Appendix Attachment

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IN THE UNITED STATES DISTRICT COURT
FOR THE NORTHERN DISTRICT OF FLORIDA
TALLAHASSEE DIVISION

AUGUST DEKKER, et al.,

Plaintiffs,

v. Case No. 4:22-cv-00325-RH-MAF

SIMONE MARSTILLER, et al.,

Defendants.
_____/

DECLARATION OF PATRICK W. LAPPERT, MD

I, Patrick W. Lappert, MD., hereby declare and state as follows:

1. I am over the age of eighteen and submit this expert declaration based on my personal knowledge.

2. I have been retained by counsel for the defendants in the above captioned lawsuit to provide an expert opinion concerning the nature of gender surgery. That opinion will be based primarily in my own experience as a physician and surgeon. It will also be based in an evaluation of the scientific publications that Plaintiffs have provided to the court in support of their complaint. It will additionally include an examination the world literature on the subject, as well as an examination of the massive public controversies that have led to near complete reversal of public health policy in multiple European states who have turned away from the social, medical, and surgical transitioning of minors.

3. I am a retired plastic surgeon, as well as a retired senior medical officer in the United States Navy. I have been a physician for 40 years. I completed my undergraduate education at the University of California, Santa Barbara. While there I had significant experience in university level research having been invited to be an undergraduate research assistant, working in the laboratory of Dr. Philip C. Laris. It gave me experience in the evaluation of research publications. We were involved in the collaborative work of elucidating the electrodynamic and stoichiometric quantification of the sodium and potassium pump, located in every living cell. I completed my undergraduate degree in four years, and went directly to medical school.

4. I completed my preliminary medical training while on active duty in the US Navy. I attended the Uniformed Services University of the Health Sciences, F. Edward Hebert School of Medicine, graduating as Doctor of Medicine in 1983.

5. I completed a surgical internship at the Oakland Naval Hospital, followed by Aerospace Medicine/ Flight Surgeon Training at the Naval Aerospace Medical Institute, Naval Air Station Pensacola.

6. I then served for 2 1/2 years with a deploying, front-line Marine Corps fighter squadron, serving in the dual functions of medical department head, and squadron Radar Intercept Officer flying in the F-4 Phantom. I was deployed to Asia and the Western Pacific. I provided medical care to squadron personnel while deployed in Japan, Korea, and the Philippines.

7. I completed my General Surgery residency at the Oakland Naval Hospital-University of California, Davis/ East Bay Consortium. Following residency, I was retained there as a staff surgeon, and was responsible for the training of surgical residents. I was awarded the inaugural "Resident's Choice" award given to the attending surgeon deemed most effective by the residents in training, and presented by Claude Organ, MD, past President, American College of Surgeons.

8. I trained in Plastic and Reconstructive Surgery at the University of Tennessee, Memphis, graduating in 1994. During that training I traveled to Peru and provided craniofacial surgical care for indigent Peruvian children. This included the publication of a case report of surgical management of a very late post traumatic ectopic frontal sinus mucocele.

9. I received Board Certification in General Surgery from the American Board of Surgery in 1992. I received Board Certification in Plastic and Reconstructive Surgery in 1997 from the American Board of Plastic Surgery. I re-certified in Plastic and Reconstructive Surgery in 2008.

10. I served as a staff plastic surgeon at Naval Hospital Portsmouth, Virginia from 1994 to 2002. I became Department Chairman in 1998, and served in that office until my retirement. We had 5 staff plastic surgeons, and 10 Enlisted and civilian members. I established the Wound Care Center, providing specialized wound care services to a global catchment area. For example, our department was responsible for the limb and pelvic reconstruction of some of the sailors wounded when the USS Cole was attacked while at anchor at Aden in Yemen. I also established and chaired the

multi-disciplinary Cleft Palate, Craniofacial Board. We provided comprehensive services for congenital pediatric deformities to a global catchment area.

11. Following selection to the rank of Captain, USN, I was selected to serve as Specialty Leader, Plastic and Reconstructive Surgery for the office of the Surgeon General, USN. In addition to being responsible for the selection and training of surgical residents, I was also responsible for Navy Medical Department policy concerning coverage for services, and medical evaluation and evacuation policy. I was responsible for the resolution of issues concerning what conditions constitute a requirement for immediate care in military hospitals, what may be purchased from civilian medical organizations and provided to eligible members on a delayed (elective) basis, and what is to be considered cosmetic surgery and therefore not an obligation of the government. I served in that position until my retirement. While serving as Department Chairman, I co-authored a textbook chapter on the management of combat injuries with the Chairman of Plastic Surgery at Harvard University, Dr. Eloff Ericksson. During that time I also published the first case report in the world literature detailing the use of endoscopic technique for reduction and plate fixation of a fronto-facial fracture.

12. I retired from the Navy after 24 years of continuous active duty. I was invited to join a surgical group in Scottsbluff Nebraska, primarily to provide comprehensive reconstructive surgery for women suffering breast cancer. I also provided reconstructive services to a very large regional catchment served by the Level II trauma center at Regional West Medical Center (RWMC). I established and chaired the Cleft Palate/ Craniofacial multi-specialty clinic at RWMC. I also established comprehensive wound care services for the many rural community hospitals in the western prairie including Nebraska, Eastern Wyoming, southwest South Dakota and northeast Colorado.

13. For reasons pertaining to the education of our six children, I moved my practice to Northern Alabama in 2005. I have been a solo practitioner here for the last 17 years. I was brought here by a local hospital that wanted to offer comprehensive breast reconstruction to women affected by breast cancer. I also started a comprehensive wound care center. I have also had a very active practice in aesthetic/ cosmetic surgery. I maintained my own surgical suite for in-office facial rejuvenation procedures as well as minimally invasive body contouring procedures. I was an early adopter of advanced techniques in autologous fat grafting for facial re- contouring as well as for the resolution of radiation burn wounds of the skin. I continued to serve in the training of medical students in my office practice.

14. Although I maintain a practice in wound consultation, skin care, and laser services, I retired from my surgical practice in 2020, after having practiced as a plastic and reconstructive surgeon for 30 years. I was an Active Member in good standing of the American Society of Plastic Surgery for all but the last two years in practice. With only two years remaining in my practice, I elected to forgo a third certification by the American Board of Plastic Surgery. The certification was no longer necessary for maintaining my hospital credentials, and I saw it as an unjustifiable expense for a solo practitioner planning retirement.

15. As can be gleaned from this summary, I have a meaningful breadth of experience, not only in the advanced surgical care of trauma, cancer, head/ neck disease, as well as cranial and facial birth defects. Many of those procedures require the use of the most advanced sensate, microvascular flaps, including composite and pre- fabricated flaps. These are all the same techniques employed by today's gender surgeons. As regards surgery of the breast, I co-authored a ground-breaking article regarding pre-operative plastic surgical planning in the care of women suffering from breast cancer. It is among the most frequently cited papers in the field of breast reconstruction.[1]

16. Since 2014 I have made a concerted effort to examine the medical literature as it pertains to the care of self-identified transgender persons including children and adults. I have had an eight year long running discussion on these issues with Family Practitioners, Pediatricians, Pediatric and Adult Psychologists and Psychiatrists, Pediatric Endocrinologists, as well as PhDs who specialize in the evaluation of the validity of scientific publications. During that time I have made many public presentations to teachers, counselors, pastors, and administrators on the subject of transgender, and the medical-scientific evidence that informs that care.

17. My curriculum vitae, which is attached as Exhibit "A", provides a list of my publications

18. I prepared a report which is attached to the Florida Agency for Healthcare Administration's "Generally Accepted Professional Medical Standards Determination for the Treatment of Gender Dysphoria" A copy of my report is attached and incorporated by reference as Exhibit "B" to this declaration.

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[1] Toth, B.A. and Lappert, P. (1991) Modified Skin Incisions for Mastectomy: The Need for Plastic Surgical Input in Pre-Operative Planning. Plastic and Reconstructive Surgery, 87, 1048-1053. http://dx.doi.org/10.1097/00006534-199106000-00006

19. I have offered testimony, both written and in person on this issue to state legislators, state health benefits management agencies, as well as to State Attorneys General.

20. I have also had experience in making judgements concerning distinctions between reconstructive surgery and cosmetic surgery. I gained this experience while serving in senior leadership for a government medical care system in which I had no financial stake. I have no financial interests in the matter in question, and the professional opinion that I offer is not influenced by my sources of income nor by my position in any organization that financially benefits from medical services that are discussed in this opinion.

21. For my services as an expert witness I am being compensated at an hourly rate of $400 for preparation of my written testimony as well for deposition and hearing. Additionally my travel expenses will be reimbursed. My compensation is not dependent upon the substance of my opinion nor upon the outcome of the litigation.

22. If called to testify in this matter I will do so truthfully, and to the best of my ability.

23. The Plaintiffs, supported by the opinions of Dr. Schechter, among others, are making the claim that "gender affirmation care" including "gender affirming (or confirming) surgery" should be paid for by the State of Florida because such care has scientifically proven efficacy, and safety. Furthermore they claim that there is such an abundance of scientific support for these treatments that they must be understood to be the standard of care, and that there is no controversy in the matter. In support of this position they have provided in their declaration an abundance of citations from professional scientific publications that they expect will furnish the court with sufficient reliable evidence that Plaintiffs claims will be upheld. As shall be seen in this response, their claims are not supported in the science. This will be seen in the examination of those scientific documents which they have cited.

24. In recent years professional medical societies have been making a concerted effort to strengthen the scientific basis upon which their particular specialties stand. This effort is commonly given the name "evidence based medicine". It is a systematic effort to categorize the quality of prognostic and therapeutic studies so that physicians reading these publications can distinguish what is vague and speculative from what is a matter of high likelihood, or grave certainty. Tools for making such

distinctions have been developed that categorize clinical or experimental findings on the basis of how that data was obtained, the reliability of the test instruments used, the variability of the results, the sample size, and the likelihood of bias among other factors. For the purposes of this response, I will use the tool developed by the American Society of Plastic Surgery.[2] For prognostic studies, the categorization of evidence is divided into Levels I- V, with Level I being the most rigorous and having the highest likelihood of scientific certainty, and Level V having the least rigor, and having very little certainty. Here are the definitions of those levels according to the American Society of Plastic Surgery:

Level I: High quality prospective cohorts study with adequate power or systematic review of these studies.

Level II: Lesser quality prospective cohort, retrospective cohort study, untreated controls from an RCT (randomized control study), or systematic review of these studies.

Level III: Case- control study or systematic review of these studies.

Level IV: Case series

Level V: Expert opinion; case report or clinical example; or evidence based on physiology, bench research or "first principles".

23. For therapeutic studies, the ASPS categorization is similar, but with a few helpful distinctions:

Leve	Type of evidence
1A	Systematic review (with homogeneity) of RCTs
1B	Individual RCT (with narrow confidence intervals)
1C	All or none study
2A	Systematic review (with homogeneity) of cohort studies

[2] The Levels of Evidence and their role in Evidence-Based Medicine Patricia B. Burns, MPH,1 Rod J. Rohrich, MD,2 and Kevin C. Chung, MD, MS3 Plast Reconstr Surg. 2011 Jul; 128(1): 305–310. http://www.plasticsurgery.org/Medical_Professionals/Health_Policy_and_Advocacy/Health_Policy_Resources/Evidence-based_GuidelinesPractice_Parameters/Description_and_Development_of_Evidence-based_Practice_Guidelines/ASPS_Evidence_Rating_Scales.html.

