Degrees and difficulty

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Simpson (2015) CiE tutorial (his italics)

Given an algorithmically unsolvable problem X, one associates to X a ***degree of unsolvability***, i.e., a quantity which measures the amount of algorithmic unsolvability which is inherent in X ... ¶ ...

The existence of unsolvable mathematical problems was discovered by Turing (1936). Indeed, Turing exhibited a ***specific***, ***natural example*** of such a problem ... ¶ ...

A scheme for classifying unsolvable problems was developed by Post (1944) and Kleene and Post (1954). Two reals X and Y are said to be ***Turing equivalent*** if each is computable using the other as a Turing oracle. The ***Turing degree*** of a real is its equivalence class under this equivalence relation. Each of the specific, natural, unsolvable problems mentioned in the previous paragraph* is a decision problem and may therefore be straightforwardly described or "encoded" as a real.

* The Halting Problem, Hilbert's Tenth Problem, Homemophisms of finite simplicial complexes, the Word Problem, the *Entscheidungsproblem*.

Simpson (2015) CiE tutorial (my italics)

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Post (1944)

Related to the question of solvability or unsolvability of problems is that of the ***reducibility*** or ***non-reducibility*** of one problem to another. Thus, if problem X has been reduced to problem Y, a solution of X immediately yields a solution of Y, while if X is proved to be unsolvable, Y must also be unsolvable. For unsolvable problems the concept of reducibility leads to the concept of degree of unsolvability, two unsolvable problems being of the same degree of unsolvability if each is reducible to the other, one of lower degree of unsolvability than another if it is reducible to the other, but that other is not reducible to it, of incomparable degrees of unsolvability if neither is reducible to the other.

"Recursively enumerable sets of positive intergers", p. 289

Shoenfield (1971)

> We can think of $X \leqslant_T Y$ as meaning that X is at least as **easy** to compute as Y. Then $X \equiv_T Y$ means that X and Y are **equally easy** to compute. Thus the degree of X is a **measure of the difficulty** of computing X; the **higher** this degree (in the partially ordered set of degrees), the **more difficult** Y is to compute.
>
> *Degrees of Unsolvability*, pp. 26-27

Should such terminology be taken seriously? Why care?

- Philosophers of mathematics tend to draw conclusions from degree-theoretic results unreflectively.
- Terms like "measure", "degree", "inherent" suggest an analogy between ***comparing the difficulty*** of computational problems and ***empirical measurement*** of mass, temperature, etc.
- ***Complexity theorists*** speak of classifying ***decidable*** problems according to their "degree of inherent computational difficulty".

Focal questions

Q1) Is there a "**Church's Thesis**" for notions like *reduction*, *easier/harder than*, and *degree/amount of difficulty*?

Q2) Does the machinery of ***measurement theory*** – *à la* Krantz, Luce, Suppes & Tversky (1971) – help to clarify this question?

Q3) Do we get the same answers for 1) and 2) in both computability theory and complexity theory?

- Computability theory is concerned with ***degrees of non-solvability*** over $2^{\mathbb{N}}$.
- Complexity theory is concerned with ***degrees of feasibility*** inside of $\mathbf{REC}$ (or even $\mathbf{EXP}$).

Focal answers (tentative)

Q1) Is there a "Church's Thesis" for notions like *reduction*, *easier/harder than*, and *degree/amount of difficulty*?

A1) At best a partial one.

Q2) Does the machinery of *measurement theory* help to clarify this question?

A2) Somewhat.

Q3) Do we get the same answers for 1) and 2) in both computability theory and complexity theory?

A3) Complicated. (In complexity theory, the *pretheoretical* comparisons of difficulty seem to have a stronger basis in practice. But we are plagued by *open separation question* for complexity classes.)

Gödel (1946) – my italtics

Tarski has stressed in his lecture . . . the great importance of the concept of . . . Turing's computability. It seems to me that this importance is largely due to the fact that with this concept one has for the first time succeeded in giving an ***absolute definition*** of an ***interesting epistemological notion***, i.e., one ***not depending on the formalism*** chosen. In all other cases treated previously, such as demonstrability or definability, one has been able to define them only relative to a given language, and for each individual language it is clear that the one thus obtained is not the one looked for. For the concept of computability, however . . . the situation is different. By a kind of ***miracle*** it is not necessary to distinguish orders, and the diagonal procedure does not lead outside the defined notion.

