Risks of lattice KEMs

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Abstract. Lattice-based KEMs under consideration within the NIST Post-Quantum Cryptography Standardization Project (NISTPQC) are much more risky than commonly acknowledged. In applications where performance constraints force the use of a lattice-based KEM, the least risky option available is NTRU Prime, specifically Streamlined NTRU Prime (sntrup) at the largest size that fits those performance constraints.

1 Introduction: Cryptanalytic overload

Years from now, when we're all looking back at today's NIST Post-Quantum Cryptography Standardization Project (NISTPQC) and asking "How did the U.S. government manage to standardize a post-quantum KEM that was then shown to be broken?", surely a large part of the answer is going to be that, even though we have *some* people publicly looking for attacks, the post-quantum attack surface is so much bigger than this that we ran out of time.

The broken standard is still a hypothetical scenario at this point. There is, however, already ample evidence of the cryptanalytic overload. Consider the following two examples:

- Round2, a "provably secure" lattice system submitted to round 1 of NIST-PQC in 2017, was broken by a very fast attack [39] from 2020 Bellare– Davis–Günther. The attack exploits the pattern of hash inputs inside the FO transform used inside Round2 to try to achieve CCA security; a mistake in these details can easily destroy all security.
- The official Frodo software from 2017—software that the submission [15, Section 3.1] claimed was "protected against timing and cache attacks"—was broken by a feasible timing attack [169] from 2020 Guo–Johansson–Nilsson. The attack exploits the fact that the software used memcmp, a subroutine well known to take variable time.

Why were these vulnerabilities not announced in 2017? 2018? 2019? These are not subtle mistakes. Presumably a large-scale attacker, having already hired and trained thousands of people to look for attacks, would have found these mistakes immediately after the submissions were posted in 2017—but there are far fewer people *publicly* working on attacks, and the attack surface is massive, so easy attacks took years to discover.

Some people will tell you something like this: "These two attacks are nothing to worry about. We're working on formal verification, which any day now is

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submission	NTRU		NTRU	Prime	SABER	Kyber	Frodo
KEM family	ntruhrss	-	sntrup	ntrulpr	saber	kyber	frodo
KEM	701	40961229	1277	1277	fire	1024	640
Basics							
pk+ct bytes	2276	3684	3914	3822	2784	3136	19336
ct bytes	1138	1842	1847	1975	1472	1568	9720
errors	Noisy	Noisy	Rounded	Rounded	Rounded	Noisy	Noisy
Q or P	Quotient	Quotient	Quotient	Product	Product	Product	Product
modulus	8192	4096	7879	7879	8192	3329	32768
dimension	700	1228	1277	1277	1024	1024	640
first 2-norm	21.6	22.6	22.2	20.7	39.2	32.0	52.4
second 2-norm	21.6	28.6	29.2	29.2	75.0	32.0	52.4
$\log_2(\text{Core-SVP})$	136	301	270	271	260	256	150
$\mathrm{ct/log_2}$ ratio	8.4	6.1	6.8	7.3	5.7	6.1	64.8
Known attack avenues not ruled out by theorems							
lattices	risk	risk	risk	risk	risk	risk	risk
derandomization				risk	risk	risk	risk
decryption failures					165	174	138
structured lattices	risk	risk	risk	risk	risk	risk	
cyclotomics	risk	risk			risk	risk	
reducibility	risk	risk			risk	risk	
quotients	risk	risk	risk				
extra samples				risk	risk	risk	risk
non-QROM FO	risk	risk	risk	risk	risk	risk	risk
non-QROM 2				risk	risk	risk	risk
Known patent threats							
patent 9094189				risk	risk	risk	
patent 9246675				risk	risk	risk	
Systemic risks							
PKE instability	2019.04			2017.12	2019.04		2019.04
instability	2019.04	2019.04	2019.04	2019.04	2019.04	2020.10	2019.04

Table 1.1. Risks of proposed lattice-based KEMs. Risks of implementations deviating from specifications and risks of side-channel attacks are not included. See Section 3 for definition of each row. Each column takes largest specified KEM size that fits public key plus ciphertext into 4KB, except that the **frodo** column allows 20KB.

going to magically eliminate all of the CCA problems and all of the software problems. This means that the security of the software will follow from the core mathematical security of the lattice PKEs inside these KEMs. What matters is that we understand this core mathematical security: we know how hard it is to break SVP, how hard it is to break Approximate-SVP, and how hard it is to break these lattice PKEs."

Unfortunately, no, we *don't* know how hard it is to break these core lattice problems. The following list of attacks published *after* the NISTPQC submission deadline shows how unstable the attack picture is for SVP, and for Approximate-SVP, and for these lattice PKEs:

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- 2018 Laarhoven–Mariano [218] saved "between a factor 20 to 40 in the time complexity for SVP".
- 2018 Bai–Stehlé–Wen [31] introduced a new variant of BKZ producing "bases of better quality" for the "same cost" of SVP.
- 2018 Aono–Nguyen–Shen [18] adapted "recent quantum tree algorithms" to enumeration.¹
- 2018 D'Anvers-Vercauteren-Verbauwhede showed [126] that "an attacker can significantly reduce the security of (Ring/Module)-LWE/LWR based schemes that have a relatively high failure rate" and [127] that for LAC-128 "the failure rate is 2⁴⁸ times bigger than estimated".
- 2018 Hamburg [174] pointed out that the first published "provably secure" Round5 design had disastrously high decryption-failure rate, 2^{-55} .
- 2019 Pellet-Mary–Hanrot–Stehlé [**311**] broke through the previously claimed $\exp(n^{1/2+o(1)})$ approximation-factor "barrier" for number-theoretic attacks against Ideal-SVP.
- 2019 Guo–Johansson–Yang [170] presented faster attacks against some systems that use error correction to (try to) reduce decryption failures. This paper violated the security claims of LAC.
- 2020 Dachman-Soled–Ducas–Gong–Rossi [128] presented slightly faster attacks against the constant-sum secrets used in three lattice submissions: LAC, NTRU, and Round5.
- 2020 Doulgerakis–Laarhoven–de Weger [136] presented "faster [sieving] methods" for SVP.
- 2020 Albrecht–Bai–Fouque–Kirchner–Stehlé–Wen [9] reduced the exponent of enumeration from $\approx 0.187\beta \log_2 \beta$ to $\approx 0.125\beta \log_2 \beta$. This improvement, in combination with [18], reduces the post-quantum security levels of a wide range of proposed lattice-based systems.²
- 2020 Albrecht–Bai–Li–Rowell [10] introduced a "practical and faster" enumeration algorithm "for reaching the same RHF in practical and cryptographic parameter ranges". This further reduces the post-quantum security levels of proposed lattice-based systems.
- 2020 Bernard–Roux-Langlois [40] improved the algorithm from [311], and showed experimentally that in small dimensions the improved algorithm reaches much better approximation factors.

¹ There is a misconception that enumeration is superseded by sieving for cryptographic sizes. In fact, because of [18], [9], and [10], the fastest quantum attacks known today use enumeration for all dimensions up to a cutoff *larger* than many of the KEMs proposed for deployment. The cutoff becomes even higher in metrics that account for communication cost, since sieving is much more memory-intensive than enumeration. See Appendix B.4.

² NIST's low-precision "categories" (see Appendix B) hide this security loss: they say that there is no change in "category" since known quantum speedups against lattice systems are still not as dramatic as the Grover speedup against AES. However, what matters for understanding the instability of the lattice attack picture is that lattice systems now have quantitatively lower post-quantum security levels than they did before, as a direct result of a dramatic improvement in enumeration speed.

- 2021 Bi–Lu–Luo–Wang–Zhang [76] introduced a hybrid dual attack that improves "the state-of-the-art cryptanalysis results by 2–14 bits, under the BKZ-core-SVP model".
- 2021 D'Anvers-Batsleer [125] improved the "state-of-the-art multitarget failure boosting attacks", showing that "the quantum security of Saber can theoretically be reduced from 172 bits to 145 bits in specific circumstances".
- 2021 May [257] improved combinatorial attacks from exponent 0.5 + o(1) to exponent 0.25 + o(1) in the case of ternary keys; 2021 van Hoof–Kirshanova– May [180] improved the exponents of quantum combinatorial attacks; 2021 Kirshanova–May [208] improved the o(1). These combinatorial attacks are now the state-of-the-art attacks against some pre-NISTPQC proposals of lattice-based PKEs, including a proposal standardized by IEEE.
- 2021 Chailloux–Loyer [99] improved quantum sieving exponents by 3%, from 0.2653...+o(1) to 0.257...+o(1). Previously 0.2653...+o(1) was believed to be optimal.³
- 2021 Heiser [176] introduced another quantum-sieving speedup that "affects the security of lattice-based encryption schemes, including NIST PQC Round 3 finalists", and conjectured that this speedup can be combined with [99].

1.2. Overconfidence. The impressive history of advances in lattice attacks is accompanied by an equally impressive history of displays of confidence that the advances had already reached their limits. For example, let's look at more of the history of number-theoretic attacks, specifically unit attacks against Ideal-SVP and their generalization to S-unit attacks:

- A breakthrough unit attack, combining a fast cyclotomic reduction algorithm by 2014 Campbell–Groves–Shepherd [96] with a fast quantum algorithm by Biasse–Song [78], extracts secret keys from the cyclotomic case (assuming "h⁺ = 1") of well-known cryptosystems introduced by Gentry [161], Smart– Vercauteren [339], Gentry–Halevi [162], and Garg–Gentry–Halevi [157].
- [123, Section 1] stated that "the above-described algorithms ... apply only to *principal* ideals" and described this as a "barrier". However, 2017 Cramer–Ducas–Wesolowski [124] showed, for cyclotomics, how to reach approximation factor $\exp(n^{1/2+o(1)})$ in polynomial time for *arbitrary* ideals.
- This approximation factor $\exp(n^{1/2+o(1)})$ was described in (for example) [307, PDF page 84], [306, minutes 61–62], [304, PDF page 75], and [309, minute 45] as a "barrier", a "natural barrier", and an "inherent barrier" for this line of work. However, as mentioned above, 2019 Pellet-Mary–Hanrot–Stehlé [311] broke through this "barrier". The algorithm of [311] reaches, for example, approximation factor just $\exp(n^{1/4+o(1)})$ in time $\exp(n^{1/2+o(1)})$, although it uses $\exp(n^{1+o(1)})$ precomputation.

³ Consider [207] saying that "one can view our lower bounds on sieving with nearest neighbor searching as a further motivation for most concrete parameter selection methods currently used in practice, which assume that the leading time complexity exponents 0.292 and 0.265 are the best an attacker can do".

- A model of concrete sizes of these attacks was presented in [138] and claimed to be "somewhat reassuring for NIST candidates"; [138] dismissed [311] as using "an exponential amount of precomputation". However, a recent talk given by Bernstein [60] introduced a variety of advances in S-unit attacks, including much faster precomputation; presented publicly verifiable (π -digit) experiments in various sizes that had been presented in [138], showing these attacks finding much shorter vectors than indicated in [138]; and conjectured subexponential scalability.
- The conjecture of subexponential scalability was disputed in [137], which explained that applying a "standard heuristic" to S-unit lattices produced the conclusion that the probability of success of the attack in [60] "would be *ridiculously* small", certainly no better than [311]. It's correct that this is a standard heuristic in the literature on lattice-based cryptography, but applying this heuristic to S-unit lattices is an error, and the conclusion of [137] is wrong. See [72].

Given how many "barriers" (and "lower bounds" and so on) have been broken, one is forced to conclude that the risk-assessment mechanisms used in lattice-based cryptography are deeply flawed.

1.3. Consequences for NISTPQC. NIST's official evaluation criteria—see Appendix A for (1) the full criteria and (2) comparison of KEMs under those criteria—state that "The security provided by a cryptographic scheme is the most important factor in the evaluation". The criteria recognize that a complicated, unstable, inadequately understood area of cryptography is a security risk:

As public-key cryptography tends to contain subtle mathematical structure, it is very important that the mathematical structure be well understood in order to have confidence in the security of a cryptosystem. To assess this, NIST will consider a variety of factors. All other things being equal, simple schemes tend to be better understood than complex ones. Likewise, schemes whose design principles can be related to an established body of relevant research tend to be better understood than schemes that are completely new, or schemes that were designed by repeatedly patching older schemes that were shown vulnerable to cryptanalysis.

Modern lattice-based cryptography is the result of many years of patching older schemes shown vulnerable to cryptanalysis. The most obvious patches are continued increases in lattice dimensions, reacting to demonstrations that older dimensions were too small to resist attack. Consider, e.g., the original NTRU proposal by 1996 Hoffstein–Pipher–Silverman estimating 2⁸⁰ security for 104byte public keys using lattice dimension 83 (see [177, Table 2, last column]). As another example, consider 2011 Lindner–Peikert [232, Section 1.1] stating that its system using key sizes of "400 kilobits" (with a public randomness source see Section 3.16) appeared to be "at least as secure as AES-128", and that using rings "we can immediately shrink the above key sizes by a factor of at least 200". Today it is well known that a 2-kilobit (256-byte) key in the ring version of the Lindner–Peikert system is not secure.

The NISTPQC submission deadline was in 2017, just six years after [232] not very long from a cryptanalyst's perspective, especially for a complex topic. Various submissions claimed that lattice attacks were "well studied", and tried to use the switch from original NTRU to Ring-LWE/Module-LWE to separate themselves from the break-and-patch history of the area. In fact, this switch had very little effect on the attacks in the literature; for a unified survey see [65, Section 6]. What had much more of an effect was the switch to much larger lattice dimensions—but lattice attacks then continued to advance, reducing security levels of all lattice submissions and breaking some lattice submissions outright.

The Kyber submission portrayed its security analysis as "conservative" [24, pages 20–21] and gave a five-step argument [24, Section 4.4] concluding that it "seems clear" that breaking kyber512, Core-SVP 2^{112} , is harder than brute-force AES-128 key search. In 2020, [54] disproved part of the argument and showed that recent attack advances had undermined the rest of the argument. The round-3 Kyber submission then presented new kyber512 security claims relying on

- replacing kyber512 with a patched cryptosystem [25, page 2] and
- presenting a new analysis [25, pages 25–27] estimating that attacks against the *new* round-3 kyber512 would use $2^{151.5}$ "gates", more than the estimated 2^{143} "gates" for AES-128 key search.

The Kyber presentation at the Third PQC Standardization Conference [26, video, 1:29–1:30] described the new $2^{151.5}$ estimate as "tentative", stating that we "need more research into this" and that there are "foreseeable improvements". A much broader range, from $2^{135.5}$ "gates" to $2^{165.5}$ "gates", appeared in [26] without the "tentative" label—but $2^{135.5}$ is not good enough⁴ if NIST requires at least 2^{143} "gates". "Conservative" in 2017; bleeding-edge patches in 2020.

When one asks for evidence that the security of the proposed lattice KEMs is "well studied", the typical response is to say that there is a long history of papers studying lattice security. For example, 2006 Silverman [337] claimed that SVP had already been "intensively studied for more than 100 years". Unfortunately, the papers on attack algorithms keep showing *losses* of lattice security. Lattice security is "well studied" in the same sense that RC4 is "well studied": yes, all of the papers [163], [210], [264], [151], [164], [150], [250], [263], [300], [301], [248], [249], [297], [295], [348], [6], [35], [79], [165], [202], [209], [243], [244], [255], [288], [298], [36], [254], [266], [296], [107], [108], [109], [267], [334], [358], [110], [111], [130], [172], [245], [274], [335], [344], [14], [112], [183], [234], [246], [242], [286], [173], [171], [184], [185], [276], [293], [326], [158], [186], [287], [327], [329], [347], [187], [188], [193], [328], [330], [353], [92],

⁴ NIST made the surprising and unexplained claim in December 2020 [269] that if the round-3 Kyber analysis is correct "then Kyber clearly meets the security categories defined in the CFP". Clarification questions [57] were ignored. NIST appears to be tweaking the boundaries of its "categories" to favor Kyber; see generally [53].

[132], [189], [192], [194], [294], [299], [100], [133], [102], [101], [103], [190] are studying the security of RC4, but this is fully explained by the fact that RC4 has many complicated weaknesses. Lattices also have many complicated weaknesses, and the list of recent attacks shows that the lattice attack picture has not stabilized.

1.4. Arguments to consider lattices despite the risks. Why are lattice-based cryptosystems being considered for standardization?

Answer #1 is pervasive advertising of the "provable security" of these systems. The reality, however, is that what has been proven by "provable security" is *not* security; it is something much weaker, controlling only certain corner risks and fundamentally not addressing the central risks in lattice-based cryptography. "Provable security" didn't stop any of the post-2017 advances in attacks, didn't save the broken systems, and won't stop further advances. See Section 5.

Answer #2 points to the ability of lattice-based cryptography to support fully homomorphic encryption, multilinear maps, etc. However:

- The concrete lattice-based KEMs proposed for standardization don't support any of these extra features.
- The recent attack literature shows that the security of systems with these features is even more poorly understood than the security of KEMs designed purely for IND-CCA2 security.

NIST says that being able to modify a scheme for extra functionality is good; however, users of broken lattice KEMs will not appreciate hearing that ample evidence of risks was submerged under advertisements of extra functionality of different lattice systems with even higher risks.

A better answer is that some applications seem⁵ to be saying things like this: "I can only give you one kilobyte in my protocol for a ciphertext and one kilobyte for a public key. That's it." Frodo doesn't fit into 1KB. Classic McEliece ciphertexts fit very easily into 1KB, but the public keys are much larger. SIKE ciphertexts *and* public keys fit easily into 1KB, but Google refused to deploy SIKE because of SIKE's consumption of CPU time.⁶ For applications with such performance constraints, one has to use a small lattice-based KEM. This creates a challenging question of how to manage the risks.

2 NTRU Prime: reducing attack surface at low cost

Let's assume that the application's performance requirements force the use of a small lattice system. Subject to this requirement, NTRU Prime is the only

⁵ For example, Google [225] reported TLS interoperability problems with 4KB keys, although it's far from clear that this is a long-term issue. Experiments with 3300-byte keys were successful.

⁶ This was reported in [217, page 26], regarding a joint Cloudflare-Google experiment with post-quantum cryptography: "Note: SIKE not cleared for Google servers due to DoS risk."

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Fig. 2.1. Predicting post-quantum disasters. See Section 1 for a review of evidence that lattice-based cryptography is on this path, including examples of broken "provably secure" lattice submissions.

submission systematically designed to eliminate unnecessary complications in security review: eliminate decryption failures, eliminate cyclotomics, etc.

Does this help? At this point there is ample evidence that the answer is yes. Subsequent advances in lattice attacks fall into two classes: attacks that work against *all* small lattice systems, including NTRU Prime; and attacks that work against only *some* lattice systems, *not* including NTRU Prime—because NTRU Prime had already eliminated the tools used in those attacks. The path to disaster in Figure 2.1 is clear, and NTRU Prime has already demonstrated success at stopping this path at its first step.

2.2. Case study: decryption failures. There have been many advances in decryption-failure attacks, including recent advances listed in Section 1 that have broken the security claims for various NISTPQC submissions. For the schemes that haven't (yet?) been broken by decryption failures, there are more and more pages of increasingly complicated analysis. Consider, e.g., [125] saying "We first improve the state-of-the-art multitarget decryption failure attack using a levelled approach", pointing out "three inaccuracies in the directional failure boosting calculation for the simplified scheme of [11]", having to do more work because "this traditional approach of calculating the directional failure boosting cost is not directly applicable to practical schemes such as Kyber and Saber due to compression of the ciphertexts", etc.

Note that compression making an attack *analysis* difficult does not imply that it makes the *attack* difficult. One is reminded of a famous quote from Hoare:

There are two ways of constructing a software design: One way is to make it so simple that there are obviously no deficiencies, and the other way is to make it so complicated that there are no obvious deficiencies.

Hoare's first way prioritizes minimizing the complexity of a thorough review.

The introduction of NTRU Prime in 2014 [44] had already noted that the problem of "figuring out whether an attacker can trigger decryption failures" was

a "mess"—so it simply eliminated decryption failures. The proof that there are no failures takes just a few lines. A thorough review of the impact of decryption failures is vastly easier for NTRU Prime than for most lattice-based KEMs.

The introduction of ntruhrss in 2017 [181], like NTRU Prime, eliminated decryption failures; ntruhps switched to eliminating decryption failures when it merged with ntruhrss to form the round-2 NTRU submission. Kyber, SABER, and Frodo still have decryption failures, and this could damage their security; see Section 3.9.

2.3. Proactive vs. reactive. More broadly, the NTRU Prime approach is *proactively* looking at the attack surface, identifying attack tools that can be eliminated at the design stage, and eliminating them.

The alternative is reacting to systems getting broken: "That was broken, stop doing that". See Figure 2.4. History shows that this reactive approach is often triggered many years after the system was designed, because that's how long the public attack development took; see the next paragraph for an illustration of cryptanalytic time scales. It is important to realize that the system *was not secure* in the meantime: the public simply didn't know that it was insecure.

Consider the problem of key recovery for the DH cryptosystem using the group $\mathbf{F}_{2^n}^*$. Discrete-logarithm techniques that were already standard in the 1960s [355] take time $\exp(n^{1/2+o(1)})$; these are based on techniques introduced in the 1920s by Kraitchik [213], relying on how frequently integers are "smooth". A sudden breakthrough by 1984 Coppersmith [120] reduced 1/2 to 1/3. The cryptosystem can survive the attack of [120] by moving to somewhat larger n, but decades later this discrete-logarithm problem was publicly smashed by a new wave of papers, culminating in a devastating quasi-polynomial-time attack by 2014 Barbulescu–Gaudry–Joux–Thomé [33]. See the recent survey [167] for further history and a review of the attack tools, notably subfields and automorphisms.

