February 17, 2026 Chris Brady

Good morning I would like to thank the chair and the committee for allowing me to speak this morning. My name is Chris Brady. I am an ophthalmologist and fellowship trained retina surgeon with a master's degree in clinical epidemiology. I've practiced at UVM for 9 years, and I conduct federally funded research developing and testing new tools for the detection of eye diseases in under-served communities worldwide.

I was asked by the Vermont Ophthalmological Society to provide my interpretation of outcome data presented on 6 specific eye procedures when performed by optometrists versus ophthalmologists. In full disclosure, I do not personally perform any of the procedures currently under review in S.64 with the exception of fluorescein angiography, though I was trained to do most of them. I hope I don't get too technical with my comments today, but my task was to dive into the details of the available evidence, so please stop me if I say something that's unclear.

I've also provided written testimony on a red-lined version of a document of evidence that was shared with VOS by this committee for comment, as well as the two

other studies presented in prior testimony. I go through each of these procedures and the specific papers presented for each one in this written testimony.

I would like to pick up with the comments in previous testimony describing optometry performed outcome data as scant. That is quite an understatement. While trying to comment on all of the clinical outcome evidence for optometry performed procedures in the United States, I was able to review a single prospective trial with three months of follow-up of 69 patients (so technically effectiveness rather than outcomes research). Previously you heard from an optometrist who has performed thousands of procedures. One of the documents presented in testimony mentions over 146,000 procedures. But the only high-quality clinical data from the United States presented that is in the peer-reviewed, published literature is a single study of one procedure without an ophthalmology comparison group, and with less than 70 patients completing the short-term follow up.

Beyond this single study, I have seen no publicly available, peer-reviewed, clinical outcomes or clinical effectiveness evidence to support the safety and

efficacy of the invasive procedures requested in S.64 when performed by optometrists.

The other documents that have been cited in the document I reviewed were either 1) conducted in other countries with very different contexts such as UK-based optometrists practicing under direct supervision of a subspecialist ophthalmologist, 2) a narrative description of the practice of a single optometrist without any clinical data presented or 3) a general patient information article describing the eye condition called chalazion without any clinical evidence of efficacy or safety or 4) MD/ophthalmology outcome data.

In short, I have seen no data proving similarity in outcomes between US optometrists and ophthalmologists.

With that I would like to transition to a discussion of the two specific papers presented in oral testimony to this committee and some of the specific reasons I do not believe these support particularly the safety of optometry performed lasers. It's important to note that from a biostatistical standpoint it is very hard to conclusively prove safety, but that is how these documents are being used.

I want to be clear I do not believe that these papers establish direct evidence that optometry performed procedures are unsafe; I just believe we have insufficient evidence of their safety. And the classic saying is that no evidence of harm is not the same as evidence of no harm.

Parenthetically, I also believe that in the public health community the way we talk about safety has evolved over the past 5-6 years. I think we've all been chastened to choose our words a lot more carefully when claiming a new type of treatment is safe and effective. So I believe the bar for declaring safety is higher than it's ever been.

I've mentioned Dr Lighthizer's paper Nd:YAG Laser Capsulotomy: Efficacy and Outcomes Performed by Optometrists already as the only prospective trial, the only US-based trial, and the only trial reporting clinical effectiveness that has been presented as evidence to this committee, and that I am aware of.

This was a prospective 3-month study of 81 eyes of 69 people. The authors compared visual acuity outcomes to over 1500 eyes reported in Table 2 of the paper. Almost all the patients in the study experienced

subjective and objective improvement in their visual function, but I'd like to discuss several significant limitations to this study design.

For example, in the most rigorous studies, I would need to see vision measured using a very strict, standardized testing method before and after the procedure. This is called "best-corrected visual acuity" with protocol refraction, and it's designed to reduce variability and bias.

Likewise there could have been a control group — meaning some patients would be randomly assigned to receive a fake (or "sham") laser treatment, or to MD treatment. That way, researchers could compare outcomes between those groups. This helps determine whether improvements are truly due to the treatment or to other factors, including random fluctuation.

Another key safeguard from bias is "masking" (in other fields they call it "blinding" but we avoid that term). The people measuring patients' vision ideally do not know who received which treatment. Nor should the patients themselves. If either group knows it can unintentionally influence the results. This is just human nature, and we strive to avoid it in the most rigorous studies.

There's also an important concept called "test-retest variability." When you repeat a test, results can naturally vary — similar to stepping on a scale twice and seeing slightly different numbers. With strict, standardized vision testing, normal variation is usually about 1 to 1.5 lines on a specialized eye chart. With less controlled testing, variation can be even greater.

In this study, the reported improvement was about 2 lines of vision. This is a meaningful improvement in vision that should be significant to patients, but that amount of change may fall within the range of normal testing variability, especially if the testing was not done under strict protocol conditions.

As far as safety, the investigators did not report any clinically significant adverse reactions.

I'm not a statistician, but I'd like to discuss a basic rule researchers use called the "rule of 3." It helps estimate the true risk of a complication when a study doesn't observe any.

The basic idea is that when a study reports zero bad outcomes, that doesn't prove the risk is zero. Instead,

you divide 3 by the number of patients in the study to estimate the highest likely risk that you could expect in a larger sample.

Using that rule here, the study supports that real complications could occur in as many as 4.3% of people.

That could be four to eight times higher than the complication rates reported for MD's in the sources cited earlier.

So "no complications observed" does not mean "no risk." It may mean the study was not large enough to detect complications that occur at a low but still meaningful rate. No evidence of harm does not mean evidence of no harm.

Again, this is not a statement that this study proves or even suggests that the risk is that high, but simply highlights the importance of an appropriately powered study when making a safety claim. As I've said it is generally very hard to prove evidence of safety. I understand the committee is looking for the best evidence to support their decision, and I do not believe this study – the only study of US-based optometrist outcomes – reaches that threshold.

I'd like to now discuss Dr. Lighthizer's other submitted paper used to support the safety of laser.

This is a narrative review, within which the authors present the results of a survey of state Boards of Optometry. The boards were asked about any patient complaints or provider reported negative outcomes.

An estimated 146,000 laser procedures were reported in the study period with 2 negative outcomes.

I would like to emphasize that this type of evidence cannot be used support the specific claim of safety that was made in testimony.

This type of data collection only captures the most serious, extreme problems. It can be valuable, but it does not track the routine or expected complications that happen in real-world medical practice and that are important to people who experience them.

Other evidence presented to this committee has shown MD-performed procedures can have complication rates of 0.5-5% or so. So the 0.00014% figure being cited (2 out of 146,000) is dramatically lower because it likely

only includes those most catastrophic events, and so using that tiny percentage as proof that the procedures are nearly risk-free is quite misleading.

I'd like to close by re-stating what I said at the outset, no data proving similarity in outcomes between US optometrists and ophthalmologists has been presented. Nor has any data establishing the safety of optometry performed procedures. I welcome the opportunity to review any other studies or reports that I am not currently aware of.

I thank you very much for the ability to present to you today, and I'd welcome any questions you have.