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Subject: Received Comments 9/1-18
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Attachments: [com9_1-18.zip](#)

During weekend, when I read the comments, I found that some of the comments are in txt version in the e-mails and hard to read. Then I converted to word version to read. I collected the comments received after September 1 in the attached zip file, for every one to read. I named them by commenter's last name except the one submitted by Microsoft, which is a PDF file.

The main commented areas are (in order of the number of comments). I think we will need to further separate the comments to the each topics.

1. Quantum security strength
2. Key exchange (KEM vs. DHish)
3. IPR
4. Hybrid mode

I did not look into comments received before September 1, which are not many. Please include if you can check the e-mails from August 2 to September 1. (I also might miss some after September 1)

Lily

**Public Comments Received on the
Draft Proposed Submission Requirements and Evaluation Criteria
for the Post-Quantum Cryptography Standardization Process**
(Comment Period Closed: 9/16/2016)

Dear Ms. Chen,

Thank you for the opportunity to submit comments on the Submission Requirements and Evaluation Criteria to be used in NIST's process for standardizing quantum-resistant public-key cryptographic algorithms.

Mozilla's mission as a non-profit organization is to promote openness, innovation, and opportunity online. Protecting the security of Internet communications is a core part of that mission. Mozilla is a major user of cryptographic standards. Our products engage in billions of HTTPS transactions per day, and we maintain one of the most widely used open-source cryptographic libraries. We are also deeply involved in the standardization of cryptographic protocols in the IETF. Eric Rescorla, a Mozilla fellow, is editor of the TLS specification, Richard Barnes is a former member of the Internet Engineering Steering Group, and several other Mozilla staff are active in cryptography-related IETF working groups. It is from this perspective that we offer our comments.

Our primary concern with the proposed process is that it needs to ensure that the algorithms standardized through it work in the real world. The draft documents provided for comment present problems at both legal and technical levels.

1. Submitted algorithms must be usable without compensation to patent holders (RAND-Z, not only RAND) and implementations must bear an open-source license

The draft Call for Proposals is correct to note that "royalty-free availability of cryptosystems and implementations has facilitated adoption". It is surprising that this observation is followed by allowances for royalty-bearing cryptosystems, e.g., in the Statement by Patent Owners 2.D.2. Allowing royalty-bearing cryptosystems to be submitted will inhibit both the thorough evaluation of proposals and their eventual adoption by industry.

As the draft CFP acknowledges in several places, contributions by the broader research community will be essential in helping NIST make a thorough evaluation of the submitted algorithms. In order to make these contributions, members of the community including researchers in the commercial and academic sectors will need be able to implement the submitted algorithms. A requirement to license patents for such implementations would make it impossible for many researchers to participate in evaluation of algorithms, undermining the completeness and the legitimacy of the NIST process.

In this context, it should be noted that U.S. Courts have all but eliminated the availability of the “experimental use defense” to patent infringement: “[R]egardless of whether a particular institution or entity is engaged in an endeavor for commercial gain, so long as the act is in furtherance of the alleged infringer’s legitimate business and is not solely for amusement, to satisfy idle curiosity, or for strictly philosophical inquiry the act does not qualify for the very narrow and strictly limited experimental use defense. Moreover, the profit or non-profit status of the user is not determinative.” *Madey v. Duke Univ.*, 307 F.3d 1351, 1362 (Fed. Cir. 2002) (finding university’s research projects with no commercial application still “unmistakably further the institution’s legitimate business objectives” in education). (See also *Soitec, S.A. v. Silicon Genesis Corp.*, 81 Fed.Appx. 734, 737 (Fed.Cir. 2003), “There is no fair use or research and development exception for infringement of normal commercial processes.”)

There is also a need for researchers to be able to use and modify the submitted implementations in order to evaluate the costs and benefits of the algorithm in different contexts. For example, a researcher might adapt the optimized implementation to run on a machine architecture common in mobile devices to see if the algorithm is suitable for use in that environment. In order to allow this usage, it is imperative that the submitted implementations be licensed under an open-source license, and in particular one that allows for the creation of derivative works. We encourage NIST to specify a small set of acceptable open-source licenses. There are several such licenses available: Many of the policies in the [US CIO’s list of licensing resources](#) recommend the CC0 or CC-BY licenses; we would also find licenses such as the MIT, BSD, Mozilla Public License, or Apache Public License acceptable.

Standardization of a royalty-bearing algorithm would strongly inhibit industry adoption of the algorithm, especially in open standards organizations and open-source projects. The IETF has historically avoided standardization of royalty-bearing algorithms, so it would be difficult to establish the ancillary protocol standards needed to integrate a NIST-standard algorithm into Internet protocols.

Open source projects such as OpenSSL and NSS are critical to the deployment of cryptographic protocols on the Internet. These projects are often unable to use algorithms that are only available through royalty-bearing licenses. In particular, it would be extremely difficult for Mozilla to include a royalty-bearing algorithm in its products (including Firefox) even if it were standardized by NIST. The only use of royalty-bearing technologies in Firefox today (the H.264 video codec) was only possible because an existing license holder offered to cover royalty costs for Firefox users and because significant engineering effort was spent enabling the codec to be distributed within the terms of the license.