Remarks before the Princeton bicentennial conf. on problems in maths., p. 150

Question: Do we expect a similar "miracle" for *reduction*, *easier/harder*, *degree of difficulty*?

Arguing for Church's Thesis

- Chuch's Thesis is a "pre-theoretical/theoretical" identification:

  $f : \mathbb{N}^k \to \mathbb{N}$ is *effectively computable* iff $f(\vec{x})$ is *recursive*

- Arguments for CT:
  i) inductive/descriptive
  ii) convergence of definitions (Kleene)
  iii) conceptual analysis (Kolmogorov & Uspensky, Gandy, Sieg)
  iv) squeezing argument (Kreisel, Smith)

- Assessing the arguments requires some "pre-theoretical data":
  1) intuitions about the **form** of a correct definition should take:

     $f(\vec{x})$ is computable just in case *there is a device of type ...*
  2) some **positive and negative examples** – e.g.
     (+) $x + y, x \times y, x^y, x{\uparrow}y, \ldots, \text{Ackermann}, \ldots$ are intuitively computable
     (-) the characteristic function of FOL VALIDITY is not (???)

Pre-theoretical and theoretical concepts

"Pre-theoretical"	"Theoretical" (e.g.)
problem	decision problem $X \subseteq \mathbb{N}$
solution	effective decision algorithm – i.e. Turing machine
reduction of X to Y	Turing reduction – i.e. oracle machine ϕ^Y which computes $c_X(x)$
X is at least as easy as Y	$X \leqslant_T Y$
X and Y are equally difficult	$X \equiv_T Y$ iff $(X \leqslant_T \ \& \ Y \leqslant_T X)$
degree of difficulty of X	$\deg_T(X) = \lvert\{Y \ : \ X \equiv_T Y\}\rvert$ – i.e. Turing degree
'amount' of difficulty of X	position of X in $\mathcal{D}_T = \langle D_T, \leqslant_T \rangle$ – i.e. the Turing degrees

NB the "e.g." ...

Pre-theoretical and theoretical concepts

"Pre-theoretical"	"Theoretical" (e.g.)
problem	decision / function / mass problem *optimization / probabilistic problem, ...*
solution	effective procedural / functional *feasible / approximation / probabilistic algorithm*
reduction of X to Y	Turing, m-1, 1-1, truth table, enumeration, Medvedev, Muchnik, ... *poly. time reduction, log-space reduction, ...*
X is at least as easy as Y	$X \leqslant_T Y, X \leqslant_1 Y, X \leqslant_m Y, X \leqslant_{\mathrm{tt}} Y, X \leqslant_e Y, X \leqslant_s Y, X \leqslant_w Y, \ldots$ $\leqslant^P_T, \leqslant^P_m, \leqslant^L_T, \leqslant^L_m, \ldots$ (Cook versus Karp)
X and Y are equally difficult	$X \leqslant_* Y$ iff $(X \leqslant_* \ \& \ Y \leqslant_* X)$
degree of difficulty of X	$\deg_*(X) = \lvert\{Y \ : \ X \equiv_* Y\}\rvert$
'amount' of difficulty of X	position of X in $\mathcal{D}_* = \langle D_*, \leqslant_* \rangle$

NB: the last three are "parametric" in the defn. of $*$.

CT for degrees and reduction?

- Claims requiring careful historical argumentation:

  1) Like *effectively computable*, the concepts of *problem* and *reduction* ***are*** **"interesting epistemological notions"** in Gödel's sense (i.e. pre-1936).
  2) But unlike *effectively computable*, our intuitions about *reducibilty* ***are not*** **"absolute"** in Gödel's sense. Nor do they admit as robust a class of positive and negative examples for testing definitions (at least in computability theory).
  3) So unlike the case of *computable*, we should expect (at best) a ***parametric*** ("formalism dependent") analysis of *reducibility* to parallel Church's Thesis.

- Now some brief, historically inadequate remarks about 1)-3).

Kolmogorov (1932)

We do not define the notion of a ***problem*** but explain it by means of some examples:

1) Find four integers x, y, z and n such that

$$x^n + y^n = z^n \quad n > 2$$

2) Prove that Fermat's theorem is false.
3) Construct a circle passing through three given points (x, y, z).
4) Given one root of the equation $ax^2 + bx + c = 0$, find the other root.
5) Assuming that the number π has a rational expression $\pi = m/n$ find a similar expression for the number e.

