Getting a Few Things Right and Many Things Wrong

Neal Koblitz, Univ. of Washington, koblitz@math.washington.edu
Outline of Talk
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• Isogenies – common wisdom might (or might not) be wrong
• Survey of my own experiences being wrong most of the time
• History of embarrassing moments in “provable security”
• What is to be done? -- the keyword that unlocks the solution to this conundrum
Reference for the first part of the talk:

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In the meantime it’s available at http://eprint.iacr.org/2008/390.pdf
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See especially Section 11 concerning the security implications of isogeny walks.
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Also see our videoabstract at
http://www.youtube.com
(search for “elliptic curve cryptography”)
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- The family life of gorillas
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• Smart houses and robotic butlers
• Controversy over whether or not Arizona’s ancient Native American tribes were warlike.
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• The impact of Alan Sokal’s “hermeneutics of quantum gravity”
Conventional wisdom
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• In cryptography, for greatest security choose parameters as randomly as possible.
• In elliptic/hyperelliptic curve cryptography it’s safest to choose the defining equation to have random coefficients.
• It’s okay to use special curves for reasons of efficiency if you insist, but some day that choice might come back to bite you.
In 1991, I proposed the use of the non-supersingular $\mathbb{F}_2$ -curves (also called anomalous binary curves)

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because they seemed to have some efficiency advantages over random curves.

The U.S. National Security Agency (NSA) liked these curves, and at Crypto 1997 J. Solinas gave a talk presenting a thorough and definitive treatment of how to optimize ECC operations on these curves.
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Some people have been mistrustful of this family of curves, in part because of the “conventional wisdom” given above.
Another reason for mistrust is the syllogism:
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“Therefore we don’t trust these curves.”
My analysis is: NSA isn’t a monolith.
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Just because an NSA mathematician recommends a family of curves, it doesn’t necessarily follow that they’re no good.
Mathematicians & Engineers
high intelligence, high ethics

Spies, Spooks & Bureaucrats
low intelligence, low ethics
Corporate world

**WHITE HATS**

Researchers & Engineers
high intelligence, high ethics

**BLACK HATS**

Marketing People & Executives
low intelligence, low ethics
University
world

Professors
high intelligence, high ethics

Administrators
low intelligence, low ethics
But please note:
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The last slide doesn’t apply to any university administrators who are present today.
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Actually, the empirical basis for that slide comes from the U.S., not from India.
Moreover, in the random-vs-special debate about curve selection, Menezes and I found reason to question the conventional wisdom that random is always more secure.
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There are various scenarios in which someone who chooses ECC with a special curve might end up better off than someone else who chooses a random curve.

Some such scenarios are suggested by recent work on isogenies. (For more details see Section 11 of the “serpentine course” paper.)
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(An isogeny between isomorphic curves is called an “endomorphism.”)
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By constructing isogenies from $E$ to other curves in its isogeny class, we can transport its discrete log problem to equivalent discrete log problems on the other curves.
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The endomorphism ring is an order in the ring of integers of the CM-field of $E$.

Within the isogeny class of $E$, how widely different in size can two endomorphism rings be?
That depends on the square part of the discriminant of $E$. 
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A random curve is almost certain to have a discriminant with very small square part.
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By a result of Jao, Miller, and Venkatesan, one can use sequences of isogenies ("isogeny walks") to efficiently travel randomly and uniformly throughout the isogeny class of B-571, which consists of approximately $2^{285}$ curves.
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It has discriminant $-7(P_{22}P_{263})^2$ divisible by the square of a 22-bit prime times a 263-bit prime.

Most of the isogenous curves have very small endomorphism ring with index divisible by the 263-bit prime, and no one knows how to construct such a large degree isogeny from K-571 to such a curve.
At the very most, one can map \( K-571 \) to the curves whose endomorphism ring has index divisible at most by the 22-bit prime.
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There are approximately $2^{22}$ – that is, 40 lakhs – such curves.
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Let’s suppose that an algorithm were found that solves the elliptic curve discrete log problem (ECDLP) in time $T_1$ in a proportion $\varepsilon$ (a very small but not negligible proportion) of all elliptic curves over $\mathbb{F}_q$, where the property of being a “weak” curve is independent of isogeny and endomorphism class.
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Let’s suppose that an algorithm were found that solves the elliptic curve discrete log problem (ECDLP) in time $T_1$ in a proportion $\varepsilon$ (a very small but not negligible proportion) of all elliptic curves over $\mathbb{F}_q$, where the property of being a “weak” curve is independent of isogeny and endomorphism class.