2. Algorithms need to be evaluated as they will be used

It is crucial for the success of this process that NIST not evaluate submitted algorithms in

the abstract, but as they will be deployed in modern information security systems. To that end, we are glad to see that the evaluation criteria place the impact on Internet protocols as a primary measure of an algorithm's utility.

Along these lines, it should be noted that verifying that an algorithm is IND-CCA2 secure in the abstract might not mean that it provides this level of security in practice. For example, if there are assumptions underlying the IND-CCA2 proof that a protocol cannot meet, then the algorithm might not provide an acceptable level of security for that protocol. The evaluation criteria should make clear that algorithms must not rely on assumptions in security proofs that cannot be satisfied by common security protocols.

While we agree with NIST's choice to rule hybrid algorithms out of scope for this process, it is nonetheless true that hybrid algorithms will be an important part of the deployment process for post-quantum algorithms. We encourage NIST to consider the suitability of algorithms for use in hybrid schemes as an evaluation criterion, with preference for algorithms that are more amenable to hybridization.

Looking at how public-key algorithms are used in modern Internet protocols, it is clear that key establishment and signature are much more important features to implement than public-key encryption. Forward secrecy in particular has been a feature that the community of TLS operators has worked very hard to make pervasive, in order to guard against temporary compromises. Even messaging security protocols, which have traditionally relied fairly heavily on public-key encryption, have been moving toward frameworks that provide more forward secrecy by relying more on key establishment instead of public-key encryption. We would be comfortable if NIST de-emphasized or dropped public-key encryption from evaluation, especially given that in many cases, it can be replaced by a combination of key establishment and symmetric encryption.

Finally, the selection of x64 as a reference platform is understandable, but perhaps not a complete reflection of modern computing environments. It is increasingly common for cryptography to be done on mobile devices, mostly using ARM architectures, and operators are increasingly selecting algorithms based on their performance in mobile environments (e.g., preferring ChaCha20 over AES). Emerging platforms for the Internet of Things will likely be bringing similar constraints in the near future. We would encourage NIST to include one or more mobile and/or IoT platforms in their evaluations, either directly or by working with the community to ensure that algorithms are evaluated in these contexts.

We are grateful to the NIST for the opportunity to comment on on this process. We look forward to working with NIST and the broader community to ensure that the Internet can be kept secure even if quantum cryptanalysis becomes feasible.

Respectfully submitted,
Richard Barnes, Firefox Security Lead
James Jones, Cryptographic Engineering Manager

the current `_exponent_` for the security of DSA against these attacks is only three years old.

Furthermore, a closer look shows that there are many more improvements that reduce concrete attack costs by reducing the "o(1)" quantities. Sloppily replacing o(1) with 0, as NIST apparently did to obtain its current recommendation of 15360 bits for DSA for $\geq 2^{256}$ security, is unjustified. Even for the simple case of single-key attacks, figuring out the o(1) with moderate precision at such large sizes is a difficult research project, extrapolating far beyond experimental ranges. At this point I wouldn't hazard a guess as to whether NIST's 2^{256} is an overestimate or an underestimate.

If someone makes enough progress on this research project to announce that the single-key attack cost is actually between 2^{245} and 2^{250} , will NIST withdraw its DSA standard? (What if there is a 2^{100} attack against RSA-2048, rather than the commonly quoted 2^{112} ?) If not, then what exactly is the point of asking people to be "confident" that 2^{256} is "met or exceeded"? More to the point, experts are `_not_` confident, even when multi-target attacks are ignored.

Some people are even more worried by the recent drastic reduction of security levels for pairing-based cryptography, by other index-calculus optimizations. Fortunately, DSA conservatively chose prime fields, avoiding the subfield/automorphism structures exploited in the latest attacks (and in the most complicated previous variants of NFS---one of the warning signals that led ECRYPT to recommend prime fields a decade ago). But the bigger picture is that index calculus is complicated and constantly improving. Would it really be so surprising to see another security loss that `_does_` affect DSA?

keylength.com reports some sources making recommendations around the same 15360-bit level recommended by NIST, but also reports Lenstra and Verheul recommending 26000 or 50000 bits. The big difference here is Lenstra and Verheul leaving a security margin in case there is progress in cryptanalysis.

So, with all these numbers in mind, how should we choose DSA key sizes to be "confident" about $\geq 2^{256}$ pre-quantum security? Should we take NIST's overconfident 15360 bits, which definitely flunks the multi-key requirement, and isn't a solid bet even for a single key? How about 26000 bits? 50000 bits? Much bigger, considering Barbulescu's 1.231? What happens if 1.231 is improved further?