The fourth and fifth problems are examples of conditional problems; the premise of the latter is false, and hence the fifth problem is meaningless or empty ... ¶ ... We believe that these examples and explanations allow us to use unambiguously the notions of "problem" and "solution of a problem" in all the cases encountered in specific fields of mathematics ... ¶ ... $a \supset b$ **is the problem "given a solution to problem a, solve problem b" or, which is the same, "reduce the solution of problem b to the solution of problem a"**.

"On the interpretation of intuitionistic logic", pp. 151-152

[Aside on mass problems]

- None of Kolmogorov's examples are decision problems $X \subseteq \mathbb{N}$.
- Nor are they ***mass problems*** ...
  - Medvedev (1955) and Muchnik (1963) both studied the use of *mass problem* to formalize the problem interpretation of IPC.
  - A mass problem is $\mathcal{X} \subseteq \mathbb{N}^{\mathbb{N}}$ – e.g. complete extensions of PA.
  - To "solve" a mass problem is to "find" a set $X \in \mathcal{X}$.
  - $\mathcal{X} \leqslant_s \mathcal{Y}$ iff $\exists e \forall Y \in \mathcal{Y}[\Phi_e(Y) \in \mathcal{X}]$.
  - $\mathcal{D}_s = \langle D_s, \leqslant_s \rangle$ forms a Brouwerian lattice.
  - By interpreting, e.g., $X \to Y = \min\{Z \ : \ Y \leqslant_s X \ \wedge \ Z\}$ Medvedev showed that $\mathcal{D}_s$ gives a model of IPC.
  - This appears to have resulted from a direct attempt to formalize the problem interpretation.
- Observations: i) unclear if we are concerned with mass problems outside computability theory; ii) but if we are, this only complicates the task of giving an analysis of *reduction*.

Remarks on Kolmogorov (1932)

$X \supset Y$ is the problem "given a solution to problem X, solve problem Y"

or equivalently (?)

"reduce the solution of Y to the solution of X"

- Let's assume that $X, Y \subseteq \mathbb{N}$ are decision problems.
- Two interpretations of ***given a solution***:
  - i) given an instance $x \in X$
  - ii) given Y as an oracle
- Corresponding interpretations of ***reduce***:
  - i′) produce an instance $y \in Y$ (for then a method for deciding Y would yield a method for deciding XF)
  - ii′) produce X or $c_X(x)$

Reduction types

- Kolmogorov speaks of a ***reduction*** as "a general method" requiring "finitely many steps".
- Two possible *types* of ***general methods***:

  i″) $m : \mathbb{N} \to \mathbb{N}$ s.t. $x \in X$ iff $m(x) \in Y$
  ii″) $m : 2^{\mathbb{N}} \to 2^{\mathbb{N}}$ s.t. $m(Y) = c_X(x)$

- Reading *recursive/recursive in* for "general method" in i′), ii″) gives the standard definitions:

  i‴) $X \leqslant_m Y$ iff there a recursive $\phi_e(x)$ s.t. $x \in X$ iff $\phi_e(x) \in Y$
  ii‴) $X \leqslant_T Y$ iff there is a e s.t. $\phi_e^Y(x) = c_X(x)$

  - Compare i″) and
    - A proof of $X \supset Y$ is a method $f(x)$ s.t. $\forall x(x : X \to f(x) : Y)$.
    - A realizer of $X \supset Y$ is an e s.t. $\forall x(x \ \mathtt{rn}\ X \to \phi_e(x)\downarrow \ \mathtt{rn}\ Y)$.
  - So reading $x : X$ or $x \ \mathtt{rn}\ X$ as "$x \in X$" *à la* Curry-Howard, suggests that (something like) $\leqslant_m$ is a potential analysis of *intuitionistic implication*.

Parameters

- Komologorov's own characterizations of "X is reducible to Y" don't seem to decide between (e.g.) $\leqslant_T$ and (e.g.) $\leqslant_m$.
- Two parametric axes:

Access: What does it mean to be "given a solution to Y"? E.g. how many oracle queries do we get in order to decide $x \in X$? how can we combine them?

