Decoding random codes: asymptotics, benchmarks, challenges, and implementations

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(Maybe better to account for multi-target; see previous talk.)
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But real-world attackers do not have unlimited computation.

The usual standard, quantified: Choose cryptosystem parameters so that attacker has success chance at most $\epsilon$ after $2^c$ computations.
These parameters depend on \( c \) and \( \epsilon \).
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Some people count number of atoms in universe. Assume $c = 384$?
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Less discussion of $\epsilon$.

Is it okay for attacker to have 1% success chance? 1/1000? 1/1000000?
How do we handle this variability in $(c, \epsilon)$?

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A. Convince big community to focus on one $(c, \epsilon)$, eliminating the variability.
B. Choose parameters.
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2B more complicated than 1B.
Helpful simplification for code-based cryptography:

All of our best attacks consist of many iterations. Each iteration: small cost $2^c$, small success probability $\epsilon$.

Separate iterations are almost exactly independent: $2^{c'-c}$ iterations cost $2^{c'}$, have success probability almost exactly $1 - (1 - \epsilon)^{2^{c'-c}}$.

So parameters are really just functions of $2^c / \log(1/(1 - \epsilon))$. 
Is this simplification correct?

Objection 1: Is $2^{c^l-c}$ an integer?
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Response: Use $\lfloor 2^{c' - c} \rfloor$.

Iteration success probability is so small that we care only about $c' \gg c$. 
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Objection 2: “Reusing pivots” makes our best attacks faster but loses some independence.
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Response: Yes, must replace $\epsilon$ by result of Markov-chain analysis.
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Objection 2: “Reusing pivots” makes our best attacks faster but loses some independence.

Response: Yes, must replace $\epsilon$ by result of Markov-chain analysis. But can still merge $(c, \epsilon)$ into $2^c / \log(1/(1 - \epsilon))$. 
Attacker’s $2^c / \log(1/(1 - \epsilon))$ depends not only on parameters but also on attack algorithm.

Maybe attacker has found a much faster algorithm than anything we know!
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All public-key cryptosystems share this risk.

Responses to this risk: a huge amount of snake oil, and one standard approach that seems to be effective.
The standard approach:
Encourage many smart people to search for speedups.

Monitor their progress:
big speedup, big speedup, small speedup, big speedup, small, small, tiny, big, small, tiny, small, small, tiny, tiny, small, tiny, tiny, tiny, tiny, small, tiny, tiny, tiny, tiny.

Eventually progress stops.

After years, build confidence that optimal algorithm is known.
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Eventually progress stops.

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that optimal algorithm is known.
... or is it?
Consider cost of multiplying two $n$-coeff polys in $\mathbb{R}[x]$, where cost means 
\# adds and mults in $\mathbb{R}$.

Fast Fourier transform (Gauss): $(15 + o(1))n \lg n$. 
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Huge interest starting 1965.

Split-radix FFT (1968 Yavne):

\[ (12 + o(1))n \lg n. \]

Many descriptions, analyses, implementations, followups; 12 was believed optimal.
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Tangent FFT (2004 van Buskirk):
$(34/3 + o(1))n \lg n$. 
Consider cost of multiplying two \( n \)-coeff polys in \( \mathbb{F}_2[x] \), where cost means 
\# adds and mults in \( \mathbb{F}_2 \).

Standard schoolbook method: 
\[ 2n^2 - 2n + 1; \text{ e.g., } 61 \text{ for } n = 6. \]
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1963 Karatsuba method:
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2000 Bernstein:
e.g., 57 for \( n = 6. \)
Consider cost of multiplying two $n$-bit integers in $\mathbb{Z}$, where cost means 
\# NAND gates.

Schoolbook: $O(n^2)$. 
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Intense work after Karatsuba. 1971 Schönhage–Strassen: \( O(n \lg n \lg \lg n) \). Used in many theorems. Was believed optimal.
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1971 Schönhage–Strassen: \( O(n \lg n \lg \lg n) \).
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2007 Fürer:
non-constant improvement, almost reaching \( O(n \lg n) \).
Possible conclusion 1:
We’ll never know the optimal algorithm for anything interesting.
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2004 − 1968 = 36;
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Algorithms are optimal if they survive 38 years.
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\[
\begin{align*}
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\]
Algorithms are optimal if they survive 38 years.