Let’s also suppose that the “weak” property can be spotted quickly once we get to the curve.
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Then a discrete log on $E$ can be found in time roughly $T_1 + T_2/\varepsilon$, where $T_2$ denotes the time for constructing an isogeny that moves the ECDLP to another curve – assuming, of course, that the class of curves to which isogenies from $E$ can be constructed in time less than $T_2$ contains more than $1/\varepsilon$ curves.
Thus, if $\varepsilon$ is much less than $1/(40 \text{ lakhs})$, we find that K-571 is safe from the attack, but B-571 is not.
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In other words, it is the possibility of random isogeny walks through an endomorphism class that under certain circumstances might make a random curve less secure than a special curve.
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Despite the reluctance of many cryptographic researchers to admit it, in fact cryptography is as much an art as a science.
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(i) $p = A^2 + B^2$ is prime;

(ii) either $n = (p+1)/2 - A$
    or else $n = (p+1)/2 + A$
    is prime.

Then the elliptic curve $E$ over $\mathbb{F}_p$ defined (for suitable $a$ in $\mathbb{F}_p$) by $y^2 = x^3 - ax$ is isolated, in the sense that it’s infeasible to construct isogenies to other curves.
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Or maybe not.
PART II:
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First major one:

In the late 1980’s it seemed (to me at least) that any elliptic curve group would be secure as long as its order is prime or almost prime.
With that condition, I thought, all curves were created equal, and were endowed with an intractable ECDLP.
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So for pedagogical reasons why not use the simplest possible curves?
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So for pedagogical reasons why not use the simplest possible curves? And this is what I often did -- in my introductory book published in 1987 and in my articles and talks in the 1980’s.
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\[ y^2 = x^3 - x \quad \text{over} \quad \mathbb{F}_p \quad \text{with} \quad 4| (p+1) \]
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Just choose \( p \) so that \( (p+1)/4 \) or \( (p+1)/6 \) is prime, and ECC is secure...
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Just choose \( p \) so that \( (p+1)/4 \) or \( (p+1)/6 \) is prime, and ECC is secure...

Or so I thought.
These curves also have some nice efficiency advantages for computing point multiples, especially over extension fields of $\mathbb{F}_2$ and $\mathbb{F}_3$. 
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And for supersingular curves, such as the two written above, the extension degree is very small.
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And for supersingular curves, such as the two written above, the extension degree is very small. Usually it’s 2, as in the above cases.
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What surprised me as much as the MOV attack that killed supersingular curves in 1991 was that 10 years later they made a roaring comeback from the grave –
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What surprised me as much as the MOV attack that killed supersingular curves in 1991 was that 10 years later they made a roaring comeback from the grave – when pairing-based crypto took the research community by storm.
Another of my misjudgments during that time period:
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In 1989, when I first proposed Hyperelliptic Curve Cryptography, if you’d asked me I would have explained what I saw as the main potential advantage of HCC over ECC by speculating that most likely the higher the genus, the more security you’d get.
In other words, an attack that might work in low genus would be less likely to work in high genus.
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But that turned out to be exactly the opposite of what happened.
I was taken completely by surprise by the Adleman—DeMarrais—Huang algorithm (1994), which gave a subexponential time solution of the HCDLP in large genus.
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My favorite illustrative example, which was a genus-191 curve over $\mathbb{F}_2$, immediately became totally insecure.

After subsequent work by Gaudry, Diem, and others, it now seems that anything bigger than genus 2 is less secure than genus 1 or 2.
The only HCC that is fully competitive with ECC is genus-2 HCC.
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I couldn’t have been more wrong in my intuition about the greater security of high genus!
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In the early 1990’s, Mike Fellows and I became captivated by the notion that, despite the fiasco with knapsacks, good cryptosystems could in fact be constructed from NP-hard combinatorial problems.

We even wrote a paper with the exuberant title “Combinatorial Cryptosystems Galore!”
There was only one actual example that we spent some time developing, and it had a sorry history.
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As I recount in my book *Random Curves*:

“Mike Fellows and I… constructed a system based on… *ideal membership*… that involved polynomials, and we challenged people to try to crack it.