2B	Individual Cohort study (including low quality RCT, e.g.
2C	"Outcomes" research; Ecological studies
3A	Systematic review (with homogeneity) of case-control studies
3B	Individual Case-control study
4	Case series (and poor quality cohort and case-control study
5	Expert opinion without explicit critical appraisal or based on physiology bench research or "first principles"

24. These distinctions are very important to physicians who seek to understand the weight of the evidence presented in support of a change in therapeutic care. Sometimes such scientific findings can be so compelling regarding an issue, that professional societies will publish clinical guidelines that strongly suggest conformity to a new treatment plan based in that evidence. Occasionally the evidence will be of such certainty, on a matter that is so grave, that professional societies and even public law will assert that there exists a standard of care based in this evidence that if ignored has a high probability of injury or harm to the patient. That is what is implied when the phrase "standard of care" is used.

25. To that end, the ASPS document provides a grading system for Practice Recommendations that helps in the decision making. It is a synthesis of the breadth of scientific data that addresses the issue in question. In the case of Grade A ther is an accompanying "Strong recommendation", versus Grade D where the evidence is so lacking in empirical value that the proposed treatment can only be offered as an option if at all, depending upon the strength of existing or alternative treatments, and the particular issues of a particular patient.

26. To summarize, it can be said that Level-V evidence is anecdotal, and in the world of surgery it is typified by the phrase "expert opinion". Such evidence is not to be dismissed since it is the known starting point for much meaningful research and discovery. A surgeon with great experience and unassailable credentials will observe something peculiar. He will form a hypothesis about its cause or treatment. Hopefully he will publish his single case report, and share his thoughts with the wider surgical community. Perhaps one of his residents will start a hunt for other cases. Perhaps

surgeons who read his paper will report similar cases. Eventually it might lead the surgeon to apply his new principal to a series of cases. The series may already be there in his own case files. If he publishes his series of cases, that would constitute an improvement to Level-IV evidence. Even then, it would be considered "poor" evidence because it suffers from the fact that it is a small collection of cases, from a single surgeon, and perhaps no one has yet replicated his observations. Additionally it may suffer from "selection bias" (as when the patient decides if he will receive long term follow up), lack of proper controls (which help us to separate out what is the result of our treatment, and what is within the range of normal variation in the population), inadequate study duration (if you claim a long term improvement in survival, you have to follow the patients long-term).

27. An example from the history of surgery will serve to illustrate how Level-IV and V evidence, when widely encouraged and applied through expert opinion, can result in grave missteps. For over 100 years, ulcer disease of the stomach was considered a surgical problem. This very debilitating disease did not appear to be manageable through medical means. Laboratory study of the stomach had already demonstrated that acid production in the stomach is regulated by particular nerves. That finding suggested that if those nerves are cut, acid production will decline, and the ulcer will heal. It was also determined that the surgery must include some form of "drainage procedure" because cutting the nerves would also impair the muscular contracture of the stomach. Through the course of the decades many of the greatest surgeons gave their names to the elegant techniques for selectively cutting the nerves, or draining the stomach in ways that hopefully would not result in a "gastric cripple" (an all too common outcome). Long hospitalizations, and many months spent accommodating to the reordering of their digestive tract was expected. There is a syndrome of bad effects from these surgeries that most people adapt to but some never do. Nonetheless, untreated peptic ulcer disease was often deadly, either from peritoneal sepsis, or bleeding to death. Because of the gravity of ulcer disease, it was ethically sound to risk "post gastrectomy syndrome" if it meant saving a life.[3] By the 1980s, level II and I studies had demonstrated that peptic ulcer disease is actually a bacterial infection that can be treated with antibiotics and an acid-reducing medication. This had been very seriously suspected for at least 30 years. However, poorly designed studies published by the greatest academic surgeons of the day had utterly suppressed the bacterial explanation in favor of the surgical solution. A very well reasoned 2014 paper by Seselja and Strasser[4] shows the heuristic pitfalls that can result in unintended harms to

[3] History and evolution of peptic ulcer surgery; John B.BlalockJr.MD1; The American Journal of Surgery Volume 141, Issue 3, March 1981, Pages 317-322

patients when surgical decision making is driven by expert opinions that aren't well supported by quality scientific evidence.

28. Generations of surgeons will follow what is taught to them by the academic surgeons. These are esteemed mentors who are responsible for training the next generation of surgeons. That is how it has always been. However today, medical science has advanced in crucial ways through the application of the "science of science". We understand better now how to examine the evidence. We are less likely to make needless errors of judgment because we are better able to analyze the data particularly with regard to its reliability. This is indispensable when studying biological systems that, in every measurable trait, demonstrate great variability. It is particularly essential when examining and caring for the human person, because you have the added dimension of their subjective interior life.

29. Dr. Schechter, by every measure known to me, is of very accomplishmed in the world of plastic surgery. It is on that basis that we can expect that the published literature which he has offered in support of Plaintiff's assertions will be of the highest level available to him. But the literature he offered does not bear out his conclusions

30. In para. 21, Dr. Schechter defines the word "transgender" on the basis of a subjective conflict within the patient's internal sense of themselves. He affirms this interior subjective division on the basis of an idea that sex is somehow "assigned" at birth, rather than scientifically discovered through tissue sampling, in utero ultrasound, or simple inspection at birth. In 99.98% of cases, simple inspection correctly detects the sex of the subject. Furthermore, this test can be administered by untrained personnel. His use of the term "assigned" implies that there can be errors of "assignment". Such an assertion demands not only that we examine the result, but we must also look at the consistency of the data. We have seen previously that consistency of the data is one of the hallmarks of good evidence. Any test that can be correct 99.98% of the time regardless of who administers the test is perhaps unequaled in scientific medicine.

31. Gender, on the other hand, as it relates to sex, is a very different matter. While there are some aspects of gender that are more fixedly related to the sex, there are large areas of gender that are learned within the milieu of the local culture, and find their origins in family life. There is no objective, repeatable test, with known error rates that can be used to detect "gender". Gender, as the term is used in the world of medicine and surgery is not objectively measurable. Such traits as hair length,

[4] Dunja Šešelja 1, Christian Straßer; Heuristic reevaluation of the bacterial hypothesis of peptic ulcer disease in the 1950s; Acta Biotheor 2014 Dec;62(4):429-54.

occupation, preference for violent sport, clothing selection, among others, may have vague gender associations, but are so variable from culture to culture as to be useless for our purposes. This is because "gender" is one of the many expressions of the interior life of the person. It is a mercurial thing because it is not entirely fixed to that part of the patient that is a reliable object for examination and treatment. That difficulty with diagnosis and prognosis is further complicated by the fact that variability in gender presentation doesn't just occur within any particular human grouping, it is also known to vary within the span of the life of a single patient.(Zucker)

33. Beginning in 23, Dr. Schechter makes the claim that "hormone therapy and gender confirmation surgeries can help alleviate gender dysphoria" and that these treatments "have been shown to be an effective treatment for gender dysphoria". In para. 24 he further states that it is his professional opinion "supported by prevailing consensus of the medical community" that these treatments are medically necessary, and are "safe and effective treatments for gender dysphoria". We will examine the claims of efficacy, and safety by examining the papers offered in support of his claim. We will examine the world literature more broadly in order to examine Dr. Schechter's claim that there is a "prevailing consensus", which is a claim that suggests that there is no meaningful controversy surrounding the use of social, medical, and surgical gender affirmation, particularly with regard to the young.

34. The first support that Dr. Schechter offers is the WPATH "Standards of Care". This document is the product of the World Professional Association of Transgender Health. It has had 7 iterations, with the 8th version now pending publication. This document has been, and continues to be produced through a process of consensus-seeking within working committees of experts. As we have seen in our discussion about the grading of scientific evidence, expert opinion is the most rudimentary level of evidence. It is the starting point of scientific investigation, not the end. As any medical subject is investigated over time, the expert opinion becomes better supported by well developed and monitored scientific processes. In short, expert consensus is only as valuable as the scientific evidence that can be reviewed and evaluated which supports the opinion. If the evidence hasn't progressed very far beyond the category of expert opinion, then we are speaking about evidence that is not sufficiently developed so as too drive either clinical decision making, nor fiduciary decision making when public or invested resources are involved.

35. It will be recalled that the use of the words "standards of care" imply that a particular treatment or clinical principle, if not employed, would have an unacceptable probability of harm to the patient. The term "standard of care" addresses issues of

duty, negligence, harm, and causation. It is a legal term that is applied when evaluating the malpractice of medicine. In its Introduction to the WPATH standards, the authors acknowledge that their document is meant to be a guideline only, and subject to individual and local adaptation, and that it is not binding in any way. On page 2 of v.7 in bold face it states "The Standards of Care are flexible clinical guidelines". This calls into question the motivation for the use of the phrase "standards of care" in all of its publications and statements.

36. If the WPATH document is actually a collection of clinical guidelines, then we must examine how such guidelines are developed. In the International Journal of Quality in Healthcare (2016) Kredo et al.[5] (&) offer a helpful examination of that process. They point out that in the past they were just consensus statements offered by experts in the field. They were "'systematically developed statements to assist practitioner and patient decisions about appropriate health care for specific clinical circumstances." With the push toward evidence based medicine, it was realized that guidelines required more scientific rigor, so in 2011 the definition was changed to, "statements that include recommendations intended to optimize patient care that are informed by a systematic review of evidence and an assessment of the benefits and harms of alternative care options'". In order to have real value, clinical practice guidelines must therefore do two things: Use high quality scientific data to evaluate risks, and beneficial results while presenting alternative approaches for the practitioner and patient to consider.

37. As Dr. Schechter has stated in his expert declaration, he is a co-author of the WPATH chapter on surgery in version 7, and is the co-lead author of the same chapter in version 8. It would be expected therefore that what Dr. Schechter has offered to the court is "high quality scientific data to evaluate risks, and results while presenting alternative approaches for the practitioner and patient to consider", since it is precisely that which is the question here.

38. The first scientific publication he offers is one that is in support of hormonal treatment of transgender persons. It is a paper by Hembree et al.[6] which is itself a clinical practice guideline promulgated by the Endocrine Society (hereafter ES). This guideline was produced in order to update an earlier guideline from 2009. It was produced using GRADE consensus methodology, and is the product of 9 experts who

[5] Guide to clinical practice guidelines: the current state of play; Kredo et al. Int J Qual Health Care. 2016 Feb; 28(1): 122–128.Published online 2016 Jan 21. doi: 10.1093/ intqhc/mzv115

[6] Wylie C Hembree, et al. Endocrine Treatment of Gender-Dysphoric/Gender- Incongruent Persons: An Endocrine Society Clinical Practice Guideline, The Journal of Clinical Endocrinology & Metabolism, Volume 102, Issue 11, 1 November 2017, Pages 3869–3903, https://doi.org/10.1210/jc.2017-01658

formed the committee. The GRADE methodology cautions its users that "inconsistency of result across multiple studies", "indirectness of evidence", "imprecision in measurement", and "publication bias" are to be watched for in its application; essentially that doctors must watch out for sloppy measurement, and bias in the working group. The scientific evidence used to support the Endocrine Society's special treatment guidelines for gender dysphoric/ gender incongruent persons appears to be of low to very low quality, since the clinical recommendations were so equivocal. It was published in 2017 and includes the statement:

"guidelines cannot guarantee any specific outcome, nor do they establish a standard of care": "The guidelines should not be considered inclusive of all proper approaches or methods, or exclusive of others. The guidelines cannot guarantee any specific outcome, nor do they establish a standard of care. The guidelines are not intended to dictate the treatment of a particular patient." P. 3895.

39. As was discussed earlier, this language of uncertainty when included in a clinical practice guideline is what we would expect with low quality evidence. This is what the ASPS would call a Grade D result that rests on level IV-V evidence, and is therefore not useful in directing clinical decision making. This consensus process described by Hembree et al. would likely appear very similar to the decision making that drove peptic ulcer surgery in opposition to evidence that it is a bacterial disease. Academic physicians of the highest caliber were making recommendations to their fellow practitioners, as they are now, based upon anecdotal experience and low level evidence.