Uniformity: What is a "general method"? E.g. does it have to be 1-1, "effective" (recursive), "finitary" (primitive recursive), "feasible" ($\mathrm{P\text{-}Time}$), "very feasible" ($\mathrm{LogSpace}$), etc.?

- If our practices/intuitions don't uniquely prefer a solution to these questions, a "miracle" seems unlikely.
- In particular, since $\mathcal{D}_T$ is not isomorphic (or even elementary equivalent) to $\mathcal{D}_m$, a "converging definitions" argument appears to be ruled out. (This is true already for $\mathcal{R}_T$ and $\mathcal{R}_m$.)

Intuitive data about "difficulty"

- Here's another way a "miracle" might occur:
  1) Maybe for certain $X, Y \subseteq \mathbb{N}$ our practices determine that X is *easier/harder/equally difficult* to decide than/as Y.
  2) This data might give rise to an "informal" (or "empirical") degree structure $\mathfrak{D} = \langle D, \preceq \rangle$.
  3) We can then compare $\mathfrak{D}$ with the formally defined structure $\mathcal{D}_*$ for different definitions of $\leqslant_*$.
  4) Maybe argue for $\mathfrak{D} = \mathcal{D}_*$ by using a variant of the inductive/ descriptive or the Kreisel squeezing argument for CT?

- Points for rest of the talk:
  i) Measurement theory clarifies this methodology and provides a *potential* justification for our use of terms like "degree", "measure", "harder/easier" wrt computational problems.
  ii) Complexity theory provides more robust data of type 1) and hence possibly a stronger case for 4).

Measurement theory (Krantz, Luce, Suppes, & Tversky 1971-1990)

Measurement theory studies the practice of associating numbers with objects and empirical phenomena. It attempts to understand which qualitative relationships lead to numerical assignments that reflect the structure of these relationships, to account for the ways in which different measures relate to one another, and to study the problems of error in the measurement process ... ¶ ... The first problem for any such theory is to justify the assignment of numbers to objects or phenomena to pass from empirical procedures and operations to a numerical representation of these procedures. [What is known as] the ***representation problem*** is first to characterize the ... abstract properties of these procedures and observations and then to show mathematically that these axioms permit the construction of a numerical assignment in which familiar abstract relations and operations, such as "is greater than or equal to" ($\geqslant$) and "plus" ($+$) correspond structurally to the empirical (or concrete) relations and operations ... ¶ ...

Suppes and Zinnes (1962)

Measurement theory (cont.)

The second fundamental problem is that of the ***uniqueness of the representation*** – i.e., how close it is to being the only possible representation of its type. The representation of mass, for example, is unique in every respect except the choice of unit [i.e. "**degree**"]; e.g., the representation is different for pounds than for grams or grains. Ordinary measurements of temperature, however, are unique in everything except the choice of both unit and origin – the Celsius and Fahrenheit scales differ not only in the size of unit but also in the zero point.

Suppes and Zinnes (1962)

- Names: Euclid, Hemholtz, Hilbert (geometry), Hölder (physics), Bentham, Pareto, von Neumann & Morgenstern (economics), Krantz, Luce, Suppes, Tversky, Narens, Zines (psychology).
- The term "**degree**" has been used for angle measurement since at least the 15th century. It had started to be used for temperature measurement by the early 18th century.

Set up

- $\mathfrak{E} = \langle E, R_1, \ldots, R_n, f_1, \ldots, f_m \rangle$ an *empirical structure* – e.g.
  - $\mathfrak{E}_1 = \langle E_1, \lesssim \rangle$, A_1 a set of rocks, $x \lesssim y$ iff x is softer than y
  - $\mathfrak{E}_2 = \langle E_2, \lesssim \rangle$, A_2 a set of cups of tea, $x \lesssim y$ iff x is cooler than y
  - $\mathfrak{E}_3 = \langle E_3, \lesssim, \circ \rangle$, A_3 a set of rigid rods, $x \lesssim y$ is x shorter than y, $x \circ y =$ concatenation
- $\mathfrak{N} = \langle N, S_1, \ldots, S_n, g_1, \ldots, g_m \rangle$ a *numerical structure* – e.g.

  $$\mathfrak{N}_1 = \langle \mathbb{N}, \leqslant \rangle \quad \mathfrak{N}_2 = \langle \mathbb{R}, \leqslant \rangle \quad \mathfrak{N}_3 = \langle \mathbb{R}^+, \leqslant, + \rangle$$