Was the system secure before it was *publicly shown* to be insecure? No. Is it reasonable to extrapolate from the lengthy public development that *attackers didn't know the attack* until 2014? No. Consider the fact that the Institute for Defense Analyses, an NSA consulting company, many years ago hired Buhler, one of the original developers of the number-field sieve [94] for integer factorization; Gordon, the first developer of a discrete-logarithm version [166] of the numberfield sieve; Miller, who as part of introducing ECC [261] was one of the first authors to probe the limits of discrete-logarithm algorithms; and Coppersmith. Much less data is available regarding the cryptanalytic capabilities of, e.g., the Chinese government. Surely large-scale attackers know many more attacks than this public does. A reactive approach is inherently incapable of protecting against these attacks, whereas a proactive approach has a chance.

2.5. Case study: cyclotomics. As a historical matter, most proposals of small lattice-based KEMs have used cyclotomic fields. These fields provide tools to the attacker that most number fields do not provide, such as subfields and automorphisms. 2013 Bernstein [43] noted the potential value of these subfields and automorphisms for finding, among other things, "short generators of ideals"



Fig. 2.4. Proactively handling advances in attacks vs. reactively handling advances in attacks.

and attacking "NTRU, Ring-LWE, FHE", and concluded that NTRU "should switch to random prime-degree extensions with big Galois groups".

Prime-degree fields are guaranteed to have no proper subfields other than **Q**. Fields with "big Galois groups" are far from having automorphisms. Notice the proactive approach, taking tools away from the attacker without waiting for attacks to be developed. As an analogy, very similar recommendations to avoid subfields and automorphisms in discrete-log cryptography were published in [29] and [42] several years *before* quasi-polynomial-time discrete-log attacks were published using these attack tools.

A complete NTRU Prime cryptosystem was introduced in 2014 [44], as noted above. This cryptosystem uses specifically the field $\mathbf{Q}[x]/(x^p - x - 1)$. This might seem similar at first glance to the cyclotomic field $\mathbf{Q}[x]/((x^p-1)/(x-1))$ used in NTRU, but two centuries of development of algebraic number theory—see Section 2.6—have established vast mathematical gaps between these fields. For example:

- The Galois group of $\mathbf{Q}[x]/(x^p x 1)$ has size $p! \approx (p/e)^p$, the maximum possible size for a field of degree p.
- The Galois group of $\mathbf{Q}[x]/((x^p-1)/(x-1))$ has size p-1, the minimum possible size for a field of degree p-1.

Along with this cryptosystem, [44] introduced a "subfield-logarithm attack", improving on previous attacks in some cases. Later in 2014, Campbell–Groves– Shepherd [96] introduced a much faster attack in the case of cyclotomic fields. Followups broke various "barriers" claimed for this line of work; see Section 1.2. These breaks have taken more and more advantage of subfields, automorphisms, and further cyclotomic structure. See also the papers [37], [229], and [77] further developing subfield-logarithm attacks for various fields.

NIST has repeatedly⁷ misstated the history here, in particular by describing NTRU Prime as a reaction to the attack of [96]. This description doesn't just get the order of events wrong; it actively hides (1) the cryptographer's ability to *proactively* protect against risks and (2) NTRU Prime's demonstrated success in doing this.

The situation since 2014 has been that some lattice-based cryptosystems are known to be broken for cyclotomic fields and *not* known to be broken for the NTRU Prime field, while there is nothing the other way around. (There are, of course, also some lattice-based cryptosystems known to be broken independently of the field choice, and some lattice-based cryptosystems not known to be broken either way.) The broken cyclotomic systems are clear evidence of a cyclotomic risk, and the tools used in the known attacks provide a clear explanation of where this risk is coming from. The risk-management conclusion is also clear: Stay away from cyclotomics.

2.6. Algorithms in algebraic number theory. Further notes are required on the mathematical context for NTRU Prime.

Algebraic number theory, the study of number fields, has been one of the primary research areas in number theory over the past two centuries. "A course in computational algebraic number theory", a well-known 1993 textbook [118] from Cohen (see also [119]), identifies five "main computational tasks of algebraic number theory", where three of them are computing the "unit group", computing the "class group", and computing generators of principal ideals. The book spends many pages describing sophisticated algorithms to compute unit groups, class groups, and generators; these topics are related to each other, and to the S-unit attacks listed in Section 1.2.

These computations have a long history before [118]: see, e.g., the systematic tables of number fields surveyed in [118, Appendix B]. Almost all of the number fields covered by these tables, and by the book, are non-cyclotomic. This does not mean that cyclotomic fields play such a minuscule part in the literature as a whole: on the contrary, there is an equally long history of algebraic number theorists exploring the special structure of cyclotomic fields (see, e.g., [352]) and exploiting this structure to efficiently carry out useful computations that nobody knows how to perform efficiently for general number fields.

An example of cyclotomic structure is the ability to instantaneously write down generators for the group of "cyclotomic units". These generators have two properties that are important for known attacks:

• The generators are *short*. See [72, Section 8] for quantification.

⁷ Examples: [8] incorrectly claims that NTRU Prime's "abandonment" of cyclotomics was "motivated by recent progress in quantum algorithms for finding short vectors in principal ideal lattices with a guaranteed short generator"; and [20, "Algebraic attacks on cyclotomics?"] states a "Long history" incorrectly claiming that, *after* discussions in 2015 of [96], NTRU Prime's field was proposed "as a defense against certain conjectured advances in this line of quantum algebraic cryptanalysis".

• The group of cyclotomic units is a *finite-index* subgroup of the unit group. Computations show that the index is generally very small, making the full unit group easy to compute via the known technique of "saturation".

For example, the cyclotomic-unit lattice is the entire unit lattice for the 512th and 701st cyclotomic fields, and has index just 3 for the 1229th cyclotomic field. See [352, page 421].

Efficiently finding short generators of a finite-index subgroup of the unit group for *general* number fields is an unsolved problem. The sophisticated unit-group algorithms in, e.g., [118] have much worse than polynomial scaling. At first glance a quantum computer might seem to help, since the Eisenträger–Hallgren– Kitaev–Song quantum algorithm [144] computes unit groups in polynomial time, but the resulting generators are not short.

For number theorists, it is not surprising that the extensive literature on faster computations for cyclotomic fields is reflected in recent literature demonstrating faster attacks against various cryptographic problems using cyclotomic fields. This is not because other fields have been ignored; it is because cyclotomics provide tremendously helpful tools for the algorithm designer.

Some people will tell you that these speedups for cyclotomics are a reason to *use* cyclotomics, rather than a reason to *avoid* cyclotomics. This is like

- saying that for a stream cipher one should select RC4 rather than ChaCha20 since, as noted above, there are many years of papers doing more and more damage to RC4's security; or
- saying that within discrete-logarithm cryptography one should use smallcharacteristic multiplicative groups rather than large-characteristic ellipticcurve groups.

History shows how dangerous it is to assume that every insecure cryptographic system will be immediately identified as insecure. Sometimes a security failure is recognized only as the culmination of a long series of papers on attack speedups. Proactive risk management requires recognizing the attack speedups as an alarm bell, not treating these speedups as something to be embraced.

3 The risk table

Table 1.1 summarizes the risks in the NISTPQC lattice KEMs. This section defines each row in the table.

3.1. Basics, part 1: "pk+ct bytes" and "ct bytes". The "ct" row states the number of bytes used by a ciphertext, while the "pk+ct" row states the total number of bytes used by a public key and a ciphertext.

The motivation for considering "pk+ct" is that this is the data communicated if a key is sent to one user, used for one ciphertext, and then discarded. The motivation for considering "ct" is that this is the data communicated if a key is sent to many users through a lower-cost broadcast channel and then used for many ciphertexts. For reasons to think that "ct" is more important than "pk+ct", see [258] and [50, Section 2.3]. It is, for example, not difficult for a server to broadcast keys through the Internet's Domain Name System, which has automatic local caching of data by Internet service providers. A client then retrieves keys locally, but still has to send a ciphertext all the way back to the server. See [51].

As noted in the table caption, each column takes the largest specified KEM size that fits pk+ct into 4KB, except that the frodo column allows 20KB. The "pk+ct" and "ct" rows for frodo are marked in red. The reason for this exception is that no specified frodo size fits into 4KB.

3.2. Basics, part 2: "errors" and "Q or P". This refers to the top-level classification of NTRU variants given in [69] and [49, Section 8].

A public key reveals a "multiplier" G and an approximation A = aG + e to the multiple aG. The "Quotient" NTRU systems take A = 0, so G is the quotient -e/a. The "Product" NTRU systems instead take random G (or something that one hopes looks sufficiently random; see Section 3.16). A ciphertext reveals an approximation B = Gb + d to Gb, and for the Product NTRU systems also reveals an approximation C = Ab + M + c to Ab + M.

"Errors" refers to how the small secrets a, b, c, d, e are chosen. In all cases a, b are chosen randomly. "Noisy" NTRU means that c, d, e are also chosen randomly. "Rounded" NTRU obtains approximations, except for the A = 0 approximation in Quotient NTRU, by rounding: d is determined by rounding Gb to B; e for Product NTRU is determined by rounding aG to A; c for Product NTRU is determined by rounding Ab + M to C.

3.3. Basics, part 3: "modulus", "dimension", first 2-norm", "second 2-norm". These are *some* quantitative features of the lattice problems that appear, meant solely to give a flavor of the variations. See [49, pages 48–52] for five tables stating the full problems for all round-2 lattice submissions. Beware that those do not all match the problems in round-3 lattice submissions.

The set of multipliers G can always be written in the form $((\mathbf{Z}/q)[x]/F)^{k \times k}$ for a monic polynomial $F \in \mathbf{Z}[x]$. "Modulus" refers to q, and "dimension" refers to $k \deg F$. For NTRU, some computations are carried out modulo $x^p - 1$ while others are carried out modulo $(x^p - 1)/(x - 1)$; for purposes of this document, F is $(x^p - 1)/(x - 1)$.

Security analyses of the difficulty of determining b, d from G and B = Gb + d are influenced not just by the modulus and dimension but also by the size of the secrets b and d. Similar comments apply to other secrets. The secrets are sometimes drawn from two different distributions: e.g., an explicit distribution of b and an implicit distribution of d from rounding. "First 2-norm" and "second 2-norm" refer to the typical Euclidean norms of the two secrets, sorted into non-decreasing order.

The numbers in the table show that submissions vary in the balance that they choose between these parameters. Some submissions take somewhat larger dimensions, for example, while others take somewhat larger secrets. Quantitative attack improvements could change the optimal balance in either direction. **3.4.** Basics, part 4: "log₂(Core-SVP)", "ct/log₂ ratio". "Core-SVP" is a particular mechanism of assigning a (pre-quantum) "security level" to each lattice system, specifically the " 0.292β " column defined in [12]. The motivation for using Core-SVP is that NIST has made comments such as "we feel that the CoreSVP metric does indicate which lattice schemes are being more and less aggressive in setting their parameters" and has repeatedly criticized submissions that used other metrics.⁸

It is important to realize that Core-SVP is a combination of underestimates, overestimates, possible underestimates, and possible overestimates, making its relationship to the actual cost of attacks highly unclear—even within the limited scope of attacks that it considers, ignoring the fact that attacks are advancing. See [65, Section 6] for a detailed review of (1) the attacks covered, (2) how Core-SVP estimates the cost of those attacks, and (3) many open questions regarding the actual costs of those attacks.

The "ct/log₂ ratio" column divides the number of ciphertext bytes by the \log_2 of Core-SVP. The motivation for computing this quotient is that sizes of optimized lattice systems are *roughly* linear in \log_2 (Core-SVP).

The Core-SVP column is marked in red when it is below 2^{256} . The ratio column is marked in red when it is above 16, corresponding to Core-SVP 2^{256} requiring ciphertext size (without key size) above 4KB.

3.5. What qualifies as a "known attack avenue"? NIST has stated [271] that it is "open to the possibility" that there is an attack against cyclotomics, and that it is also "open to the possibility" that there is an attack against $x^p - x - 1$. Why, then, does Table 1.1 have a row for "cyclotomics" and not a row for " $x^p - x - 1$ "? More generally, if there's a split of systems, some systems having feature X and some systems having feature Y, then shouldn't there be a row for the possibility of X being a problem, and a separate row for the possibility of Y being a problem?

To see that this is not the right rule, consider the following special case of the rule: an analysis of SARS-CoV-2 transmission risks via different systems for faculty meetings should consider

- the possibility of transmission via aerosols and
- the possibility of transmission via Zoom.

⁸ However, NIST has not criticized the round-3 Kyber submission for switching to a different definition. The metric in [12] measures the LWE problem for KEMs that are based on LWE, Ring-LWE, and Module-LWE, while it measures the LWR problem for KEMs that are based on LWR, Ring-LWR, and Module-LWR. The new metric in [25, Table 4] measures a mixed LWE/LWR problem, even though the Kyber submission continues to state [25, Sections 1 and 4.4] that it is "based on the hardness of ... MLWE" and that its security claims are "based on the cost estimates of the best known attacks against the MLWE problem underlying Kyber". For clarity, this document uses "Core-SVP" for the metric in [12] and "revised-Core-SVP" for the metric in [25]. The new round-3 version of Kyber-512 has Core-SVP 2¹¹² and revised-Core-SVP 2¹¹⁸.

The first possibility is a legitimate subject of risk analysis: there is an explanation of a *mechanism* by which SARS-CoV-2 could transmit via aerosols (see, e.g., [351]), and one can scientifically study this mechanism to understand the risk better. There is no explanation of any such mechanism for Zoom.

There has been a long history of attacks damaging the security of latticebased cryptography; see, e.g., many examples listed in Section 1. The tools used in these attacks are mechanisms that could enable further attacks doing even more damage. The attack avenues listed in Table 1.1 are a categorization of known attack tools. Every tool known for attacking $x^p - x - 1$ also applies to cyclotomics, while the opposite is not true; this is why there is a "cyclotomic" row and not an " $x^p - x - 1$ " row. (Some literature claims that cyclotomics are "uniquely protected" against a particular attack strategy, but this claim is false; see Section 5.6.)

As an example unrelated to cyclotomics, one could consider the *possibility* of "Noisy" being broken while "Rounded" survives, and the *possibility* of "Rounded" being broken while "Noisy" survives. (Theorems stating that a "Rounded" attack implies a "Noisy" attack are too weak to apply to these KEMs; see Section 5.3.) But what's the *mechanism* that threatens one and not the other? The lack of an answer to this question is why "Noisy" and "Rounded" are not listed as attack avenues separately from "lattices".

Similarly, one could consider the rank-2/3/4 module systems being broken while the ring-based systems survive, or vice versa. (Theorems stating that a ring attack implies a module attack are too weak to apply to these KEMs; see Section 5.3. Theorems stating that a module attack implies a ring attack are also too weak to apply to these KEMs; see Section 5.4.) But there's again no explanation of the *mechanism* that threatens one and not the other. This is why "rings" and "modules" are not listed as attack avenues separately from "structured lattices".

One can also generically view any attack that quantitatively benefits from lower dimensions as a reason to prefer higher dimensions, or generically view any attack that quantitatively benefits from smaller secrets as a reason to prefer larger secrets. However, these attacks do not have sharp cutoffs at any particular dimension or any particular size of secret, and there is no explanation of a mechanism that could create such cutoffs. (Some literature, notably [16], claims that hybrid combinatorial-lattice attacks are "induced by the fact that secrets are ternary", but this is another false claim; see [55].) This is why "lower dimension" and "smaller secret" are not listed as attack avenues separately from "structured lattices".

3.6. Implementation attacks are not included. Table 1.1 does not cover attacks that depend on the implementation being attacked: for example, timing attacks, and attacks exploiting bugs. This exclusion should *not* be taken as a statement that these attacks are unimportant. On the contrary:

• The timing attack from [169] was a disaster for the Frodo software. Other submissions, such as NTRU Prime, already have software written within frameworks that verify immunity to timing attacks; this is a clear advantage.

- Other NISTPQC software implementations have been broken because of incorrect generation of random objects. See, e.g., [235] and [316]. The use of rounding in NTRU Prime and in SABER eliminates some (not all!) of these random objects.
- Optimizations are a major driver of cryptographic software complexity and bugs. Many vectorized subroutines in the NTRU Prime software have already been computer-verified to match the reference subroutines on all inputs. See [61].

The reason for this exclusion is that merely *assessing* implementation risks is a major research project that has not been completed. The risks in Table 1.1 are thus limited to risks that are intrinsic in the KEM specifications, risks that cannot be addressed by changes in implementations.

One can argue that implementation risks are less important than specification risks in the long run. It is clearly feasible to eliminate timing attacks with current technology. One *hopes* that research into high-assurance cryptographic software will eliminate KEM bugs within a few years. It is more difficult to stop other sidechannel attacks such as electromagnetic attacks (see Section 7.10), but one can still hope for safe deployments on devices kept far enough away from attackers.

3.7. Known attack avenue: "lattices". All of these KEMs are broken if one finds a sufficiently short nonzero vector in a given lattice. Note that "short" does not mean specifically "shortest". This row is for attack tools that seem to apply to all lattices. Half of the recent attacks listed in Section 1, such as the sieving advances, are of this type. The risk is that lattices in general continue to lose security.

3.8. Known attack avenue: "derandomization". This row marks systems where the underlying PKE is randomized: PKE decryption recovers only part of the randomness used to produce a ciphertext. As part of the "CCA transform" used to try to protect against chosen-ciphertext attacks, the KEM derandomizes the PKE, choosing the random coins in the PKE as deterministic functions of the PKE input.

Incorrectly ignoring proof looseness (see Section 5.8) leads to the unjustified conclusion that derandomization cannot lose security. Fixing this error shows that there is a *possibility* of losing security. This, by itself, would still not qualify derandomization for listing in Table 1.1. However, Bernstein [59] recently found examples of randomized PKEs that have attacks exploiting derandomization: the KEM is approximately q times easier to break than the PKE. Here q is the number of hash queries, around 2^{100} for a large-scale attacker today.

These attacks rely on a partial information leak from the PKEs. The core risk here is that algorithms can similarly extract partial information from lattice PKEs. This risk is structurally avoided by the KEMs using deterministic PKEs.

3.9. Known attack avenue: "decryption failures". This row marks KEMs where the underlying PKE does not always succeed in decrypting the ciphertexts that it produced. The number listed in the row is λ when decryption is claimed

to fail with probability at most $1/2^{\lambda}$. The number is marked in red when λ is below $\log_2(\text{Core-SVP})$.

There is a common perception that decryption failures are not an issue if they are rare enough that *legitimate users* will not encounter them. However, as noted in [49, Section 5], attackers can search offline for decryption failures. The core algorithmic question is how reliably attackers can *recognize* decryption failures offline (based on, e.g., the magnitude and pattern of errors in each ciphertext), not sending most of the ciphertexts to the legitimate user. Proofs do not address this risk; see Section 5.7.

Some of the recent attacks listed in Section 1 are decryption-failure attacks, arising in some cases from λ having been calculated incorrectly (which is not listed here as a separate risk) and in some cases from the impact of decryption failures having been underestimated. The risk is that these attacks continue to advance. This risk is structurally avoided by systems without decryption failures.

3.10. Known attack avenue: "structured lattices". Recall that the set of multipliers G can always be written in the form $((\mathbf{Z}/q)[x]/F)^{k \times k}$ for a monic polynomial $F \in \mathbf{Z}[x]$. This row marks systems where deg F > 1.

Section 1.2 listed various claimed "barriers" broken by S-unit attacks. These attacks exploit structured lattices. There is a risk that the latest "barriers" claimed between these attacks and Ring-LWE/Module-LWE will also fall. The general risk of structured lattices is highlighted in the Frodo submission [15, Section 1.2.1]:

Given the unpredictable long-term outlook for algebraically structured lattices, and because any post-quantum standard should remain secure for decades into the future—including against new quantum attacks—we have based our proposal on the algebraically unstructured, plain LWE problem with conservative parameterizations (see Section 1.2.2).

The risk is structurally avoided by the Frodo design.

3.11. Known attack avenue: "cyclotomics". Within "structured lattices", this row marks lattices where the underlying number field is a cyclotomic field. In the case of SABER and Kyber, the field is defined by the cyclotomic polynomial $x^{256} + 1$. In the case of NTRU, the field is defined by the cyclotomic polynomial $(x^p - 1)/(x - 1)$ where p is, e.g., 701 or 1229.

Cyclotomic structure is exploited in some of the number-theoretic attacks listed above; see Section 2.6. The risk is that cyclotomic structure enables further attacks. This risk is structurally avoided by Frodo, which does not use number fields in the first place, and by NTRU Prime, which uses non-cyclotomic fields.

3.12. Known attack avenue: "reducibility". This row is marked when $(\mathbf{Z}/q)[x]/F$ is not a field; in other words, when there are ring morphisms from $(\mathbf{Z}/q)[x]/F$ to smaller nonzero rings. There is, for example, a ring morphism from $(\mathbf{Z}/8192)[x]/((x^{701}-1)/(x-1))$ to $(\mathbf{Z}/2)[x]/((x^{701}-1)/(x-1))$, and there is a ring morphism from $(\mathbf{Z}/3329)[x]/(x^{256}+1)$ to $(\mathbf{Z}/3329)[x]/(x^2+17)$.

Such morphisms are exploited in the attacks of [338], [145], [146], [105], and [106]; the attacker applies the ring morphisms to public ring elements. Some of the attacks are on cases known to be breakable in other ways, but [106] breaks some prime-cyclotomic cases with larger error distributions than have been attacked in any other way. So far the attacks have not affected any NISTPQC KEMs, but the risk here is that the attacks are further extended. Note that this is not the same as the cyclotomic risk, although it is incurred by the same KEMs in Table 1.1.