40. Just 2 years later, in 2019, the ES , along with an international panel of endocrinology societies, concluded "**the only evidence-based indication for testosterone therapy for women is for the treatment of HSDD** [Hypoactive sexual desire disorder]," and that "There are **insufficient data to support the use of testosterone for the treatment of any other symptom or clinical condition,** or for disease prevention." Also, "The **safety of** long-term testosterone therapy has not been established."[7] It is somewhat alarming to note that these findings are entirely consistent with a consensus statement from 5 years earlier in 2014. In the span of just 5 years, the Endocrine Society consensus has swung from "no other indication for androgen in women" to something akin to, "it is crucial that androgens be given to women who are

[7] Endocrine Society Susan R Davis, et al, Global Consensus Position Statement on the Use of Testosterone Therapy for Women, The Journal of Clinical Endocrinology & Metabolism, Volume 104, Issue 10, October 2019, Pages 4660–4666, https://doi.org/10.1210/jc.2019-01603.

gender dysphoric", and then back to "no other indication for androgen". This kind of consensus oscillation is what you would expect when there is such scant scientific basis for the decision making.

41. Dr. Schechter presented the ES guidelines as the "criteria for initiation of surgical treatment". Between para. 27 and 28 he claims that gender surgery is "often necessary and effective" citing the same ES document. He also correctly reports parallel, committee based consensus statements by the numerous academic societies, produced in the same way, and resting upon similar evidence as discussed above re: ES guidelines, and WPATH "standard of care".

42. In para. 29 Dr. Schechter offers a listing of all of the surgeries that would be considered "generally accepted in the medical community". He does not explain how he measured the level of acceptance that transgender breast and genital surgery enjoys in the medical community. He further explains that all of the listed surgeries are "consistent with the standard of care". By the phrase "Standards of Care", he appears to refer to the WPATH document. The main salient chapter in that document was authored by Dr. Schechter, in his capacity as a leading academic expert in the field of transgender surgery. It is his own expert opinion that he cites as an authoritative guide in surgical decision making in matters relating to the care of transgender persons. Ipse dixit.

42. In para. 30, while discussing surgeries for women who seek to present as men, he describes the removal of healthy breasts and the cosmetic masculinizing procedure as "reconstructive". This is somewhat problematic, particularly as regards the duties of third party payors, including the State of Florida Medicaid program, which includes the management of payment for services. The distinction between what is cosmetic (aesthetic) versus reconstructive is one of the key issues in the just management of payments by insurers, including the State of Florida. This distinction was a daily issue for me when I was in Navy Medical Department leadership. The distinction between cosmetic and reconstructive surgery rests upon objective, observable, repeatably measurable markers of conditions that diminish or impair human functioning. It is based in comprehensive objective knowledge of human structure and function and is in the service of human flourishing.

Reconstructive surgery is the restoration of form and function for a person who has suffered a loss through genetic or developmental in utero accident, trauma, infection, or surgery for infectious events or cancer. It begins with the most comprehensive knowledge available concerning the nature and function of the injured part, and seeks to optimize function as the primary goal, while seeking to restoring the

natural form. Both form and function are understood objectively, and both have subjective effects. Restoration of form and function in a combat injured leg has measurable effects on mobility, range of motion, strength, and capacity for work. Subjectively, the impact is profound as well, but it is not the central purpose of the operation.

In contrast, aesthetic surgery begins in the subjective life of the patient. The patient presents seeking an opinion concerning the aesthetics of a particular feature, such as the nose. They will express a dislike of the feature. Their hope is that by modifying its appearance, they will improve their interior subjective life. What the patient is seeking is a normal human objective: to improve the aesthetics of things, for ourselves and for the people around us. When the surgeon is planning and performing the operation on the nose, there is great objective precision, however all of it is placed in the service of the subjective life of the patient. It is of no use for the surgeon to impress himself with a technically perfect result if the patient loathes it. The surgeon additionally has the grave duty of managing the risk for the patient and weighing it against the potential benefit. The patient must not be submitted for a surgery which entails a significant risk of loss if the surgery is being performed only to achieve an aesthetic outcome. If there is a certainty of loss, then there is a certainty of error. To give a young woman a perfect nose, and in the process destroy her ability to breath through it would be a terrible error of surgical decision making. It is axiomatic in plastic surgery that we are to avoid a predictable sacrificing function in the pursuit of cosmetic improvement.

The procedure which Dr. Schechter labels “chest reconstruction surgery” in female to male presentation patients actually begins with the known expectation that the surgery will produce a degradation or loss of two essential human functions, namely: sexual arousal, and breast feeding. Both functions are permanently and irretrievably lost, and that loss is one of the expected results or the surgery. This step is then followed by the cosmetic shaping of the chest through the use of liposuctioning in an effort to further masculinize the appearance of the chest. This surgery is now being routinely performed on minor girls, and version 8 of the WPATH “standards of care”, written by Dr. Schechter and presented here as authoritative, actually recommends mastectomy in girls as young as 15 years of age. This surgery does not involve the restoration of form and function, and is therefore not reconstructive. It is an operation that begins in the subjective life of the patient, and aims at a result that also resides entirely within the subjective life of the patient. It is thus by definition an aesthetic (cosmetic) surgery. Because it includes the 100% likelihood of a massive functional loss, it must be considered unsupportable as a matter of policy.

Similarly, genital surgery procedures listed in Dr. Schechter's declaration can not be considered reconstructive because they do not meet the definition of reconstructive surgery. They begin with an obsessive concern or anxiety in the subjective life of an otherwise normal, healthy person. They involve the planned destruction of an essential human function, and they are not restoring a form that is missing due to trauma, genetic accident, in utero event, or disease. The surgery seeks to create counterfeit structures that never could have existed in the patient, except as an artifact of surgery. I have done many reconstructive surgeries involving the entire genital area in patients with military injuries and infectious illnesses. If the injury is so devastating as to require a counterfeit structure, and that is all that can be offered, then there is no question as to how the surgeon must proceed.

In contrast, gender affirmation surgery only produces counterfeit structures that are created to serve the subjective life of the patient. Because these surgeries are cosmetic, and because they are 100% certain to produce grave functional losses, they must not be supported as a matter of public policy, and never be paid for by use of public funds.

43. In para.31 Dr. Schechter discusses what he calls "medical necessity criteria for initiation of surgical treatment". His sole reference is the WPATH "standards of care" of which he is the principal author. It should be remembered that the WPATH standard of medical necessity is not supported in reliable scientific evidence, but only on rudimentary, low level data. His first criteria is that the patient must have "the capacity to make fully informed decisions and to consent for treatment". He adds that mental health concern must be "reasonably well controlled prior to surgery".

It is firmly established in high quality research that persons with gender dysphoria have a greater than 30% likelihood of being on the autism spectrum, and a nearly 40% probability of a diagnosis of depression or major anxiety disorder. The proponents of gender surgery will rightly point to the high probability of self-harming behavior, including suicide attempts and completed suicide among self-identified transgender persons.

In my experience as a surgeon, it is considered imprudent to obtain consent from patients suffering from psychological conditions that provoke the patient to acts of self-harm and/ or suicidal ideation. These psychological disturbances are known to impair the patient's capacity for understanding the information offered by the surgeon, interpreting that information, and reasoning from that information. If those capacities are

impaired by psychological disturbances sufficient to consider suicide, then meaningful consent must be questioned. Certainly in the case of conditions that constitute a threat to life and limb in a patient with decreased competence. consent may be obtained with the assistance of family, guardian, or in particularly urgent cases a group of professionals who agree on the grave necessity to proceed with surgery.

The difficulty here is that none of the surgeries on Dr. Schechter's list are emergency operations performed to save the life of the patient. They are all elective (scheduled when convenient and patient is deemed ready). Furthermore, an ever-growing percentage of patients submitted for gender surgeries are minors who by definition are not competent to consent. The claim is made that these surgeries are in fact "life saving". This is a claim that is not supported in high quality scientific evidence. In fact high quality evidence, which I will present below, shows that while self-harm and suicide rates are improved in the very short term for some sub-groupings of patients, in the long-term these problems remain if not worsen.

When Dr. Schechter speaks of the need to have these psychological disturbances "well- controlled" prior to surgery, he must certainly mean that self-harming or suicidal thoughts have been well controlled. If that is the case, then the chiefly claimed reason for the surgery has been successfully treated medically, and the patient would no longer require the surgery.

What is troubling is that the co-morbid conditions of autism spectrum disorder, clinical depression, and major anxiety disorder are never examined as the possible causes of the gender identity disturbance. These are conditions that, if treated, might improve if not resolve the gender problem. To the contrary, these serious problems are viewed as mere impediments to gender surgery that must be "reasonably well-controlled" so that surgery may proceed. This is consistent with the regnant WPATH model that there is a single explanation (disconnection between biological reality and subjective identity which has an as-yet undiscovered cause), and has only a single solution (social, medical, and surgical affirmation). Such a single cause/ single solution assumption would seem to be unlikely, given the massive range and the recent complete reversal in the demographics of transgenderism. What used to be a condition that was nearly exclusively found in little boys (and resolved nearly 90% of the time), is now predominantly a condition affecting young women, and at a rate that has risen between 4000 and 5000% in the course of the last decade.(Shreier et al.)

44. In para.33 Dr. Schechter begins the evidentiary support for the assertion that "Gender Confirmation Surgeries are Medically Necessary". In para.34 he states that the "medical community generally considers those surgeries to be medically

necessary". He makes this assertion of broad acceptance in the medical community without evidence to support it. How was the data gathered and interpreted to support the assertion that the medical community broadly shares his opinion? I am not aware of any published study that has measured the acceptance of this therapeutic model among the members of the medical community. The only evidence he has presented concerning the broader medical community is the collection of consensus statements, produced by the various specialty committees which was produced using the faulty methodology described above, and supported with the lowest levels of evidence.

45. In para.34 Dr. Schechter asserts that "gender confirming surgeries are not cosmetic because, when performed in accordance with the Standards of Care, they are clinically indicated to treat the underlying medical condition of gender dysphoria". He appears to be saying that medical necessity exists because he said so himself when he wrote the chapter for the Standards of Care. He is again standing upon his considerable expertise, but he hasn't up to this point supported the assertion with scientific evidence. This is entirely analogous to the history of peptic ulcer surgery: driven by the highest level of academic expertise, but lacking rigorous scientific support.

46. In para. 35 he continues his assertion that gender surgery is not cosmetic by making the claim that gender dysphoria is a medical diagnosis. This is an extremely problematic statement for him to make, given that the diagnosis comes to us from the Diagnostic and Statistical Manual of Mental Disorders (DSM) vol. 5 (2013). As Dr. Schechter described in his outline of the diagnostic and pre-operative selection for surgery, the patients are referred to him by a process that begins in psychology, continues with psycholgical support, and concludes with certification by psychological services that the patient is ready for surgical modification. At no point has he described any medical diagnostic process of history-taking, physical examination, laboratory evaluation, or radiographic examination that he has used to confirm a diagnosis which was produced and sustained by psychological services. The indication for surgery begins in the subjective life of the patient. Surgery is offered to the patient with the assurance that it is likely to improve the subjective life of the patient, and is therefore by definition cosmetic surgery.