What NIST is asking post-quantum submitters to figure out is far more difficult than this DSA example, for several reasons:

- As one would expect given the history of how cryptanalytic effort has been allocated, the security picture for most post-quantum public-key algorithms is even less stable than the security picture of DSA. Example: Current algorithms for the famous shortest-vector problem take (conjecturally) time $2^{(0.29...+o(1))d}$ in dimension d , a vast improvement compared to $2^{(0.40...+o(1))d}$, the best result known just a few years ago.
- At this point we have only crude guesses as to the ultimate costs of different quantum operations. I understand that NIST wants to define 2^b post-quantum security as 2^b quantum AES computations, but what is the relative cost of a quantum AES computation and a lookup in an N -entry table using a quantum index? How does this depend on N ? How much harder is it if the table entries are themselves quantum?
- I agree with NIST's comment that a 256-bit preimage search with Grover is actually harder than a 256-bit collision search (even if qubits are magically as cheap as bits), since Grover parallelizes poorly. I agree that the optimum value of $T\sqrt{S}$, subject to $ST=2^{128}$ and $T\geq\sqrt{S}$, is $2^{(2*128/3)}$. But $T\sqrt{S}$ is not a user-comprehensible cost metric, and not a metric for which many subroutines have been analyzed.
- Algorithm designers benefit tremendously from being able to try out their algorithms on small-scale and medium-scale problems. An experiment can show with minimal effort that an algorithm doesn't produce the desired outputs, or that it doesn't run at the desired speed. Designers of quantum algorithms don't have this tool yet.
- How is a submitter supposed to be confident of reaching, e.g., 2^{128} post-quantum security? Submitters will end up making ill-informed random guesses of which parameters to assign to which security levels. Security analysis will then throw some submissions into the Scylla of being "broken", while others will have thrown themselves into the Charybdis of being "inefficient", even though those submissions might simultaneously be more secure and more efficient than other submissions that simply happened to make luckier initial guesses of target security levels.

To summarize: Well-informed long-term security assessments will not simply supersede obsolete guesswork. The guesswork will continue having an influence long after it should have been thrown away. This is a serious failure mode for the evaluation process.

Does "meet or exceed each of five target security strengths" mean that each submission has to separately target all five levels, giving designers five chances to be artificially thrown into the rocks? Is it enough to target just the top level, namely 2^{256} pre-quantum security and 2^{128} post-quantum security?

I found it particularly striking that this choice of top target security level was based on the security achieved by a secret-key system (in this case AES-256, for some reason ignoring multi-target attacks), rather than on any attempt to assess what users actually need. I'm reminded of the ludicrous requirement of 2^{512} preimage resistance for SHA-3, forcing permutation-based SHA-3 submissions such as Keccak to be much larger and slower than they would otherwise have been.

If a public-key system naturally has 2^{2b} pre-quantum security and more than 2^b post-quantum security (I predict that this will be a common case), then choosing parameters to successfully target 2^{256} pre-quantum security will be overkill for 2^{128} post-quantum security---and also overkill for what users actually need. Why is this a sensible target?

If a public-key system naturally has 2^b post-quantum security and more than 2^{2b} pre-quantum security (I know one example like this), then choosing parameters to successfully target 2^{128} post-quantum security will be overkill for 2^{256} pre-quantum security. Why should the designer have to bother evaluating the pre-quantum security level?

Let me suggest a different approach:

- Leave it up to submitters to decide exactly what post-quantum security level to aim for.
- Tell them that security levels $<2^{64}$ will be viewed as "breakable", and that security levels $>2^{128}$ are unlikely to be viewed as more valuable than security level 2^{128} , except possibly as a buffer against future cryptanalytic progress.
- Ask them to do the most accurate job that they can of analyzing post-quantum security. Don't ask for fake confidence.
- Scrap the requirement of a pre-quantum security analysis. Users will use cheap ECC hybrids to obtain the pre-quantum security that they want.

Of course, many submissions will do a pre-quantum security analysis and then say "We don't think Grover will reduce the exponent by a factor beyond 2". Is there any problem with this? Should the number of submissions be limited by the current availability of expertise in quantum cryptanalysis?

Followup analysis will improve our understanding of the actual post-quantum security levels of various algorithms, and then NIST will look at a two-dimensional plot of speed vs. security level and decide which options are most interesting.

2. My understanding is that NIST is asking for two specific types of encryption, which NIST labels as "public-key encryption" and "key exchange". This is too

narrow: it omits important variants of public-key encryption that people should be allowed to submit.

What I suspect will be most important in the long run is a CCA2-secure "KEM". A KEM can be viewed as a limited form of public-key encryption: the only thing a ciphertext can do is to communicate a random session key. As a simple pre-quantum example, Shoup's "RSA-KEM" chooses a random number $r \bmod pq$ and transmits a session key $\text{SHA-256}(r)$ as the ciphertext $r^3 \bmod pq$. This is easier to design and analyze and implement than, say, RSA-OAEP.

(Proponents of RSA-OAEP will respond that RSA-OAEP can encrypt a short user-specified message as a ciphertext with the same length as pq . Some applications will notice the bandwidth difference. Obviously NIST should continue to allow public-key encryption as a target.)

One can easily combine a KEM with an authenticated cipher to produce a full-fledged public-key encryption scheme. But this understates the utility of a KEM: the same session key can be reused to encrypt any number of messages in both directions, whereas wrapping the KEM in a public-key encryption scheme hides this functionality. Using this public-key encryption scheme to encrypt another level of a shared session key would be frivolous extra complexity. Why not let submitters simply submit a KEM, skipping the cipher?