"Modulus switching" relates some problems in $(\mathbf{Z}/q)[x]/F$ to problems in $(\mathbf{Z}/r)[x]/F$ for a replacement modulus r (although it adds considerable noise, as noted in [69, page 37]). Every polynomial F is reducible modulo various r, so attacks of this type are included under the general "structured lattices" risk.

3.13. Known attack avenue: "quotients". The problem of finding a, e given G and A = aG + e could be easier in the homogeneous case that A = 0, the problem of recovering a, e given the quotient G = -e/a.

The analyses of [206] and [139] conclude that standard BKZ attacks for the homogeneous case do better than the standard analysis indicates if the modulus q is large, specifically above $0.004n^{2.484}$ where n is the dimension. Both papers also claimed that attacks of this type exist only for the homogeneous case,⁹ but this was disproven by [65, Section 6.3]. The analysis of the known attacks against the inhomogeneous case is an open question; it could be that the standard analysis is wrong in all cases.

Given the lack of knowledge regarding the performance of known attacks, this row in Table 1.1 presumes that there is a security loss specific to the homogeneous case. Accordingly, this row marks the Quotient NTRU systems. The core risk is that the security loss can be pushed to smaller values of q, small enough to apply to NISTPQC submissions.

3.14. Known attack avenue: "extra samples". This row is marked for systems that release more than n "samples", in the terminology typically used for dimension-n LWE. These are exactly the Product NTRU KEMs.

For each error distribution, known attacks suddenly break all of the lattice problems considered here once enough samples are released to the attacker. See, e.g., [134], [22], and [11]. This row covers the risk of attack advances reducing the cutoff for the required number of samples.

One can also consider the risk that an attack already starts working at n samples, affecting all of the KEMs in the table. This is already covered by the general "lattices" risk and is not covered in this row. As an analogy, one can

⁹ For example, [206, Section 7] claimed that an "important difference between NTRU and Ring-LWE instances is the fact that in NTRU lattices, there exists many short vectors". NIST [7] then stated a confused version of the same claim, namely that "the NTRU construction produces a lattice that has somewhat more structure than in similarly efficient RLWE and MLWE cryptosystems (due to having shorter than expected vectors)" and similarly that sntrup uses "a lattice with unusually short vectors". Note the difference between "many short vectors"—something that [206] emphasizes as critical—and "shorter than expected vectors"—which is simply false.

also consider ways that an improved attack against Quotient NTRU could be accompanied by an improved attack against Product NTRU; this is covered in the general "lattices" row, not the "quotients" row.

3.15. Known attack avenue: "non-QROM FO". All of these KEMs use variants of the Fujisaki–Okamoto transformation [153] as CCA conversions. These variants use a hash function. It's trivial to write down hash functions that eliminate all KEM security even if everything else is unbroken; the risk in this row is that the selected hash function also loses security.

There is a misconception that hash-function weaknesses are outside the scope of KEM evaluation as long as KEMs use standard hash functions. The problem here is that there is a gap between

- the security properties for which standard hash functions were reviewed (e.g., preimage resistance) and
- the security properties that these KEMs require from the hash functions (this is more difficult to write down—essentially, one has to state what security means for the specific KEM at hand).

This gap needs to be addressed by further security analysis. Claims that standard hash functions are "indistinguishable from random" are ill-defined and are not a substitute for security analysis.

3.16. Known attack avenue: "non-QROM 2". The Product NTRU KEMs use, inside their PKEs, another hashing layer. The motivation for this layer is that a random multiplier G takes space in public keys, and having one standard G shared by everyone raises security concerns. These PKEs eliminate the sharing and almost all of the space issue by sending a short seed that is hashed to obtain a "random" G.

It's again trivial to write down hash functions that eliminate all security here. The risk in this row is that the selected hash function loses security. This is analogous to the previous row but requires a different security analysis.

NIST [8, page 17] criticized two of the three options for this hashing layer in the Round5 submission. NIST stated that "there is no proof of security" for those options. However, as in Section 3.15, there is *also* no proof of security for the use of standard hash functions in this context. Each combination of hash function and KEM raises a different security question.

3.17. Known patent threats: "patent 9094189" and "patent 9246675". Patent risks and attack risks involve different mechanisms, but patent risks are like attack risks in that they can easily trigger the worst-case scenario that the cryptographic user ends up being unprotected.

Another feature shared between patent risks and attack risks is that properly evaluating them requires expertise. For example, non-experts tend to think that a patent covers only what is literally included in the patent's claims; but the "doctrine of equivalents" states [346, page 732] that a patent "is not limited to its literal terms but instead embraces all equivalents to the claims described". The patent holder can show infringement of a patent claim under this doctrine

by showing for each element of the claim that "the accused product performs substantially the same function in substantially the same way with substantially the same result" [345, page 1312].

U.S. patent 9094189, expiring 2032, is on essentially the LPR cryptosystem, and was filed before the LPR cryptosystem was published. It was filed after *unstructured* lattice systems were published, so it cannot cover those systems. Given the doctrine of equivalents and other procedures used by patent courts, all efficient LPR derivatives using *structured* lattices are threatened; there is no reason to think that one can escape the doctrine of equivalents by switching from Ring-LWE to Ring-LWR or by switching to low-rank modules. The "patent 9094189" row in the table is marked for the Product NTRU systems that use structured lattices.

U.S. patent 9246675, expiring 2033, is on a variant of the LPR cryptosystem with compressed ciphertexts, and was filed years before a 2014 paper [302] that claimed novelty for compressing LPR ciphertexts. Given court procedures, all LPR variants using shorter ciphertexts than LPR are threatened, even if the compression mechanisms are not identical. The "patent 9246675" row in the table is marked for the Product NTRU systems that use structured lattices and have smaller ciphertexts than LPR.

For unclear reasons, NIST has discouraged public analysis of NISTPQC patent threats; see, e.g., [268] and [62]. NIST has not released its own patent analysis. In response to a FOIA request regarding NISTPQC patents, NIST refused to provide 3 documents totaling 31 pages; see [58]. For years NIST also carried out patent-buyout negotiations; the details are secret and appear to have failed.¹⁰

There has nevertheless been some public analysis, including a paper [241] claiming "non-applicability" of the first patent "to Kyber and Saber", and an accompanying summary [240] claiming that Kyber/SABER patent risks "should be ignored". The paper fails to recognize the definitions and procedures used in patent courts, such as Markman hearings and the doctrine of equivalents.

There was also some litigation, starting in 2017, against the European version of the first patent. The first round of litigation failed to kill the patent. An appeal also failed to kill the patent, and ended on 20 October 2021. See [147] for the list of litigation documents. For comparison, [310] claimed in 2020 that the patent was "likely" to be invalidated by prior art.

3.18. Systemic risks: "PKE instability" and "instability". These rows look at the history of the *family* of KEMs, rather than any specific KEM size. The "PKE instability" date is the publication date of the most recent change

¹⁰ See, e.g., [93] (another patent holder stating in June 2019 that "discussions started Friday"); [62] (quoting NIST email in December 2020 stating "We are working to clear up the IP situation, but it is a slow process"); [58] (quoting NIST document in January 2021 stating "We're still mostly waiting for them to give us a number"); and [270, video, minute 1:08-1:09] (NIST statement in June 2021 that "just the ballpark figures that were being mentioned made it clear that this wasn't something NIST was gonna be able to do. It was definitely more than our overall budget for our crypto group. We're continuing to talk to them").

in the family of underlying mathematical PKEs. The "instability" date is the publication date of the most recent change in the family of KEMs, including the PKEs, CCA conversions, and encoding details.

Instability contributes to attack risks because a moving target (1) adds to the overall reviewer load, (2) discourages researchers whose work on a previous version no longer applies to the current version, and (3) leaves a shorter period for analysis. "PKE instability" is separated from "instability" because most of the attack analysis has focused on the underlying PKEs.

Instability also contributes to general patent risks because a change could bring a system within scope of a patent filed in the meantime. Normally each patent application is published 18 months after filing (for the full picture see, e.g., [252]), so patent applications filed by April 2019 would normally have been online by October 2020; serious effort by NIST to organize labor-intensive searches and public analyses of patent applications might have completed within a year; but no such effort has been announced. Separating "PKE instability" from "instability" also makes sense in this context, since it allows separate analyses of PKE-related patents and CCA/encoding-related patents.

4 Conclusions from the risk table

Let's now use Table 1.1 to rationally select the least risky lattice KEM. Of course, if new events—for example, sudden success in patent buyouts—warrant updates in the table then the selection process should be revisited.

4.1. The size decision. Within each KEM family (for example, ntruhps), all available evidence is that the larger KEMs are harder to break. Perhaps a larger KEM also protects against future advances. One can construct arguments that a smaller KEM *might* do better; but a larger KEM is less risky. Consequently, the application should select the largest KEM that fits its performance constraints. The remaining question is simply which of the seven families to take.

4.2. The structured-lattice decision: frodo vs. everything else. The "ratio" row shows that frodo requires an order of magnitude more space to achieve any particular Core-SVP level than the other lattice KEMs. If the goal is to handle applications that require a small lattice system, as in the Google example from Section 1.4, then frodo simply isn't an option.

What happens if the importance of these applications has been overstated, and the applications of interest can actually afford 10KB ciphertexts?

These applications reach Core-SVP only 2^{150} with frodo, and much higher security margins with the other submissions. This includes ntruhrss, which has an ntruhrss1373 option that wasn't listed in the table because the ciphertexts are 2401 bytes. The structured-lattice submissions could specify parameter sets at much higher Core-SVP levels using 5KB or even 10KB ciphertexts. A closer look suggests that this would be problematic for kyber without some tweaks to the kyber structure (see Appendix A.3), but higher Core-SVP levels would be straightforward for, e.g., ntruhps.

Perhaps much larger security margins from other KEMs will be destroyed by advances in structured-lattice attacks. For comparison, the $\mathbf{F}_{2^n}^*$ discretelogarithm catastrophe mentioned in Section 2.3 was a quantitatively larger loss of security levels for that problem. On the other hand, selecting **frodo** would

- exacerbate the general lattice risks—half of the recent attack advances listed in Section 1—exactly because of the much lower security margin and
- incur a separate risk of losing all security if we're wrong about applications being able to afford 10KB.

Even if the devastating-structured-lattice-attack risk is assigned higher weight than the general-lattice-attack risk, it's hard to see how it can also be assigned higher weight than the application-can't-afford-this risk. This eliminates frodo.

Note that the decision-making process in this section is assuming that the goal is to select *one* of the lattice-KEM families. Selecting multiple families would reduce the application risk, although it would also increase systemic risks: for example, the risk of error increases when effort is spread across multiple families. On the other hand, this risk is lower for the two NTRU Prime families, sntrup and ntrulpr: there is extensive sharing of the work across these NTRU Prime options, notably in the parameter choices, security analysis, software, testing, and verification. Providing both options via NTRU Prime is simpler and less error-prone than gluing together two non-unified submissions. There is also some sharing of work between ntruhps and ntruhrss, and some of the software work has been shared between NTRU and NTRU Prime.

4.3. The Quotient-vs.-Product decision: ntruhrss and ntruhps vs. saber and kyber. These four systems share some risks. Beyond the shared risks, ntruhrss and ntruhps also incur the "quotients" risk, while saber and kyber also incur the "derandomization", "decryption failures", "extra samples", "non-QROM 2", "patent 9094189", and "patent 9246675" risks.

There is a myth that Product NTRU systems such as **saber** and **kyber** have been proven to be as secure as Quotient NTRU systems such as **ntruhrss** and **ntruhps**. Any such theorem would logically imply that the "derandomization", "decryption failures", "extra samples", and "non-QROM 2" risks cannot favor the Quotient NTRU systems. But there is no such theorem; the myth arises from an indefensible misrepresentation of known theorems. See Section 5.

There are generic arguments that attacks keep improving so the "quotients" risk should be taken seriously. For the same reasons, the "derandomization", "decryption failures", "extra samples", and "non-QROM 2" risks should be taken seriously. There are no obvious risk-management principles that would justify saying that the "quotients" risk outweighs the other four risks.

More importantly, the "quotients" risk does not outweigh the patent risks. As NIST put it in the call for NISTPQC submissions [279], it is "critical that this process leads to cryptographic standards that can be freely implemented in security technologies and products".¹¹ A patent threat doesn't need to be

¹¹ For comparison, [**323**] says "I fully trust that NISTPQC does exactly what it set out to do in 2016 and selects post-quantum cryptography algorithms primarily based

a guaranteed disaster to interfere with free implementability. This eliminates saber and kyber; it also eliminates ntrulpr, even though ntrulpr avoids some of the cryptanalytic risks.

4.4. The number-theoretic decision: ntruhrss and ntruhps vs. sntrup. Within the remaining three systems, there are four shared risks; ntruhrss and ntruhps also have the "cyclotomics" risk and the "reducibility" risk; sntrup has no extra risks. This eliminates ntruhrss and ntruhps, leaving sntrup as the least risky system.

This analysis did not account for the systemic risks of instability, but these are also in favor of sntrup. The PKE family inside sntrup was already published in 2016, years before the other submissions made changes to their PKE families. Looking beyond the PKE family at the entire KEM family shows changes in sntrup in 2019, but other submissions also changed their KEM families at that point. In short, sntrup provides the maximum stability of any of these KEM families.

4.5. Why did NIST downgrade NTRU Prime to alternate? NIST's procedures are not transparent, but a FOIA request led to the publication of [20], including the following statements:

[bullet item:] Among the remaining, structured lattice schemes, our assessment was that cyclotomics (esp power-of-2 cyclotomics) are the clear "community standard"

[bullet item:] So, we moved Kyber, Saber, NTRU on as Finalists, but kept NTRUprime too

NIST appears to have made its decision regarding NTRU Prime entirely upon this basis, never mind any of the other NTRU Prime features.

Procedurally, NIST appears to be requiring Kyber to be broken outright as a prerequisite for considering NTRU Prime, whereas it certainly isn't requiring NTRU Prime to be broken outright as a prerequisite for considering Kyber. There's extensive literature carrying out efficient computations for cyclotomics where the best known algorithms for most number fields are much slower; NIST doesn't treat this as an alarm bell regarding cyclotomics. This literature includes, much more recently, efficient breaks of famous cryptosystems using cyclotomics; NIST still doesn't treat this as an alarm bell regarding cyclotomics. Cyclotomic KEMs win by default because they're supposedly the "community standard".

Back in 2016, Google's NewHope experiment [224] triggered development and then submission of many NewHope variants to NISTPQC, all using cyclotomics by default. However, 41% of the round-1 lattice submissions provided options

on their technical and security merits (while of course also considering the advice from their own legal professionals.) This is what the security industry, other U.S. government departments, various military branches, international standardization bodies, etc expects it to do." The claim regarding NISTPQC is contradicted by [279], and the claim regarding expectations by "the security industry" etc. is contradicted by [256].

that do *not* use cyclotomics. This includes submissions from submission teams with many years of lattice publications, such as Frodo and Titanium. Most of these submissions paid heavily in performance for this choice. Most of these submissions expressed concerns regarding security. Most of these submissions provided *no* cyclotomic options. This is not the story of a community confident in the security of cyclotomics.

With this background in mind, let's think through what NIST's "community standard" statement from [20] is saying:

- The word "remaining" means that this statement isn't about, e.g., Titanium; NIST had already thrown Titanium away on the basis of performance.
- The word "structured" means that this statement isn't about Frodo.

The statement, by its own terms, is limited to a selection of just four submissions: NTRU, NTRU Prime, SABER, and Kyber. Yes, three of those use cyclotomics, but this is selection bias, not a "community standard". One could just as easily exclude any other targeted submission by selecting a distinguishing feature of that submission and claiming that the feature is not the "community standard". This is a completely unprincipled way to be making decisions. What we should all be doing instead is proactively minimizing risks.

5 No, theorems have not ruled out these risks

Let's look more closely at the pervasive advertising of the "provable security" of lattice KEMs. The main function of the advertisements is to make the reader believe that various lattice risks have been mathematically proven not to exist.

This section summarizes what has actually been proven regarding each topic. The main conclusion is that, because of important limitations in the theorems, the theorems *do not* rule out any of the risks listed in Table 1.1.

5.1. There is no proof that breaking newhope1024 or kyber768 is as hard as breaking ntruhrss701. Typical advertisement [140] making the reader think that there *is* a proof: "The preference for using Ring/Module-LWE is due to the fact that this problem is at least as hard as NTRU".

The reader is led to believe that KEMs based on Ring-LWE or Module-LWE, meaning Product NTRU KEMs such as newhope or kyber, have been proven to be at least as secure as KEMs based on NTRU, meaning Quotient NTRU KEMs such as ntruhrss. Perhaps there's an exception if the NTRU dimensions are larger than the Ring-LWE/Module-LWE dimensions, but surely the advertising means that newhope1024 and kyber1024 and kyber768 have been proven to be at least as secure as ntruhrss701. This, in turn, means that there cannot be a security reason to take ntruhrss701 rather than kyber1024; ergo, the extra risks of kyber1024 in Table 1.1 cannot exist.

However, what was actually proven (in [305, page 33]) is vastly weaker than this; is useless for comparing the KEMs submitted to NISTPQC; and does not rule out any of the risks in Table 1.1. Section 5.2 looks more closely at the proof.

5.2. What the "Ring-LWE is at least as hard as NTRU" proof actually says. Say one has an algorithm A that with high probability, say probability $1-\epsilon$, solves the search Ring-LWE problem with n samples and error distribution χ for the ring $R = (\mathbf{Z}/q)[x]/F$, where F has degree n. This means that one can find χ -distributed secrets b, d given a uniform random $G \in R$ and B = Gb + d, with b, d, G being chosen independently.

Let's use this algorithm to solve the problem of distinguishing a quotient e/afrom uniform, where e, a are χ' -distributed elements of R^* , the set of invertible elements of R. The distinguisher works as follows: choose b, d randomly from distribution χ , apply the algorithm A to inputs G = e/a and B = Gb + d, and see whether A returns (b, d). Notice that B = G(b + a) + d - e, so if there's a big overlap between the distributions of (b, d) and (b + a, d - e) then A can't tell whether it should be returning (b, d) or (b + a, d - e) in the overlap, so it will frequently fail, say with probability $\geq \delta$, where one can calculate δ from seeing how small χ' is related to χ . (One can also consider (b + ma, d - me) for other small ring elements m.) If, however, e/a is replaced by something uniform then, by assumption, A returns (b, d) with probability $1 - \epsilon$. If $\delta > \epsilon$ then this guarantees a distinguisher with probability at least $\delta - \epsilon$.

But what does this have to do with KEM security? The advertisement abuses naming to sweep this critical question under the rug. The reader is invited to confuse the first problem with the security of Ring-LWE-based KEMs, and to confuse the second problem with the security of NTRU-based KEMs. Instead of skipping the question, let's look at how little these problems have to do with the security of, say, newhope1024 and a hypothetical ntru1024 using the same ring $R = (\mathbf{Z}/12289)[x]/(x^{1024} + 1).$

Starting from the distribution χ in newhope1024 and the distribution χ' in ntru1024, the proof calculates some $\delta < 1$; the exact value of δ won't matter for the following analysis. The proof concludes that a solution to the *search* Ring-LWE problem with n samples and with success probability $1 - \epsilon > 1 - \delta$ implies a distinguisher between e/a and uniform with distinguishing probability at least $\delta - \epsilon$.

But where's the proof that a distinguisher for e/a, even a high-probability distinguisher, breaks Quotient NTRU? Sure, e/a is (modulo irrelevant details) the public key; so what? One of the useful features of Quotient NTRU is that the KEM ROM IND-CCA2 security analyses boil down to the simple question of whether the underlying PKE is *one-way*, meaning that a random plaintext is hard to find from a ciphertext and public key; one doesn't have to analyze the more subtle problems of ciphertext distinguishers or public-key distinguishers. See [49, Sections 6 and 7].

Furthermore, the proof that "Ring-LWE is at least as hard as NTRU" does *not* convert any of the following disaster possibilities for NewHope, Kyber, etc. into a distinguisher:

• There could be an *n*-sample search Ring-LWE attack that succeeds with probability $1 - \epsilon$ for some $\epsilon \geq \delta$. The proof says nothing in this case. This wouldn't be an issue if δ were *extremely* close to 1, but the newhope1024

distribution isn't wide enough to achieve that; also, Kyber uses narrower distributions than NewHope does, very close to NTRU's traditional ternary distributions.

- There could be a high-probability search Ring-LWE attack exploiting the extra samples released by NewHope, Kyber, etc. This is covered by the "extra samples" risk in Table 1.1. The proof says nothing about this—it assumes specifically an *n*-sample attack.
- There could be a KEM attack that uses a distinguisher for the underlying PKE to exploit the derandomization in NewHope, Kyber, etc. This is covered by the "derandomization" risk in Table 1.1, a risk that doesn't apply to Quotient NTRU; see also Section 5.8 below. The proof says nothing about this—it assumes a *search* Ring-LWE attack.
- There could be a KEM attack finding outputs of encapsulation that don't decrypt correctly in NewHope, Kyber, etc. This is the "decryption failures" risk in Table 1.1. The proof says nothing about this.
- There could be a loss of security from the hash function that's actually used to compute G in NewHope, Kyber, etc. This is the "QROM 2" risk in Table 1.1. The proof says nothing about this.

Structurally, the proof hypothesis considers just one type of attack, ignoring all of the above attack avenues. Meanwhile the proof conclusion isn't a successful attack against the Quotient NTRU KEMs. The advertising nevertheless leads readers to believe that the proof says that any attack against NewHope or Kyber implies an attack against Quotient NTRU KEMs.

