How do humans succeed in tasks like proving Fermat's Theorem or predicting the Higgs boson?*

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Abstract

I discuss issues of inverting feasibly computable functions, optimal discovery algorithms, and the constant overheads in their performance.

Our computers do a huge number of absolutely wonderful things. Yet most of these things seem rather mechanical. Lots of crucial problems that do yield to the intuition of our very slow brains are beyond our current computer arts.

Great many such tasks can be stated in the form of inverting easily computable functions, or reduced to this form. (That is, finding inputs/actions that could produce a given result in a given realistic process.)

We have no idea about intrinsic difficulty of these tasks. And yet, traveling salesmen do get to their destinations, mathematicians do find proofs of their theorems, and physicists do find patterns in transformations of their bosons and fermions ! How is this done, and how could computers emulate their success?

Of course, these are collective achievements of many minds engrossed in a huge number of papers. But today's computers can easily search through all math and physics papers ever written. The limitation is not in physical capacity.

And insects solve problems of such complexity and with such efficiency, as we cannot dream of. Yet, few of us would be flattered by comparison to the brain of an insect. What advantage do we humans have ?

One is the ability to solve **new** problems on which evolution did not train zillions of our ancestors. We must have some pretty universal methods, not dependent on the specifics of focused problems. Of course, it is hard to tell how, say, mathematicians find their proofs. Yet, the diversity and dynamism of math achievements suggest that some pretty universal mechanisms must be at work.

Let me now focus on a specific technical problem: Consider, for instance, algorithms that 3-color given graphs¹. Is it true that every such algorithm can be sped-up 10 times on **some** infinite set of graphs ?

Or, there is a "perfect" algorithm, that cannot be outsped 10 times even on a subset of graphs?

Note, there is a 3-coloring algorithm that cannot be outsped by more than constant factors on **any** subset. The question is, must these constants get really big?

But before further discussion, some history:

In the 50s, in the Russian math community there was much interest in the works of Claude Shannon. But many of Shannon's constructions required exhaustive search of all configurations. There was an intense interest in whether these exponential procedures could be eliminated (see [9]).

And Sergey Yablonsky wrote a paper that he interpreted as showing that no subexponential method could work on a problem that is, in today's terms, co-NP. It is a problem of finding a boolean function of maximal circuit complexity.

Kolmogorov saw this claim as baseless since the proof considered only a specific type of algorithms. Unhappy with such misleading ideas being promoted, Kolmogorov advocated the need for efforts to find valid proofs for common beliefs that complexities of some popular problems are indeed unavoidable.

This task required a convincing definition of the running time. But Turing Machines were seen as too restricted to use for meaningful speed lower bounds. Kolmogorov formulated (see [6]) a graph-based model of algorithms that had time complexities as they are understood today.

He also ran a seminar where he challenged mathematicians with quadratic complexity of multiplication. And an unexpected answer was soon found by Anatoly Karatsuba, and improved by Andrei Toom: multiplication complexity turned out nearly linear.

This was an impressive indication that common sense is an unreliable guide for hardness of computational problems, and must be verified by valid proofs.

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 $^{^{1}}$ This is a complete problem, i.e. one to which all other inversion problems are reducible.

I, at that time, was extremely excited by some other work of Kolmogorov. He (and independently Ray Solomonoff) used the Turing's Universal Algorithm for an optimal definition of informational complexity, randomness, and some other related concepts.

I noted that similar constructions yield an optimal up to a constant factor algorithm for a problem now called Tiling, and thus for any search problem, as they all have a straightforward reduction to Tiling.

To my shagreen, Kolmogorov was not impressed with the concept of optimality, saw it as too abstract for the issue at hand. (Indeed, finding specific bounds did not look as hopeless then as it now does.) But he was much more interested in my remark that Tiling allows reduction to it of all other search problems. He thought I should publish **that** rather than the optimal search.

I thought it would only be worth publishing if I can reduce it to some popular problems. My obstacle was that combinatorics was not popular in Russia, and my choice of problems that might impress the math community was rather limited. I saw no hope for something like factoring, but spent years in naive attempts on things like graph isomorphism, finding small circuits for boolean tables, etc.

Meanwhile an interesting angle was added to the issues. In 1969 Michael Dekhtiar, a student of Boris Trakhtenbrot, published a proof [3] that under some oracles inverting simple functions has exponential complexity. In the US, Baker, Gill, and Solovay did this independently [1].