  NB: N is classically assumed to be a subset of $\mathbb{R}$.
- A *numerical assignment* is a $\psi : E \to N$. $\psi(x)$ is *homomorphism* if
  - for all $1 \leqslant i \leqslant n$ and $\vec{x} \in E^{r_i}$, $R_i(x_1, \ldots, x_{r_i})$ iff $S_i(\psi(x_1) \ldots, \psi(x_{r_i}))$
  - for all $1 \leqslant i \leqslant m$ and $\vec{x} \in E^{q_i}$, $\psi(f(x_1, \ldots, x_{q_i})) = g(\psi(x_1), \ldots, \psi(x_{q_i}))$

  NB: A homomorphism will typically be neither 1-1 nor onto.

Extensive measurement and representation

- A paradigm case of *extensive measurement* is where the empirical structure is $\mathfrak{E} = \langle E, \lesssim, \circ, e \rangle$ s.t.
  - $E =$ (e.g.) set of weights or rods (idealized)
  - We can check if $a \lesssim b$ holds by performing an empirical measurement – e.g. placing a and b in a pan balance.
  - $a \circ b = c$ if (e.g.) if a, b balance with c.
  - $e \in E$ is a choice of unit (e.g. a "standard" weight or rod).
- In such cases, we often expect that $\mathfrak{E}$ will satisfy axioms Γ.
- A ***representation theorem*** is of the form

  If $\mathfrak{E} \models \Gamma$, then there is a numerical structure $\mathfrak{N}$ and a homorphism
  $$\psi : E \to N$$
- E.g. $\mathfrak{E} \models \Gamma =$ the axioms of an Archimedean, totally ordered group.
- Holder's Theorem (part i): If $\mathfrak{E} \models \Gamma$, then there is a homomorphism $\psi(x)$ from $\mathfrak{E}$ into $\mathfrak{N} = \langle \mathbb{R}, \leqslant, +, 0 \rangle$.

Scales and uniqueness

- The ***set of scales for*** $\mathfrak{N}$ ***based on*** $\mathfrak{E}$ is the the set $\mathcal{S}$ of homomorphisms from $\mathfrak{N}$ to $\mathfrak{E}$.
  - $\mathcal{S} \neq \varnothing$ iff there is a representation theorem for $\mathfrak{N}$ wrt $\mathfrak{E}$.
  - A ***uniqueness theorem*** is a characterization of $\mathcal{S}$.
  - E.g. Holder's Theorem (part ii): If $\mathfrak{E} \models \Gamma$ and $\psi, \chi : E \to \mathbb{R}$ are homomorphisms from $\mathfrak{E}$ into $\mathfrak{N} = \langle \mathbb{R}, \leqslant, +, 0 \rangle$, then $\exists a \in \mathbb{R}^+$ s.t. $\chi(x) = a \cdot \psi(x)$.
- Scale types:
  - ratio: $\psi \in \mathcal{S} \Rightarrow \forall a \in \mathbb{R}^+, \ a \cdot \psi(x) \in \mathcal{S}$
    zero and ratios meaningful – e.g. mass, length, duration, angle
  - interval: $\psi \in \mathcal{S} \Rightarrow \forall a \in \mathbb{R}^+ \forall b \in \mathbb{R}, \ a \cdot \psi(x) + b \in \mathcal{S}$
    differences and their ratios meaningful – e.g. temperature
  - ordinal: $\psi \in \mathcal{S} \Rightarrow \forall \text{ strict mono } f : \mathbb{R}^+ \to \mathbb{R}^+, f \circ \psi(x) \in \mathcal{S}$
    only order meaningful – e.g. hardness (Mohs), wind (Beaufort)
  - nominal: $\psi \in \mathcal{S} \Rightarrow \forall \ f : \mathbb{R}^+ \to \mathbb{R}^+, f \circ \psi(x) \in \mathcal{S}$
    arbitrary relation to number – e.g. social security numbers

Artefacts

- A ***representational artefact*** occurs when we "read back" a property of the numerical structure $\mathfrak{N}$ into the empirical structure $\mathfrak{E}$ which isn't justified by a uniqueness theorem.
- E.g. maps don't represent color, conformal maps don't represent size (e.g. Greenland vs. Australia).
- Other examples:
  - interval: no "intrinsic" relation b/t the meter bar and $1 \in \mathbb{R}$
  - ratio: $100°\mathrm{C}$ is not "twice as hot as" $50°\mathrm{C}$
  - ordinal: *fluorite* is not "twice as hard as" *gypsum*, nor is $|h(\textit{fluorite}) - h(\textit{gypsum})| = 2 \cdot |h(\textit{gypsum}) - h(\textit{talc})|$ meaningful
  - nominal: I am not not odd (even though my social security number ends in 3).