5.3. Weaponization of selective proof exaggeration. If it is acceptable to disregard proof limitations then one can argue exactly the opposite position—namely, that one should prefer Quotient NTRU since Quotient NTRU has been proven to be at least as hard as Ring-LWE—by saying that the Stehlé–Steinfeld cryptosystem [341] is a Quotient NTRU cryptosystem having a proof that an attack implies a Ring-LWE attack. However, the limitations of *this* proof have been prominently advertised: most importantly, the e, a distributions in that cryptosystem are much bigger than in the Quotient NTRU KEMs in NISTPQC.

One can similarly argue that one should prefer rounding to noise as follows: disregard the proof limitations of [32, Theorem 3.2], claim that LWR has been proven to be at least as hard as LWE, claim that Ring-LWR has been proven to be at least as hard as Ring-LWE, etc. (Even better for fans of rounding, there don't seem to be any proofs pointing in the opposite direction.) But this proof is another case where the limitations have been prominently advertised.¹²

Why, within lattice-based cryptography, does common proof advertising

¹² As an example of limitations being ignored in *some* logically inapplicable proofs, consider [20], which claims that "security proofs generally assume Gaussian noise" as a reason for expressing concern regarding the security of fixed-weight noise, but does not point to the LWR-vs.-LWE proofs as a reason for expressing concern regarding the security of LWE. On the contrary, [20] says "No known reduction from LWE to LWR, but no known attacks on normal parameter ranges for LWR KEMS", so the reader is thinking about whether there are unknown attacks against LWR. Why

- exaggerate most of the proofs considered in this section, such as the "Ring-LWE is at least as hard as NTRU" proof reviewed above, but
- highlight the limitations of other proofs, such as the proofs from [341] and [32]?

Perhaps this inconsistency is related to the fact that the proof exaggerations all make Kyber look better, while ignoring the limitations of [341] and [32] would make Kyber look worse. In any case, whatever the intent might be, exaggeration of security proofs damages the risk-assessment process, and defenses need to be put into place to protect against this damage.

5.4. There is no proof that breaking kyber1024 is as hard as breaking newhope1024. Here [227] says that "Mod-SIVP can trivially be shown to be no easier than Id-SIVP" and that "M-LWE and M-SIS are obviously no easier than R-LWE and R-SIS". These refer to proofs that one can solve various dimension-1024 lattice problems for the ring $R = (\mathbf{Z}/q)[x]/(x^{1024}+1)$ if one can solve, e.g., dimension-4096 lattice problems for the module R^4 . But if another cryptosystem uses the module S^4 over the vastly smaller ring $S = (\mathbf{Z}/q)[x]/(x^{256}+1)$, then these proofs say merely that an S^4 attack implies an S attack—which

- is useless, since S is too small to be secure, and
- does not say that an S^4 attack implies an R attack.

Meanwhile proofs pointing in the opposite direction, such as "large modulus Ring-LWE \geq Module-LWE", have the "large modulus" limitation prominently displayed and are (correctly) not treated as reasons to prefer NewHope to Kyber.

A different proof works around the S-vs.-R size gap as follows: S is isomorphic to the subring $(\mathbb{Z}/q)[x^4]/(x^{1024}+1)$ of R. Then R is an S-module isomorphic to S^4 , and multiplication by an element of R is a restricted case of multiplication by a 4×4 matrix over S. However, [56] lists reasons that this still doesn't give a proof that kyber1024 is as hard to break as newhope1024. For example, the newhope1024 error distribution is much wider, adding up 16 bits modulo 12289 where kyber1024 adds up 4 bits modulo 3329. "Modulus switching" is too noisy to compensate for this.

5.5. There is no proof that breaking kyber1024 is as hard as breaking ntrulpr953. One can object to the newhope1024 comparison in Section 5.4 as a distraction from the comparisons that remain in NISTPQC. Surely what the proofs must mean is that module KEMs are at least as secure as ring KEMs *if* the error rate is at least as large, the dimension is at least as large, etc.: for example, the proofs must say that the module KEM kyber1024 is as hard to break as as the ring KEM ntrulpr953.

But, no, there's no hope of any such proof, even if the moduli are equalized. The proof mentioned above, exploiting a subring of $(\mathbf{Z}/q)[x]/(x^{1024}+1)$ that is isomorphic to $(\mathbf{Z}/q)[x]/(x^{256}+1)$, obviously doesn't apply to the ntrulpr953 ring $(\mathbf{Z}/q)[x]/(x^{953}-x-1)$, which is chosen to be a field.

does [20] not say "No known reduction from LWR to LWE, but no known attacks on normal parameter ranges for LWE KEMs"?

For a number theorist, the subrings of $(\mathbf{Z}/q)[x]/(x^{1024} + 1)$ are interesting attack tools to explore. As noted in Section 2.6, there is already a long literature on related computations exploiting these tools. It would be unsurprising for $(\mathbf{Z}/q)[x]/(x^{1024} + 1)$ to turn out to be weaker than $(\mathbf{Z}/q)[x]/(x^{953} - x - 1)$; and there certainly isn't a proof to the contrary.

As a direct result of this gap, a full proof that attacks against a rank-4 $(\mathbf{Z}/q)[x]/(x^{256} + 1)$ KEM imply attacks against a $(\mathbf{Z}/q)[x]/(x^{1024} + 1)$ KEM wouldn't be able to draw any conclusions regarding a $(\mathbf{Z}/q)[x]/(x^{953} - x - 1)$ KEM. Furthermore, the structure enabling such a proof, namely the relatively small ring $(\mathbf{Z}/q)[x]/(x^{256} + 1)$ (with even smaller subrings), raises a variety of security questions that the proof is incapable of addressing. This is reminiscent of the situation described in [44]: "The structures used in the 'proofs of security', such as automorphisms, are also some of the structures exploited in this attack."

Is it clear at this point that these attack tools *will* damage the security of kyber1024? No. But readers should not be deceived into believing that the use of modules in kyber1024 has been proven to not lose security.

5.6. Cyclotomics are not uniquely protected against reductions. The typical advertising here [303, page 15] claims that "cyclotomic fields, used for Ring-LWE, are uniquely protected" against a particular class of attacks. The reader is led to believe that this attack avenue threatens $x^p - x - 1$ and that it does *not* threaten cyclotomics, contradicting what Section 3.5 said and implying that Table 1.1 should have an $x^p - x - 1$ row.

However, checking the proof shows that the proof is limited to saying that cyclotomic fields are protected against certain attacks. The proof never showed that cyclotomic fields are *unique* in being protected. It is straightforward to see that the NTRU Prime fields are protected too.

The class of attacks is as follows:

- Starting from the polynomial F, find a modulus q and a root α of F modulo q where α has very low order. As a concrete illustration, let's assume α has order 4.
- Consider the ring morphism $(\mathbf{Z}/q)[x]/F \to \mathbf{Z}/q$ that maps x to α . This maps the powers of x to the powers of α , namely ± 1 and $\pm \alpha$ since α has order 4, so small linear combinations of powers of x map to small linear combinations of 1 and α .
- Apply this map to G and B = Gb + d. The objective of the attack is to distinguish B from uniform. (One can similarly consider more samples.)
- The usual distributions of b and d are small combinations of powers of x, so b and d map to small linear combinations of 1 and α , constraining the relationship between G and B. If q is sufficiently large then this distinguishes B from uniform.

One can convert this into a distinguishing attack for other moduli by "modulus switching", although this adds noise, requiring q to be larger.

This attack fails against the polynomial $F = x^p - x - 1$ used in NTRU Prime. Indeed, if $\alpha \in \mathbb{Z}/q$ has order 4 then it is a root of $x^2 + 1$. If it is also a root of $x^p - x - 1$ then q must divide the resultant of $x^p - x - 1$ and $x^2 + 1$ in $\mathbb{Z}[x]$. This resultant is either 1 or 5, depending on p mod 4, so $q \leq 5$, which is far too small for the attack to work: the images of b, d are very well distributed across the integers modulo 5. The attack similarly fails against power-of-2 cyclotomics, since the resultant is 4.

The above calculations were only for order 4, but replacing $x^2 + 1$ with further cyclotomic polynomials shows that the attack fails against all of the NTRU Prime fields for the entire range of orders of interest. These calculations are explained in [52]. So, no, cyclotomics are not *uniquely* protected against this class of attacks. It is also interesting to note that attacks in this class *do* work against prime cyclotomics in some cases; see Section 3.12.

See also [52] for a survey of known extensions to this class of attacks. The extended class of attacks has a huge parameter space, and it is not clear how to find optimal parameters; there is no proof that cyclotomics—or any other fields—are immune to the extended attacks. There is also no proof ruling out further attacks. The risks here are covered in Section 3.12.

5.7. There is no proof that the decryption failures in kyber1024 are safe. Typical advertisement [25, Section 4.3.1] on this topic: "Tight reduction from MLWE in the ROM ... The following concrete security statement proves Kyber.CCAKEM's IND-CCA2-security ... the security bound is tight". The reader understands this to mean that the KEM has been proven to reach its target ROM IND-CCA2 security level, under the stated assumption regarding the underlying mathematical problem, in this case Module-LWE.

This, however, has *not* been proven for the KEMs where "decryption failures" is listed in red in Table 1.1. See [49, Section 5] for a detailed analysis of what has been proven.

For example, for kyber1024, the ROM proof allows an attacker to succeed with probability $q/2^{172}$ where q is the number of hash computations carried out by the attacker. The proof thus does not guarantee any security against an attacker carrying out 2^{172} computations. This is a very large number of computations, but it is not the claimed 2^{256} security level.

A useful way to understand the risk here comes from the proof modularization in [80]. For the PKEs under discussion, it is reasonable to expect that plaintexts do not collide under encryption, so there *exists* an oracle that, unlike the actual decryption algorithm, always decrypts correctly. If the attacker cannot find a failing ciphertext then, for this attacker, the actual decryption algorithm behaves the same way as the oracle. The risk is that a ROM attack *can* find a failing ciphertext. Similar comments, with quantitative differences, apply to QROM attacks; see [80]. Non-QROM attacks are covered separately in Table 1.1.

5.8. There is no proof that derandomization in kyber1024 is safe. The derandomization risk is the most deeply hidden risk considered in this section: the risk where the first attacks (against *any* KEMs, never mind lattice KEMs) were developed most recently, and the risk that takes the most work to see from the previous literature. General awareness of the risk is low enough that there

has been little motivation for advertisements specifically mentioning this risk. However, more general advertisements such as

- "Ring-LWE is at least as hard as NTRU" (as mentioned above) and
- "there is no security . . . reason to prefer an NTRU NewHope [Quotient NTRU using the NewHope ring] to NewHope" [240]

lead readers to believe that Product NTRU KEMs are provably as hard to break as Quotient NTRU KEMs. This belief forces, among other things, denial of the "derandomization" row in Table 1.1: Quotient NTRU KEMs do not have a derandomization risk, so the belief logically implies the belief that Product NTRU KEMs do not have a derandomization risk. Given these advertisements, it seems necessary to emphasize that this specific belief is incorrect.

For the Quotient NTRU KEMs under consideration, the underlying PKEs are deterministic: decapsulation naturally recovers all randomness that was used to generate a ciphertext. Deterministic PKEs have a proof that any ROM IND-CCA2 attack against the most popular KEM construction implies an attack with essentially the same speed and success probability against the one-wayness of the PKE. For a careful proof, see [73], which also presents counterexamples to some previously claimed "theorems" regarding other PKEs.

The Product NTRU KEMs do *not* have a proof that a ROM IND-CCA2 attack implies an attack with essentially the same speed and success probability against the one-wayness of the underlying PKE. What has been proven is more limited:

- There is a proof that loses a factor q in success probability, where q is the number of hash computations carried out by the attacker. This limitation is important: a current large-scale attack can reach $q \approx 2^{100}$, and NISTPQC KEMs are supposed to look ahead to a future of even larger attacks.
- There is a tight proof under the assumption that the PKE provides IND-CPA security, indistinguishability against chosen-plaintext attacks. This change of assumption is important: algorithm designers study IND-CPA attacks less often than they study one-wayness, the occasional studies have found much faster attacks in some cases, and there are more IND-CPA attack avenues than one-wayness attack avenues. See generally [49, Section 6].
- There is a tight proof starting from the newly formulated assumption that the PKE provides "q-OW-CPA security". This is implied by IND-CPA security, and perhaps it is satisfied even when IND-CPA is not, but it has received essentially no study from algorithm designers. The new attacks from [59] show that q-OW-CPA security can be approximately q times easier to break than one-wayness.

The underlying issue is that the original PKE is unable to recover all randomness that was used to generate a ciphertext. These KEMs begin by derandomizing the underlying PKE to obtain a deterministic PKE—but there is no proof that derandomization preserves security; [59] gives examples where it loses security. **5.9.** There is no proof that breaking kyber1024 is as hard as breaking worst-case Ideal-SVP. Common advertising: "Attractive features of lattice cryptography include ... security under worst-case intractability assumptions" [305]; in particular, there is a "very strong hardness guarantee" [238] for Ring-LWE, assuming the hardness of worst-case Ideal-SVP.

The reader, easily putting everything together, is led to believe that breaking kyber1024 is provably as hard as breaking the underlying Module-LWE problem for $((\mathbf{Z}/q)[x]/(x^{256}+1))^4$, which in turn is provably as hard as breaking a Ring-LWE problem for $(\mathbf{Z}/q)[x]/(x^{1024}+1)$, which in turn is provably as hard as breaking a worst-case Ideal-SVP problem.¹³

See [72, Appendix A] for examples of how the last step in this chain, the Ideal-SVP guarantee, has been highlighted in a series of proposals of lattice KEMs leading into NISTPQC. The Ideal-SVP guarantee plays several roles in influencing decision-making processes: in promoting the notion that changing moduli is guaranteed safe,¹⁴ for example, and in discrediting Quotient NTRU.¹⁵ (Meanwhile, for some reason, the importance of Ideal-SVP is downplayed in response to advances in attacks against Ideal-SVP.¹⁶)

A closer look shows that, in fact, these proofs do not apply to kyber1024 or any of the other KEMs under consideration for deployment, even if one considers only the underlying problems such as Ring-LWE. What the theorems say, in a nutshell, is that one can extract a vastly less efficient worst-case Ideal-SVP attack from an attack against a related Ring-LWE problem. The efficiency gap is polynomially bounded but so large as to make the theorems logically inapplicable to NISTPQC. See generally [49].

This issue was buried for many years inside theorem statements that said "polynomial" without saying what the polynomial was. Applying such theorems to any specific size was never logically justified: the theorem statements did not rule out the possibility of a gigantic polynomial. Micciancio–Regev [259] characterized taking the polynomial into account as "overly conservative" but did not indicate how large the polynomial was.

¹³ Sometimes the chain of exaggerations extends another step, for example with the wording of [308]: "the underlying worst-case problems—e.g., approximating short vectors in lattices—have been deeply studied by some of the great mathematicians and computer scientists going back at least to Gauss, and appear to be very hard."

¹⁴ For example, [236] stated that "we have a line of research that states that avoiding a modulus q that supports NTT is (at least asymptotically) unnecessary—the worst-case to average-case reductions don't care about the ring modulus (assuming that worst-case lattice problems are actually hard)".

¹⁵ For example, NIST [8] stated that NTRU "lacks a formal worst-case-to-average-case reduction".

¹⁶ For example, in [21], NIST claimed that there was a "barrier" between Ideal-SVP and Ring-LWE, as one of two reasons for judging that [60] "does not impact the ongoing standardization process as-is". (The other reason relied on the incorrect conclusions of [137]; see Section 1.2.) If Ideal-SVP attacks don't matter, then why is it a negative feature for NTRU to supposedly not have an Ideal-SVP guarantee?

The shocking magnitude of the polynomial in these proofs finally came to light when Chatterjee–Koblitz–Menezes–Sarkar [104] "analyzed Regev's worst-case-to-average-case reduction for a cryptosystem that Regev had proposed, took lattice dimension 1024 with security target 2^{128} as a case study, and found an astonishing 2^{504} tightness gap in the proof", in the words of [49, Section 9]. The more recent proof in [159] has a somewhat smaller polynomial but is similarly inapplicable to NISTPQC.

6 The importance of proper benchmarking

Performance problems can create security risks when they lead users to reduce security levels or to disable cryptography entirely. A thorough risk analysis needs to include a performance analysis. Frodo's much lower Core-SVP level at each size, compared to the other KEMs, already appeared in Table 1.1 and played a decisive role in Section 4.2, but smaller performance differences among the other KEMs could also influence risks.

This section and Section 7 study performance in much more detail. Given the conclusion of Section 4 that sntrup is the least risky lattice KEM, Section 7 looks in particular at the NTRU Prime performance numbers. This section looks at the general question of how to evaluate costs.

There are many traps and pitfalls in cost evaluation, as illustrated by [212] analyzing many different types of "benchmarking crimes".¹⁷ The cryptographic case studies in this section illustrate that careful attention to benchmarking procedures is required to protect evaluations and comparisons against errors and sabotage.

6.1. Do benchmarks reflect the performance that the cryptographic user will see? Many years ago, "optimal extension fields" in elliptic-curve cryptography were advertised as allowing "faster multiplication and much faster inversion", in the words of [42]. However, a closer look at performance in [42] showed that these extension fields damaged overall performance:¹⁸

- The big inversion speedup didn't make much difference in context, since inversions weren't actually a serious bottleneck.
- These fields "have a huge disadvantage: even if they are slightly faster on some CPUs, they are much slower on other CPUs".

Presenting inversion microbenchmarks as if these represented total costs is an example of "benchmarking crime" B1 from [212], "microbenchmarks representing overall performance". Optimizing a design for one platform, and then presenting

¹⁷ One can criticize the word "crime" as implying *mens rea* that is not necessarily present, even when the source obviously benefits from the crime. What matters for this document is the damage that these "crimes" do to comparisons, whether or not the damage was intentional.

¹⁸ This is separate from the point made in the same paper that prime fields "have the virtue of minimizing the number of security concerns" for ECC.

results for that platform as if these represented performance across platforms, is an example of "benchmarking crime" A3, "selective data set hiding deficiencies".

Unfortunately, the same "benchmarking crimes" are a recurring problem in NISTPQC. Microbenchmarks selected to favor specific submissions are often presented as if they were overall performance, while other benchmarks showing deficiencies are suppressed.

6.2. Case study: ntruhrss vs. kyber. Consider recent claims that ntruhrss is "15X behind in speed" [240] compared to a lattice system proposed in 2021, which in turn was portrayed in [140] as having similar speed to "Ring/Module-LWE" systems. Readers can easily fall into the trap of believing that ntruhrss is $15 \times$ less efficient than kyber.

More broadly, readers can easily fall into the trap of believing that ntruhrss is poorly designed—for example, that it ignores "the huge research efforts that have been made in this area in the last 25 years" [240] and could easily be replaced by "a (much) better" variant in "6 months"—and therefore shouldn't be standardized. Such perceptions are influential: consider, e.g., [82] expressing concern that standardizing NTRU would be standardizing a "less-optimal" KEM.

But are the claims correct? Let's check encapsulation speed on an ARM Cortex-M4 microcontroller. This is not an obscure scenario: NIST said [19] that microcontroller comparisons should use ARM Cortex-M4, and common sense suggests that people sending encrypted messages from such lightweight platforms are more likely to encounter speed bottlenecks than people sending encrypted messages from Intel chips. The benchmarks of M4-optimized software collected by the ongoing pqm4 project [201] show

- kyber512 taking 551681 cycles,
- kyber512-90s taking 445609 cycles, and
- ntruhrss701, with higher Core-SVP, taking 375948 cycles.

This looks like a solid win for NTRU over Kyber. How is this possible, if NTRU is "15X behind"?

6.3. Does key-generation time matter? Let's look more closely at where the "15X behind" claim comes from. The underlying paper [140, Table 3] shows the following Intel Skylake cycle counts:

- "319.9K" for "NTRU-HRSS-701",
- "20.4K" for the new "NTTRU",
- "23.3K" for "Kyber-512", and
- "38.9K" for "Kyber-768".

The ratio between 319.9K and 20.4K is 15.7; more to the point, the ratio between 319.9K and 23.3K is 13.7.

All of these are totals of keygen+enc+dec time. The 319.9K is, according to [140, Table 3], dominated by keygen time. But wait a minute: where's the evidence that the user cares about keygen time in any of these systems? There is a long history of user annoyance at the latency of RSA key generation, but this does not justify drawing any conclusions regarding much faster lattice KEMs.

All of these KEMs are designed so that keys can be freely reused. Eliminating key-generation time immediately drops the ntruhrss701 cost below 100K cycles. Declaring that keys have to be ephemeral has negligible effect on this speedup: a dual-core 2GHz Skylake laptop generating a new ntruhrss701 key each minute is spending under 1/1000000 of its cycles on key generation.

Note that *ephemeral* and *one-time* are different concepts. The security goal of "forward secrecy" means *promptly* erasing secret keys, so that an attacker later stealing the device doesn't have a copy of those keys. This is exactly what *ephemeral* keys—keys "lasting for a very short time"—accomplish. Having keys be *one-time* is neither necessary nor sufficient for this. Compared to a protocol properly designed to erase each secret within a specified number of minutes (see, e.g., [48]), a protocol using one-time keys

- is less efficient—because it generates a new key for each ciphertext—and
- fails to achieve the security goal—see, e.g., [223].

Erroneously equating one-time keys with ephemeral keys plays a prominent role in arguments to assign a high weight to key-generation time.

Furthermore, even *if* an application really wants one-time keys, inversions can be made much faster via "Montgomery's trick". See [66], which estimates a $2\times$ speedup for ntruhrss701 keygen and demonstrates a much larger speedup for sntrup761 keygen—including demonstrating web browsing on top of TLS 1.3 using sntrup761 with fast key generation.