Possible conclusion 3:
Should choose parameters aiming at a slightly larger $c$ so that speedups on this scale don’t compromise security.
Can also find examples of bigger speedups in well-studied problems, but these examples are less common.

Reasonable to hope that the standard approach (encouraging many smart people to search for speedups) finds near-optimal attacks.

Doesn’t eliminate risk, but historical examples suggest that the risk is much higher for cryptosystems that do not take the standard approach.
Sometimes I see papers taking steps that *discourage* this research:

1. Excessively optimistic algorithm analyses.

2. Excessively pessimistic algorithm analyses.

3. Nonsensical machine models.

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1. Excessively optimistic algorithm analyses.
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Why do they do this?
Napoleon: “N’attribuez jamais à la malveillance ce qui s’explique très bien par l’incompétence.”
McEliece public key:
linear map $G : \mathbb{F}_2^k \rightarrow \mathbb{F}_2^n$.

McEliece plaintext:
$m \in \mathbb{F}_2^k$;
and $e \in \mathbb{F}_2^n$ of weight $w$.

McEliece ciphertext:
$Gm + e \in \mathbb{F}_2^n$.

Typical parameter choices:
$k = Rn$ with $R = 0.8$;
$w = (n - k) / \lceil \log n \rceil$
$\approx (1 - R)n / \log n$. 
Basic information-set decoding, given $G$ and $y \in \mathbb{F}_2^n$:

Choose uniform random size-$k$ subset $S \subseteq \{1, 2, \ldots, n\}$.

Hope that the composition $\mathbb{F}_2^k \xrightarrow{G} \mathbb{F}_2^n \xrightarrow{} \mathbb{F}_2^S$ is invertible ($S$ is an “information set”). If not invertible, try new $S$.

Project $y$ from $\mathbb{F}_2^n$ to $\mathbb{F}_2^S$.

Apply inverse, obtaining $m$.

Compute $e = y - Gm$.

If weight of $e$ is not $w$, try new $S$. 
Idea introduced by 1962 Prange.

Easy to analyze speed of one iteration (one choice of $S$): Gaussian elimination to invert $k \times k$ matrix; matrix-vector multiplication; etc.

Easy to analyze probability for almost all choices of $G$:

0.288... chance of invertibility; 
\[
\frac{n-k}{w} \times \frac{n}{w} \times 0.288... \frac{n-k}{w} \times \frac{n}{w}.
\]

that $e$ is 0 on $F_2^S$;

overall iteration success chance 0.288... \[
\frac{n-k}{w} / \binom{n}{w}.
\]
1978 McEliece repeats same idea but has different analysis:

“A more promising attack is to select \( k \) of the \( n \) coordinates randomly in hope that none of the \( k \) are in error . . .

The probability of no error, however, is about \((1 - \frac{t}{n})^k\), and the amount of work involved in solving . . . is about \( k^3 \).

. . . one expects a work factor of \( k^3 \cdot (1 - \frac{t}{n})^{-k} \).

For \( n = 1024, k = 524, t = 50 \) this is about \( 10^{19} \approx 2^{65} \).”
McEliece probability analysis was excessively optimistic; lazy approximations are too small.

1988 Adams–Meijer:

$$k^3 \left/ \left(0.288 \ldots \left(\frac{n-k}{w}\right) / \left(\frac{n}{w}\right) \right) \right) \approx 2^{83}.$$
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\[ k^3 / \left( 0.288 \ldots \left( \frac{n-k}{w} \right) \left( \frac{n}{w} \right) \right) \approx 2^{83}. \]

How can someone publish an interesting new speedup from \( 2^{83} \) to \( 2^{73} \), if McEliece said \( 2^{65} \)?