47. One of the essential mechanisms that third-party payors (including state Medicaid agencies) have for distinguishing reconstructive surgery from cosmetic surgery is found in the pathology department. Two operations may be outwardly identical even while one is reconstructive and the other cosmetic. An excellent example is the breast reduction surgery which I discussed in my previously submitted assessment. This surgery may be reconstructive when the size of the breast is being

sufficiently reduced in order to remedy a condition of orthopedic pain. These patients suffer from chronic neck, back, and shoulder pain caused by the orthopedic effects of their heavy breasts. The same, technically identical operation, might be done for purely cosmetic reasons. In the case of reconstruction, the patient has a condition that causes lost time from work, frequent covered visits to physical therapy, or to pain clinics, chiropractors and radiologists. There is abundant actuarial data, based upon the highest levels of scientific support, that a breast reduction of sufficient weight (based upon the anthropometric measurement of the patient) has a very high probability of resolving the chronic pain. High quality medical literature that addresses this issue, and its importance to insurance plans, is typically very precise in its data gathering and actuarial interpretation.[8] However, pain cannot really be measured. Pain is reported by the patient. Health insurance plans are able to distinguish cosmetic breast lift from reconstructive breast reduction based upon the reported weights of the breast tissue that is submitted to pathology. An objective, repeatable medical test, with known error rates is used to confirm the diagnosis, ensure correct care for the patient, and separate cosmetic surgery from reconstructive surgery in the interest of preserving medical resources and preventing fraud. No such process exists in the case of mastectomy for chest masculinization. There is no physical, biochemical, hormonal or tissue pathology, that can be demonstrated to localize the patient's condition in her healthy breasts. It is the young woman's subjective sense of revulsion when she looks at herself that has caused her to believe that mastectomy might make her feel better.

48. Similarly, the pathology department helps us to make distinctions between reconstructive and cosmetic surgery in the case of gynecomastia, which is a condition that affects biological males, and results in the growth of female glandular breast tissue. Men who have enlargement of the soft tissue of the chest area may come to the surgeon seeking improvement in appearance. Most often this problem is caused by varying degrees of obesity, and in such cases surgical improvement would be rightly categorized as cosmetic. The patient is motivated by a dissatisfaction with his appearance, which he wishes to improve, so that he can feel better about his appearance. In contrast, the man with gynecomastia might present with a mass behind one nipple, that is tender, and disfiguring. Perhaps it interferes with his occupation because it is impinged upon by safety equipment that he has to wear in his occupation. In this case, the surgical removal of the mass (gynecomastectomy) would rightly be considered reconstructive because surgery is used to restore function and form in a person who has an objective finding (female glandular tissue in a male chest). As in the earlier case, insurance plans rely on the evaluation provided by the pathology

[8] Accuracy of Predicted Resection Weights in Breast Reduction Surgery: Kung, Theodore A. MD; Plastic and Reconstructive Surgery - Global Open: June 2018 - Volume 6 - Issue 6 - p e1830

department when surgical specimens are submitted. The lab, using proven, repeatable, accurate testing methodologies can report either the presence of nothing more than adult fat tissue, as would be expected in the case of the cosmetic chest re-contouring, or they would report the presence of "fibroglandular breast tissue", which would be the expected and necessary finding that would warrant payment for a reconstructive surgery that is a covered benefit in the insurance plan. Thus, the findings provided by the pathology department serve to confirm what expert opinion, supported by high quality evidence, diagnosed before the patient was brought to surgery. Such objective, measurable, repeatable findings from pathology help to advance the quality of care because outcomes of care can be evaluated in the long term, and the consistent language and data can be compared among multiple studies from multiple centers of care, thus increasing the reliability of expert opinion throughout the medical community.

In footnote #8 of Dr. Schechter's declaration he says that "Dr. Lappert incorrectly refers to breast growth in transgender women as "gynecomastia", and that "Gynecomastia refers to enlargement of the male breast, not to breast growth in transgender women" (biological males who present as women). From the standpoint of the validity of scientific-medical information, this is a very troubling statement for Dr. Schechter to make. He is essentially saying that the validity of objective, measurable, repeatable findings of the pathology department are completely negated by the subjective condition of the patient. The patient's subjective interior sense of themself cancels the objective validity of medical knowledge.

49. In para. 38 Dr. Schechter begins the assertion that gender surgery "has been demonstrated to have other salutary effects, such as improving quality of life and reducing negative health outcomes". In support of this assertion he cites a 2019 publication by Miller et al. He summarizes the findings of the paper with a declaration of subjective satisfaction on the part of the patients (that 100% would do it again). Upon examination of Dr. Miller's paper we see that it is a report of a single surgeon retrospective review. It begins with a chart review of only 34 patients, only 12 of whom responded to the quality of life questionaire. This means 74% of the study patients dropped out (patient self-selection bias, with drop out rate greater than 20%). All of the data is based in subjective reporting by the patients, rather than objective findings (such as substance abuse rates, psychiatric hospitalization rates, suicide attempt etc.). Such reporting which is purely subjective (satisfaction survey, quality of life survey etc.) is characteristic of the cosmetic surgery literature, not the reconstructive surgery literature. He reports that "every patient surveyed at 1 year" reported that "their life had changed for the better". This statement is again reporting only subjective data, this time following a meaninglessly short follow-up of a very small group that has been biased by

self-selection. The overall study is little better from the standpoint of the duration of the study because the final follow-up was only 4 to 7 years. This paper presents level-IV and level-V (low to very low quality) evidence, and is therefore only useful in suggesting further research, particularly since it is authored and cited by subject matter experts. It is not useful for clinical decision making. Neither can it be presented as evidence for anything more than a cautiously worded practice guideline (as in the ES guideline concerning the use of cross-sex hormones presented above), and certainly can never be used in support of a "standard of care". It is important that this was the first paper Dr. Schechter cited in support of efficacy, given the weakness of its content.

50. In para. 40, Dr. Schechter continues his presentation of the scientific evidence that demonstrates the efficacy of gender surgery by citing a 2006 paper by Newfield et al. Dr. Schechter summarizes the paper's findings saying that mastectomy and chest masculinization in transgender biological females "increases self-esteem and improves body image" while providing the patient with "some security and safety for those who remove their shirts in public areas such as gyms or beaches". In reading the entirety of the paper I found that it did not demonstrate that at all. The passage Dr. Schechter quotes is a personal editorial opinion expressed by the authors in support of transgender surgery. It is found in the preamble to the paragraphs that describe their study of the self-identified, but never verified, post surgical patients.

This paper is a report of an anonymous survey. It claims to provide meaningful information about the effect of female to male transitioning medicine and surgery without even verifying that the subjects who responded to the survey have in fact undergone medical and surgical gender transition. Subjects were recruited **"via online promotion and printed materials, including flyers and postcards that were distributed to San Francisco Bay Area community centers, cafes, stores, and health clinics that serve the transgender community."** In terms of self-selection bias (patient determines who is followed by the study) it is hard to imagine a more problematic patient selection process. The researchers even admit that they were unable to determine how many surveys may have been submitted multiple times by the same study respondent. They write: "Although this procedure *helped* (italics mine) prevent duplicate submissions by the same participant, we could not employ more sophisticated computerized systems due to administrative and financial constraints".

All of the demographic information contained in the study was self-reported but not verified, including age, sex, health status, history of hormonal therapy, and history of gender surgery. The study uses a quality of life survey with 36 questions in 8 areas of interest, producing only self-reported subjective information. Even the claim of simple

benefit is poorly support, as is reflected in the conclusion to the paper. The authors write, **"The 376 US FTM transgender participants analyzed in this sample had diminished mental-health related QOL compared with the general US population, as measured by the SF36v2. These findings are consistent when compared against specific age and sex norms."** This statement demonstrates the lack of value in the study. The study participants demonstrated a quality of life that is statistically significantly lower than the age/ sex comparison cohort, and the authors can only speculate as to the cause. There is no way to tell if treatment helped, had no effect, or harmed the patients because there was no information available about the anonymous subjects. This is because the anonymous test subjects hadn't received pre-treatment evaluation using the same or comparable test instrument. This study, selected by Dr. Schechter to support the claim of efficacy is of the lowest evidentiary value, may be useful for suggesting future research, but is of no value in directing clinical decision making, or meaningful allocation of public resources in the service of health.

On the Safety of Transgender Surgery

51. A discussion of surgical safety must include anticipated losses which are either expected, or even remotely possible. In order to examine the comparable issues in transgender versus reconstructive surgery our effort is simplified by comparing identical operations.

On several occasions I have performed the reconstructive surgery called "Sensate radial- forearm microvascular free flap hypopharyngeal reconstruction". I operated in order to reconstruct the tongue and throat of patients who had suffered a grievous wound of the mouth and throat that resulted from removal of an aggressive cancer. The defects caused by that wounding needed to be replaced with thin, pliant, abrasion and lfluid resistant tissue. It needed to provide the patient with sensation in the reconstructed area so that they can feel the food and liquids in their mouth, and manipulate the food so as to swallow it. We selected an area of skin on the inside of the forearm that has regular and robust blood allow, is thin and durable, and has an easily dissected sensory nerve that can be attached to the nerves in the wound. The forearm flap satisfied all the requirements. An operation of this complexity, duration, and technical requirements has many issues, big and small, that can diminish or destroy the result.

That throat reconstruction operation is in almost every way identical to the second most commonly performed female to male (FtM) genital surgery, the "Sensate

radial forearm microvascular free flap phalloplasty", which I understand is performed by Dr. Schechter. In that operation, the identical flap is raised and transferred. It also must be resistant to abrasion, be water tight, pliant, sensate, and of correct volume. Through a process of incision, plication and suturing, a tubular phallus is constructed within which is a skin lined tube which will serve as the urethra. The suture closures in both flaps is where most things go wrong because the skin edges that define the suture line can lose sufficient blood supply. In the phalloplasty, when this happens, the patient suffers from delayed healing, urine leakage, varying degrees tissue death, and scarring. All of those problems can happen with either the throat flap, or the phallus flap. When the phallus flap fails, the patient suffers due to varying degrees of tissue loss, chronic urinary leakage, or urinary obstruction due to scarring that can cause kidney injury if left un-treated. When the throat flap fails, bacteria laden saliva will leak into the neck where it can cause fulminant infections, or erode into a major artery and cause the patient to bleed to death in a matter of moments. A singularly terrible event.

In the case of the throat operation, if the removal of the cancer had not been performed, there was a known and significant probability that the cancer would have eroded into the tissues of the neck and caused a fulminant infection, or eroded into a large blood vessel, as described above. In contrast, if the phallus flap operation, had not been performed, the patient would have remained fully functional in every human capacity, though suffering from an inner subjective disturbance called gender dysphoria, which has not yet been adequately addressed.

Both operations involve the use of a highly complex surgical techniques to remedy a wound. In the case of the cancer operation the wound was the result of a cancer that would have ended in a terrible death. In the case of the phallus operation the surgeon is creating multiple physical wounds (castration, loss of pelvic organs of reproduction, de-gloving injury of the forearm, skin graft donor site injury), with their associated risks of complications. The surgery is performed in attempt to remedy a subjective, patient-reported sense of their identity.

Clearly the pre-operative condition of the cancer patient is far more grievous than the condition of the young person who is suffering from gender dysphoria. The cancer patient would likely be more than willing to endure significant loss, such as voice, or teeth, or the sense of smell. And yet, if I were discussing surgical risk pre-operatively with my patient who has the throat cancer, and told him that there was a certainty that in the course of the operation he would lose all of his reproductive organs, he would be justified in asking why he was being subjected to such an unsafe operation. The patient wouldn't be even slightly interested in any discussion of infection risk, wound healing

problems, or scarring. The question of safety addresses itself to the question of potential loss. Transgender surgery of the genital apparatus predictably causes grievous loss that dwarfs such complications as infection, local tissue loss, urinary leakage or scarring.

52. Returning from safety to efficacy in para. 43, we have a reiteration of an expert, professional opinion, and referring to a "substantial body of peer reviewed research". This is the next issue we must address: the use of the phrase "peer reviewed". Peer review is the very important process by which highly educated and trained experts review scientific medical papers for publication. They are examined in order to ensure that the corpus of medical literature is protected from imprecise, substantively erroneous, or conceptually flawed publications. It is an essential part of the historic, magisterial process in medicine. In fact, it is so much a part of the life-long learning process of the doctor that any worthy training program will have a robust "journal club" in which doctors at every level of training take a turn at publicly "peer reviewing" an article and leading a lively discussion of its value. A good doctor is constantly peer reviewing. It is an essential element of good medical care.

