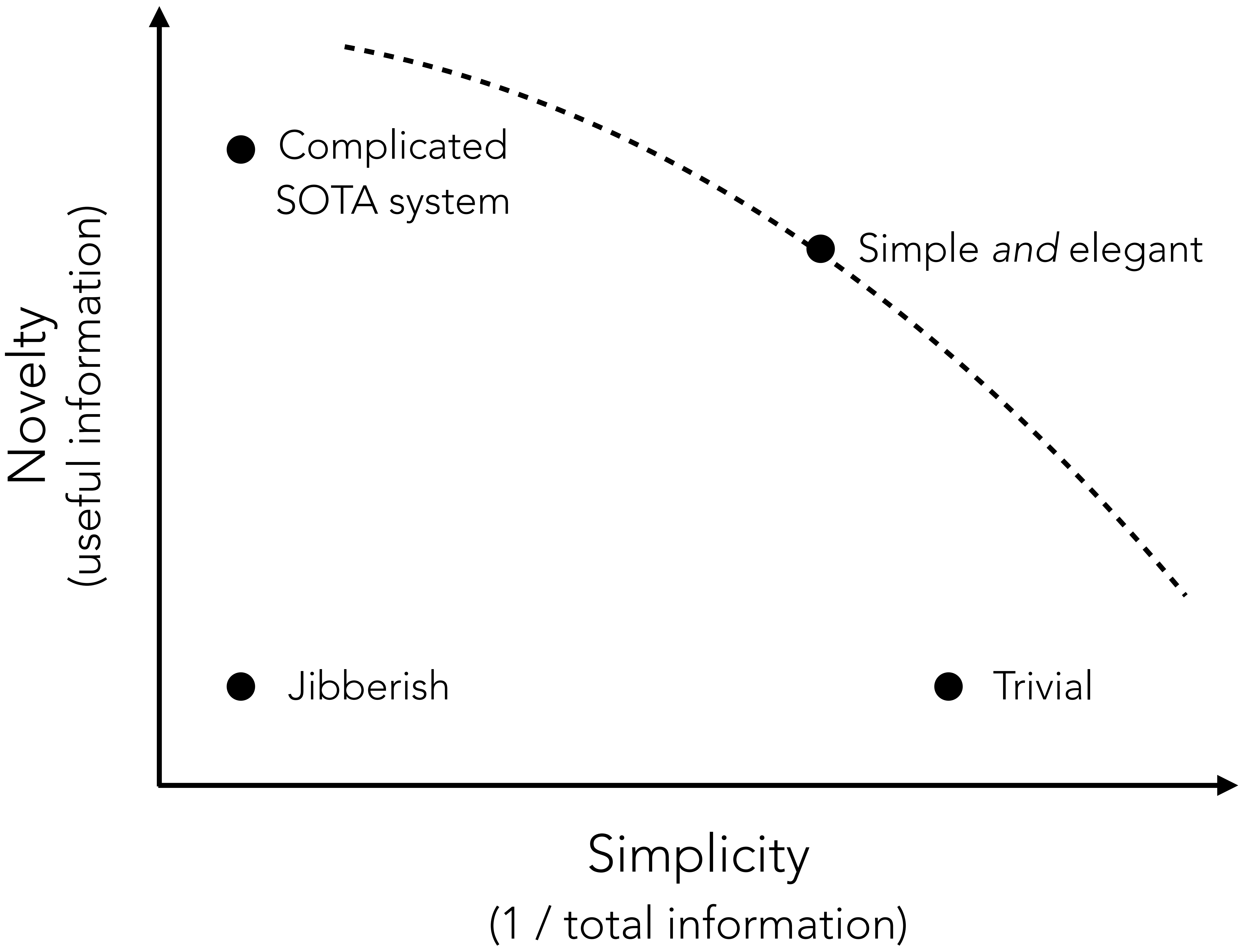
Do the most, with the least

$$\frac{\begin{array}{l}\text{Discoveries}\\ \text{Explanations}\\ \text{Results}\\ \text{Tools}\\ \ldots\end{array}}{\begin{array}{l}\text{Words}\\ \text{Equations}\\ \text{Concepts}\\ \text{Lines of code}\\ \text{GPUs}\\ \text{People, Time, Money}\\ \ldots\end{array}} = \text{Metric for research}$$