“The most attractive feature of our cryptosystem was the name that Mike thought up for it: *Polly Cracker*.

“It was very inefficient, and before long some papers were published that indeed cracked the code.”
Back to ECC:

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But otherwise you could use whatever field you most enjoy working with, and security is unaffected by that choice.
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That’s what I thought.
But I was wrong about this.
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His idea was to transport the ECDLP to the DLP on a high-genus curve over a subextension, where it could be attacked by the faster high-genus algorithms.
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Soon Gaudry, Hess, Smart, Galbraith, Menezes, Teske, and others found weak curves over certain binary fields of composite extension degree.

Fortunately, other people (such as Scott Vanstone) had had better instincts than I had, and all commercial implementations and all ECC standards used prime fields or prime-degree extensions of $\mathbb{F}_2$. 
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In early 1998 I published *Algebraic Aspects of Cryptography*. In a section titled “Cultural Background” I discussed the Birch and Swinnerton-Dyer Conjecture, after which I essentially apologized to my readers for taking up their valuable time with something that, while mathematically important, has no relevance for cryptography.
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What was most alarming for ECC people was that Silverman used the heuristics of the BSD Conjecture (and an analytic rank formula of Mestre) to boost the likelihood of a successful attack on the ECDLP.
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So the exact same mathematics that 8 months earlier I had been dismissing as irrelevant to cryptography turned out to be at the center of our fears about xedni.
The heuristics of this conjecture were very hard to analyze from a practical computational standpoint.

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After a lot of initial worry about xedni (fueled by our concern that RSA would use xedni as a weapon in their public relations battle with ECC, which was still going strong in 1998), I found that we could use the height function to show that xedni wouldn’t work.
I was so thrilled about this success in defending ECC that I gave a talk at ECC 2000 titled

“Miracles of the Height Function: A Golden Shield Protecting ECC”
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At ECC 2007, Silverman made a similar analysis for all 4 ways one could try index or xedni with liftings to global fields.
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I’ve learned the hard way that it’s foolish to go around talking about “Golden Shields” that protect security of ECC protocols or anything else. And I don’t believe in miracles anymore.
In each case of misjudgment, I thought I had a good mathematical reason to feel confident about what the future would bring.
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It likes to play jokes on us.

I learned that, much as we might wish to convey an impression of self-confidence and mathematical certainty to the outside world, to our colleagues, and to ourselves, such self-confidence is rarely justified.
PART III:
PART III: SOME SCANDALS IN THE HISTORY OF “PROVABLE SECURITY”
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For more details, please see the series of “Another Look” papers by Menezes and me on the eprint.iacr.org website.
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Soon after, MasterCard and Visa included OAEP in their SET electronic payment standard.
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This type of thing causes a credibility problem.
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In February of that year, Hugo Krawczyk – one of IBM’s top cryptographers – submitted a paper to Crypto 2005 in which he claimed to have found flaws in the Menezes—Qu—Vanstone (MQV) key agreement protocol.
A case that I think is even more scandalous occurred in 2005.

In February of that year, Hugo Krawczyk – one of IBM’s top cryptographers – submitted a paper to Crypto 2005 in which he claimed to have found flaws in the Menezes—Qu—Vanstone (MQV) key agreement protocol.

He especially criticized MQV for lacking a “proof of security.”
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If his claims had been valid, this would have been a major embarrassment not only to Menezes and his coauthors, but also to the NSA, which had licensed MQV (and 25 other patented protocols) from Certicom and whose experts had studied it closely.
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When Menezes was finally sent a copy – after the Program Committee had already accepted it – he saw that, first of all, Krawczyk’s objections to MQV were without foundation.

Moreover, he discovered that the paper’s main argument (“proof”) was fallacious.
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Menezes quickly found not only that the proof was fallacious, but that certain of the HMQV protocols succumb to the same attacks as MQV would have if those checks had not been put in it.
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So-called “proofs of security” often do more harm than good because they lull people into a false sense of security and cause them to take leave of common sense.
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Abstracts and introductions to papers often read as if they were written by marketing people or as part of a patent application, full of hype with little connection to reality.