Establishing that an article is peer-reviewed is a basic, and essential practice. You might read a medical paper with level-III evidence of high quality, or a paper that is level-V evidence of low or questionable quality, both of which undergo peer review, and are published. The level-III will likely drive decision making, and possibly a recommendation as high as "standard of care", while the poor-evidence paper drives research, or perhaps the consideration of an alternative approach, if that approach does not put the patient in any significant risk.

53. In para. 44 he presents another peer reviewed article from 2013 by Weigert et al. which he summarizes. He asserts that the article demonstrates a statistically significant improvement in "psychosocial well-being" following cosmetic breast augmentation in biological males who are presenting as women. This paper is very simple to analyze and classify as not meaningful for informing clinical-therapeutic decision making. At the bottom of the published abstract, and on the front page of the full article is written, "Clinical question/level of evidence: Therapeutic, IV." As was discussed above, this paper is at the same low level, because it is a single-center sampling of a small cohort of patients, that relies on subjective, self-reporting through questionnaire, over a short study duration. Patient collection was made between 2008-2012. The paper was published in 2013. If the peer review process followed the usual time-line, it is likely that there are a significant number of patients in the study who were followed for less than a year. The authors, in the abstract only claim the pre-surgery, and the 4th month post-surgery as assured time points. This is remarkably short follow

up even for a cosmetic breast augmentation cohort. The article is perhaps useful in suggesting inquiry into why their cohort reported no improvement in physical well-being, given the known association between emotional health and physical health. There is nothing in the article that would rise even to the level of a guardedly worded clinical guideline suggestion.

54. The next citation offered in support of gender surgery is another peer-reviewed study by Horbach et al. published in 2015. This is a review of transgender surgical literature published between 1995 and encompassing nearly 20 years. It yielded 26 papers that satisfied the search criteria, and includes 1,461 patients. As Dr. Schechter quoted, the paper claims that "transgender women (biological males presenting as women) who had vaginoplasty found that study participants' mean improvement in quality of life after surgery was 7.9 on a scale of one to ten". In the conclusion of the paper the authors write, ""Sexual function and patient satisfaction were overall acceptable, but many different outcome measures were used. QoL was only reported in one study. Comparison between techniques was difficult due to the lack of standardization."

Of the merely 26 studies out of a sampling that spanned 20 years, only one paper was found to have used a standardized metric, one that only measures subjective, patient reported information, and the rest could not even be compared to each other. The authors write, **"The available literature is heterogeneous in patient groups, surgical procedure, outcome measurement tools, and follow-up. Standardized protocols and prospective study designs are mandatory for correct interpretation and comparability of data."**

56. This result is startlingly reminiscent of a paper published by Tolstrup et al. published in 2020. It is a comprehensive literature review on the subject of breast surgery in transgender patients, including both male to female, and female to male presentation. It is a scoping review that yielded 849 papers of which 47 papers met the inclusion criteria based upon title, abstract, and full text. In the study results,the authors report that,

"The summary of outcome domains and classifications showed that there are large variations in outcome evaluation between studies. Although several studies reported on similar outcome categories, there was a high level of heterogeneity of domains and classifications of outcomes." The authors then conclude by explaining that **"Evaluation of outcomes in gender-confirming chest**

surgery showed large variations in reporting, and further streamlining of reporting is therefore required to be able to compare surgical outcomes between studies."

None of the articles examined rates of psychiatric hospitalization, substance abuse,

self-harm behaviors, or suicide. This tells us that the most compelling reason offered for performing these surgeries (psychological distress and suicide risk) isn't even evaluated with regard to efficacy in the world transgender surgery literature.

55. Professionally speaking, these are very disappointing findings from comprehensive examination of the transgender surgery literature. To have a surgical sub-specialty working diligently, and guided by professionals at the highest levels of academic expertise, that has only produced case-series reports, retrospective case collections, and fruitless 20 year literature reviews, and still only have level-IV and V evidence to show for it is alarming. It shows that the sub-specialty has not developed uniform descriptive language, standardized reporting nor test instruments that might raise the value of expert opinion to a level that could make reliable recommendations that might help in surgical decision making, rightly inform the consent process, or guide decision making by officials entrusted with the care of public and private medical resources. It would cause me to make a sober review of the medical and surgical principles that are guiding this work.

56. The next article cited by Dr. Schechter is one presented in support of masculinizing chest surgery in minors and young adults. This is the mastectomy and liposuctioning surgery discussed above. It is a 2017 paper, published in the peer reviewed journal JAMA Pediatrics. Dr. Schechter asserts that the paper demonstrates that "surgical intervention (mastectomy)positively affected both minors and adults". This paper is perhaps the most alarming of all the citations offered.

The principle author, Dr. Olson-Kennedy is also an academic expert in her capacity as Associate Professor of Clinical Pediatrics, Keck School of Medicine of USC, and Medical Director of The Center for Transyouth Health and Development in Los Angeles. She holds professional membership in The Society for Pediatric Research, the World Professional Association for Transgender Health(WPATH), and the Society for Adolescent Health and Medicine. If any expert would be in a position to offer high quality, evidence based publications, she would certainly be in contention.

In their summary of findings, the authors report that "chest dysphoria" is common

among “trans males” (natal females seeking to present as males), and that the dysphoria is decreased by surgery. They claim that regret for surgery is “rare”. The article reports breast removal surgery on at least one girl aged 13 years. The average age was 19. Children were entered into the study through recruitment from among patients visiting the clinic, and by telephone over a six month period. The authors found that patients recruited from among visitors to the clinic (convenience sampling) yielded an abundance of non-operated patients, so they were forced to reach out to all the post surgical patients by phone. 26% of the clinic’s post surgical patients could not be reached for various reasons including no working phone, or failure to respond to multiple messages. A 26% drop-out rate is never questioned by the authors. Were they lost to follow up because of dissatisfaction, psychiatric hospitalization, or suicide? This problem is called “self-selection bias”, and is evidence of careless study design. Of the remaining 74% of patients, only 72% of them (only 53% of the study patients) completed the survey. This is a second example of self-selection bias. Why would some post surgical patients who had been successfully contacted, not complete the survey? The authors do not ask the question.

In the study, dysphoria was measured using “a novel measure” (an unproven test instrument) which was a series of subjective questions about happiness. Among the designers of this novel test instrument were some of the adolescent patients themselves. Their flawed methodology included the use of an entirely unvalidated test instrument, with no known error rates, or proven predictive power, **that was in part designed by the minors and young adults who were the subject of the study.**.

Furthermore, the post surgical patients were given the survey at varying time intervals post surgery. The longest interval between surgery and the satisfaction survey was 5 years, but children less than a year post surgery were included in their flawed sample, and yet the authors claim evidence of “negligible regret.” This is a remarkable claim given that long term, longitudinal population studies show that there is a dramatic rise in post surgical problems such as depression, hospitalization, substance abuse, and suicide beginning at around year 7 post surgery(Dhejne). Surely Dr. Olson-Kennedy is familiar with the international literature on transgender outcomes?

Having promised in the introduction to her paper that “chest dysphoria” is reduced by surgery, at the conclusion they confess the fact that the study design and execution produced very low quality data that is not useful for patient selection, or prediction of outcomes. They even confess that the study does not address the efficacy of surgery in improving outcomes regarding the single most compelling reason for

performing the operation: mitigation of depression and suicide. The authors write: **" An additional limitation of the study was the small sample size. The nonsurgical cohort was a convenience sample, recruited from those with appointments during the data collection period. There could be unknown imbalances between the nonsurgical and postsurgical cohorts that could have confounded the study findings.**

Finally, the Chest Dysphoria Scale is not yet validated, and may not represent distress or correlate with validated measures of quality of life, depression, anxiety, or functioning."

This paper is a typical example of a publications which are used to support transgender medicine and surgery, written by board certified transgender expert physicians who practice in our nation's largest pediatric gender clinics, and was published in peer-reviewed medical journals. The article is essentially useless in making any clinical decisions regarding who should be offered surgery, what the likelihood is that they will benefit from it, or the likelihood that they will regret their decision. Most importantly, it can not even vaguely estimate if the risk of hospitalization, incarceration, or suicide will be reduced. For the same reason that the paper is not useful in clinical decision making, it is likewise not meaningful in decision- making for persons charged with the just management of public and private medical resources.

On the Experimental Nature of Transgender Surgery

57. In para. 47 Dr. Schechter brings into question the assertion that transgender surgery is experimental in nature. He begins this response with the claim that referring to transgender surgery as experimental is "unsupported by the professional medical consensus and prevailing standards of care for treating gender dysphoria, and is inconsistent with mainstream medical standards." We previously discussed the lack of intrinsic validity of consensus methodology when evidence to support the consensus is weak. We discussed the evidentiary requirements for claiming a "standard of care"; that one must present evidence that is high (III or better) and consistent over multiple studies (with obvious exceptions when the consequences of error are minor). We also reviewed the historic meaninglessness of the phrase "mainstream medical standards" when trying to resolve the issue of the validity of novel research finding.

It is to be remembered that whenever there is a breakthrough in medical-scientific knowledge, it is characteristic that academic experts are proven to have been

in error. This is in the nature of the scientific method. As will be recalled from earlier discussion, the greatest names in academic surgery for 100 years wrote papers, published books, trained residents, wrote board questions, and examined candidates for board certification fully convinced that ulcer disease of the stomach is a surgical disease that demanded some of the most advanced operations known at the time. They considered it not only necessary, but in fact lifesaving beyond a shadow of doubt. These were great, and well-intentioned surgeons laboring under a poverty of information for 70 years, then laboring under an abundance of confidence that they could not possibly be wrong about their condemnation of the infection theory of ulcer disease. Eventually, high quality evidence proved them wrong, and now surgery for gastric ulcer disease is rarely seen. Through the whole history of ulcer surgery, the principle of acid reduction through nerve sectioning and drainage was "mainstream". This test of mainstream acceptance in the medical community, which Dr. Schechter offers, is a test with no predictive value in making either clinical or medical executive decisions on the question of the validity of transgender surgery.

58. One of the important usages of the term "experimental" in the world of medical care is in the domain of insurance services, both public and private. Leadership in these agencies is charged with the responsibility of managing medical resources in a way that both preserves resources, while at the same time applying those resources to the patient as correctly as medical science and their own actuarial information will allow. Whenever a novel therapy is proposed for a given condition, insurance services will examine the medical and actuarial data to see if the proposed therapy is likely to yield a result that serves those two purposes (health of the patient, and financial soundness of the insurance process). Typically, in the early years of a new treatment there is resistance on the part of the payors because early on (as discussed in detail above) all that the proponents are able to present is low-level scientific papers that present anecdotal case collections without controls, or multiple studies that can not be compared due to methodological variation, or are methodologically questionable due to unvalidated test instruments. We surgeons are inclined to think uncharitable thoughts when reading letters from insurance plan managers. But history has shown, and the fact remains, that good surgery demands good evidence, particularly when permanent damage to the client is a possible result.

Nonetheless, if the reviewer see evidence that a new approach may be helpful, they prudently insist that therapies of known value be tried to their reasonable limit , and that they be found to fail in solving the patient's condition, before considering the new treatment.

This dynamic process between the patient, the physicians, and the insurance industry has many problems, but good, well validated scientific evidence is not one of those problems. In fact well validated science is typically the best remedy for those problems. Sometime the good science is from the doctors, and sometimes the good science comes from the actuaries. In both cases the patient benefits.