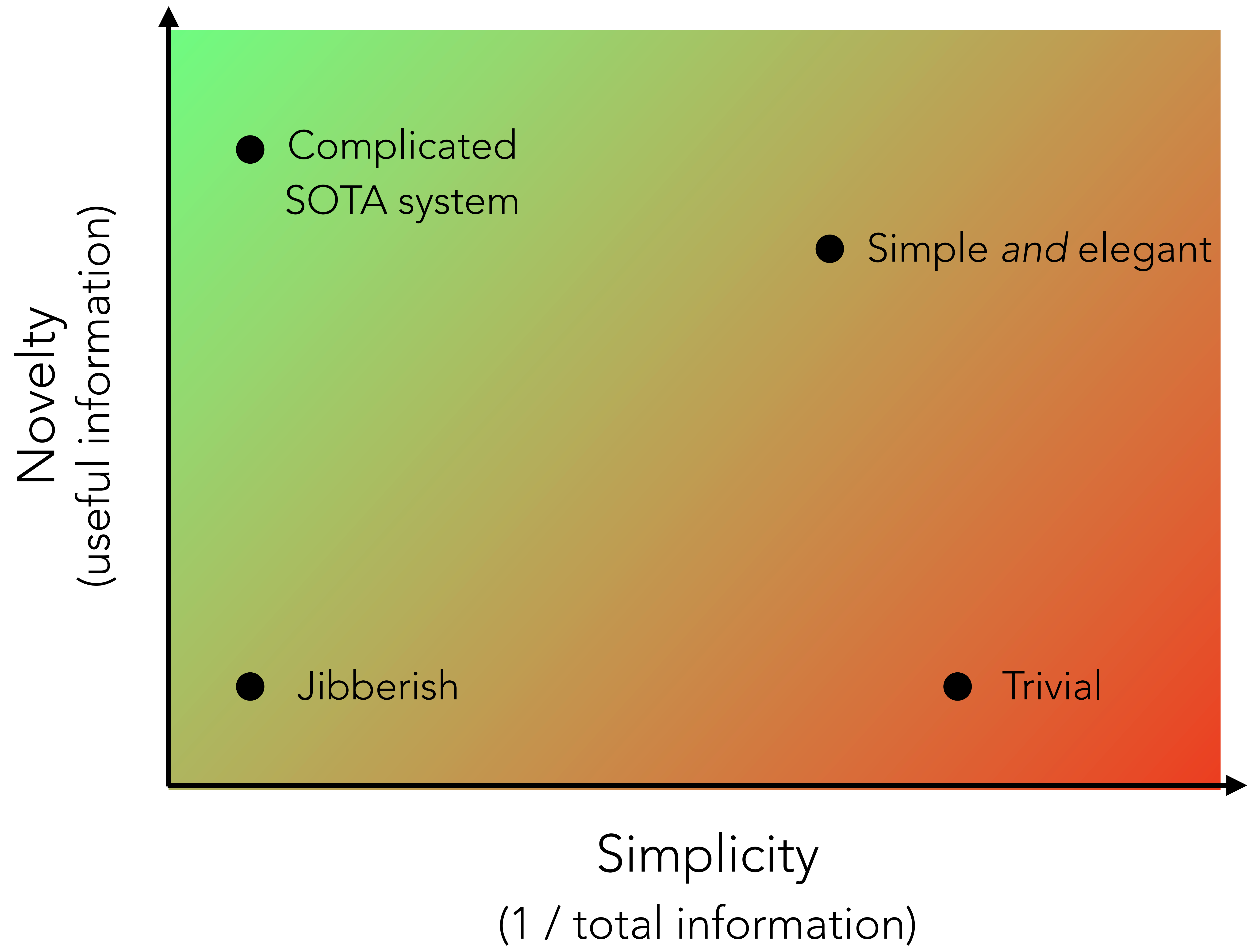
Products

Costs

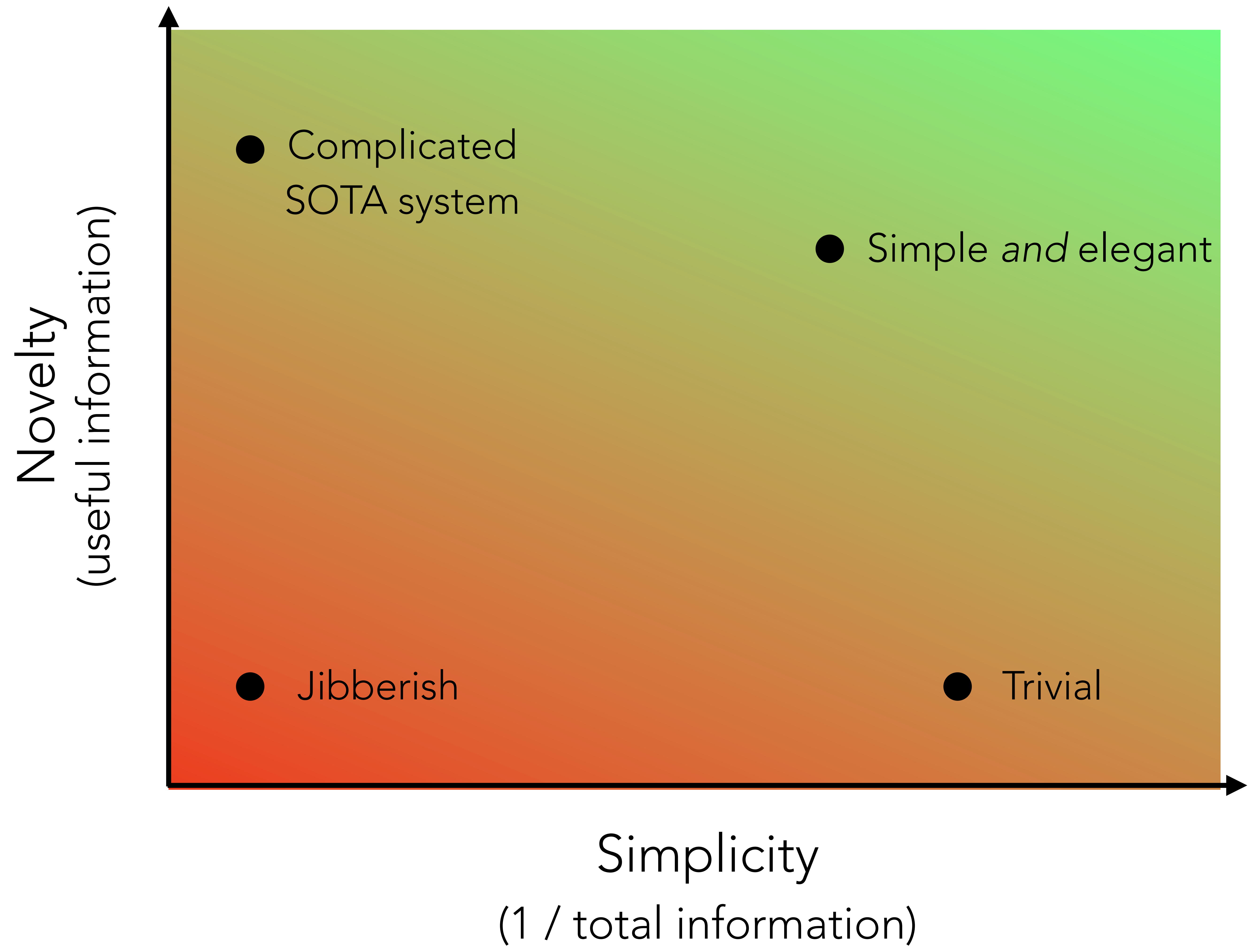
Pareto front of simplicity vs novelty



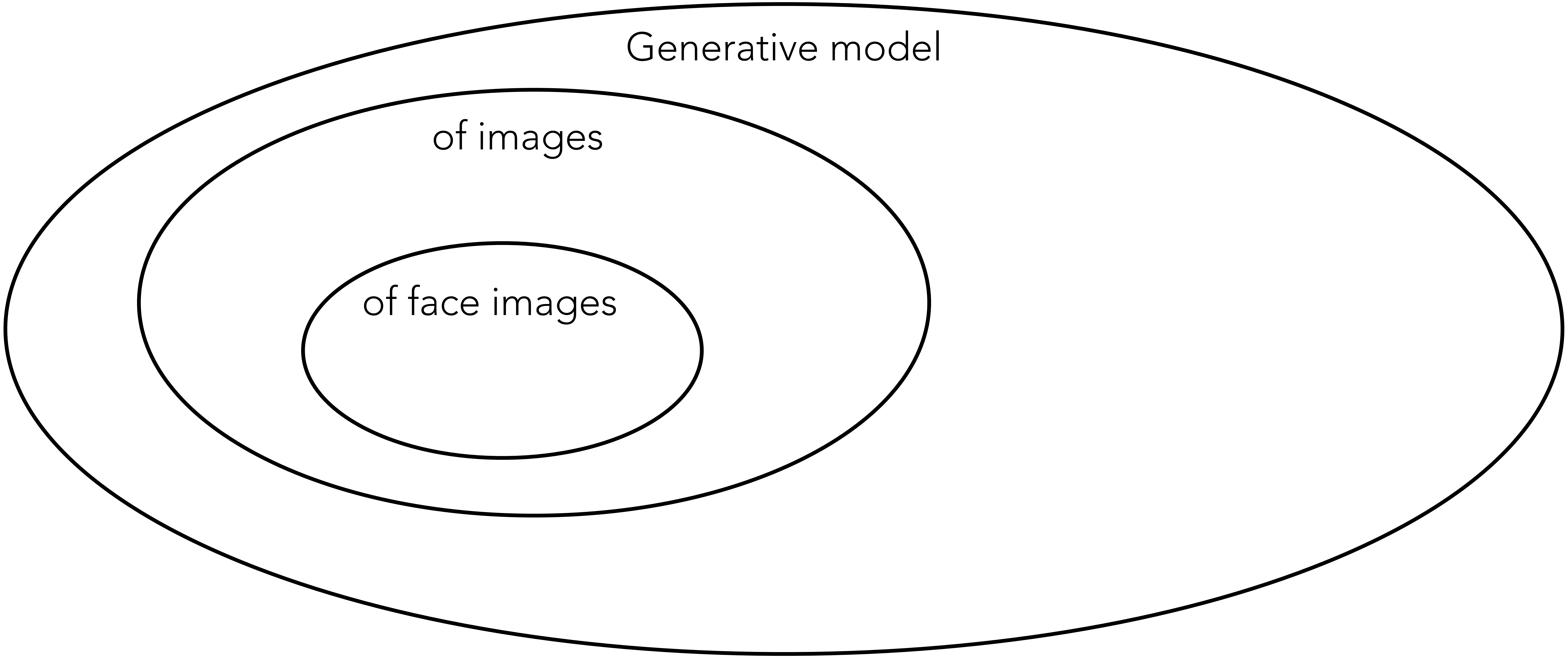
What the reviewing system rewards



What stands the test of time



Scope your contribution



Pick the broadest scope under which your work is novel. Too broad and your work is indistinguishable from prior theory. Too narrow and you lose impact.

The regularizing force of human fallibility

- The vast majority of information at any given academic conference is forgotten — that's a good thing
- We tend to forgot all but the simplest and starkest discoveries
- Complex and subtle discoveries are usually either overfit or unimportant

[https://www.youtube.com/watch?v=mrw4KlP5en0]