Setting up the computational case

- The "empirical" structure $\mathfrak{C} = \langle \mathcal{C}, \lesssim \rangle$ consists of
  - a class of *problems* $\mathcal{C}$ – i.e. sets $X_0, X_1, \ldots \subseteq \mathbb{N}$
  - $X \lesssim Y$ iff we **judge** X to be *at least as easy to decide* as Y
- (Much of measurement theory was developed to account for similar sorts of judgements – e.g. of *pitch*, *intensity*, *preference*, etc. – which don't determine extensive structures.)
- Possible "empirical data" about $\lesssim$:

  $$\textsc{RelPrime} \lnsim \textsc{Primes} \lnsim \textsc{Factors} \lnsim \mathrm{K} \lnsim \textsc{Tot} \lnsim \textsc{Rec}$$

  Also $\lesssim$ should presumably be reflexive and transitive.
- So the "numerical structure" will be $\mathfrak{D} = \langle D, \preceq \rangle$ where $X \approx Y$ iff $X \lesssim Y$ & $Y \lesssim X$, $D =$ the set of $\approx$ equivalence classes.
- Do we have any *pretheoretical* reasons to expect that D can or cannot be a subset of $\mathbb{R}$?

Setting up the computational case (cont.)

$$\mathfrak{C} = \langle \mathcal{C}, \precsim \rangle \xrightarrow{\mathfrak{C}/\approx} \mathfrak{D} = \langle D, \preceq \rangle \overset{?}{\cong} \begin{cases} \mathcal{D}_T = \langle D_T, \leqslant_T \rangle \\ \mathcal{D}_m = \langle D_m, \leqslant_m \rangle \\ \mathcal{D}_{tt} = \langle D_{tt}, \leqslant_{tt} \rangle \\ \mathcal{D}_1 = \langle D_1, \leqslant_1 \rangle \\ \quad \vdots \end{cases}$$

Philosophical claims about this methodology:

1) When we talk about degree structures "measuring difficulty", the measure-theoretic framework is in background.
2) The "empirical data" is messy and probably fails to yield a robust axiomatization of $\mathfrak{C}$.
3) So we should be concerned about the *representation* and *uniqueness* problems when we speak of a given definition $\mathcal{D}_*$ as a scale for "measuring difficulty".

Difficulties with the computational case

- Since we can't have $2^{\aleph_0}$ many judgements, maybe we need to restrict to (e.g.) r.e. or arithmetical degrees.
- Maybe $\textsc{Primes}, \mathrm{K}, \textsc{Tot}$ are "natural" examples of problems such that $X \lnsim Y \lnsim Z$ and. $\deg_T(X) = \mathbf{0}, \deg_T(Y) = \mathbf{0}', \deg_T(Z) = \mathbf{0}''$.
- But are these judgements about *difficulty* or *definability* when extended to $\mathbf{0}^n$ for $n \geqslant 2$?
- We make judgements *within* $\mathbf{0}$ – e.g. $\textsc{Primes}$ vs. $\textsc{Factors}$.
- Do we have judgements about incomplete or incomparable r.e. degrees? [The "naturalness" debate.]
- Do our judgements attest to what kind of scale $\mathfrak{D}$ should be? E.g. should $\preceq$ be a total order? lattice? distributive? minimal pairs? automorophisms?
- Without such answers, there's little chance of ***axiomatizing*** our judgements about $\lesssim$ and hence little chance of ***proving*** representation or uniqueness theorem about $\mathfrak{C}$ wrt $\mathcal{D}_*$.