59. From the perspective of the case in question, this sense of the term "experimental surgery" may be the most important. How did the affirmation care scientific model and its associated social, medical and surgical treatments enter into the mainstream of the medical community. Did it follow this same process of gradual acceptance in both the medical and insurance communities through a process of steadily improving evidence levels? Was it used on a careful trial basis after having exhausted treatment by established methods? The answer is, "in a way, yes it did".

60. The historically validated treatment model for what is today called gender dysphoria is what is called "watchful waiting". On hearing the name one is tempted to think of this as a resignation to inaction. It is not. It is a psychotherapeutic process that is rooted first in an examination of the cognitive processes of the child, and seeing how the child has responded to the reality of their life. For this reason, in order to be effective, it must include family therapy. The goal is to keep the anxious and confused child in loving contact with reality, while seeking to understand and remedy the dynamic that is provoking the condition of distress. Occasionally, short courses of anxiety medications may be used to manage the intensity of obsessive thinking that has become centered on their physical appearance. It is essentially the same process used in helping persons who suffer other obsessive- compulsive issues, like eating disorders. Psychological research, having high level evidence, has shown that over the course of time this approach results in over 80% resolution of the cross-sex self identification during adolescence, and nearly 92% by young adulthood.[9]

61. This watchful-waiting approach is likely the reason why transgenderism used to be a rare diagnosis. The vast majority of people with the condition resolved the issues, and went on to live their lives without the need for life-long medications, without destructive surgeries, without the loss of their sexual faculties, and without the loss of fertility. What has happened, however, is that the dynamic between patient, physician,

[9] Zucker, K. J. (2018). The myth of persistence: response to "A critical commentary on follow-up studies and 'desistance' theories about transgender and gender nonconforming children" by Temple Newhook et al. International Journal of Transgenderism, 19(2), 231–245. Published online May 29, 2018. http://doi.org/10.1080/15532739.2018.1468293

and insurance services has been severely disrupted. The science based medical and actuarial management of this condition has been separated from the evidence, and now rests entirely on the opinions of academic experts who have managed to influence the decision makers in their favor. In large part, they have accomplished this by never speaking about watchful waiting except to dismiss it as folly. This process of silence and dismissal is exactly what ulcer surgeons did to the proponents of the scientifically correct infection model of ulcer disease.

62. Sadly, silence and dismissal about watchful waiting is not the only reason for the 5,000% increase in the diagnosis of transgender over the past decade. Surgeons who were seeking to achieve the best results for their transgender patients came to realize that most of the difficulty with good cosmetic results was that young men seeking to present as women looked too masculine, and young women seeking to present as men looked to feminine. If their masculine or feminine development had been arrested early, the surgeons theorized that they would achieve better results. It was reasonable, in light of their treatment model, to think that a better cosmetic result would mean a better resolution of the gender incongruence. Thus the idea was born that the earlier the child was transitioned, the better the cosmetic surgical result, and thus the better psychological result, which was the goal.

This theoretical improvement in outcomes for transgender persons through early transition was certainly an idea worth investigating. Because the lifelong effects of the approach might include some really bad outcomes for the children, and because the actual outcomes were unknown at the time, it would have been prudent, and scientifically consistent to categorize this from the above described insurance industry perspective, as experimental. It would have required that the patients exhaust the fully established and proven treatment model of watchful waiting. If that treatment failed to resolve the issue, then on a trial basis, and supervised under very strenuous human experimentation oversight, the affirmation model could be tried.

In order for this highly supervised experimental approach to pass ethical standards in human experimentation there would have to have been a previously established diagnostic and patient selection process of very high specificity. If the proven and established method of watchful waiting is yielding 92% resolution, then what the ethically minded surgeon is really supposed to be doing is trying to find that 8% of children who would have failed watchful waiting, and select them out for surgery earlier in their life. Then studying the result on a very long-term and comprehensive basis, he would have been able to provide high-value evidence that his hypothesis about the

successful early management of transgenderism is a safe and valid option for his patient. This was not done. Instead, the routine social and medical transitioning of children began, which includes puberty blockade, and cross-sex hormones in children and youth.

Instead of seeking the historic, and small cohort of patients who would have carried the condition into adulthood and treating them, physicians and surgeons are treating all of those children now. Instead of seeking the scientific methodology with which to make a correct diagnosis so as to increase the likelihood that you are operating on the right person, the transgender treatment model is essentially turning all affected children into "the right person". By the time the youth or young adult person arrives in the surgeon's office the process has been locked into place.

63. It would seem that the best course of action for those who serve the insurance industry is to return this process to the time tested dynamic model of patient, physician, and insurance plan discussed above. Because the affirmation model rests upon such low quality evidence, it seems justifiable to suspend financial support until such time as testing and patient selection processes are improved to acceptable levels of reliability. Given the serious, life long consequences of mis-diagnosis, and the misapplication of surgery, levels of patient selection reliability would have to be quite high.

64. There is the additional issue of liability for insurance providers, including the state. The massive surge of patient numbers over the past 10 years would predict that large numbers of patients are reaching the point where affirmation interventions will have been exhausted, and the numbers of treatment failures will start to rise dramatically. We are beginning to see this in the increasing numbers of "regretters" whose support groups and legal representatives are sounding the alarm. This may represent a serious liability to insurers in addition to the physicians and hospital systems that have taken such an active role in promoting this unproven treatment model.

65. A review of the European literature on this topic forms the basis for this anticipation of liability and merits review here. As we have seen in this review of Dr. Schechter's expert declaration, the American literature used in support of the claim of benefit is of low reliability. We make this assertion based on the expectation the Dr. Schechter has provided the court with the best evidence available. One of the features of all those papers taken in aggregate is that they report studies of short duration. Follow up durations of less than 3 years are common. Some, as we have seen are as

short as 4 months. This fact helps us to understand why proponents of the affirmation model are enthusiastic. We see in the European literature that there is a trend line that looks favorable for up to 7 or 8 years, after which result decline precipitously.

66. As described in my previous report, the Swedish medical establishment maintains an excellent and centralized data base of all episodes of care for beneficiaries. It uses uniform language, and captures treatment events at all levels, from school clinics, to psychiatric hospitals, to prison infirmaries, to public clinics. The database can be analyzed for such things as emergency room visits, drug addiction treatment, hospitalization for suicide, psychiatric admissions for self-harming event etc.

In 2011, Dhejne et al.,published a population based, longitudinal cohort study of the database that sought to examine the lifetime hazard ratio of such things as substance abuse, incarceration for violent behavior, psychiatric hospitalization, and completed suicide.[10] This is level-III evidence of high order given the methodology employed and the proven reliability of the database. It examined persons who have fully completed transition and compared them to age and sex matched controls in the Swedish population. It did not use anonymous surveys, or other faulty convenience sampling. It found the post-transition patients by finding the associated episodes of care, such as i hormone therapy prescriptions began, or admission for gender surgery occurred. The data set spans 30 years. What it shows is that fully transitioned subjects showed a relative risk of suicide roughly equal to the age/ sex matched controls, but the effect appears to last for just a few years. The trend line for death from any cause begins a sharp drop at approximately 10 years and continues to drop massively over the subsequent 15 years. When the researchers looked at the aggregate life-time relative risk of suicide, persons who fully transitioned were 19.1 times more likely to have killed themselves when compared to age and sex matched controls. If you only look at the subgroup of biological females who transitioned to male-presentation, the risk of suicide is 40 times higher than the control group. Results such as these, because they are obtained using tested and reliable methodology, are able to help meaningfully in clinical and administrative decision making, and in several European countries it has.

67. Over the past several years, the medical systems in Great Britain, Sweden, Finland, and France have stepped away from early medical and surgical

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[10] Long-Term Follow-Up of Transsexual Persons Undergoing Sex Reassignment Surgery: Cohort Study in SwedenPLoS One. 2011; 6(2): e16885.
Published online 2011 Feb 22. doi: 10.1371/journal.pone.0016885

transitioning of the young. The Tavistock-Portman Institute in London, which was the sole provider of these services to children in Great Britain was closed recently following the public declaration by a review committee that the Institute was "unsafe for children".[11] Similarly, the Karolinska Institute in Stockholm reversed its policy by suspending the medical and surgical transitioning of the young in favor or psychological support and treatment.[12] Similar changes in treatment guidelines for self-identified transgender youth have been published in Finland. Based upon these developments in Europe, it is very troubling to read assessments or declarations by leaders in the field of transgender surgery which assert that these treatments are mainstream and beyond controversy, or that they are part of a core curriculum of surgical training, or that an oral board examiner might fail a candidate surgeon if their answers reveal a reticence to join the mainstream that Dr. Schechter has described. The world literature demonstrates emphatically that early medical and surgical transitioning is in fact so controversial that medical leadership in multiple countries has put a stop to it.

68. In summary, transgender surgery is based in a treatment model of affirmation that lacks scientific support based in quality evidence. The scientific support offered by the leaders in the field is entirely composed of small studies, single provider /single center studies that are lacking in control cohorts, and are often rendered uninterpretable due to haphazard case-collections such as the solicitation of study participants without methodology to confirm that the patient is a treatment subject. All of the studies cited have short follow-up, and most studies suffer from massive self-selection bias and high drop-out rates. The studies often employ untested assessment methodologies, and all of the literature cited by experts report only subjective data, which is typical of papers that address outcomes in cosmetic surgery. Transgender surgery is, by definition, cosmetic surgery. The move towards surgery begins in the subjective life of the patient, is conducted with the aim of improving the subjective life of the patient, and outcomes are measured in subjective terms based in satisfaction surveys. Transgender surgery violates fundamental principle of cosmetic surgery, because it predictably destroys essential functions of the human person. It is not reconstructive surgery because the patient is physically healthy before the surgery, and has no definable deficit that can be objectively characterized to be the cause of the presenting complaint. There is no objective test to confirm the diagnosis of transgender, and no way to correctly select patients for surgery from among the young. The enterprise of gender affirmation medicine and surgery is based entirely in a consensus

[11] NHS to close Tavistock child gender identity clinic https://www.bbc.com/news/uk-62335665

[12] rSweden's Karolinska Ends All Use of Puberty Blockers and Cross-Sex Hormones for Minors Outside of Clinical Studies https://segm.org/Sweden_ends_use_of_Dutch_protocol

of expert opinion of low reliability because it is supported by unreliable scientific evidence.

69. Based upon all of the above examined findings, it is my professional opinion that leaders in the medical community must stop the application of the affirmation care model to the young. The earlier model of watchful waiting, which gender affirmation unjustly replaced, must be restored as the tested and proven basis for care until such time as better, scientifically proven methods are found. Leaders in medical administration have a duty to the patients, and to their healthcare systems, to stop the financial support of this untested, demonstrably harmful treatment model, and to direct practitioners to the fullest use of the proven treatment model of watchful waiting. American physicians who care for persons who experience gender discordance likewise have a duty to follow evidence that has been offered by our European colleagues, and to seriously consider the fact that long term evidence now shows that gender transitioning medicine and surgery do not reduce lifetime risk of self-harm. Physicians must instead seek more effective ways to treat this community of persons who are suffering greatly, and who deserve care based in the objective truths that are revealed by good science.

I declare under penalty of perjury that the foregoing is true and correct. Executed this 2nd day of October, 2022.

//s//Patrick W. Lappert, MD
Patrick W. Lappert, MD

EXHIBIT "A"

Curriculum Vitae- Patrick W. Lappert, MD

Education and Training :

— Bachelor of Arts in Biological Sciences at the University of California, Santa Barbara, 1979. Research in cell membrane physiology with Dr. Philip C. Laris, studying stoichiometry of the sodium: potassium ATPase pump.

— M.D., Doctor of Medicine degree at the Uniformed Services University of the Health Sciences, 1983 at Bethesda, Md.

— General Surgery Residency at the Naval Hospital Oakland/ UC Davis East Bay Consortium, 1987-1991

— Chief Resident, Department of Surgery, Naval Hospital Oakland, 1990-1991.