All of these points are covered in the literature: see, e.g., [65, Section 5.3, "Does key-generation time matter?"]. All of these points are ignored in the presentation of "319.9K" as overall performance.

6.4. Are benchmarks reproducible and properly labeled? Anyone who checks the SUPERCOP benchmarks [71] for Skylake (samba) sees that

- kyber512 takes 37336 cycles for enc and 28629 cycles for dec, total 65965 cycles, while
- ntruhrss701 takes 25740 cycles for enc and 61530 cycles for dec, total 87270 cycles.

These numbers show 32% more cycles for ntruhrss701 than for kyber512, but ntruhrss701 also has 15% higher $\log_2(\text{Core-SVP})$ than kyber512 does. Also, ntruhrss701 wins in pure enc time, and it is easy to argue that this is what matters if the sender (for example, a server doing enc) is busy while the receiver (for example, a laptop doing dec) is not.

For comparison, [140, Table 3] reports "34.6K" cycles for "NTRU-HRSS-701" enc and "65K" cycles for dec. The total of these cycle counts is 14% higher than what one sees for ntruhrss701 in SUPERCOP. Why is a claim of a speedup over ntruhrss701 comparing to ntruhrss701 timings that are slower than previously published, publicly verifiable timings for ntruhrss701 on the same platform?

A more striking gap, $4 \times$ rather than 14%, is between

• [140, Table 3] claiming that "Kyber-512" takes "23.3K" cycles for the total of keygen+enc+dec and

• SUPERCOP showing 89657 cycles for kyber512: 23692 for keygen, and the 37336 and 28629 mentioned above for enc and dec.

This $4 \times$ gap comes from the fact that the "Kyber-512" in [140, Table 3] is *not* the Kyber-512 KEM submitted to NISTPQC. One might guess that it's the Kyber-512-90s KEM—but, no, it isn't that KEM either. Compared to Kyber-512-90s, this KEM is further modified to perform less hashing. To avoid confusion, let's refer to this KEM as Kyber-512-90s-PartialHash.

The Kyber presentation at the Third PQC Standardization Conference [26, video, 1:23:00, text on slide] advertised Kyber as being "conservative" in four ways. These four ways included

- "all symmetric crypto based on Keccak"—which is exactly what the "90s" version of Kyber abandons—and
- "shared key depends on full transcript"—which is abandoned by the further modification from 90s to 90s-PartialHash.

The mislabeling of Kyber-512-90s-PartialHash as "Kyber-512" in [140] will make many readers believe that Kyber-512, with its advertised features, achieves the speeds displayed in [140]; in fact, it is $4 \times$ slower.

Now that we've seen the actual Kyber performance figures, let's look again at the idea that, since ntruhrss701 is supposedly "15X behind" the new "NTTRU", NTRU is poorly designed and shouldn't be standardized. Even if we accept the questionable idea that Skylake keygen+enc+dec is the sole metric of interest, a more reasonable estimate is $9\times$, given the keygen speedups from [66] and the faster enc+dec in SUPERCOP. Meanwhile Kyber-512 is $4\times$ slower in this metric than the new Kyber-512-90s-PartialHash. Does this $4\times$ slowdown mean that Kyber-512 was poorly designed and shouldn't be standardized?

The Kyber submission [25, Section 2] tells a very different story:

As also mentioned in Section 1.5, hashing the public key and the ciphertext is not required for CCA security, but is instead done to make the function more robust and directly usable in applications that require the shared key to depend on the entire transcript. Our rationale is that because the basic operations comprising Kyber are extremely fast, we can afford to pay a time penalty and make the default version of Kyber as robust and misuse-resilient as possible.

One can similarly find the NTRU submission stating design goals other than performance. So, no, the fact that new designs use many fewer cycles doesn't mean that these submissions were poorly designed.

6.5. Does faster on some CPUs mean faster across platforms? There's extensive literature studying performance variations across platforms, including many examples where speedups on one platform are slowdowns on another. Chip designers make different decisions regarding which packages of bit operations to bundle into CPU "instructions" (and into "coprocessors", and into FPGA "slices", and so on); these decisions interact in many ways with algorithm optimization.

Kyber was designed from the outset to streamline a particular NTT-based multiplication strategy for Intel CPUs. The Kyber presentation at the round-3 NISTPQC conference claimed [26, video, 1:22–1:23] that

the fastest way to perform arithmetic in the polynomial rings you find in, well, structured lattice-based cryptography is to use NTT-based multiplication. Now, by now I think all candidates use NTT-based arithmetic but Kyber was built to make this particularly efficient, which, well, makes it particularly efficient.

These blanket statements were promptly contradicted by the next presentation at the same conference: SABER does use NTTs on some platforms, but on other platforms it (1) is implemented without NTTs and (2) achieves speeds that Kyber still has not matched and perhaps cannot match. Perhaps most importantly, all available evidence is that Kyber hardware is less efficient than SABER hardware, primarily because Kyber uses more bit operations than SABER for arithmetic.

What about NTRU? The literature does not seem to contain the necessary bit-operation analysis, but the best bet from current knowledge is that—outside key generation—NTRU will use even fewer bit operations than SABER.

The main arithmetic bottleneck in NTRU is multiplying elements of, e.g., the ring $(\mathbb{Z}/8192)[x]/(x^n - 1)$. One element is small, having *n* coefficients in $\{-1, 0, 1\}$, while the other looks random, having *n* 13-bit coefficients. Schoolbook polynomial multiplication performs n^2 multiplications of 13-bit coefficients by small coefficients and accumulates the results; one is free at any moment to replace x^n with 1 and to replace 8192 with 0. For a full analysis one needs to not just count bit operations in this process but also see what speedups are possible with fancier multiplication methods: Karatsuba, Toom, and NTTs. See generally [41].

In SABER, an *n*-coefficient ring is replaced by (e.g.) 3 elements of an n/3coefficient ring, which one has to multiply by a 3×3 matrix. The $3 \times$ smaller ring makes schoolbook multiplication $9 \times$ faster, but there are 9 multiplications—no obvious difference in speed, and no obvious reason that fancier multiplication methods would favor SABER. However, the small coefficients in SABER are larger, requiring considerably more bit operations for each coefficient operation. Furthermore, the high-level structure of Product NTRU, used in the SABER submission, uses more arithmetic for enc+dec than the high-level structure of Quotient NTRU, used in the NTRU submission. Operations beyond arithmetic, such as hashing, do not obviously favor SABER.

If one thinks of a multiplication of two 16-bit integers—this means hundreds of bit operations—as one "instruction" then it becomes difficult to see the bitoperation advantage of smaller inputs. The NTTs in Kyber take advantage of these instructions. However, people building ASICs, cryptographic coprocessors, etc. see the intrinsic hardware cost of each operation, and can build much more efficient hardware that takes advantage of how small the inputs are. SABER does better than Kyber on ASICs by using non-NTT multipliers (see [320]), and one would expect an NTRU ASIC to do even better. There are no good choices for a Kyber ASIC:
- An ASIC using NTTs is bottlenecked by full-size multiplications, which are much more expensive than multiplications with small inputs.
- An ASIC trying to avoid using NTTs faces serious problems since various objects communicated by Kyber are in the NTT domain.

Kyber's cross-platform difficulties go beyond ASICs. Consider the paper [13] targeting a "commercially available smart card chip (SLE 78)" with an RSA coprocessor, stating that Kyber "requires the usage of the Number Theoretic Transform (NTT), which we cannot realise efficiently with our approach", and achieving much better speeds by introducing a new design—labeled as a "Kyber variant" but actually much closer to SABER. The same approach works even more efficiently for NTRU and NTRU Prime, not requiring any modifications.

In short, Kyber's NTT-related design decisions look good in *some* benchmarks but look bad in others. It is critical to take a wide enough range of benchmarks to be able to see the full performance picture.

6.6. The dominance of communication costs for lattice KEMs. The total costs of a cryptographic system go beyond the costs of computation, such as the cost of encapsulation; they also include the costs of communication, such as the cost of sending the ciphertext produced by encapsulation.

These costs are naturally measured in different units, such as cycles and bytes. How does one put these on the same scale? One answer—used in [65, Section 5.4] and the earlier documents cited there—is to consider a quad-core 3GHz CPU handling a 100Mbps Internet connection. In one millisecond, the CPU runs 12 million cycles, while the Internet connection transmits only 12500 bytes. Let's model the CPU and the Internet connection as having similar costs; more precisely, let's model each byte on the Internet connection as having the same cost as 1000 cycles on the CPU.

This model provides a way to quantify the idea that communication costs are much more important for lattice KEMs than they are for ECC:

- If an ECDH system uses 200000 cycles and sends 64 bytes, the total cost in this model is 264000 cycles, about 3/4 from computation. If another system saves half the bytes, 32 bytes, the total cost drops to 232000 cycles, 88% of the first cost. If a third system then saves half the cycles, the total cost drops to 132000 cycles, 56% of the second cost.
- If a lattice system uses 100000 cycles and sends 1000 bytes, the total cost in this model is 1100000 cycles, about 90% from communication. If another system saves half the cycles, the total cost is 1050000 cycles, 95% of what it was a moment ago. Because so much of the cost is from communication, the cycle-count improvements make far less of a difference than they did in the ECDH example.

As a real example, let's consider ntruhps4096821 (Core-SVP 2^{179}). The sender spends 44119 cycles on enc, and sends a 1230-byte ciphertext, costing 1230000 cycles; the total cost is 1274119 cycles, 97% from communication. The receiver receives the 1230-byte ciphertext, costing 1230000 cycles, and spends

81114 cycles on dec; the total cost is 1311114 cycles, 94% from communication. If the costs of a new key are included then there's a more noticeable 414070 cycles for keygen, but also another 1230 bytes to send the key and another 1230 bytes to receive the key; the total cost across both sides is 5459303 cycles, with 90% from communication. Making keygen $2\times$ faster might sound dramatic if keygen microbenchmarks are considered in isolation, but saves only 4% of the total cost.

It is hard to see how emphasizing the cycle counts of ntruhps4096821, rather than the sizes, can be justified in this context.¹⁹ Even if the costs of a new key are included, saying that 539303 cycles are *as expensive as* 4920 bytes means saying that a quad-core 3GHz CPU is as expensive as a 1Gbps Internet connection. This seems hard to explain, given that a \$500 computer with a quad-core 3GHz CPU will typically last for several years while Google Fiber 1Gbps will accumulate \$500 in charges within several months.

See [50, Section 2.3] for more evidence backing the idea that "communication volume is by far the most serious performance problem with lattice systems". For example, the Google–Cloudflare experiments with lattice systems (see [224] and [217]), including a variant of ntruhrss701, have consistently seen much more impact of bytes transmitted than of cycles. See also [168, page 6], which describes a smartcard with a "100 MHz" Cortex-M3 CPU and "< 100 kB/s" communication—i.e., communicating a byte takes more time than 1000 cycles.

6.7. Performance loss #256. Consider the following competition between NTRU and Kyber. What's the smallest ciphertext size provided by the KEMs specified in each submission, subject to requiring Core-SVP to be at least 2^{128} ?

Answer: NTRU's ntruhps2048677 fits into 931 bytes. Kyber's kyber768 is noticeably worse: 1088 bytes, 17% larger. This is another solid win for NTRU over Kyber.

If one changes from requiring Core-SVP 2¹²⁸ to requiring Core-SVP 2¹¹⁸, exactly the Core-SVP level²⁰ of kyber512, then suddenly Kyber wins instead: kyber512 fits into 768 bytes, 18% smaller than ntruhps2048677. But why should these comparisons be carried out at the levels that Kyber happened to select?

One might think that, well, NTRU happened to pick some examples of sizes, and Kyber happened to pick some examples of sizes, and if there's a request for an intermediate size then NTRU and Kyber can simply specify intermediate sizes meeting the request. This is true for NTRU, but it's *not* true for Kyber.

Structurally, Kyber uses a rank-k module over $(\mathbf{Z}/q)[x]/(x^{256}+1)$, so the dimension is 256k. There are no Kyber options between kyber512 and kyber768 and kyber1024: those are k = 2 and k = 3 and k = 4. If kyber512's Core-

¹⁹ One could, as noted in [65, Section 5.4], artificially reduce the communication-vs.computation ratio by pairing "an expensive radio inside a recent smartphone" with "a cheap microcontroller manufactured on a 90nm fab". It was proposed in [65] to have the community "agree upon a spectrum of data points regarding the costs of communication and the costs of computation in various environments".

²⁰ This gives kyber512 the benefit of switching from Core-SVP to revised-Core-SVP.

SVP 2¹¹⁸ isn't acceptable²¹ then one has to jump all the way up to kyber768, a remarkable 42% increase in ciphertext size. SABER has the same problem. The NTRU structure is different, supporting a much more tightly packed list of dimensions.

See Figure 7.3 below for a comparison across all security levels. After the 42% jump from kyber512 to kyber768 in the volume of data communicated, there is another 44% jump from kyber768 to kyber1024. One can draw a superficial analogy to the jumps in key sizes between, e.g., AES-128, AES-192, and AES-256, but (1) AES keys are much smaller than Kyber ciphertexts, (2) AES keys are rarely sent over the wire, and (3) setting some AES key bits to constants allows smooth tradeoffs between key size and security, whereas the only way that Kyber can save ciphertext size is by jumping far down in security level.

The importance of this performance issue in lattice-based cryptography is not a new observation. This issue was, for example, motivation #1 stated for Lyubashevsky–Peikert–Regev [239], a paper that generalized previous work for power-of-2 cyclotomics to provide an arbitrary-cyclotomic "toolkit for Ring-LWE cryptography":

While power-of-two cyclotomic rings are very convenient to use, there are several reasons why it is essential to consider other cyclotomics as well. The most obvious, practical reason is that powers of two are sparsely distributed, and the desired concrete security level for an application may call for a ring dimension much smaller than the next-largest power of two. So restricting to powers of two could lead to key sizes and runtimes that are at least twice as large as necessary.

Kyber's 768/512 is just 1.5 rather than 2, but this doesn't eliminate the problem, as illustrated by the 17% Kyber loss described above.

6.8. Consequences of performance loss #256 for NISTPQC. The call for NISTPQC submissions states that each submission should—if possible—"specify *several* parameter sets that allow the selection of a range of possible security/performance tradeoffs" (emphasis added). NIST proposed five broad security "categories" (see Appendix B) and said that submitters could specify even more than five parameter sets to demonstrate flexibility:

Submitters may also provide more than one parameter set in the same category, in order to demonstrate how parameters can be tuned to offer better performance or higher security margins.

The evaluation criteria (see Appendix A) state "security" as the top evaluation factor, then "cost", then "algorithm and implementation characteristics". The third-factor criteria say that

Assuming good overall security and performance, schemes with greater flexibility will meet the needs of more users than less flexible schemes, and therefore, are preferable

²¹ This again gives kyber512 the benefit of switching from Core-SVP to revised-Core-SVP.

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and list "It is straightforward to customize the scheme's parameters to meet a range of security targets and performance goals" as an example of "flexibility".

Consequently, according to the NISTPQC rules, NTRU's amply documented flexibility to provide a full spectrum of dimensions is a positive feature, compared to Kyber's enforced leaps from dimension 512 to dimension 768 to dimension 1024. The size competition in Section 6.7 illustrates how this flexibility lets NTRU reach combinations of security targets and performance goals that Kyber cannot reach.

The same comments apply to SABER: Kyber and SABER share the design decision to require modules over a 256-coefficient ring, a big step backwards from the traditional flexibility of NTRU. Note also that these performance losses turn into quantitative security losses if the application has, e.g., ciphertext-size limits that don't happen to match what's favorable to Kyber and SABER.

The problem is even more extreme for NewHope, which leaps from dimension 512 to dimension 1024. NIST's round-2 report [8] pointed out Kyber's advantage in exactly this respect over NewHope, as part of explaining why NIST eliminated NewHope:

KYBER naturally supports a category 3 security strength parameter set, whereas NewHope does not.

This is also an advantage for NTRU over Kyber: NTRU "naturally supports" more "categories" than Kyber does. Also recall that the call for submissions valued "better performance or higher security margins" even within "the same category".

Surprisingly, the same NIST report [8] fails to recognize this advantage of NTRU over Kyber, and even issues a blanket statement that NTRU is less efficient than Kyber:

While NTRU is very efficient, it is not quite at the level of the highestperforming lattice schemes ... NTRU has a small performance gap in comparison to KYBER and SABER ...

See [53] for a detailed analysis of how a pro-Kyber "discretization attack" would limit comparisons to security levels favorable to Kyber, producing the incorrect conclusion that Kyber consistently outperforms NTRU. In fact, the performance winner between Kyber and NTRU depends on the application requirements.

7 Performance of NTRU Prime

The NTRU Prime design process has a clear priority order. First, within the design space of lattice systems, eliminate unstructured lattices, because the goal is to handle applications that *require* something small. Second, *within small lattice systems*, eliminate unnecessary complications in security review, because these complications exacerbate lattice security risks. Third, *within the systems that remain*, optimize the tradeoff between

- size and
- security against known attacks.

One would guess that this produces much worse performance than submissions that give performance optimization a higher priority, balancing performance with other concerns. A key insight—this is not something obvious; it is the result of detailed performance analysis—is that this guess is incorrect. NTRU Prime reduces the attack surface at surprisingly low cost. Sometimes NTRU Prime outperforms all of the other submissions.

7.1. A simple example where NTRU Prime has the best performance. As in Section 6.7, let's ask for the smallest ciphertext size provided by the KEMs specified in each lattice submission, subject to requiring Core-SVP to be at least 2^{128} , but now let's look at all five submissions, not just NTRU and Kyber. NTRU Prime is the winner:

- NTRU Prime (sntrup653): 897 bytes.
- NTRU (ntruhps2048677): 931 bytes.
- Kyber (kyber768): 1088 bytes.
- SABER (saber): 1088 bytes.
- Frodo (frodo640): 9720 bytes.

Obviously NTRU isn't far behind. Kyber and SABER are noticeably worse, with 21% larger ciphertexts than NTRU Prime, as a direct result of the module-related performance loss explained in Section 6.7.

7.2. Competitive size-security tradeoffs. Let's vary the example from Section 7.1 by asking, for each b, what ciphertext sizes are available when Core-SVP is required to be at least 2^b . The results are plotted in Figure 7.3. This is an update of [50, Figure 3.5] to account for

- extra parameter sets specified for NTRU,
- extra parameter sets specified for NTRU Prime,
- a modified version of kyber512,²² and
- the SABER submission's announcement that, because of an error in [12], pre-round-3 versions of the SABER submission had reported incorrect (too high) Core-SVP results.

See [50, Section 2.3] for reasons to use size metrics, specifically ciphertext-size metrics; and see [50, Section 3] for visualization principles.

Each b corresponds to a horizontal line, and better submissions are farther left on that line. The smallest ciphertexts are achieved by sntrup for $118 < b \le 129$, ntruhps for $129 < b \le 145$, sntrup for $145 < b \le 153$, saber/kyber for $153 < b \le 181$, saber for $181 < b \le 189$, sntrup for $189 < b \le 209$, saber for $209 < b \le 260$, etc. Both sntrup and ntruhps can easily improve their position in the graph by adding further parameter sets, whereas kyber and saber cannot; see Section 6.7.

²² This figure, and the subsequent figures in this section, permits kyber512 to substitute revised-Core-SVP for Core-SVP.



Fig. 7.3. Core-SVP (vertical axis, log scale) vs. ciphertext size (horizontal axis, bytes, log scale). Better security-performance tradeoffs are farther up and to the left.

One can also use the same graph to see how size constraints limit Core-SVP levels, exacerbating security risks. Each size limit corresponds to a vertical line, and better submissions are farther up on that line. For example, if an application is limited to 1024 bytes then it obtains Core-SVP 2^{145} with NTRU (ntruhps), Core-SVP 2^{129} with NTRU Prime (sntrup), and bleeding-edge Core-SVP with saber and kyber. Other size limits obtain the best Core-SVP with saber, in some cases tied with kyber, so this is not a clear argument against any particular KEM except for frodo.

Beware that lumping security levels into "categories" would (1) hide most of the information in Figure 7.3 and (2) let attackers manipulate the category boundaries so as to favor particular submissions. See [53] and Appendix B.

7.4. Competitive speeds. Section 6.6 explained the rationale for considering size, specifically ciphertext size, as the primary performance metric. But what if you have a slow ARM Cortex-M4 microcontroller—again, the ARM Cortex-M4 is NIST's designated microcontroller for comparisons [19]—and you really want to know how many cycles are used for computation?

One scenario is that the microcontroller is encrypting messages, typically to send to a larger device. Figure 7.5 shows the time for encapsulation. This graph



Fig. 7.5. Core-SVP (vertical axis, log scale) vs. ARM Cortex-M4 time for encapsulation (horizontal axis, cycles, log scale). Better security-performance tradeoffs are farther up and to the left.

shows that sntrup is competitive, including various security requirements (e.g., Core-SVP 2^{128}) for which it is the most efficient option.

Another scenario is that the microcontroller is decrypting messages. Figure 7.6 shows the time for decapsulation. In this graph, sntrup is the most efficient option at every security level—except for Core-SVP 2^{118} (and below), which, for security reasons, the NTRU Prime submission refuses to support.

All of these measurements are average cycle counts reported by the pqm4 benchmarking project as of 17 October 2021 for M4-optimized software. The new ntruhrss1373 and ntruhps40961229 are not included since they do not yet have M4-optimized software.