In particular, they highlight their reductionist security argument (that reduces a supposedly intractable problem to a successful attack of a specified sort) using terminology designed to convince the reader that their protocols have been “proved” to be secure.
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Here’s an example: A paper by Boldyreva—Gentry—O’Neill—Yum available from eprint.iacr.org/2007/438 gave a pairing-based construction of sequential aggregate signatures (in which several people in sequence put together a single compact signature). They claimed it “…permits savings on bandwidth and storage… substantially improves computational efficiency and scalability over any existing scheme with suitable functionality…”
“In contrast to the only prior scheme to provide this functionality, ours offers improved security...
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The amusing thing about this example is that about a year later Huang, Lee, and Yung showed that a crucial security proof in this paper was fallacious.
“In contrast to the only prior scheme to provide this functionality, ours offers improved security… We provide formal security definitions and support the proposed scheme with security proofs…”

The amusing thing about this example is that about a year later Huang, Lee, and Yung showed that a crucial security proof in this paper was fallacious.

They also broke the corresponding protocol.
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The amusing thing about this example is that about a year later Huang, Lee, and Yung showed that a crucial security proof in this paper was fallacious.

They also broke the corresponding protocol. (The flaw was catastrophic – details are given in my March 2010 AMS Notices article with Menezes.)
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“…cryptographic constructions can be proven secure with respect to a clearly-stated definition of security and relative to a well-defined cryptographic assumption.

“This is the essence of modern cryptography, and what has transformed cryptography from an art to a science. The importance of this idea cannot be over-emphasized.”
Meanwhile, anyone who’s dismayed by the large number of fallacious proofs in the provable security literature is supposed to be consoled by the prospect that advances in “theorem-proving” software will soon make it possible to prove the security of our protocols automatically.
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Human mistakes and failings will supposedly disappear from the process of establishing guarantees of security.

And anyone who’s bewildered by the exotic nature of some of the cryptographic assumptions that underlie security proofs for many of the pairing-based protocols is supposed to be reassured by Boyen’s exuberant explanation at the Pairings-2008 conference:
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“...in comparison to the admittedly quite simpler algebraic structures of twentieth-century public-key cryptography...
“The newcomer to this particular branch of cryptography will… be astonished by the sheer number, and sometimes creativity, of these assumptions…

“…in comparison to the admittedly quite simpler algebraic structures of twentieth-century public-key cryptography… the new ‘bilinear’ groups offer a much richer palette of cryptographically useful trapdoors than their ‘unidimensional’ counterparts…”
In our March 2010 article in the AMS Notices, Menezes and I explain why we do not share Boyen’s enthusiasm for the bold and exotic assumptions that populate the landscape in pairing-based cryptography.
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Indeed, some of the assumptions used by leading researchers turned out to be significantly weaker than they had thought.
In our March 2010 article in the AMS Notices, Menezes and I explain why we do not share Boyen’s enthusiasm for the bold and exotic assumptions that populate the landscape in pairing-based cryptography.

Indeed, some of the assumptions used by leading researchers turned out to be significantly weaker than they had thought.

And the jury is still out on most of the other assumptions, since hardly any of them have been investigated thoroughly.
Thus, on the one hand, we see the trend of bold and boastful writing by cryptographic researchers.
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On the other hand, we see a long history of misjudgments, faulty assumptions, uncertainty, and catastrophic blunders in cryptography that continues to the present day.
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How can we reconcile the disciplinary culture of our field with reality?
To paraphrase Lenin,
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What is to be done?
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Should we retreat into despair and cynicism?
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That is not the message of my talk.
To paraphrase Lenin,

What is to be done?

Should we retreat into despair and cynicism?

That is not the message of my talk.

There is a better answer to this conundrum.
There is a keyword that unlocks the puzzle!
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This keyword is revealed if we look for guidance in the wisdom of ancient India.
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In Chapter 13, Verses 8-12 of the Bhagavad Gita
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In Chapter 13, Verses 8-12 of the Bhagavad Gita, the all-knowing one, Sri Krishna, describes the qualities that are necessary for knowledge.
Here is the list, first in Sanskrit, then in transliteration, then in English.
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Note what the very first quality is.
7. Humility; unpretentiousness; non-violence; forbearance; uprighteness; service of the teacher; purity; steadiness; self-control;
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and if He were to glance at some of the boastful introductions to articles on eprint.iacr.org, at Krawczyk’s HMQV paper, at Boyen’s *Pairings-2008* paper, and at the preface to the book of Katz and Lindell,
…He would conclude that what the cryptographic research community badly needs is a healthy dose of
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*Amaanitvam.*