Sometimes people reduce the security goals and design KEMs to encrypt just one message, without chosen-ciphertext security. Here is the application to keep in mind:

- a client generates a KEM public key;
- a server uses this to transmit a random session key;
- messages are signed by long-term keys for authentication;
- the KEM private key and session key are erased after the session.

This is how New Hope works inside TLS. The signatures (if handled properly) prevent attackers from choosing any ciphertexts. So why not let people submit single-message non-CCA2-secure KEMs?

(I don't like the TLS/SIGMA approach to secure sessions: it is error-prone and excessively complex. This is not a broadcast scenario; authentication does not require signatures. I prefer the simplicity of using pure encryption: the long-term key is an encryption key, and the soon-to-be-erased short-term key is another encryption key. This requires multiple-message support and CCA2 security, but my current impression is that this robustness has only minor costs, and I wouldn't be surprised if the New Hope team decides to move in this direction. However, if they instead decide that CCA2 security is too expensive, they shouldn't be rejected for targeting TLS!)

What NIST calls "key exchange" in the draft sounds to me like a poorly labeled KEM with intermediate security requirements: chosen-ciphertext security seems to be required, but the interface sounds like it allows only one message before the key is thrown away. NIST should make clear if it instead meant a full-fledged KEM allowing any number of ciphertexts. Either way, NIST should explicitly allow non-CCA2-secure single-message KEMs such as New Hope.

Calling any of these systems "key exchange" is deceptive for people who expect "key exchange" to be a drop-in replacement for DH key exchange. In DH, Alice and Bob both know a shared secret as soon as they see public keys from Bob and Alice respectively, with no additional communication. As a concrete example, consider the very small number of network round trips needed to efficiently authenticate data from hidden client identities in the "CurveCP" and "Noise_XK" protocols. Here's Noise_XK using ECC:

- Alice sends her ephemeral public key eG to Bob. New session key: hash of ebG , where b is Bob's long-term key.
- Bob responds with his ephemeral public key fG , encrypted and authenticated. New session key: hash of ebG and efG .
- Alice sends her long-term public key aG to Bob, encrypted and authenticated. New session key: hash of ebG , efG , and afG .

This third packet can already include data authenticated under the last session key, and Bob immediately knows that the data is from Alice. Pure public-key encryption (without signatures) needs another round trip for authentication: Bob has to send data to Alice's long-term public key and see the reply before Bob knows it's Alice talking.

There is one notable post-quantum example of the DH data flow, namely isogeny-based crypto. Security analysis of isogeny-based crypto is clearly in its infancy, but if isogeny-based crypto does survive then the data flow will be an interesting feature. People who submit isogeny-based crypto should be allowed to submit it in a way that makes this data flow clear, rather than having to wrap it in public-key encryption.

I understand that for signatures NIST explicitly decided to disallow one data flow of clear interest, namely stateful signatures, since there is already separate ongoing standardization of stateful hash-based signatures, which are the main reason for interest in this data flow. (The security of hash-based signatures is much better understood than the security of most other public-key systems.) But for encryption I don't see how a similar limitation could be justified.

To summarize, there are at least three clearly different types of data flow of interest: public-key encryption, KEMs, and DH. Within KEMs, there are at least

two security targets of interest: passive security for one message, and chosen-ciphertext security for many messages. I suggest that NIST explicitly allow

- all four of these targets;
- also the intermediate type of KEM labeled as "key exchange" in the current draft, if NIST has an application in mind; and
- any further encryption targets that NIST identifies this year as being useful.

I also suggest defining some standard conversions that NIST will apply automatically: e.g., converting a CCA2-secure KEM into CCA2-secure PKE by composition with AES-256-GCM, and converting the other way by encrypting a random 256-bit key. NIST won't want to listen to pointless arguments such as "yes we know we're worse than this PKE but it wasn't submitted to the KEM category" from KEM submitters, and won't want to have to wade through artificially bloated PKE+KEM submissions that are really just one design but want to compete in every category.

3. I have three suggestions regarding terminology.

First, the draft refers frequently to "key exchange", which as noted above ends up deceiving people. I suggest scrapping this terminology in favor of more precise terminology such as KEM and DH. (There's already a NIST standard introducing relevant names such as "C(0,2)", but I don't know how many people are familiar with these names.)

Second, the draft uses "forward secrecy" (even worse, "perfect forward secrecy") to refer to the obvious security benefits of erasing a private key. This terminology also ends up deceiving people. Last week I was speaking with a banker who thought that TLS's "perfect forward secrecy" would protect his communications against future quantum computers. I suggest avoiding this terminology and instead saying something like "Fast key generation is useful for high-frequency generation of new key pairs, which in turn allows each private key to be promptly erased."

Third, the draft says that post-quantum cryptography is "also called quantum-resistant or quantum-safe cryptography", and makes occasional use of the "quantum-resistant" terminology after that. It's true that Google finds some hits for "quantum-resistant cryptography" and "quantum-safe cryptography" (1630 and 4340, compared to 47100 for "post-quantum cryptography"), but I'm not at all sure that the people using these terms are using them with the same meaning as post-quantum cryptography, and I predict that users seeing algorithms labeled as "resistant" and "safe" will be deceived into thinking that we are more confident than can be scientifically justified.