Later I ran into problems with communist authorities. And friends advised me to quickly publish all I have while the access to publishing is not yet closed to me. So I submitted several papers in that 1972, including the one about search [7] (where Kolmogorov agreed to let me include the optimal search). I guess I must thank the communists for this publication.

But the greatest developments by far were going on in the United States. S. Cook [2], R. Karp[5], and Garey and Johnson [4] made a really revolutionary discovery. They found that 3-SAT reduces to great many important combinatorics problems.

Combinatorics received much attention in the West and these results became a coup !

Kolmogorov asked several questions at that time, still open and interesting. One was: Are there polynomial time algorithms that have no **linear** size circuits ? We knew that some slow polynomial time algorithms cannot be replaced by faster **algorithms**. But can linear-sized circuits families replace **all** of them ?

His other interesting comment was a bit more involved. We proved at that time that mutual information between strings is roughly symmetric. The proof involved exponential search for short programs transforming a strings x into y. Kolmogorov wondered if such search for short fast (meant in robust terms, tolerating +O(1) slacks in length and in log time) programs would not be a better candidate than my Tiling to see if search problems are exponentially hard.

He said that, often, a good candidate to consider is one that is neither too general, nor too narrow. Tiling, being universal, may be too general, lacking focus. Some other problems (say, factoring) – too narrow. And search for fast short programs looked like a good middle bet to him. It still does to me !:-)

Such search is involved in another type of problems that challenge our creativity: extrapolating the observed data to their whole natural domains. It is called by many names, "Inductive Inference", "passive learning", and others. Occam Razor is a famous principle of extrapolation. A version attributed to Einstein suggests: hypothesis need be chosen as simple as possible, but no simpler :-).

Ray Solomonoff gave it a more formal expression: The likelihoods of various extrapolations, consistent with known data, decrease exponentially with the length of their shortest descriptions. Those short programs run about as fast as the process that had generated the data.

There have been several technical issues that required further attention. I will stay on a simple side, not going into those details. Most of them have been clarified by now, **if** we ignore the time needed to find such short fast programs. This may be hard. Yet, this is still an inversion task, bringing us back to the issues of optimal search. I have a little discussion of such issues in [8].

Now, back to my focus. The concept of optimal algorithm for search problems ignores constant factors **completely**. So, it is tempting to assume that they must be enormous.

However, this does not seem so to me. Our brains have evolved on jumping in trees, not on writing math articles. And yet, we prove Fermat's Theorems, design nukes, and even write STOC papers. We must have some quite efficient and quite universal guessing algorithms built-in.

So, I repeat a formal question on these constants:

Can every algorithm for complete search problems be outsped 10 times on an infinite subset ? OR, there is a "perfect" one that cannot be, even on a subset ?

Of course, careless definitions of time can allow fake speed-ups. For instance if we ignore the alphabet size and reduce the number of steps just by making each step larger due to the larger alphabet. Or if we exclude the required end testing of the input/output relation, and choose a relation that itself allows a nonconstant speed-up. But it is easy to carefully define time to preclude such cheating.

Let me now go into some little technicalities to see what issues are involved in understanding these constant factors. We look at the optimal search for an inverse w of a fast algorithm f, given the output xthat f must produce from w.

We refine Kolmogorov Complexity with **time**, making it computable. The time-refined complexity **Kt** of w given x considers all prefixless programs pby which the universal algorithm U generates w from x in time T. (T includes running f(w) to confirm it is x.) **Kt**(w|x) is the minimum of length of p, plus log T.

The Optimal Inverter searches for solutions w in increasing order of this complexity **Kt** of w given x, **not** of length of w. For instance, shorter proofs may be much harder to find, having higher complexities. The Inverter generates and checks in time 2^k all wup to complexity k.

Btw, the optimal search makes the concept of complexity applicable to individual instances of search tasks, not just to families of instances which we now call "problems" and complexities of which we study. So we can ask how hard is, say, to find a short proof for Fermat's theorem, not for theorems in general. Would not this notion fit tighter ?

The big **catch** here is that **each** wasteful bit U requires of p **doubles** the time. We would need a **very** "pure" U, frugal with wasting bits. Do our brains have such a one built-in? It seems so to me. We do seem to have little disagreement on what is "neat" and what is cumbersome. There are differences in our tastes, but they are not so huge that we could not understand each other's aesthetics. But this is just a feeling. The formal question remains:

Is there an algorithm for a complete search problem that cannot be outsped ten times, even on an infinite subset ?

(Of course, this 10 is a bit arbitrary, can be replaced with your favorite reasonable constant.)

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