— Plastic Surgery Residency at the University of Tennessee- Memphis, 1992-1994.

Board Certifications in Medicine :

— Board Certified in Surgery — American Board of Surgery, 1992-2002

— Board Certified in Plastic Surgery — American Board of Plastic Surgery, 1997-2018

Medical Staff Appointments :

— Staff General Surgeon at the Naval Hospital Oakland, CA 1991-1992

— Associate Professor of Surgery, UC Davis-East Bay, 1991-1992.

— Plastic and Reconstructive Surgeon, Naval Medical Center, Portsmouth, VA 1994-2002

— Chairman, Department of Plastic and Reconstructive Surgery, Naval Hospital Portsmouth, VA 1996-2002.

— Clinical Assistant Professor, Department of Surgery, Uniformed Services University of the Health Sciences, 1995-2002

— Founding Director, Pediatric Cleft Palate and Craniofacial Deformities Clinic, Naval Hospital Portsmouth, VA 1996-20002

— Founding Director, Wound Care Center, Naval Hospital Portsmouth, VA 1995-2002.

— Staff Plastic Surgeon in Nebraska, and Alabama.

U.S.N. Surgeon General Service:

— Specialty Leader, Plastic and Reconstructive Surgery, Office of the Surgeon General-USN, 1997-2002

Faculty Appointments:

— Teaching Faculty at Eastern Virginia Medical School, Division of Plastic Surgery, 1995-2002

Military Service :

— Aviation Officer Candidate, Naval Aviation Schools Command, NAS Pensacola, 1978

— Commissioned an Ensign, MC, USNR 1979 and Commissioned as a Lieutenant, MC, USN 1983 .

— Designated Naval Flight Surgeon, Naval Aerospace Medical Institute, 1985

— Flight Surgeon, Marine Fighter/ Attack Squadron-451

— Radar Intercept Officer in the Marine F-4S Phantom, accumulating 235 flight hours, and trained for qualification as an Air Combat Tactics Instructor.

— Deployed to the Western Pacific as UDP forward deployed fighter squadron in Korea, Japan, and the Philippines.

— Service in the US Navy for 24 years

— Service in the US Marine Corp. for 3 years.

— Retired with the rank of Captain, USN in 2002

Military Awards:

— Navy Commendation Medal - For service with Marine Fighter/Attack Squadron - 451

— Meritorious Unit Citation- 3rd award

— Humanitarian Service Medal - For service in the aftermath of the Loma Prieta earthquake.

Publications - Peer Reviewed Medical Journals :

— Lappert PW. Peritoneal Fluid in Human Acute Pancreatitis. Surgery. 1987 Sep;102(3):553-4

— Toth B, Lappert P. Modified Skin Incisions for Mastectomy: The Need for Plastic Surgical Input in Preoperative Planning. J Plastic and Reconstructive Surgery. 1991; 87: 1048-53

— Lappert P. Patch Esophagoplasty. J Plastic and Reconstructive Surgery. 1993; 91 (5): 967-8

— Smoot E C III, Bowen D G, Lappert P, Ruiz J A. Delayed development of an ectopic frontal sinus mucocele after pediatric cranial trauma. *J Craniofacial Surg.* 1995:6(4):327–331.

— Lappert PW. Scarless Fetal Skin Repair: "Unborn Patients" and "Fetal Material". J Plastic and Reconstructive Surgery. 1996 Nov;98(6):1125

— Lappert PW, Lee JW. Treatment of an isolated outer table frontal sinus fracture using endoscopic reduction and fixation. P!astic and Reconstructive Surgery 1998;102(5):1642-5.

Publications - Medical Textbooks:

— Wound Management in the Military. Lappert PW, Weiss DD, Eriksson E. Plastic Surgery: Indications, Operations, and Outcomes, Vol. 1; 53-63. Mosby. St. Louis, MO 2000

Operations and Clinical Experience - Consultations and Discussions : As a physician and surgeon, I have treated many thousands of patients in 7 states and 4 foreign nations. My practice has included Primary Care, Family Medicine, Aerospace Medicine, General Surgery, Reconstructive Surgery for combat injured, cancer reconstructive surgeries including extensive experience with microvascular surgery, Pediatric Congenital Deformity, and the care of chronic wounds. I have practiced in rural medicine, urban trauma centers, military field hospitals, university teaching hospitals, and as a solo private practitioner. In my private practice I have had occasion to treat many self-identified transgender patients for skin pathologies related to their use of high dose sex steroids, laser therapies for management of facial hair both in transitioners and detransitioners. I have performed breast reversal surgeries for detransitioning patients. My practice is rated as "LGBTQ friendly" on social media. I have consulted with families with children who are experiencing gender discordance. I have given many presentations to professional meetings of educators and counselors on the subject of transgender, and the present state of the science and treatment. I have discussed the scientific issues relevant to the case with many physicians and experts over a number of years and also discussed related issues with parents and others.

EXHIBIT "B"

Florida Medicaid Project: Surgical Procedures and Gender Dysphoria

Patrick Lappert, M.D.

May 17, 2022

Florida Medicaid Project: Surgical Procedures and Gender Dysphoria

Patrick Lappert, M.D.

Overview

The "Gender Affirmation" care model for children who suffer from gender identity issues is experimental in nature because it is based in low to very low-quality scientific evidence. There is no body of quality scientific evidence to support the hypothesis that gender dysphoria with its associated problems of self-harm and suicide, is improved long-term by gender affirmation surgical procedures.

The best evidence available today demonstrates that transgender is not a single condition that can be explained by any single factor. There are vast differences in age of presentation, predominant sex, persistence into adulthood, and resolution during adolescent development. Moreover, there are numerous and common co-morbid conditions such as autism-spectrum disorder, major anxiety disorders, and clinical depression that severely affect any sense of certainty about the true cause of the child's dysphoria, as well as their capacity to understand and give assent to irreversible medical and surgical procedures that lead to permanent sterility, sexual impotence, and a lifetime of medical problems associated with affirmation care.

The process of obtaining medical informed consent as part of gender affirming surgery is morally indefensible, and likely legally indefensible as well. Parents of suffering children are led by medical professionals to believe that there is only one valid option of care (affirmation medicine and surgery), utterly concealing the historic reality that greater than 92% of children desist in their cross-sex self-identification when treated using the "watchful waiting" therapeutic strategy. Parents are told that if they do not consent to affirmation care, there is a high likelihood that their child will die from suicide. This is not informed consent, but rather consent under duress.

Gender identity is being presented as a fixed and unchanging, biologically determined, personal characteristic. It is not. The medical literature has consistently shown over many years that the vast majority of children with cross-sex gender identity resolve the issue during adolescence and adopt a gender identity that is congruent with their biological sex.

Because surgeons who perform gender affirmation surgeries have no diagnostic test to predict who among the self-identified transgender minors would have persisted in their cross-sex self-identification into adulthood, and who among those children would have desisted, they have no way to know, in any particular case if the irreversible surgery is being performed on a person who would have continued to self-identify in the cross-sex persona into adulthood. Given the historically well-known desistance rate, it is possible that as many as 90% of children are undergoing surgery based upon an incorrect diagnosis.

"Gender Affirming" breast surgery for self-identifying transgender minors is not medically and ethically equivalent to similar procedures performed for objectively identifiable medical conditions. Transgender breast surgery is always cosmetic (aesthetic) in nature because the indication is a hoped-for improvement in the interior emotional life of the patient. Transgender surgery is not based in any medical diagnosis and does not seek to restore any form or function that may have been lost due to trauma, disease, or developmental accident. It begins with normal structures and changes their appearance in order to achieve a subjective improvement and is therefore cosmetic surgery.

Because gender affirming surgery is cosmetic (aesthetic) in nature, such surgeries must never be offered if they are known to predictably produce an irreversible loss of function. To knowingly sacrifice a human capacity (breast feeding, capacity for sexual intimacy, fertility) in the pursuit of a cosmetic result in a minor who is incapable of giving informed consent, is morally indefensible. The hoped-for subjective improvement that is sought in transgender surgery is a short-lived improvement and is only supported by low to very low-quality scientific evidence. Long term longitudinal cohort studies that are based in level III evidence show that affirmation surgical care is of no benefit in reducing self-harm including suicide.

Problems with Informed Consent

The protection of children in situations requiring informed consent is a crucial problem that the state has a historic and abiding interest in. In the particular situation of self-identified transgender children, it becomes a most significant problem, given that they are being submitted for permanently life-altering interventions. In my opinion as a plastic and reconstructive surgeon, the life-altering nature of hormonal and surgical interventions needs to be addressed from the moment of the child's entry into the gender-transition system, given the fact that the overwhelming majority of children who first begin puberty blockade, go onto the physically altering and permanent changes produced by cross sex hormones, and many ultimately also pursue surgery, as is attested to by multiple papers, the content of which is examined below. Informed consent has several requirement that need to be met if such consent is to be deemed valid. These requirements include a thorough discussion of the details of the proposed procedure including risks, known complications, and some measure of the likelihood of a favorable outcome. The discussion must include alternative treatments, and their risks, known complications and their likelihood of a favorable outcome. In the case of the interventions associated with gender-transition medicine and surgery, the favorable outcomes should be evident over the lifetime of the patient, given that they are permanently sacrificing structures and capacities (breasts and breast-feeding, or genitals and fertility).

Because the commonly cited medical literature used in support of these surgeries is of low to very low quality, it must be recognized that such surgeries must be considered experimental in nature given the unknown long-term effects of treatment, and the vast uncertainty in the patient selection and diagnostic processes. Yet the experts who provide opinion in support of these surgeries speak with absolute certainty of their efficacy, and the absence of any alternative treatment. Considering these factors severally and together it becomes difficult to imagine a

more flawed consent process. It also becomes understandable how parents can be drawn into uninformed participation given the simultaneous presentation of dire consequences if gender dysphoria is left untreated, and the insistence that affirmation care including surgery is the only way to bring lasting happiness to the child.

Chest Masculinization" in Natal Females is Not Ethically Equivalent to Mastectomies for Breast Cancer

When mastectomy is performed for the management of breast cancer, or to mitigate the proven risk of developing breast cancer in women, it is done on the basis of objective diagnoses either by pathological examination of biopsy tissue, or as in the case of prophylactic mastectomy, on the basis of genetic analysis that shows known markers of increased risk of developing breast cancer. These tests (microscopic examination of tissue specimens, detection of cell surface markers with proven association with malignancy, and genetic screening of at-risk patients) have known positive predictive value for the diagnosis of breast cancer, and these tests have known error rates that can be used when obtaining informed consent for mastectomy. The validity of these tests has been proven using scientific methodologies that produce high quality evidence in longitudinal population studies with control populations, and very long follow up. As the result, when a woman gives consent for mastectomy to control or prevent the potentially lethal disease, it is with a clear and proven evaluation of the risks and benefits that consent is obtained. Mastectomy is being performed based upon an objective diagnosis of a potentially lethal condition, and the surgical procedure has proven benefit in management of that condition.

In stark contrast, this is not the case when mastectomy is performed to "masculinize" the chest of girls and women who self-identify as transgender or who self-report symptoms of dysphoria. In the self-identified transgender adolescent, breasts are being removed on the basis of a diagnosis that is made by the patient since there are no tests with known error rates that can be used to predict who will benefit from this disfiguring and irreversible surgery. The claim is made that chest masculinization has proven benefit in reducing dysphoria and the associated risk of suicide. But published studies that make this claim of benefit offer evidence that is low to very low quality, typically small case collections with self-selection bias, very short follow up, and no case controls.