To whom it may concern:

My name is David Jao. I am the designer of post-quantum cryptosystems based on the isogeny problem over supersingular elliptic curves.

I would like to draw your attention to the following areas in your post-quantum cryptography draft requirements and evaluation criteria which I believe would benefit from further clarification:

(Section 2.B.1) In prior NIST standardization processes, there was only one functionality being evaluated (e.g. block ciphers for AES, and hash functions for SHA3). In this draft, we have potentially up to three distinct pieces of functionality (encryption, signatures, and key exchange) being evaluated at the same time. Will NIST be evaluating all the algorithms in a single submission package together, or will the three types of schemes be evaluated separately? If the latter, why not just accept separate submissions in each category rather than combining schemes of each type into one submission? It would be helpful if NIST could clarify the rationale behind accepting multiple items in one submission. For example: "If a submission includes more than one type of scheme, NIST will evaluate the schemes of each type separately. However, submitters may choose to combine different types of schemes into a single submission in order to share software code among multiple schemes within the submission."

(Section 2.C.1) Does the requirement for ANSI C source code preclude the use of assembly language optimizations? Your draft proposal does not specifically address this question. I would like to see assembly optimizations (at least inline ASM) allowed for the optimized implementation, because otherwise the implementation would not be representative of real-world conditions, especially for number-theoretic cryptography which relatively speaking benefits more from assembly optimization than other families of cryptosystems. It seems to me to be a little inconsistent to specify a target platform (Intel x64) and not allow platform-specific optimizations.

(Section 4.A.2) IND-CCA2 makes perfect sense for public-key encryption, as well as key transport, but does not apply to key establishment in isolation. It is not clear what security model NIST is proposing for key establishment. All existing security models for key establishment that I'm aware of are rather heavyweight, and the vast majority are tailored to authenticated key exchange, which you mention only in Section 4.C.1. As I am not an expert in security models for key establishment, I defer to others on the question of what model to use. If NIST requires external assistance in this regard then a public request for input would be appropriate.

(Section 4.A.4) Typically, it is not possible to tune the classical and quantum security levels of a scheme separately; a given choice of parameters will imply a fixed classical security level and a fixed (possibly different, but not independently tunable) quantum security level. For example, any isogeny-based scheme with 128-bit classical security

The current document is still inconsistent in what categories NIST is asking for submissions. This matches the discussions in February when it was left open whether NIST would ask for a key exchange mechanism. The current document first speaks of 'key exchange' and later of 'key establishment'. The API documentation uses both words interchangeably.

It should be made clear what precisely is asked for. Most people understand key exchange to match the functionality that Diffie-Hellman key exchange is offering: two keypairs determine a shared secret without communication; keys can be reused, e.g. A's published key can lead to a shared secret with Bob, using Bob's public key, and one with Charlie, using Charlie's public key. This is the functionality matching eBACS's DH function API: given one public key and one secret key, compute the shared key.

The functionality described early in the draft call for proposals changes this to distinguish between an initiator message and a responder message. This does not match common understanding of key exchange, which is also why the eBACS API does not fit.

Later on the call document -- now speaking about key establishment -- highlights the desired result: key transport and forward secrecy. The latter implies that new public keys must be generated frequently, requiring efficient key generation and small key sizes for transmission. I think it makes much more sense to ask for submissions for this scenario and a scenario with long-term public keys instead of asking for submissions for key exchange and encryption.

Realistically, public-key encryption is used only to transmit a key which is then used in a symmetric cipher; this is also recognized in the call document. The formal treatment of this is most advanced in the KEM/DEM framework: the public-key system is used as a key-encapsulation mechanism, which ensures that sender and receiver obtain the same key, and that key is then used in the data-encapsulation mechanism to encrypt the message. This avoids issues of padding.

To summarize, I recommend asking for submissions for two types of KEM:

- KEMs in which the receiver has a long-term public key; obtaining the key is outside the scope of specifying the KEM and
- KEMs in which at least one side generates and transmits a fresh public key.

instead of submissions for encryption and key exchange/establishment.

The second type of KEM scheme should be efficient enough that keys can be generated for each key transport, but ideally not break down completely if keys are reused. It would be good for the submitters to specify how key reuse would affect the security of

their system. I understand that the latter might be captured under the property of 'robustness'.

The minor issue is that I would recommend to request a constant-time implementation of each retained algorithm in the second round. Timing attacks are one of the most basic and thus most powerful attacks and each implementation (in software or hardware) needs to be protected against it. The call currently says that you'll take ease of SCA protection into account; that will be much easier and more meaningful if the submitter has to send in a protected implementation. This might be seen as a burden by some, so I wouldn't require it as part of the submission package, but each proposer group can get help by the time the second round comes along.