Discoveries and artefacts

- Properties of $\mathcal{D}_T$: *non-distributive* upper-semilattice with least element, embeds every countable partial order, not very homogenous – e.g. $\mathcal{D}_T \not\equiv \mathcal{D}_T(\geqslant \mathbf{0}^\omega)$, few (one?) automorphisms, bi-interpretable with true second-order arithmetic, *not absolute* wrt ZFC.
- Properties of $\mathcal{D}_m$: *distributive*, all ideals are upper-semilattices with least element & countable predecessor property of cardinality $\leqslant 2^{\aleph_0}$, homogeneous (all cones are isomorphic), many $(2^{2^{\aleph_0}})$ automorphisms, bi-interpretable with true second-order arithmetic, *absolute* wrt ZFC.
- Properties of $\mathcal{D}_1$: *not* an upper or lower semilattice, bi-interpretable with true second-order arithmetic.
- Methodological Questions:
  - Are any of these genuine "discoveries" about our pre-theoretical concept of *difficulty*?
  - Are any of the properties of $\mathcal{D}_*$ artefacts of using one definition of $\leqslant_*$ over another to measure difficulty?

A brief early history

1965 Cobham noted the robustness of polynomial time computability, defined the class $\mathbf{P}$, proposed $X \in \mathbf{P}$ as a *necessary* condition for ***feasible*** decidability.

1965 Edmonds gave an informal characterization of $\mathbf{NP}$ and suggested (essentially) $\mathbf{NP}$-completeness as a *sufficient* condition for ***intractability***.

1971-3 Cook defined *polynomial time Turing reducibility* ($\leqslant^P_T$) and proved that SAT is $\mathbf{NP}$-complete (Levin did this with $\leqslant^P_m$).

1972 Karp defined *polynomial time m-1 reducibility* ($\leqslant^P_m$) and showed that 21 "specific, natural" problems were $\mathbf{NP}$-complete relative to $\leqslant^P_m$.

- Of Karp's 21 problems, at least 11 had been previously stated in the literature of graph theory, combinatorial optimization, operations research, etc.