Beware that neither graph includes the costs of communicating the ciphertext being handled. Including such costs would narrow the gaps between the systems, as in Figure 7.3, and would reduce sntrup's overall advantage, but in any case sntrup is an attractive option for microcontrollers.

7.7. How is it possible for sntrup to be outperforming kyber? As noted above, NTRU Prime has a rule of not allowing performance improvements that would complicate security review. Kyber has no such rule: it emphasized speed from the outset and built a narrative of being the most efficient lattice KEM.



Fig. 7.6. Core-SVP (vertical axis, log scale) vs. ARM Cortex-M4 time for decapsulation (horizontal axis, cycles, log scale). Better security-performance tradeoffs are farther up and to the left.

However, Figures 7.3, 7.5, and 7.6 show sntrup outperforming kyber at many security levels in size, in encapsulation speed, and in decapsulation speed.

These numbers show that **sntrup** is doing something positive for performance that **kyber** failed to do. There are actually several differences contributing to **sntrup**'s performance wins. It's helpful to look at the details, to understand why **sntrup** can be expected to provide competitive cross-platform performance.

First, kyber is structurally limited to dimensions 256, 512, etc., while sntrup provides many more choices. See Section 6.7. The losses are easily visible from the big jumps of kyber90s/kyber costs in each of the figures; same for saber. On the other hand, this obviously can't be the whole story, given how consistently sntrup wins in Figure 7.6.

Second, as noted in [65, Section 5.5], all NTRU Prime operations take time $b^{1+o(1)}$ where b is the number of key bits. This is not true for kyber: the design decision to use modules over a 256-coefficient ring forces kyber to spend time handling $256k^2$ coefficients for lattice dimension 256k. The cost per coefficient is small on Intel CPUs, but the cross-platform slowdown is more significant.

Note that modules do nothing to eliminate complications in security review: on the contrary, they *add* complications to security review. See [65, Section 4.8]. Consequently, modules have never been candidates for the NTRU Prime design. Fortunately for NTRU Prime's performance, modules also don't help performance. Regarding the idea that modules make optimized implementations easier, see Appendix A.3.

Third, by selecting a Product NTRU structure rather than a Quotient NTRU structure, Kyber sacrificed enc speed and dec speed in favor of keygen speed. From a performance perspective, this is making a highly questionable bet; see Section 6.3 and Section 7.11. Of course, if one simply declares that the metric of interest is keygen+enc+dec, rather than investigating the needs of applications, then the bet looks good.

For comparison, the NTRU Prime submissions in every round of NISTPQC have equally supported Product NTRU and Quotient NTRU. (This starts from the fact that each option avoids some security-review complications created by the other option. See, e.g., [59], and see generally Table 1.1.) From a performance perspective, under the reasonable assumption that enc speed and dec speed are much more important than keygen speed, NTRU Prime benefits from the Quotient NTRU option, sntrup.

Fourth, the way that Kyber bakes NTTs into its specification effectively ties implementors to a particular NTT-based multiplication strategy. This works well on Intel CPUs but turns out to damage cross-platform performance. See Section 6.5.

Fifth, even though the polynomial $x^p - x - 1$ used in NTRU Prime is slightly larger (in relevant metrics) than competing polynomials such as $x^p - 1$, the resulting performance loss turns out to be small, not dominating the performance picture. Similar comments apply to eliminating decryption failures.

7.8. Secret-key storage. Consider again a microcontroller using a KEM to decrypt messages. If the microcontroller is cycling through N secret KEM keys for, e.g., kyber768, then it needs to store 2400N bytes, since each KEM key uses 2400 bytes. This can be a problem, depending on

- how big N is,
- the total storage available in the microcontroller, and
- what else is competing for space in the microcontroller.

Figure 7.9 shows the number of bytes per secret key for all of the KEMs. Kyber and Saber consume the most space of all of the small KEMs; ntrulpr and ntruhps consume the least; sntrup and ntruhrss are in the middle.

A closer look at the KEM designs shows that each design has some natural flexibility regarding how much data is precomputed during key generation and stored in the secret key, rather than being recomputed during decapsulation. KEM designers make judgment calls regarding the best tradeoffs here. None of the KEMs opted to store only a short seed and regenerate keys from that seed during decapsulation; and none of the KEMs selected the opposite extreme, such as precomputing all possible transforms of multiplication inputs.

Secret-key holders can vary this decision while retaining interoperability, at the expense of a more complicated implementation ecosystem. A full graph of



Fig. 7.9. Core-SVP (vertical axis, log scale) vs. secret-key size (horizontal axis, bytes, log scale). Better security-performance tradeoffs are farther up and to the left.

all of these options would require merging Figures 7.6 and 7.9 into a threedimensional picture showing the tradeoffs between size, decapsulation cycles, and security, after implementation work to measure those tradeoffs.

Given that Kyber, with its own selection of secret-key size, is consistently outperformed by sntrup in Figure 7.6 (decapsulation time) and in Figure 7.9 (secret-key size), it is reasonable to guess that implementing more options for both Kyber and sntrup will show a three-dimensional picture favorable to sntrup.

Beware that the extra dimension here makes it even more important than usual to be on alert regarding improper benchmarking. This is illustrated by the range of benchmarking errors that appeared in response to [63], a recent message correctly disputing the idea "that Kyber is the most efficient lattice KEM in NISTPQC". Concretely, [63] observed that "if I want an ARM Cortex-M4 to decrypt messages, specifically with Core-SVP $\geq 2^{128}$ " then Kyber uses 50% more cycles than NTRU Prime, according to pqm4, and also receives 20% more bytes than NTRU Prime. The conclusion of [63] was as follows:

There are other benchmarks where Kyber does better, but cherry-picking pro-Kyber benchmarks and pretending that Kyber always wins is highly inappropriate. The actual situation is that the performance winner depends on the application.

There were three directions of responses. First, there were various responses saying that Kyber does better on other benchmarks—shifting to Kyber's favorite security levels, highlighting key-generation time, and ignoring communication costs. Structurally, these responses were not addressing the point of dispute.

Second, there were objections to the RAM consumption and code size of the sntrup software. See [23] and [322] ("clearly not practical for use in embedded engineering"). A closer look shows, however, that

- the reported code-size numbers were dominated by *key generation*, which has no relevance to the scenario under discussion; and, furthermore,
- memory usage wasn't an optimization target for this code (see Section 7.10).

Experience suggests that NTRU Prime code fitting into much less memory will not lose much speed. As an analogy, compare the Saber results in [116], fast but not space-optimized, to the followup Saber results in [2], space-optimized without much loss of speed.

Third, there were responses such as [237] saying that Kyber can save time in decapsulation by spending more RAM per secret key. Quantitatively, increasing Kyber's secret key in this scenario from 2400 bytes to 5856 bytes was estimated to reduce Kyber from 839000 cycles to 445000 cycles and the "90s" version from 749000 cycles to 431000 cycles, while NTRU Prime uses 486707 cycles. The conclusion of [237] is that this makes Kyber "faster" than NTRU Prime. This conclusion ignores all of the following facts:

- In a reasonable scenario of a 100MHz CPU with 100kBps communication (see Section 6.6 and [168]), Kyber is still slower: it would need to decapsulate in 295000 cycles to catch up.
- 5856N bytes of RAM for N secret keys for Kyber (assuming keys are stored in RAM) are much larger than 1518N bytes for the competition. To put these numbers into perspective, consider [23] highlighting "16,088 bytes of RAM" used by (not RAM-optimized) NTRU Prime software vs. "3,520 bytes of RAM" used by (RAM-optimized) Kyber software. This refers to RAM used during a computation and reused for the next computation, not perkey RAM; this is outweighed by the difference between 5856N and 1518N as soon as $N \geq 3$.
- NTRU Prime, like Kyber, has the flexibility to precompute transforms of multiplication inputs, so if enough RAM is available for expanded keys then NTRU Prime will save time too.

In short, Kyber starts out using more RAM (assuming N is not very small), more communication, and more cycles than NTRU Prime. The argument that Kyber should be able to use fewer cycles than NTRU Prime relies on (1) using even more RAM and (2) ignoring the flexibility of NTRU Prime to also exploit extra RAM. Furthermore, even if Kyber is allowed to use much more RAM while NTRU Prime is not, Kyber uses so much more communication that it will still be slower than NTRU Prime. **7.10.** More platforms. There are many more platforms of interest, and many variations in cost metrics of interest. The ecosystem of optimized NTRU Prime implementations is expanding to include more and more targets. Here are some examples.

FPGAs: When an application needs to fit many different processing units into the space available on an FPGA, the space consumed by each unit is critical. Speed matters only for units that are bottlenecks in the application. Marotzke [251] presented a *complete* constant-time sntrup761 implementation that fits into a small corner of an Artix-7 FPGA (the FPGA designated by NIST for comparisons) with better throughput than needed for any known application of FPGAs. In case applications do need better speeds, very recent results in [312] show that NTRU Prime supports a wide range of area-throughput tradeoffs on the same Artix-7 FPGA:

- keygen fits into 629367 cycles in 7579 LUTs or 64026 cycles in 39200 LUTs,
- enc fits into 29245 cycles in 6379 LUTs or 5007 cycles in 40879 LUTs, and
- dec fits into 85628 cycles in 6279 LUTs or 10989 cycles in 36789 LUTs,

in all cases running at ≥ 131 MHz. The combined keygen-enc-dec units in [312] are not much larger than single units. On a Zynq Ultrascale+, LUT counts are smaller and frequencies are higher, competitive with recent ntruhps Ultrascale+ results [129] for that FPGA. It is interesting to note that the current FPGA speed records for ntruhps, sntrup, and saber do not use NTTs, even though these FPGAs include multipliers.

ASICs: Central predictors of hardware efficiency, such as energy consumption, include bit operations—weighted by the cost of each bit operation in hardware— and communication costs. Investigations are underway regarding the number of bit operations required for all sizes of sntrup and ntrulpr. See Section 6.5.

Smartphones: There are many different smartphone CPUs. The main targets are 32-bit ARMv7-A and 64-bit ARMv8-A. Each of these targets includes many variations in microarchitectures. Work is in progress to optimize NTRU Prime software for these targets: for example, sntrup761 decapsulation currently runs in 212981 Cortex-A72 cycles.

Software memory usage: Given how little space NTRU Prime uses on an FPGA, it is safe to extrapolate that NTRU Prime can also be squeezed into very little space in software, including code size, stack usage, etc. Work is underway to construct such software, both for the single-parameter-set scenario and for the multiple-parameter-set scenario. This may be useful for some applications on small devices.

Side-channel protection: All supported NTRU Prime implementations are protected against timing attacks, but many applications also need protection against power attacks, electromagnetic attacks, etc. A twist in the story here is 2021 Ngo–Dubrova–Guo–Johansson [280], an attack very efficiently breaking the *masked* implementation of SABER from [38]: the attack recovers the user's secret key from just 16 decapsulation traces. This attack, together with other recent side-channel attacks, has called into question the entire idea that these KEMs can be protected by low-order masking; see generally [28]. Investigations

Haswell cycles/key	enc	dec	sntrup761 report
>6000000	59456	97684	round-1 submission
946772	55252	70464	round-2 update talk
156317	46914	56241	round-3 update talk
Haswell cycles/key	enc	dec	ntrulpr761 report
47396	77280	95316	round-2 update talk
42515	69103	82071	round-3 update talk

Table 7.12. History of improvements in Haswell cycles for keygen, enc, and dec for the dimension recommended in the NTRU Prime submission. See [66].

are underway regarding automated conversions of sequences of bit operations for NTRU Prime into *high-order* masked implementations.

7.11. Key generation, part 1: speedups. It's far from clear that keygen time matters in these systems (see Section 6.3), so optimizing keygen hasn't been the top priority—but there has nevertheless been some work. This work has dramatically sped up sntrup keygen, so any concerns that might have been raised at the beginning regarding keygen time are now obsolete *even if* the application is bottlenecked by keygen time.

To see the speedups, let's focus specifically on

- Intel Haswell (NIST's designated large CPU for comparison) and
- dimension 761 (the dimension that has always been recommended in the NTRU Prime submission)

since this is where the longest history of performance data is available.

The round-1 submission reported key generation taking more than 6 million cycles; see Table 7.12. The round-2 update talk reported key generation more than 6 times faster. The round-3 update talk reported key generation another 6 times faster. See [66], including a web-browsing demo on top of TLS 1.3 using sntrup761 with fast key generation.

Table 7.12 also covers ntrulpr761, the Product NTRU option in the NTRU Prime submission. This has faster keygen than sntrup761 and smaller key sizes, but slower enc, slower dec, and larger ciphertexts. The round-3 cycle counts in the table allow a direct speed comparison between Product NTRU and Quotient NTRU: the ring is the same, there is extensive sharing of optimized software between the options, and the subroutines specific to each option have also been rewritten for performance.

The numbers show that, as soon as a key is used for 3 ciphertexts, sntrup761 uses fewer cycles than ntrulpr761. It also sends fewer bytes, since its ciphertexts are smaller. A popular server broadcasting a key for clients around the Internet to use for the next few minutes is going to have the key used for many more than 3 ciphertexts.

7.13. Key generation, part 2: comparison to Kyber. Let's take Kyber's favorite scenario: N keys are used for only N ciphertexts; the sender and receiver

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each have an Intel CPU; also, let's ignore ntrulpr. Here are the full costs of two options, accounting for the latest benchmarks for publicly available software.

For kyber768, the receiver spends

- 1184N bytes sending keys plus 1088N bytes receiving ciphertexts, and
- 44178N cycles on key generation plus 47766N cycles on decapsulation;

total cost 2360944N cycles in the model of Section 6.6. The sender spends

- 1184N bytes receiving keys plus 1088N bytes sending ciphertexts, and
- 60258N cycles on encapsulation;

total cost 2332258N cycles. The sender and receiver together spend 4693202N cycles.

For sntrup653, the receiver spends

- 994N bytes sending keys plus 897N bytes receiving ciphertexts, and
- 164260N cycles on key generation plus 55778N bytes on decapsulation;

total cost 2111038N cycles. The sender spends

- 994N bytes receiving keys plus 897N bytes sending ciphertexts, and
- 44155N cycles on encapsulation;

total cost 1935155N cycles. The sender and receiver together spend 4046193N cycles. Notice that keygen is only 4% of this cost.

If the sender and receiver cannot afford the 16% extra cost of kyber768 then for Kyber they have to jump down to kyber512, which has²³ Core-SVP only 2^{118} , exacerbating risks compared to sntrup653's Core-SVP 2^{129} .

By taking a different cost limit one can construct an argument in the opposite direction, where the limit forces sntrup to a lower Core-SVP level than kyber. Keygen has negligible impact on these total-cost comparisons: the comparisons boil down to the sizes favoring sntrup at some security levels and favoring kyber at others, as in Figure 7.3.

7.14. Key generation, part 3: TLS. Typical pro-Kyber speed narratives select the new-key-for-every-ciphertext scenario as above, select an Intel CPU as above, implicitly select target security levels favorable to Kyber, and *ignore communication costs*. What is benchmarked is thus keygen+enc+dec on, e.g., Intel Haswell.

Kyber outperforms all the other small lattice KEMs in this benchmark, and advertises this as TLS performance. However, there is also ample evidence that this Kyber speedup doesn't matter, because *all* of the small lattice KEMs are much faster than necessary for this scenario. In other words, this scenario should be given very low weight in comparing the performance of these KEMs.

Let's look at the numbers. In 2017, Cloudflare $[\mathbf{214}]$ reported that 73% of its connections were TLS connections, and that 68% of its TLS key exchanges

²³ This once again gives kyber512 the benefit of switching from Core-SVP to revised-Core-SVP.



Fig. 7.15. Copy of [66, Figure 5]. Each curve in the figure shows, for one cryptosystem, the cumulative distribution of TLS 1.3 handshake speed observed in 100 experiments between two consumer-grade workstations over a fast local network. Each experiment measures the elapsed wall-clock time on the client side for 8192 sequentially established TLS 1.3 connections. The P-256 and X25519 experiments use the most optimized implementations available in OpenSSL 1.1.1 for pre-quantum ECC key exchange. The sntrup761 and sntrup857 experiments use the software from [66]. CPUs are Intel Core i7-6700 (client) and AMD Ryzen 7 2700X (server).

used the NIST P-256 elliptic curve. OpenSSL 1.1.1 (which is newer than [214]) takes 48000 Haswell cycles for NIST P-256 key generation and 235000 Haswell cycles for NIST P-256 scalar multiplication, in total 283000 cycles for Alice, and similarly 283000 cycles for Bob.

For comparison, the sntrup761 Haswell software in [66] takes 259472 cycles for keygen+enc+dec. This is the *total* of Alice's time (212558 cycles) and Bob's time (46914 cycles), so sntrup761 is more than twice as fast as NIST P-256. Beware that this comparison ignores the much higher communication costs of lattice systems compared to ECC, as noted in [66].

Macrobenchmarks of TLS connections easily see this speedup if one uses a fast local network, masking the communication costs. Figure 7.15, copied from [66], shows the results of an end-to-end experiment examining fully established TLS connections, including a new key for each connection on the client side, enc

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on the server side, dec on the client side, validation against a traditional RSA certificate, and communication over a local network. In this experiment, NIST P-256 completed 520 TLS connections/second, and sntrup761 completed 600 TLS connections/second.

Does this speedup matter for the application? Cloudflare reported in [214] that only 1.8% of its CPU cycles were spent on TLS and concluded that "Using TLS is very cheap, even at the scale of Cloudflare". Most of the public-key cycles were spent on RSA. NIST P-256, handling not just 68% of the key exchanges but also 75% of the signatures, used just 8% of the cryptographic time, i.e., 0.15% of the total CPU cycles. Evidently handling *every* connection with TLS using NIST P-256 would have consumed only 0.22% of the total CPU cycles. A hybrid deployment, protecting each connection with sntrup761 and NIST P-256, would still have consumed a negligible percentage of the total CPU cycles.

Does it matter that, in the same Haswell keygen+enc+dec benchmark, saber uses 286511 cycles, ntruhrss701 uses 359076 cycles (here one should expect speedups using the techniques of [66]), and kyber768 uses 152202 cycles? No. Again, all of these small lattice KEMs are much faster than necessary for this scenario. Increasing security levels would increase the KEM costs, but runs into communication bottlenecks long before running into computation bottlenecks; see Section 6.6.

Today most web browsing uses X25519, which is faster than NIST P-256. Does this mean that NIST P-256 is a bottleneck on Intel CPUs? No. The move to X25519 has been driven much more by other factors, such as the speed of X25519 in constrained environments (Apple deployed X25519 for ARM devices starting in 2010), the advanced X25519 software ecosystem (see, e.g., [332]), and security concerns regarding NSA-generated elliptic curves (see, e.g., [68]).

7.16. Performance overview and outlook. Ultimately what matters is whether users around the world can upgrade to post-quantum cryptography without having to compromise security. Google's first post-quantum experiment in 2016 concluded [224] that newhope1024 would be "practical to quickly deploy". The Google–Cloudflare TLS experiment [217] showed that the communication costs of ntruhrss701 had little impact on TLS, and the computation costs had even less impact, as one would expect from Sections 6.6 and 7.14.

Compared to newhope and ntruhrss, one obtains better tradeoffs between size and Core-SVP from NTRU's ntruhps, NTRU Prime's sntrup, SABER, and Kyber. See [50, Figure 3.5] and Figure 7.3. The highest Core-SVP in Figure 7.3 could be from sntrup, from ntruhps, or from saber (sometimes matched by kyber), depending on the application's exact size limit.

These TLS experiments did not include post-quantum authentication. Using post-quantum KEMs for authentication, as in [45], [48], and [333], is more efficient than using post-quantum signatures, and can be expected to double or triple the KEM costs per connection, depending on the number of authentication layers. On the other hand, servers distributing their long-term keys and perminute keys through a broadcast network such as DNS will eliminate almost all of the costs of key distribution. These improvements should put much lower weight on public-key size than on ciphertext size, and much lower weight on key-generation time than on encapsulation time and decapsulation time. In any case, communication costs will remain dominant.

There are more reasons to think that cycle counts are an issue for applications running on small devices. This is most likely to be a problem for Kyber, since the way Kyber integrates NTTs for speedups on Intel CPUs creates cross-platform slowdowns. Applications building post-quantum coprocessors to address speed bottlenecks are likely to obtain the best efficiency from ntruhps and sntrup, somewhat worse efficiency from saber, and the worst efficiency from kyber. See Section 6.5.

A critical caveat for all of these KEMs is that the attack picture is unstable. See Section 1. The Core-SVP figures in the performance graphs consider only *known* attacks. Applications can and should react in two ways. The first is to take the largest dimensions they can afford; this gives a further advantage to NTRU and NTRU Prime, which scale better than SABER and Kyber. The second is to select a KEM family designed in light of the risks from *unknown* attacks. NTRU Prime's performance profile demonstrates that eliminating as many of these risks as possible, subject to the requirement of being a small lattice KEM, is compatible with attractive performance.

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A The NISTPQC evaluation criteria

The NISTPQC procedures are mandated by a United States Federal Register notice [277] pointing to specific "evaluation criteria which will be used to assess the submissions"; those criteria are listed in [279]. This appendix reviews those criteria (minus footnotes), and compares NTRU Prime to the other lattice KEMs according to these criteria.

The "categories" inside the criteria are marked as a *proposal* and are thus not mandated. See Appendix B for analysis of these "categories".

A.1. Security. The following quotes are from [279, Section "4.A Security"].

The security provided by a cryptographic scheme is the most important factor in the evaluation.

The lattice KEMs are all designed to be secure, but the approaches to security are different. The main point of this document is that NTRU Prime's approach minimizes security risks. See especially Sections 1 through 4.