Editorial comments:

1. p.2 "in the event that large-scale quantum computers" should be replaced by "to prepare for the event that large-scale quantum computers" (or similar). It is too late to change once a QC is built. Even if long-term security is not a concern, roll out would take too long. (This is captured well a few paragraphs further down but confusing here).
2. p.4 It is unusual for key-exchange schemes to distinguish between initiator and responder messages. It is normal to request that the scheme defines a shared secret for each pair of public keys. If the definition is different this should be stated early on. It might be that you're instead asking for public-key encryption schemes for one-time use public keys with fast key generation (which is different from the typical DH message flow). See above.
3. p.5 In case a submitter has submitted his implementation to eBACS there will be benchmarks on a multitude of processors. Describing all the platforms and all the results would unnecessarily blow up the submission document. I recommend to allow inclusion by reference to the page for the primitive on bench.cr.yp.to. Of course, the submitter should still be free to highlight some processors and implementations if he chooses to and then be required to describe the platform, so this is a suggestion to permit a reference in addition.
4. p.7 Do you really want to receive all pdf files of papers or are links to public versions of the paper sufficient? Can people set up a webpage with supplemental material including links? I foresee a problem with copyrights: authors usually have the right to put their author copy online; if their work is relevant to my submission, I can put a link to their work on my homepage without violating any copyright, but I cannot submit the paper to NIST and make a statement about the copyright. This basically means that I cannot use papers published by others.
5. What do you mean by "unusual vulnerabilities"? Would this be e.g. key reuse in a scheme where decryption failures can be used to determine the secret key? or the fragility in ECDSA with nonce reuse? It would be good if you could be more

specific. For the avoidance of doubt, please specify whether assembly subroutines or intrinsics are acceptable.

6. p.10 "the quantum-resistant algorithm evaluation process": elsewhere you've changed to 'post-quantum' so I suggest to adjust the phrasing here to match.
7. p.13 Same comment regarding key exchange being asymmetric in initiator and responder as above.
8. p.16 You mean 2^k executions of AES on the given architecture? See the detailed analysis of the costs of using Grover on AES (PQCrypto 2016); are you considering the estimated cost of 2^{32} to equal 1? I've seen the FAQ on this topic, but that didn't help. Some algorithms suffer much more from requiring the steps to be reversible than others, so it will be necessary for cryptographers to understand quantum algorithms in any case. In principle this is not a new problem -- 1 ECC operation is not the same as an AES operation and we don't even know how to define the exact security level of elliptic curves. Counting operations in quantum algorithms is at least as hard. While I think that we cannot reach a way of comparing security between AES and post-quantum systems, I strongly suggest that systems using similar primitives count their efficiency the same way, e.g. code-based systems against which information-set decoding is the most efficient, should have a consistent way of using the cost of one loop; same for lattice-based systems using sieving. These ways might not be accurate in the end, but at least they allow comparisons within one class of algorithms. Eventually it is necessary to compare systems across different primitives, but by then more detailed research on current and quantum attacks will have happened.
9. p.17 I often encounter practitioners who take "perfect forward secrecy" to mean that a later attack cannot do harm and misunderstand it to mean that they can continue to use ECC till quantum computers arrive. They are surprised when they understand that having the public messages + keys is enough to later on break the scheme with a quantum computer. Due to this confusion I have started to use "key erasure" for this concept; given that this is not yet a common term it is necessary to add a parenthetical comment "(also known as perfect forward secrecy)" for now. Please be more specific when referring to this concept. Do you accept schemes that become insecure if the same key is reused or do you mean to ask for schemes which have very fast key generation time and do not require much space for the key transmission?
10. p.17 While it is grammatical to say 'resistance to side-channel attack' I would suggest to use the plural 'attacks' here, because there are many different attacks and a system might not be equally defensible against all of them. It might be useful to include a ranking of what types of attacks must be covered, e.g. timing attacks are applicable in significantly more situations than power analysis.
11. Regarding multi-key attacks: a brute force attack is always sped up when many targets can be attacked; you might want to specify that this would be with respect to the best attack or be more precise in the 'an advantage'. Also it

We have suggestions that we believe will improve the proposed standardization process and the outcome. Our comments focus on the following areas:

- Intellectual Property Rights
- Performance Measurement
- Evaluation Under Real-World Scenarios
- Security Levels
- International Standardization

A. Intellectual Property Rights:

The current draft includes the following statement on Intellectual Property in Section 2.D:

NIST has observed that royalty-free availability of cryptosystems and implementations has facilitated adoption of cryptographic standards in the past. As part of its evaluation of a PQC cryptosystem for standardization, NIST will consider the 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.

Further, the proposed required Statement by Patent Owner(s) in Section 2.D.2 explicitly allows for a patent holder to select an option of RAND (reasonable and on discriminatory) licensing with compensation.

This is a change from the SHA-3 competition in that royalty-free licensing is not required by the proposal but is merely a factor to be considered. We have seen in the past how ambiguity and licensing have hampered the adoption of new cryptographic technologies. It is critical that NIST maintain the same intellectual property rights disclosure and release requirements that were set out for the SHA-3 competition, namely that all submitters be required to release any and all IP claims as a condition of entry, and that each submitter agree to unrestricted, royalty-free use of their work.