Karp's diagram

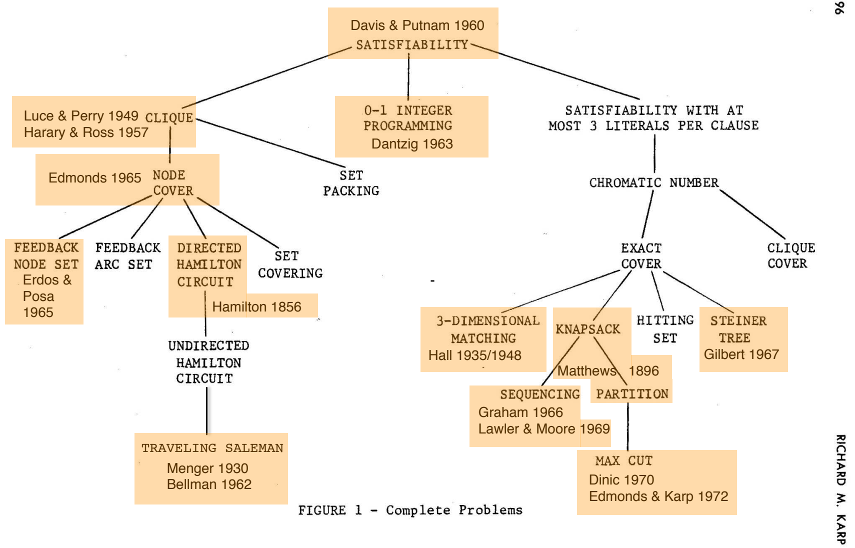


FIGURE 1 - Complete Problems

= Already in the literature

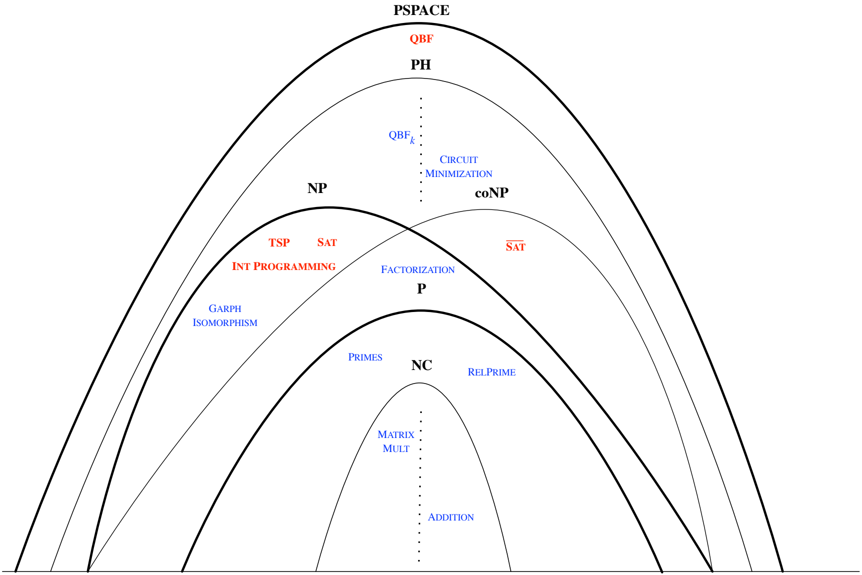
Polynomial time degrees in practice

- Karp's problems all have degree $\mathbf{0}$ (in fact, they are all in $\mathbf{EXP}$).
- Pre- and post-1971 much effort had already been spent searching for efficient algorithms for certain X which *seem* hard in practice.
- Some outcomes:
  - Feasible algorithms – e.g. $\textsc{Bipartite Matching}, \textsc{Primes}$
  - Parameterized complexity – e.g. $\textsc{Vertex Cover}$.
  - Approximation algorithms – e.g. $\textsc{Bin Packing}$
  - Average case complexity – e.g. $\textsc{Sat}$ solvers.
  - Dynamic programming – e.g. $\textsc{Tsp}$.
  - Little to no progress – e.g. $\textsc{Set Cover}$.
- Practical questions:
  - When can we conclude X is ***intractable*** – i.e. "feasibly unsolvable" or "unsolvable in practice"? (complicated)
  - Which reduction notion should we use?

Polynomial time degrees in practice (cont.)

- Completeness proofs for "natural" problems (like Karp's) generally yield m-1 reductions. However ...
  - $\mathcal{D}_m^P$ (distributive) is not are not elementarily equivalent to $\mathcal{D}_T^P$ (non-distributive).
  - $\leqslant_T^P$ and $\leqslant_m^P$ don't coincide on $\mathbf{EXP}$ – e.g. there are problems which are $\mathbf{EXP}$-complete wrt to $\leqslant_T^P$ (or $\leqslant_{tt}^P$) but not $\leqslant_m^P$.
  - This conjectured to be true for $\mathbf{NP}$ (but unknown).
- Complementation and $\mathbf{NP}$.
  - Note that $\overline{X} \leqslant_T X$ and $\overline{X} \leqslant_T^P X$.
  - But compare SAT and $\overline{\mathrm{SAT}}$:
    i) $\varphi \in \mathrm{SAT}$ iff $\exists v[\![\varphi]\!]_v = 1$.
    ii) $\varphi \in \overline{\mathrm{SAT}}$ iff $\forall v[\![\varphi]\!]_v = 0$
  - It certainly *seems* like $\overline{\mathrm{SAT}} \not\leqslant \mathrm{SAT}$.
- Presuming $\mathbf{NP} \neq \mathbf{coNP}$, this seems like a point in favor of using $\leqslant_m^P$ over $\leqslant_T^P$ to measure feasibility.

Locating problems



"Difficulty" in complexity theory

$$\textsc{Matrix Mult} \lnapprox \textsc{Primes} \lnapprox \textsc{Factors} \lnapprox \begin{cases} \textsc{Sat} \\ \overline{\textsc{Sat}} \end{cases} \lnapprox \text{QBF}_k \lnapprox \text{QBF}$$

- These are all "**specific, natural**" problems of degree $\mathbf{0}$.
- There are both practical and theoretical arguments for each instance of "$\lnapprox$".
- It is currently unknown if $\lnapprox$ can be replaced by $\lneq^P_m$.
- As $\textsc{Factors} \in \mathbf{NP} \cap \mathbf{coNP}$, a heuristic case can be given that its $\leqslant^P_m$-degree is ***intermediate*** between $\mathbf{P}$ and $\mathbf{NP}$
  - I.e. a candidate for a "natural" non-complete member of $\mathbf{NP} - \mathbf{P}$?
- Upshot: more robust "empirical data" $\lesssim$, fewer separation results about (e.g.) $\leqslant^P_m$.

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