Schemes will be judged on the following factors:

4.A.1 Applications of Public-Key Cryptography NIST intends to standardize post-quantum alternatives to its existing standards for digital signatures (FIPS 186) and key establishment (SP 800-56A, SP 800-56B). These standards are used in a wide variety of Internet protocols, such as TLS, SSH, IKE, IPsec, and DNSSEC. Schemes will be evaluated by the security they provide in these applications, and in additional applications that may be brought up by NIST or the public during the evaluation process. Claimed applications will be evaluated for their practical importance if this evaluation is necessary for deciding which algorithms to standardize.

All of the lattice KEMs are designed to provide IND-CCA2 security. Many protocols can be built from an IND-CCA2 KEM. Prototype software libraries such as Open Quantum Safe [340] show how to integrate these KEMs into various applications; these integrations do not ask the KEMs for anything beyond IND-CCA2. The critical question is whether the KEMs *do* provide IND-CCA2 security.

Some KEMs provide further features that *might* have security value. As an example, recall that Kyber [26, video, 1:23:00, text on slide] advertised "shared key depends on full transcript" as one of four ways that Kyber is "conservative"; but protocols such as TLS already handle their own transcript hashing, covering more data than a KEM can see. Transcript-hashing security analyses need to be carried out at the protocol layer in any case; see, e.g., [74]. The NTRU Prime submission is like Kyber in hashing the full KEM transcript, but warns reviewers "that the security consequences of this hashing need to be formalized and proven, and that it is safer for protocols to rely on simpler promises from primitives". See [65, Section 7].

In the context of anonymity, SABER [34, Section 7] says that, as a "result of the power-of-two moduli", public keys and ciphertexts are indistinguishable from uniform random strings. It is not correct that this is limited to power-of-2 moduli: standard encoding techniques easily achieve the same for other moduli, as explained in [65, Section 4.7, "Encodings of sequences of integers"]. The widely applied pre-quantum baseline here is Elligator [70], which encodes points on Montgomery/Edwards curves over prime fields as uniform random strings.

4.A.2 Security Definition for Encryption/Key-Establishment NIST intends to standardize one or more schemes that enable "seman-

tically secure" encryption or key encapsulation with respect to adaptive chosen ciphertext attack, for general use. This property is generally denoted *IND-CCA2 security* in academic literature.

NTRU Prime was very early in recognizing KEMs with IND-CCA2 security as the most important design goal. The original NTRU Prime paper in May 2016 already included a complete reference implementation of a KEM designed for IND-CCA2 security; see [69, 2016 version, Figures 2.1–2.2] and [69, 2017 version, Appendix Z].

Kyber's predecessor NewHope had been published several months earlier [16], but with no protection against chosen-ciphertext attacks. The first version of Frodo [85] was published in June 2016, again with no protection against chosenciphertext attacks. Adding chosen-ciphertext security is often described as easy but is in fact a major source of security issues, as illustrated by [64] breaking HILA5, [39] breaking Round2, and various related risks in Table 1.1.

Concrete NTRU proposals before NTRU Prime were typically *PKEs* designed for IND-CCA2 security, more complicated than KEMs designed for IND-CCA2 security. This extra complexity is not justified by typical applications. A KEM transmitting a session key does exactly what is needed for hybrid protocols such as TLS, which use the session key together with symmetric cryptography to transmit many user messages, whereas a PKE transmitting a single user message is aiming at the wrong target.

The above security definition should be taken as a statement of what NIST will consider to be a relevant attack. Submitted KEM and encryption schemes will be evaluated based on how well they appear to provide this property, when used as specified by the submitter. Submitters are not required to provide a proof of security, although such proofs will be considered if they are available.

Regarding known attacks, the NTRU Prime submission document [65, Section 6, pages 46–70] includes a more thorough security evaluation than any of the other lattice-KEM submissions.

Regarding the risk of unknown attacks, NTRU Prime is unique in its approach of proactively eliminating unnecessary attack tools. The security benefits of this approach have already been demonstrated by subsequent attacks within latticebased cryptography, notably decryption-failure attacks and cyclotomic attacks. See Section 2.

Regarding proofs: For each KEM, there are plausible claims that QROM IND-CCA2 security of the KEM is provably related to a simpler security property of the underlying mathematical PKE. To *some* extent these claims are backed by literature giving careful proofs: see, e.g., [73]. Known proofs are

- stronger for ntruhps, ntruhrss, and sntrup than for ntrulpr (because of derandomization), and
- stronger for ntrulpr than for saber, kyber, and frodo (because of the rate of decryption failures).

Most of the attack advances in Section 1 and risks in Table 1.1 are regarding the mathematical PKEs. Available proofs did not stop these attacks and do not control these risks. See [49] and Section 5.

For the purpose of estimating security strengths, it may be assumed that the attacker has access to the decryptions of no more than 2^{64} chosen ciphertexts; however, attacks involving more ciphertexts may also be considered. Additionally, it should be noted that NIST is primarily concerned with attacks that use classical (rather than quantum) queries to the decryption oracle or other private-key functionality.

User devices are of course limited in how much computation they will carry out. The exact limit does not seem relevant to comparisons of these KEMs.

4.A.3 Security Definition for Ephemeral-Only Encryption/Key-Establishment While chosen ciphertext security is necessary for many existing applications (for example, nominally ephemeral key exchange protocols that allow key caching), it is possible to implement a purely ephemeral key exchange protocol in such a way that only passive security is required from the encryption or KEM primitive.

For these applications, NIST will consider standardizing an encryption or KEM scheme which provides semantic security with respect to chosen plaintext attack. This property is generally denoted *IND-CPA security* in academic literature.

The above security definition should be taken as a statement of what NIST will consider to be a relevant attack. Submitted KEM and encryption schemes will be evaluated based on how well they appear to provide this property, when used as specified by the submitter. Submitters are not required to provide a proof of security, although such proofs will be considered if they are available. Any security vulnerabilities that result from re-using a key should be fully explained.

All of these submissions spend some cycles on protection against chosenciphertext attacks. Some submissions advertise the potential speedup of using a key just once, but these speedups are (1) small in context and (2) outweighed by the cost of generating a new key for every ciphertext; the benefit is thus limited to a small speedup for applications that have other reasons to generate a new key for every ciphertext. Meanwhile supporting any such option creates risks, as stated in [226]: "CPA vs CCA security is a subtle and dangerous distinction, and if we're going to invest in a post-quantum primitive, better it not be fragile." Systems-security issues are not generally covered by the evaluation criteria, but NTRU Prime's focus on an IND-CCA2 KEM as the sole user interface should at least be recognized under the simplicity criterion below.

Regarding "purely ephemeral", the intent appears to have been to refer to *one-time* keys. It is important to realize that (1) "one-time" is neither necessary nor sufficient for "ephemeral"; (2) "ephemeral" is the right concept for the security goal at hand; and (3) this affects performance evaluations. See Section 6.3.

4.A.4 Security Definition for Digital Signatures [not relevant here; omitted]

4.A.5 Security Strength Categories [covered in Appendix B]

4.A.6 Additional Security Properties While the previously listed security definitions cover many of the attack scenarios that will be used in the evaluation of the submitted algorithms, there are several other properties that would be desirable:

One such property is perfect forward secrecy. While this property can be obtained through the use of standard encryption and signature functionalities, the cost of doing so may be prohibitive in some cases. In particular, public-key encryption schemes with a slow key generation algorithm, such as RSA, are typically considered unsuitable for perfect forward secrecy. This is a case where there is significant interaction between the cost, and the practical security, of an algorithm.

The SUPERCOP benchmarking framework reports RSA-2048 key generation (using OpenSSL 1.1.1) taking a median of 257 million cycles on a Haswell core (hiphop, supercop-20210604). For comparison, [66] reports just 156317 cycles per key for sntrup761: 1600 times faster, with a much higher security level against known attacks. The overall CPU cycles used per TLS 1.3 handshake in [66], including a new sntrup761 key for each handshake, are smaller than the cycles used on the same CPU for X25519, the standard choice of curve for TLS 1.3; a hybrid of both cryptosystems is easily affordable and is less expensive than NIST P-256, the other standard option in TLS 1.3. See also Sections 6.3 and 7.14. NTRU Prime also has an ntrulpr option with even faster key generation, but this option is overkill for purposes of this evaluation criterion.

Available NTRU software does not yet include keygen speedups analogous to [66], but it would be unreasonable to claim on this basis that NTRU does not support "perfect forward secrecy". If there is a performance problem with new keys for any of these KEMs, the problem is much more likely to come from communication cost than from computation time. See Section 6.6.

Another case where security and performance interact is resistance to side-channel attacks. Schemes that can be made resistant to side-channel attack at minimal cost are more desirable than those whose performance is severely hampered by any attempt to resist side-channel attacks. We further note that optimized implementations that address side-channel attacks (e.g., constant-time implementations) are more meaningful than those which do not.

The NTRU Prime C software, both reference and optimized, has always been constant-time. As mentioned in Section 3.6, the software is written within a framework that verifies immunity to timing attacks. On the other hand, any KEMs not in this state should be expected to catch up eventually.

As for more invasive side channels: SABER was early in producing a masked implementation, but that implementation was broken by [280]. Protection "at minimal cost" does not seem to be achievable for any of these KEMs; see [28].

The path towards NTRU Prime implementations with advanced side-channel protection is described in Section 7.10.

In principle, masking of any order can be applied to any computation, but the costs depend on the computation. For example, linear operations, such as multiplication by a public ring element, are generally less expensive to mask than non-linear operations, such as hashing. It is thus not safe to predict costs of masked implementations given only costs of unmasked implementations.

The SABER submission suggests [34, Section 7] that prime moduli are a speed problem for conversion between arithmetic masking and xor masking. However, these conversions are only a small part of the cost of a masked KEM, whereas NTRU and NTRU Prime should be expected to have larger advantages in bit operations for multiplications; see Section 6.5. The SABER submission also suggests that masked noise generation is a speed problem compared to rounding; NTRU Prime uses rounding too.

In the aforementioned context of anonymity, the SABER submission says "Saber is naturally constant time over different public keys in contrast to primemoduli schemes". This issue applies to the prime moduli in Kyber but does *not* apply to the prime moduli in NTRU Prime. There is a different issue for kyber90s and ntrulpr, namely that if the symmetric primitive used for LPR generator expansion is chosen to be AES then on many platforms the public key will leak through timing. None of this applies to sntrup.

A third desirable property is resistance to multi-key attacks. Ideally an attacker should not gain an advantage by attacking multiple keys at once, whether the attacker's goal is to compromise a single key pair, or to compromise a large number of keys.

Most cryptanalytic papers focus on single-target attacks. The exceptions have found many speedups for multi-target attacks; see, e.g., the discrete-logarithm speedups listed in [46]. So there is clearly a risk here, and the risk has even less evaluation than the risk of single-target attacks.

Some submissions advertise public-key hashing in the context of multi-target protection. (NTRU Prime also hashes the public key.) Public-key hashing can help against *some* types of multi-target attacks. However, the general multitarget attack surface for lattice KEMs is much larger than the corner addressed by public-key hashing.

Fortunately, the multi-target risk has a clear quantitative limit that is small enough to be useful. The NTRU Prime submission [65, Section 7.1] says "We recommend handling multi-target attacks by aiming for a very high single-target security level, and then relying on the fact that T-target attacks gain at most a factor T. This approach is simple and effective, and is not much more expensive than merely stopping single-target attacks."

A final desirable, although ill-defined, property is resistance to misuse. Schemes should ideally not fail catastrophically due to isolated coding errors, random number generator malfunctions, nonce reuse, keypair reuse (for ephemeral-only encryption/key establishment) etc.

See above regarding implementation security.

4.A.7 Other Consideration Factors As public-key cryptography tends to contain subtle mathematical structure, it is very important that the mathematical structure be well understood in order to have confidence in the security of a cryptosystem. To assess this, NIST will consider a variety of factors. All other things being equal, simple schemes tend to be better understood than complex ones. Likewise, schemes whose design principles can be related to an established body of relevant research tend to be better understood than schemes that are completely new, or schemes that were designed by repeatedly patching older schemes that were shown vulnerable to cryptanalysis.

All of the lattice KEMs score poorly on this criterion. If "well understood" is a yes-no prerequisite for standardization then none of these KEMs should be standardized. See Section 1.

A closer look shows that some of the KEMs are structurally immune to *some* classes of lattice attacks. See Table 1.1. In particular, NTRU Prime's ntrulpr structurally avoids three risks incurred by saber and kyber without incurring any additional risks, and NTRU Prime's sntrup structurally avoids two risks incurred by ntruhrss and ntruhps without incurring any additional risks.

The comparison with Frodo is more difficult, since Frodo incurs some risks while avoiding others. See Section 4.

Regarding "simple schemes", the original NTRU Prime reference software [69, Appendix Z] fits into two pages, and all of the subsequent lattice KEMs under consideration have similarly concise descriptions. However, it is important to realize that the *security analysis* is vastly more complicated. See the many papers listed in Section 1. This is not a new phenomenon in cryptography: consider, e.g., RC4, which is a very simple scheme but has a very complicated security analysis.

NIST will also consider the clarity of the documentation of the scheme and the quality of the analysis provided by the submitter. Clear and thorough analysis will help to develop the quality and maturity of analysis by the wider community. NIST will also consider any security arguments or proofs provided by the submitter. While security proofs are generally based on unproven assumptions, they can often rule out common classes of attacks or relate the security of a new scheme to an older and better studied computational problem.

In addition to NIST's own expectations for the scheme's long-term security, NIST will also consider the judgment and opinions of the broader cryptographic community.

See above regarding proofs and quality of analysis. Regarding maturity of analysis, NTRU Prime is the oldest of the lattice KEMs under consideration; see the "instability" rows in Table 1.1.

A.2. Cost. The following quotes are from [279, Section "4.B Cost"].

As the cost of a public-key cryptosystem can be measured on many different dimensions, NIST will continually seek public input regarding which performance metrics and which applications are most important. If there are important applications that require radically different performance tradeoffs, NIST may need to standardize more than one algorithm to meet these diverse needs.

The available evidence indicates that communication costs are dominant for these lattice KEMs; see Section 6.6. All of the lattice KEMs except for Frodo have similar communication costs; see Figure 7.3.

Other metrics show larger performance differences, depending on the metric and the target security level. See, e.g., Figures 7.5 and 7.6. However, it seems difficult to point to diverse application performance requirements as an argument for standardizing more than one of NTRU, NTRU Prime, SABER, and Kyber.

4.B.1 Public Key, Ciphertext, and Signature Size Schemes will be evaluated based on the sizes of the public keys, ciphertexts, and signatures that they produce. All of these may be important consideration factors for bandwidth-constrained applications or in Internet protocols that have a limited packet size. The importance of public-key size may vary depending on the application; if applications can cache public keys, or otherwise avoid transmitting them frequently, the size of the public key may be of lesser importance. In contrast, applications that seek to obtain perfect forward secrecy by transmitting a new public key at the beginning of every session are likely to benefit greatly from algorithms that use relatively small public keys.

NTRU Prime scores well here—as do NTRU, SABER, and Kyber. The winner depends on the target security level. See Figure 7.3.

In "bandwidth-constrained applications or in Internet protocols that have a limited packet size", the highest security could be from NTRU Prime, or from NTRU, or from SABER (in some cases matched by Kyber), depending on the exact size constraint. Again, see Figure 7.3.

Each of these KEMs has public-key size close to ciphertext size, but there are slight differences, as illustrated by sntrup (smaller ciphertexts) and ntrulpr (smaller keys), so it is helpful to establish the role of these sizes:

- In applications that transmit a new key for every ciphertext, the correct metric is pk+ct.
- In applications where keys are cached, the correct metric is ct plus a fraction of pk that depends on the cache effectiveness. Even if the cache works for only 90% of the key lookups, the effective fraction of pk drops to 10%, and for these KEMs the resulting metric is tantamount to simply ct.

Given the $\approx 2 \times$ performance improvement from caching, the first step towards improving performance in performance-sensitive applications is to cache keys,

so the ct scenario should be given higher weight than the pk+ct scenario in performance evaluations. See [258] and Section 3.1. Contrary to popular belief, "forward secrecy" is fully compatible with the first scenario; see Section 6.3. Regarding TLS, see Section 7.16.

4.B.2 Computational Efficiency of Public and Private Key Operations Schemes will also be evaluated based on the computational efficiency of the public key (encryption, encapsulation, and signature verification) and private key (decryption, decapsulation, and signing) operations. The computational cost of these operations will be evaluated both in hardware and software. The computational cost of both public and private key operations is likely to be important for almost all operations, but some applications may be more sensitive to one or the other. For example, signing or decryption operations may be done by a computationally constrained device like a smartcard; or alternatively, a server dealing with a high volume of traffic may need to spend a significant fraction of its computational resources verifying client signatures.

NTRU Prime scores well here too, and is again often the winner, depending on the application environment. See, e.g., Figures 7.5 and 7.6 for encapsulation and decapsulation operations on "a computationally constrained device like a smartcard", specifically on an ARM Cortex-M4 microcontroller, and Figure 7.9 for secret-key size.

Regarding "evaluated both in hardware and software", it is important to realize that almost all NISTPQC speed evaluations have been in environments with fast multipliers. This includes Cortex-M4 evaluations and FPGA evaluations. True hardware evaluations—ASIC evaluations—see the intrinsic cost of each multiplication; as noted earlier, this is favorable to NTRU and NTRU Prime, less favorable to SABER, and unfavorable to Kyber. See Section 6.5.

4.B.3 Computational Efficiency of Key Generation Schemes will also be evaluated based on the computational efficiency of their key generation operations, where applicable. As noted in Section 4.A.6, the most common scenario where key generation time is important is when a public-key encryption algorithm or a KEM is used to provide perfect forward secrecy. Nonetheless, it is possible that key generation times may also be important for digital signature schemes in some applications.

See above regarding key generation.

4.B.4 Decryption Failures Some public-key encryption algorithms and KEMs, even when correctly implemented, will occasionally produce ciphertexts that cannot be decrypted/decapsulated. For most applications, it is important that such decryption failures be rare or absent. For algorithms with decryption/decapsulation failures, submitters must provide the failure rate, as well as an analysis of the impact on security that these failures could cause. While applications can always obtain an

acceptably low decryption failure rate by encrypting the same plaintext multiple times, and interactive protocols can simply restart when key establishment fails, these types of solutions have their own performance costs.

The view of decryption failures as a *performance* issue is not relevant to any of these KEMs: decryption failures are too rare to be triggered by normal usage. However, there is a *security* risk: *attack* algorithms can search many more ciphertexts, and perhaps can recognize failing ciphertexts. See Section 3.9 and Section 5.7. From this security perspective, NTRU and NTRU Prime have an advantage over Kyber, SABER, and Frodo: NTRU and NTRU Prime are perfectly correct, i.e., have no decryption failures.

A.3. Flexibility, simplicity, adoption. The following quotes are from [279, Section "4.C Algorithm and Implementation Characteristics"].

4.C.1 Flexibility Assuming good overall security and performance, schemes with greater flexibility will meet the needs of more users than less flexible schemes, and therefore, are preferable.

Some examples of "flexibility" may include (but are not limited to) the following:

a. The scheme can be modified to provide additional functionalities that extend beyond the minimum requirements of public-key encryption, KEM, or digital signature (e.g., asynchronous or implicitly authenticated key exchange, etc.).

See above regarding IND-CCA2 etc. More advanced protocols such as fully homomorphic encryption cannot be built from these KEMs; see Section 1.4.

b. It is straightforward to customize the scheme's parameters to meet a range of security targets and performance goals.

This is a clear win for NTRU Prime compared to SABER and Kyber. See below regarding NTRU.

Structurally, SABER and Kyber support only dimensions that are multiples of 256, while NTRU Prime supports many intermediate dimensions. See Section 6.7; also compare the large kyber jumps in, e.g., Figure 7.6 to the demonstrated flexibility of sntrup (red dots).

Many more NTRU Prime parameter sets, including intermediate dimensions and larger dimensions, have their Core-SVP levels and sizes displayed in [65, pages 104–111]. The generation and Core-SVP evaluation of parameter sets are fully automated.

As noted in Section 4, larger parameter sets appear problematic for Kyber. There are two basic reasons for this:

• The Kyber modulus 3329 causes increasing decryption-failure difficulties as the dimension increases. It is not clear how to compensate for this without (1)

significantly reducing Core-SVP or (2) revisiting Kyber's structural decision to use the same modulus for all parameter sets.

• The matrix structure of Kyber produces quadratic slowdowns. For example, merely *generating* the matrix reportedly takes 318000 Cortex-M4 cycles for kyber768 and 713000 Cortex-M4 cycles for kyber1024, and each operation also has to *use* the entire matrix.

Interestingly, even though Kyber's limitation to $(\mathbb{Z}/3329)[x]/(x^{256}+1)$ creates these difficulties and directly damages Kyber's performance in, e.g., Figure 7.5, Kyber advertises this as a performance feature: "Optimized implementations only have to focus on a fast dimension-256 NTT and a fast Keccak permutation. This will give very competitive performance for all parameter sets of Kyber." This is listed in [25, Section 7] as the first "unique advantage" of Kyber. This advantage is summarized as "Ease of implementation", but the way that NTTs are baked into the Kyber specification makes simple implementations of Kyber more complicated than simple implementations of competitors; evidently the intent was to say how easy optimized implementations are.

For comparison, the AVX2-optimized software released for NTRU Prime for all parameter sets is automatically generated from a unified code base, sharing a size-512 NTT across all sizes. The reference software is also shared across sizes. The same generator trivially produces optimized code for intermediate dimensions that Kyber, because of its module structure, is structurally incapable of handling. The Kyber submission is incorrect when it claims [25, Section 6.4.3] that changing ring requires "completely re-implementing all the operations".