Additionally, we note that the proposed approach to Intellectual Property Rights for this competition conflicts with NIST's stated commitment in NISTIR 7977 on this specific issue. See NISTIR 7977, Section 7 ("Policies and Procedures for the Life Cycle Management of Cryptographic Standards and Guidelines), Subsection 4 ("Define a Specific Plan and Process"), bullet point "Hold a Competition" (bottom of page 18 of NISTIR 7977), where NIST writes [emphasis added]:

If NIST decides to pursue the development of a standard or guideline, it may use an open competition. When a competition is used, interested parties will have an opportunity to participate in the competition by reviewing core requirements and evaluation criteria, publishing research papers, submitting comments, and attending public workshops. Researchers worldwide may contribute candidate

designs and papers on the theory, cryptanalysis and performance of the candidates. The winning submitters are recognized, *but agree to relinquish claim to intellectual property rights for their design so that the winning candidate can be available for royalty-free use.*

This process is clearly a competition as defined in NISTIR 7977, so NIST must adhere to the IPR commitments it made for competitions in that document. To that end, Microsoft strongly suggests that the “reasonable terms and conditions” IPR language be struck from the proposal in favor of the exact language used in the SHA-3 competition, guaranteeing that the selected algorithms be “available on a worldwide, non-exclusive, royalty-free basis.”

B. Performance Measurement

i. Constant Time Implementations

Section 2.C.1 (“Implementations”) references “optimized implementations” that will be used for performance benchmarking. Real-world applications of cryptographic schemes require constant-time implementations as a minimum to protect against timing and cache-timing attacks.

To ensure that “optimized implementations” reflect what would be deployed, and to enable apples-to-apples comparisons, all “optimized implementations” submitted for this effort should be designed to be constant-time. Second-round updates to submissions may make updates to fix constant-time-related bugs in first-round submissions.

ii. Performance Tooling

The performance evaluation of “optimized implementations” must be done by NIST directly or by an independent and neutral third party not affiliated with any party involved in any submission. The tools used in this evaluation must be open, independent, auditable and neutral, their code must be freely published for inspection, and must not be owned by or affiliated with any party involved in any submission. No submitter can be involved in performance evaluation in any capacity.

iii. Performance Testing Scenarios

The performance evaluation should cover the following platforms at a minimum: a 64-bit processor “server class” and a 32-bit processor “mobile class”. In addition, testing should be conducted on 8-bit and 32-bit microcontrollers, and be evaluated on at least one alternative hardware platform (e.g., FPGA).

C. Evaluation Under Real-World Scenarios

i. Hybrid Modes:

In Section 1, NIST writes that “hybrid modes” which combine quantum-resistant cryptographic algorithms with existing (not necessarily quantum-resistant) cryptographic algorithms are out of scope for the competition. We believe this limitation is overly restrictive for two reasons. First, some proposed quantum-resistant schemes may have benefits when combined with certain classical schemes, and NIST’s evaluation process should be able to consider such benefits¹. Second, ease of integration and engineering compatibility with classical cryptography must be a consideration in the evaluation of submitted algorithms as a desirable property. It is most likely that post-quantum cryptographic schemes will be deployed in such hybrid modes first and be used alongside classical cryptography for a significant amount of time. Candidate quantumresistance schemes must be evaluated in the wider context in which they will be applied, which will include integration with classical cryptography.

ii. Protocol Scenarios

NIST should identify several high-priority protocol scenarios, such as TLS, for evaluating and testing submitted schemes. Ease of integration with the most commonly used security protocols and performance in such scenarios must be an important evaluation criteria.

In the second round, those candidates selected to continue on should be asked to apply their submissions to selected real-world protocols, such as TLS, to further the evaluation.

D. Security Levels

In Section 4.A.4 (“Target Security Strengths”), NIST identifies five target security strengths for which submitters will be asked to provide parameter sets. We are concerned that the lowest security strengths identified are too low: the requirements should encourage strong and conservative security levels. There are also too many security strengths specified. Reducing the number of parameter sets required of submissions will simplify the evaluation. We suggest that NIST remove target levels (1), (2) and (3) and replace them with a target level of 128 bits classical security / 128 bits

¹ For a practical example of such ancillary benefits see C. Costello, P. Longa and M. Naehrig, *Efficient Algorithms for Supersingular Isogeny Diffie-Hellman*, recently presented at Crypto 2016 and available online at <http://eprint.iacr.org/2016/413>. In this paper the authors present a post-quantum key agreement scheme based on supersingular isogenies, and in Section 8 they present a strong ECDH+SIDH hybrid (“BigMont”) that leverages the underlying field arithmetic of the post-quantum scheme to provide a parallel ECDH key exchange for very little overhead. NIST’s current proposed language would prohibit NIST from considering hybrid benefits from such schemes.

General Comments:

- * The terminology in the document should be more consistently used; for example, quantum-resistant vs. post-quantum.
- * The document should make the use of the term "parameters" clearer; whether "parameters" refer to a general parameter set for the primitive, a specific choice of public parameters or even a full suite of test parameters. For example, ECDH with a 384-bit prime curve, ECDH with the P-384 curve or ECH with curve P-384 and test key pairs.
- * There should be more care with the use of pseudo-normative language (shall/should and must/may) as this could lead to problems later in the competition. As an example, if the README file for a submission doesn't list all of the files on the CD, then someone could claim that it is not a "complete and proper" submission as it fails to meet the mandatory requirements in section 2.C.4:
 - o The "README" file shall list all files that are included on this disc with a brief description of each.