Extra work for authors to convince reviewers that McEliece was wrong. Where’s McEliece’s erratum?
1999 Barg et al.: Huge speedups from “supercode decoding.”

\[ \text{Cost } 2^{(0.101\ldots + o(1))n} \]

if \( n \rightarrow \infty \), assuming

\( w/n \rightarrow W \) and \( k/n \rightarrow 1/2 = 1 + W \log W + (1 - W) \log(1 - W) \).

Best previous result:

\[ \text{Cost } 2^{(0.115\ldots + o(1))n}. \]
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Best previous result:

Cost $2^{(0.115...+o(1))n}$.

But 1999 Barg et al. is wrong!

Critical error in “Corollary 12” kills analysis and conclusions.

2009 Finiasz–Sendrier:

“To evaluate the cost of the algorithm we will assume that only the instructions (ISD i) are significant. . . . It is a valid assumption as we only want a lower bound. . . . WF_{ISD} \approx \cdots”
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No, a lower bound is not enough! Need to state actual attack cost to encourage future research. Lower bound is too optimistic, discourages future research.
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2011 Bernstein–Lange–Peters found most important idea in “ball-collision decoding” by analyzing supercode decoding.

2009 Finiasz–Sendrier missed speedup because they had an overly pessimistic analysis: lazy approximation $\binom{k+l}{p} \approx \binom{k}{p}$. 
Perhaps the biggest drain on research in this area: nonsensical machine models.
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1998 Canteaut–Chabaud: “We give here an explicit and computable expression for the work factor of this algorithm, i.e., the average number of elementary operations it requires.”

How can someone write a followup paper demonstrating a smaller “work factor”? Where is the definition of “elementary operations”?
Canteaut et al. obviously aren’t counting memory access, copies, communication costs. “Elementary operations” are fully explained by arithmetic.

Write speedup paper that counts these “operations” and doesn’t count memory access.
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Reviewer: “When the authors compute the complexity of one iteration of the algorithm they neglect (or deliberately forget) the cost of the join operation between the sets $S$ and $T$. ”
How do we get out of this mess? Surely we can cite definitions from computational complexity? Typical “RAM” definition?
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Typical “RAM” definition? Nonsensical results: can do $\Theta(n^2)$ bit ops in “time” $n$. Okay for poly-time theorems, not for serious optimization.
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Typical “RAM” definition? Nonsensical results: can do $\Theta(n^2)$ bit ops in “time” $n$. Okay for poly-time theorems, not for serious optimization.

“Pointer machines” — much more restrictive? 1980 Schönhage: Can multiply $n$-bit integers in $\Theta(n)$ operations on a pointer machine.
Count # NANDs in a circuit?
Mathematically pleasing.
Not obviously nonsensical.
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Circuits have fixed connections.
Simulate RAM by sorting.
Some work, but reasonably easy.
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Still physically unrealizable:
ignores wire delay, wire cost.
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1981 Brent–Kung AT theorem:
n-bit multiplication on
realistic size-n parallel circuit
has to take time \( n^{1/2} \)
even without wire delay.
A few suggestions

Want correct analyses in clear cost metrics.

Brent–Kung: realistic; not excessively complicated; suitable for asymptotics.
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# NANDs: trades realism for attractive simplicity; suitable for asymptotics.
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# NANDs: trades realism for attractive simplicity; suitable for asymptotics.

Time on CPU X: realistic; not as easy; not asymptotic; allows computer verification.
RSA factoring challenges have encouraged and recognized progress in integer factorization. Several new attempts to do this for post-quantum cryptography.

Mistakes to learn from: ECC challenges are too widely spaced; ECC and RSA solutions don’t measure time.

2011 Bernstein–Lange–Peters: new “partly wild” challenges, reasonably tight spacing; will keep track of time.
q = 13
m = 3
n = 451
s = 24
t = 2
u = 48
k = 307
w = 25
ciphertext = [11, 9, 12, 11, 10, ..., 1, 11]
recovered_plaintext_using_secret_key = True
pubkeycol144 = [0, 10, 3, 5, 1, ..., 4, 12,

pubkeycol145 = [12, 6, 8, 3, 2, ..., 10, 7,

...  

pubkeycol450 = [7, 10, 8, 10, 11, ..., 11, 8]