It is easy to see from the NTRU specification that NTRU can be customized to meet a range of security targets and performance goals, the same way that NTRU Prime can. The reason this is less obvious for NTRU than for NTRU Prime in Figure 7.5 is that NTRU has specified only four ntruhps sizes and two ntruhrss sizes, and has released optimized code for fewer sizes. The NTRU documentation includes Core-SVP evaluations of many more parameter sets.

c. The algorithms can be implemented securely and efficiently on a wide variety of platforms, including constrained environments, such as smart cards.

All of these KEMs can be implemented on a wide range of platforms, except that Frodo would have trouble on *extremely* small devices. Efficiency varies: see Section 7.4 for NTRU Prime's performance wins on an ARM Cortex-M4 microcontroller. Kyber is unusual in having its performance profile tilted towards large devices; see Section 6.5.

Regarding secure implementation, important challenges include protecting against side channels (see above) and eliminating bugs. Some submissions point to specific implementation features (e.g., power-of-2 moduli in Table 1.1 mean that software does not have to reduce modulo odd primes) and claim that these features are important advantages, but these claims are negatively correlated with observed implementation security. For example, the Frodo submission [15, Section 6.1, "Ease of implementation"] stated as Frodo's first "advantage" that

"One of the features of FrodoKEM is that it is easy to implement and naturally facilitates writing implementations that are compact and run in constant-time", but Frodo's official software was then

- broken by the timing attack of [169],
- modified to avoid the timing attacks, and then
- broken by an even easier attack [321] because of a bug in the modification.

The NTRU Prime submission is at the opposite extreme. The NTRU Prime web pages have always included a dedicated "Warnings" page [284] covering, among other things, the need for software review. The software has, since 2019, been factored into modules with separate tests and optimizations. As mentioned in Section 3.6, new tools [61] have computer-verified for most of those modules that

- the existing AVX2-optimized implementation and
- the existing (simpler) reference implementation

produce the same outputs for all possible inputs. Some modules, notably the multiplier, are too large for the current tools to handle, but [66, Appendix A] announced computerized range-checking of the critical NTT software inside the AVX2-optimized multiplier. Work is continuing towards verification of all subroutines and towards verification for more platforms.

d. Implementations of the algorithms can be parallelized to achieve higher performance.

All of the lattice KEMs have demonstrated successful vectorization. Further parallelization is clearly possible for the multiplications. Parallel hash functions are also available, although care is required regarding security; see Sections 3.15 and 3.16.

e. The scheme can be incorporated into existing protocols and applications, requiring as few changes as possible.

It isn't clear what this means for KEMs beyond the performance requirements (e.g., ciphertext size). Aside from performance, all of the KEMs have the same externally visible data flow.

4.C.2 Simplicity The submitted scheme will be judged according to its relative design simplicity.

The KEM designs have many similarities, but each KEM (even if claimed to be "simple") has complications that do not appear in some of the other KEMs. For example:

• The underlying PKEs are simpler for Quotient NTRU than for Product NTRU: for example, the Quotient NTRU ciphertext is just Gb + d, whereas the Product NTRU ciphertext is (Gb + d, Ab + M + c).

- Product NTRU needs extra work for derandomization, while Quotient NTRU does not.
- Combining polynomials with matrices (modules, as in Kyber and SABER) is more complicated than using just polynomials (NTRU, NTRU Prime) or using just matrices (Frodo).
- Using binomials $x^n + 1$ (Kyber, SABER) is simpler than using $x^p x 1$ (NTRU Prime), which in turn is simpler than using $x^p 1$ with its factor x 1 (NTRU).
- Kyber's NTT-based representation of polynomials is more complicated than the traditional representation used in NTRU, NTRU Prime, and SABER.
- The error distribution in Frodo is more complicated than the sum-of-bits error distributions in Kyber and SABER and the ternary distributions in NTRU and NTRU Prime.

The best way to see what varies from one KEM to another is through detailed cross-KEM comparison charts, as in [49, pages 48–52] and Table 1.1. Another source of information regarding Quotient NTRU vs. Product NTRU is the NTRU Prime software, which is mostly shared but has, e.g., inversion code only for Quotient NTRU and derandomization code only for Product NTRU.

4.C.3 Adoption Factors that might hinder or promote widespread adoption of an algorithm or implementation will be considered in the evaluation process, including, but not limited to, intellectual property covering an algorithm or implementation and the availability and terms of licenses to interested parties. NIST will consider assurances made in the statements by the submitter(s) and any patent owner(s), with a strong preference for submissions as to which there are commitments to license, without compensation, under reasonable terms and conditions that are demonstrably free of unfair discrimination.

This is a problem for ntrulpr, saber, and kyber, and appears to be decisive regarding those KEMs since the call for submissions says that it is "critical that this process leads to cryptographic standards that can be freely implemented in security technologies and products". See Sections 3.17 and 4.3.

OpenSSH integrated the round-1 version of sntrup761 as a fully supported SSH key-exchange option in April 2019, and replaced it with the current version of sntrup761 (which has the same underlying mathematical PKE) in March 2021 [291]. OpenBSD now [290] supports sntrup761 for IPsec key exchange. Google and Cloudflare have carried out web-browsing experiments [217] with a variant of ntruhrss701.

B The NISTPQC categories

The standard scientific way to understand tradeoffs between two variables—for example, performance vs. security—is with two-dimensional scatterplots, such

as Figures 7.3, 7.5, 7.6, and 7.9. There are some common pitfalls in the graphing process, but these pitfalls are avoidable. See [50, Section 3].

The scatterplots are only as good as the data they use. For the figures above, the numbers on the vertical axis are Core-SVP; recall from Section 3.4 that Core-SVP is a combination of underestimates, overestimates, possible underestimates, and possible overestimates of the number of operations used in certain lattice attacks. The standard scientific reaction to crude models is to study the topic more closely and build better models, as in [65, Section 6].

Comparing costs of attacks with different mixes of operations—for example, asking whether a lattice attack is as cheap as an attack against AES-128—generally requires defining a cost metric, a way to compute a cost for each operation. The details are important. There are many examples in the literature of algorithms where different choices of cost metrics make a huge difference in algorithm cost, often reversing comparisons between algorithms and comparisons between problems. See, e.g., [91] and [211, Section 5.4].

With these issues and standard scientific practices in mind, let's look at what NIST tried to accomplish with its "categories", and what actually happened.

B.1. Symmetric primitives as a security floor. NIST's August 2016 draft of the evaluation criteria [278, Section 4.A.4, "Target security strengths"] began by asking submitters "to provide parameter sets that meet or exceed each of five target security strengths". Submitters were asked to be "confident that the specified security target is met or exceeded".

"Strength" 1 was labeled as "128 bits classical security / 64 bits quantum security", the stated intent being to be at least as secure as "brute-force attacks against AES-128". "Strength" 2 was labeled as "128 bits classical security / 80 bits quantum security", the stated intent being to be at least as secure as "brute-force collision attacks against SHA-256/ SHA3-256". Et cetera.

This approach was critiqued in [46]:

Quantitatively comparing post-quantum public-key security levels is going to be a nightmare. I see only two ways that submitters a year from now can possibly be "confident that the specified security target is met or exceeded": (1) overkill; (2) overconfidence. Many users will not be satisfied with overkill, and NIST should not encourage overconfidence.

Various illustrative examples and (correct) predictions of evaluation problems appeared in [46], along with a proposal to focus on the problem at hand ("Scrap the requirement of a pre-quantum security analysis. Users will use cheap ECC hybrids to obtain the pre-quantum security that they want") and to prioritize accuracy ("Ask them to do the most accurate job that they can of analyzing post-quantum security. Don't ask for fake confidence").

B.2. Pseudo-definitions of "category" boundaries. NIST's final call for submissions [279] in December 2016 renamed "strengths" as "categories", said that each "category" will be defined by a symmetric primitive "whose security will serve as a floor", and made some changes in the details—including a giant leap of security for SHA3-256, which was assigned

- "128 bits classical security / 80 bits quantum security" in [278], but
- " 2^{146} classical gates" with no quantum speedup in [279].

The leap from 2^{80} to 2^{146} is much larger than can be explained by a switch from counting hash calls to counting "gates".

The Brassard–Høyer–Tapp algorithm [90] finds SHA3-256 collisions in about 2^{83} quantum operations. These operations use far fewer than 2^{146} quantum "gates" with the set of "gates" defined by, e.g., Ambainis [17]. Clearly NIST must have in mind a more restrictive set of "gates"—but which set? A definition is critical for comparisons. Five years later, despite clarification requests, NIST's "categories" still do not have clear definitions. See [53, Section 5.4] for further analysis.

B.3. Lattice guesswork. Analyzing the number of operations used in known lattice attacks for cryptographic sizes remains an open research problem—see [65, Section 6]—even if faster attacks are assumed not to exist. Lattice submissions in round 1 of NISTPQC took different approaches to handling (1) the unknown number of operations used in these algorithms and (2) the lack of clarity from NIST regarding how to assign operation counts to "categories".

Let's look in particular at Kyber. Like NewHope, Kyber calculated "the core-SVP hardness" of its parameter sets, and said [24, page 17] that this is "a very conservative lower bound on the cost of an actual attack". However, this "lower bound" was far below 2^{128} for kyber512. So the submission argued that, because of various other unquantified factors, kyber512 was still as difficult to break as AES-128, meaning that kyber512 qualified for "category" 1.

But is it true that kyber512 qualified for "category" 1? Does the round-3 kyber512, a patched version of the previous kyber512, qualify for "category" 1? The answers to these questions remain unclear even if, again, faster attacks are assumed not to exist. See Section 1.3.

B.4. How "categories" have damaged analyses. See [53] for examples of how NIST's emphasis on "categories" has damaged performance evaluations. What follows is an example of how the same emphasis has damaged security evaluations.

Consider the problem of solving dimension- β SVP. The best enumeration algorithms known have exponent $\Theta(\beta \log \beta)$. The best sieving algorithms known have exponent $\Theta(\beta)$, and are therefore faster for all sufficiently large β .

However, this does not say anything about concrete sizes. It is important to realize that the slow growth of $\log \beta$ makes the cutoff between sieving and enumeration extremely sensitive to improvements in either algorithm. See the numerical examples in [65, Section 6.9].

When NISTPQC began, enumeration had asymptotic exponent approximately $\approx 0.187\beta \log_2 \beta$, and sieving had asymptotic exponent $\approx 0.292\beta$. Including lowerorder terms is more favorable to enumeration than to sieving, and quantum computers chop the enumeration exponent in half (see [18]) while reducing the sieving exponent by only about 10%, but all this is outweighed by $\log_2 \beta$ once β is large enough. The question is how large. The round-1 Kyber submission stated [24, Section 5.1.1] that "sophisticated enumeration, with serious optimization ... and with quantum speedups", was outperformed by sieving "for dimensions larger than 250, quite possibly already earlier", and was thus not a threat since the "smallest dimension that we are interested in for the cryptanalysis of Kyber is 390". The sieving-vs.-enumeration comparison was under an assumption "that access into even exponentially large memory is free"; this is important because sieving is memory-intensive while enumeration is not.

In other words, the submission was saying that sieving-with-free-memory was cheaper than enumeration. Obviously it's also cheaper than sieving-with-realmemory, so an attack-cost analysis based on sieving-with-free-memory would be a lower bound for an attack-cost analysis based on enumeration or sieving-withreal-memory.

Then attacks advanced. Section 1 noted dramatic recent improvements in enumeration speed, including [9], which reduced the asymptotic exponent from $\approx 0.187\beta \log_2 \beta$ to $\approx 0.125\beta \log_2 \beta$, and [10, page 547], which estimated the new cutoff between quantum enumeration and quantum sieving as $\beta = 547$, far above the 250 mentioned above. Every proposed lattice system with pre-quantum Core-SVP below about 2^{160} has, as a direct result of these attacks, less post-quantum security than previously believed.

Seeing exponents drop by $1.5\times$, and seeing various post-quantum proposals suddenly having less post-quantum security, perfectly illustrates the instability of security analysis of lattice-based cryptography. There is a clear risk of further enumeration improvements further damaging post-quantum security and even damaging pre-quantum security. The advance in enumeration also means that the known pre-quantum-to-post-quantum loss for lattice systems is larger than it was before. But now let's look at what "categories" do to this analysis.

AES has an even larger pre-quantum-to-post-quantum loss. The question of whether a lattice KEM reaches the AES floor is thus unable to see the known loss of post-quantum security. As a direct result of NIST's promotion of "categories", this question is emphasized in [10, Section 1], which says that "this work does not invalidate the claimed NIST Security Level" of the affected systems.

The round-3 Kyber submission claims that round-3 kyber512 qualifies for "category" 1, as hard to break as AES-128. The analysis in the submission says that a sieving attack on round-3 kyber512 could use as few as $2^{135.5}$ "gates"—which, according to the submission, is possible for known attacks—would not be "catastrophic, in particular given the massive memory requirements that are ignored in the gate-count metric". Let's review the logic here:

- The round-3 submission says a sieving attack against round-3 kyber512 could use hundreds of times fewer "gates" than an AES-128 attack.
- How, then, does round-3 kyber512 qualify for "category" 1? Answer: this potential speedup over AES-128 is sieving-with-free-memory; sieving-with-real-memory is much more expensive. This answer sounds reasonable if the cost metric includes realistic costs for memory.

• But what about enumeration? Answer: enumeration doesn't matter, since for these sizes it seems more expensive than sieving-with-free-memory. This answer sounds reasonable if the cost metric includes free memory—but then how can kyber512 qualify for "category" 1?

NIST's "categories" are being interpreted as allowing free memory for purposes of skipping an enumeration analysis, but as including realistic costs for memory for purposes of allowing kyber512 to qualify for "category" 1. These inconsistent interpretations are enabled by NIST's continued failure to define a cost metric.

A back-of-the-envelope calculation suggests that—assuming realistic memory costs, as kyber512 does in its argument to qualify for "category 1"—the latest enumeration algorithms are

- easily the fastest known quantum attacks against kyber512 and
- quite possibly the fastest known non-quantum attacks against kyber512—more research is needed regarding lower-order factors.

Even if the algorithms are still slower than AES-128 key search, this change in the security picture is big news. Focusing on "categories" suppresses this news. Scientific quantification is buried under the question of whether a KEM reaches a particular "floor".

B.5. What "categories" were supposed to accomplish. Recall that the "Target security strengths" section of NIST's August 2016 draft [278] started by asking submitters "to provide parameter sets that meet or exceed each of five target security strengths". NIST's goal was clear: to be able to say that each of the symmetric primitives was matched or surpassed in strength by a post-quantum parameter set.

In the final call [279], this no longer sounded like the goal of the "categories". Instead "categories" were portrayed as addressing other problems. Let's look at this list of problems and at how "categories" have failed to help.

NIST anticipates that there will be significant uncertainties in estimating the security strengths of these post-quantum cryptosystems. These uncertainties come from two sources: first, the possibility that new quantum algorithms will be discovered, leading to new cryptanalytic attacks; and second, our limited ability to predict the performance characteristics of future quantum computers, such as their cost, speed and memory size.

This is a surprisingly limited list of sources of uncertainty regarding security. What about advances in *non-quantum* algorithms? What about difficulties in analyzing the *number of operations* used in algorithms, even when the cost of each operation is well understood?

The normal scientific way to handle uncertainties regarding the performance of future quantum computers is to analyze multiple possibilities. For example, [47] reviewed the difficulty of carrying out 2^{128} operations and then compared the following possibilities:

• "quantum op costs 2¹⁰ pre-q ops \Rightarrow 2¹¹⁸ quantum ops aren't a threat"

- "quantum op costs 2^{20} pre-q ops $\Rightarrow 2^{108}$ quantum ops aren't a threat"
- "quantum op costs 2^{30} pre-q ops $\Rightarrow 2^{98}$ quantum ops aren't a threat" "quantum op costs 2^{40} pre-q ops $\Rightarrow 2^{88}$ quantum ops aren't a threat"
- "quantum op costs 2^{50} pre-q ops $\Rightarrow 2^{78}$ quantum ops aren't a threat"

Different possibilities could end up favoring different submissions: for example, aiming for more attack operations is less of an issue for a submission with linear scalability than for a submission with quadratic scalability.

As for the possibility of unknown attacks, cryptographers traditionally handle this possibility by searching for attacks—and, sometimes, by explicitly managing risks of attack advances, as in NTRU Prime.

In order to address these uncertainties, NIST proposes the following approach. Instead of defining the strength of a submitted algorithm using precise estimates of the number of "bits of security," NIST will define a collection of broad security strength categories. Each category will be defined by a comparatively easy-to-analyze reference primitive, whose security will serve as a floor for a wide variety of metrics that NIST deems potentially relevant to practical security.

NIST's pseudo-definitions of low-precision "categories" haven't done anything to address uncertainties regarding new quantum algorithms and the performance of quantum computers. On the contrary, by emphasizing comparisons to AES and thus emphasizing pre-quantum security, these "categories" took attention away from what should have been the whole point of NISTPQC: protecting against quantum computers. This was predictable, and was predicted in [46].

A given cryptosystem may be instantiated using different parameter sets in order to fit into different categories. The goals of this classification are:

1. To facilitate meaningful performance comparisons between the submitted algorithms, by ensuring, insofar as possible, that the parameter sets being compared provide comparable security.

Standard scientific scatterplots, such as Figure 7.3, directly show securityperformance tradeoffs (1) within each submission and (2) across submissions. Low-precision "categories" are a giant step backwards in comparisons, hiding most of this information. Compare [53, Figures 4.2 and 4.5] for a visualization of how much information is lost.

2. To allow NIST to make prudent future decisions regarding when to transition to longer keys.

Everyone who has an idea of what "category 1" means also knows how to compare a number to 128.

3. To help submitters make consistent and sensible choices regarding what symmetric primitives to use in padding mechanisms or other components of their schemes requiring symmetric cryptography.

Most submissions, including NTRU Prime, sensibly choose to use ≥ 256 -bit symmetric cryptography everywhere, even when the target security level is lower. This simplifies the review process and has negligible impact on performance. A few submissions, such as Frodo, use smaller symmetric cryptography to be "consistent" with the target security level.

4. To better understand the security/performance tradeoffs involved in a given design approach.

This differs from #1 above: #1 was about comparisons between designs, whereas #4 is about tradeoffs inside one design. "Categories" don't completely hide the jumps in Kyber cost—where's the "category 2" Kyber parameter set?— but scientific scatterplots such as Figure 7.5 do much better at showing the full picture.

In accordance with the second and third goals above, NIST will base its classification on the range of security strengths offered by the existing NIST standards in symmetric cryptography, which NIST expects to offer significant resistance to quantum cryptanalysis. In particular, NIST will define a separate category for each of the following security requirements (listed in order of increasing strength):

[breaking categories 1, 2, 3, 4, 5 "must require computational resources comparable to or greater than those required for" AES-128 key search, SHA-256 collision search, AES-192 key search, SHA-384 collision search, AES-256 key search; resource-estimation details omitted]

NIST asks submitters to provide a preliminary classification, according to the above categories, for all parameter sets that they intend to be considered for standardization. All submitters are advised to be somewhat conservative in their preliminary classifications, but submitters of algorithms where the complexity of the best known attack has recently decreased significantly, or is otherwise poorly understood, should be especially conservative.

Here NIST's goal is again clear: to be able to say that each of the symmetric primitives is matched or surpassed in strength by a post-quantum parameter set. This is, however, a distraction from the scientific task of evaluating attacks.

NIST will not require submitters to provide distinct parameter sets for all five security strength categories. Submitted parameter sets meeting the requirements of a higher category will be automatically considered to meet the requirements of all lower categories. Submitters may also provide more than one parameter set in the same category, in order to demonstrate how parameters can be tuned to offer better performance or higher security margins.

The reference to "higher security margins" within "the same category" suggests that "categories" are not the end of the story. However, NIST's text puts much more emphasis on "categories", and these "categories" have played a major role in NISTPQC. Scatterplots do a much better job of showing the same information.

NIST recommends that submitters primarily focus on parameters meeting the requirements for categories 1, 2 and/or 3, since these are likely to provide sufficient security for the foreseeable future. To hedge against future breakthroughs in cryptanalysis or computing technology, NIST also recommends that submitters provide at least one parameter set that provides a substantially higher level of security, above category 3. Submitters can try to meet the requirements of categories 4 or 5, or they can specify some other level of security that demonstrates the ability of their cryptosystem to scale up beyond category 3.

All of this could have been stated just as easily in terms of quantitative security levels.

B.6. The way forward. What the evaluation criteria *require* is evaluations of security and performance.

Cost evaluations of known attacks should be quantified. In recognition of the limited evaluation time and the need for clarity regarding the security goals, NIST should pick a *very short* list of *clearly defined* cost metrics for attacks. Comparisons of security-performance tradeoffs should use standard scientific scatterplots. "Categories" have damaged this process and should be scrapped.

For lattice systems in particular, there are huge error bars in current estimates of the cost of known attacks. Consider again the round-3 Kyber presentation saying that the fastest attack known against kyber512 uses somewhere between $2^{135.5}$ and $2^{165.5}$ "gates"—never mind the question of exactly which "gates" are allowed. These error bars should be included in security comparisons between lattice systems and other systems. Note that "categories" damage this process too: they have far too low precision to communicate error bars.

For comparing security-performance tradeoffs among lattice KEMs, whether one takes a low, medium, or high "gate" count for attack cost, it *seems* that the results are highly correlated with Core-SVP. Lattice-comparison scatterplots that use Core-SVP do not obviously need to be discarded. However, it would be better to upgrade from Core-SVP to a more realistic estimate. NIST should designate a shared cost-estimation method to be used consistently across all lattice submissions.