Specific Comments:

Section 3.3.c - The scheme shall be capable of supporting a message size up to 2^{63} bits.". For hash-based signatures, the number of messages that can be signed is limited by the parameter set used. If the submission is supposed to provide concrete values for the parameter sets, should there also be a requirement on the number of messages that it must be capable of signing? A bound of 2^{64} messages is given in Section 4.A.3 but then it states that "attacks involving more messages may also be considered".

Section 4.A.2 - "NIST intends to standardize at least one scheme that enables "semantically secure encryption" with respect to adaptive chosen ciphertext attacks". This statement is slightly confusing. Does this imply that NIST might also standardize other encryption schemes that are not secure against active attacks? It is not clear as well what security model is expected for key agreement.

Section 4.A.4, paragraphs 4-7 - While understanding the desire to find a way to fairly address the issues with parallelization, this discussion somewhat undermines the clear definition of security targets given in the previous paragraphs. One can imagine arguments similar to those about what does and doesn't constitute an attack during the SHA-3 competition.

Section 4.A.4, paragraph 7 (starting "Since NIST's goal") - implies that they are equating s bits of quantum security with the time t it takes to break a $2s$ -bit key by Grover's algorithm given w much space. It is commonly held that for Grover's algorithm, we have $t^2 * w = 2^{(2s)}$. If that is NIST's definition of quantum security, it should be explicitly stated. Does this mean that an attack that uses 85 bits of quantum memory and 85 bits

Comment on Post-Quantum Cryptography Requirements and Evaluation Criteria

September 16, 2016

Thank you for the opportunity to provide feedback on the upcoming NIST Post-Quantum Cryptography project.

The draft submission requirements set out a clear plan for a transparent process that will lead to the identification of one or more post-quantum technologies with confidence in the result. Please find below comments on six aspects of the submission requirements which I believe will further improve the process.

1) Security levels and refinement

The draft submission requirements ask for parameter sets at five target security levels, the lowest 3 of which are at 64, 80, and 96 bits of quantum security. Reducing the number of target security levels will simplify submissions and allow cryptanalytic research to focus on fewer parameters. I suggest omitting security levels below 128 bits of quantum security.

Given that some submissions will be based on mathematical problems for which cryptanalysis continues to advance, it seems plausible that security levels of parameter sets may evolve, either favourably or unfavourably, during the evaluation process. It would be unfortunate if promising submissions were disqualified because of cryptanalytic advances shaved e.g. 10 bits of security off of a 128-bit-level submission. I suggest that NIST aim to include some room for refinement of parameters in these scenarios, possibly (a) between the first and the second round, or (b) during a round with a minor update, or (c) by incorporating an even higher security level (say, 192-bit quantum security) to provide breathing room.

2) Royalty-free

NIST should require submitters to meet the same intellectual property requirements in the previous SHA-3 competition, namely that if their submission is selected then the submitters agree to place no restrictions on use of the algorithm, making it available on a worldwide, non-exclusive, royalty-free basis.

3) Key establishment / KEM security

Early text from NIST asked which model to use for key establishment; the current submission requirements say IND-CCA security. I support this security requirement. Among other reasons, it is compatible with the notion used in security proofs of TLS [Jager et al. CRYPTO 2012, Krawczyk et al. CRYPTO 2013].

4) Application-level performance

NIST should identify a variety of application scenarios and evaluate submissions in these scenarios. Given that many post-quantum schemes will involve trade-offs between runtime, memory, and communication, evaluating these schemes in applications may provide surprising results compared to standalone evaluation. TLS should certainly be one of these application scenarios. Given the complexity of adding new algorithms to TLS implementations and fairly benchmarking such a system under realistic loads, NIST may want to apply this evaluation only to second round candidates, and NIST may also want to provide a standard interface and code for integration, rather than requiring submitters to each implement this themselves. For example, it should be possible to modify OpenSSL or another open source TLS implementation to include a ciphersuite that calls in to the NIST-specified PQC API.

5) Transition from traditional to post-quantum algorithms

In the years following this NIST process, standards and implementers will likely gradually transition to post-quantum cryptography, running traditional and post-quantum algorithms side-by-side. While the NIST process rightly focuses on evaluating the security and practicality of post-quantum algorithms, one aspect of practicality is enabling a smooth transition. NIST may want to include as a positive evaluation criteria any characteristics of the scheme which enable a smoother transition, for example a post-quantum scheme that can be easily run in a hybrid mode alongside a traditional algorithm and (safely) share some parameters.

6) Key exchange API

The current C API for key exchange has four functions:

```
crypto_keyestablishment_initiator_generate  
crypto_keyestablishment_responder_generate  
crypto_keyestablishment_initiator_recover  
crypto_keyestablishment_responder_recover
```

The responder_generate function outputs a responder public key (ker) and a responder private key (skr), and then the responder_recover function outputs the shared secret (ss). This makes sense for plain Diffie-Hellman protocols, but may not make as much sense for some other protocols. KEMs generically are separated into 3 algorithms:

others it will be $1/10$. As the focus is on quantum security, it may be tempting to focus on quantum bit-security targets, possibly with an additional requirement of not getting below a certain (and higher) classical bit security.

Best regards,
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