The best data presently available on the long-term effects of medical and surgical transitioning are long-term, longitudinal, population-based studies. For example, Dehjne, et al., examined the putative long-term benefit of full transitioning (including hormonal and surgical treatments) found in the Swedish medical database. (See Long-Term Follow-Up of Transsexual Persons Undergoing Sex Reassignment Surgery: Cohort Study in Sweden; Cecilia Dhejne, Paul Lichtenstein, Marcus Boman, Anna L. V. Johansson, Niklas Långström, Mikael Landén; PLOSOne February 22, 2011 https://doi.org/10.1371/journal.pone.0016885). That database includes all persons in the Swedish medical system, from pre-natal to death. It reports all episodes of care and all demographic information in a uniform vocabulary. Furthermore, Sweden has been on the forefront of "gender affirmation" long before the American medical

system seriously considered its claims. Because of the nature of Sweden's database, it is possible to study a cohort of patients that very closely matches the inquiry group with regards to age, sex, economic status, etc. It is possible to ask with great precision such questions as, "What is the likelihood that a fully transitioned transgender male will be hospitalized for psychiatric illness when compared to the age/sex matched control group?" Even more, one could urgently ask, "What is the relative risk of suicide in transgender persons, when compared to age/sex matched controls?"

Why are such longitudinal, population-based studies superior to the case-collection/case series methodology? Because confounding variables such as age, sex, and self-selection biases are removed. In the flawed case-collection methodology, the reported cases are typically only those who return for follow up. You have no way of knowing if the patient had a good outcome or didn't return for follow up because they were in a psychiatric hospital, were incarcerated, or committed suicide. In the Swedish longitudinal study, the suicide is in the same database, as are the other issues of hospitalization, incarceration, and addiction treatment, among other rates of comorbidity. Thus the longitudinal population study can give us what is called a "hazard ratio" for a particular study population (patients who have completed transgender transition in this case).

What this Swedish study shows us that the risk of completed suicide in all transgender persons is 19.1 times higher than in the control cohort. If you look only at patients who have transitioned — patients after "treatment" — from female to "male presentation," the risk of completed suicide is 40 times higher than in the general population. (Note: this finding is consistent with the historic Branstrom 10-year follow up study, which found no benefits to "transitioning treatments" but did note an increased risk of serious suicide attempts and anxiety disorders AFTER "treatment.") (Correction to Bränström and Pachankis, Am J Psychiatry 177:8, August 2020; see detailed citations in the "Notes" section of this report below).

Another cautionary note was added to the literature by the reputed Cochrane Review, a UK based international association of researchers who examine the quality of scientific evidence used in medical decision making. The Cochrane Review recently published findings concerning the medical evidence used to support the decision to give young women cross sex hormones as part of the transition process. The authors summarize the world literature review thus: "We found insufficient evidence to determine the efficacy or safety of hormonal treatment approaches for transgender women in transition. This lack of studies shows a gap between current clinical practice and clinical research." (Does hormone therapy help transgender women undergoing gender reassignment to transition? See, Haupt C, Henke M, Kutschmar A, Hauser B, Baldinger S, Saenz SR, Schreiber G., Cochrane Review, 28 Nov 2020).

Similar issues of very poor, low quality scientific support for chest masculinization surgery can be seen in a recent article by Tolstrup et al. published in the journal Aesthetic Plastic Surgery (See Anders Tolstrup, Dennis Zetner, Jacob Rosenberg, Outcome Measures in Gender-Confirming Chest Surgery: A Systematic Scoping Review, Aesthetic Plast Surg 2020 Feb;44(1):219-228. doi: 10.1007/s00266-019-01523-1. Epub 2019 Oct 29). The article reports a

comprehensive review of the world literature concerning the efficacy of "gender confirming" chest surgery in transgender patients. The authors found 849 articles on the subject, published in peer reviewed medical journals. Of these 849 articles, only 47 could be included in the review. This means that only 5.5% of all the published, peer-reviewed transgender surgery articles demonstrated even rudimentary scientific rigor. Of those 47 articles, the authors report that only 29 of the articles addressed mental health outcomes (3.4% of all the articles). What is startling is that the mental health outcomes were judged only on the basis of uncorroborated, untested, and unassessed patient subjective reporting with descriptors that varied so widely from article to article that results could not even be compared. The authors summarize by saying, "Evaluation of outcomes in gender-confirming chest surgery showed large variations in reporting, and further streamlining of reporting is therefore required to be able to compare surgical outcomes between studies." None of these negligent articles even bothered to examine rates of psychiatric hospitalization, substance abuse, self-harm behaviors, and suicide. This tells us that the main reason for performing these surgeries (psychological distress and suicide risk) isn't even evaluated with regard to efficacy.

An example of an article with very low-quality data, reckless (now banned practices), and methodology, published in a "leading journal," and promoted as evidence for the efficacy of "chest masculinization" surgery makes this fact very clear. The lead author (Olson-Kennedy, a leading national advocate for the transgender treatment enterprise) is a board-certified pediatrician who leads the gender clinic for the Los Angeles Children's Hospital. The article appeared in 2018 (See J. Olson-Kennedy, J. Warus, MD1, et al., Chest Reconstruction and Chest Dysphoria in Transmasculine Minors and Young Adults; Comparisons of Nonsurgical and Postsurgical Cohorts., JAMA Pediatr. 2018;172(5):431-436. doi:10.1001/jamapediatrics. 2017.5440. In their summary of findings, the authors reported that "chest dysphoria" is common among "trans males" (natal females seeking to present as males) and claimed that dysphoria is "decreased by surgery." They claim that regret for surgery is "rare." The article reports breast removal surgery on at least one girl aged 13 years. (Note that this reckless, experimental practice has now apparently been abandoned as unethical/experimentation on children by England, Sweden, and Finland. The average age of patients in the study was 19. Children were entered into the study through recruitment from among patients visiting the clinic and by telephone over a six-month period. The authors found that, of the patients recruited from among visitors to the clinic (convenience sampling), there was an over-representation of non-operated patients, so the authors were forced to reach out to all the post-surgical patients by phone. Twenty-six percent of the clinic's post-surgical patients could not be reached for various reasons including no working phone, or failure to respond to multiple messages. The 26% drop-out rate is never even questioned by these authors. Were surgical patients lost to follow up because of dissatisfaction, psychiatric hospitalization, or suicide? This problem is called "self-selection bias," and it is evidence of careless study design. Of the remaining 74% of patients, only 72% completed the survey. This is a second example of self-selection bias. Why would some post-surgical patients who had been successfully contacted, not complete the survey? The authors — demonstrating multiple levels of confirmation bias — do not even ask such essential questions. (See detailed citations in the "Notes" section of this report below).

In the study, dysphoria was evaluated using what the author called "a novel measure," which amounted to a series of subjective questions about happiness that was in part designed by the adolescent test subjects themselves. Essentially, the methodology used an entirely unvalidated ("junk science") test instrument, with no known error rates and no proven predictive power. Furthermore, the post-surgical patients were administered the survey at widely varying time intervals post-surgery. The longest interval between surgery and the satisfaction survey was 5 years, but children less than a year post-surgery were included in this obviously flawed sample, and yet the authors claim evidence of "negligible regret." This is a remarkable, misleading, and deceptive claim given that long-term, longitudinal population studies show that there is a dramatic rise in post-surgical problems such as depression, hospitalization, substance abuse, and suicide beginning at around seven years post-surgery (Ibid). Surely the authors are familiar with the world literature on transgender outcomes?

Having deceptively or negligently promised in the introduction to their paper that "chest dysphoria" is reduced by surgery, at the conclusion the authors confessed to the fact that the study design and execution produced very low-quality data that is not useful for patient selection, or prediction of outcomes. They even confessed that the study does not address the efficacy of surgery in improving outcomes regarding the single most compelling reason for performing the operation: mitigation of depression and suicide. The authors write, "An additional limitation of the study was the small sample size. The nonsurgical cohort was a convenience sample, recruited from those with appointments during the data collection period. There could be unknown imbalances between the nonsurgical and postsurgical cohorts that could have confounded the study findings."

Finally, the authors did not even bother to validate their "Chest Dysphoria Scale." Such a "made-up" scale is unlikely to accurately represent distress or correlate with properly validated measures of quality of life, depression, anxiety, or functioning. Their own analysis at the conclusion or the paper directly contradicts the deceptive claim made in their introduction.

This is the kind of "junk science" that is used to support transgender medicine and surgery. The paper is only a few years old. It was written by board certified physicians who practice in one of the nation's largest pediatric gender clinics and was published in a peer-reviewed medical journal. It is essentially useless in making any clinical decisions regarding who should be offered surgery, what is the likelihood they will benefit from it, and what is the likelihood they will regret their decision. Most importantly, it does not even measure the effect of therapy on suicide risk. The very morbidity (the risk of suicide) that they claim is improved by surgery is not even measured in their low-quality study.

Because of the very low-quality scientific support for mastectomy in the management of gender dysphoria, valid consent would demand that these procedures be described as experimental, would need the approval of ethics panels to monitor human experimentation, and would require the use of valid controls found in long-term, longitudinal population-based study models. These are the kinds of patient protections now endorsed in England, Sweden and Finland but still

ignored in the US environment where proper scientific critiques of such studies can get faculty "cancelled."

Even though the transgender treatment industry has been performing these surgeries for over 50 years, gender treatment centers continue to publish the same low quality, methodologically defective studies based upon collected cases that are degraded in value by self-selection bias, confirmation bias, and short-term follow-up, while continuing to deceptively claim that such defective research provides a sufficient scientific basis for performing irreversible, disfiguring, and ultimately sterilizing hormonal treatments and surgeries on children.

"Chest Masculinization" in Natal Females is Not Ethically Equivalent to Gynecomastectomy

Gynecomastectomy is the surgical treatment of gynecomastia, a fairly common condition in which males develop female-type breast gland tissue. Proponents of "masculinization" mastectomy in natal females erroneously equate the ethics of removing healthy breast tissue from gender dysphoric children with the removal of abnormal breast tissue in men (gynecomastia). In the case of gynecomastectomy in male patients, the operation is performed to remove the objectively diagnosed presence of female type glandular breast tissue present in a male patient. Physical examination demonstrates the presence of a dense retro-areolar mass which is tender and sometimes disfiguring. Pathological examination of the removed tissue will demonstrate the presence of female-type fibroglandular tissue in a male patient. This is an objectively abnormal condition. It should further be noted that the absence of such abnormal, female-type fibroglandular tissue in the submitted surgical specimen places the chest recontouring in the category of cosmetic surgery and is therefore not typically paid for by third-party payors.

A comprehensive literature review on the subject of gynecomastectomy and suicidal behavior conducted by Sollie in 2018 (Management of gynecomastia—changes in psychological aspects after surgery—a systematic review: Gland Surg. 2018 Aug; 7(Suppl 1): S70–76.doi: 10.21037/gs.2018.03.09) did not produce a single paper claiming improvement in suicide rate in patients who underwent this surgery. There were many reports concerning improvement in the pain that men with this objective condition suffer with. The remainder of the reported data was in the category of subjective "satisfaction survey". This tells us that the author did not distinguish between medically indicated and aesthetic surgeries. Nonetheless, no claim is made of decreased suicide rates in a suicidal population of male patients. This is because any male patient seeking removal of abnormal, female-type, breast tissue who reported suicidal ideation would be considered incompetent to give consent and would require a psychiatric evaluation and treatment to manage suicidal thinking before being considered for surgery. This kind of decision in favor of psychiatric support does not appear to be at work in the transgender affirmation world. There, and there alone, is suicidal thinking considered a qualification for a surgery.

"Chest Masculinization" in Natal Females is Not Ethically Equivalent to Breast Reduction

It should be obvious that "Chest Masculinization" surgery in natal females is not ethically equivalent to breast reduction surgery in non-transgender females. In the case of breast reduction for females with excessively large breasts (macromastia, or gigantomastia), the operation is performed to relieve a debilitating orthopedic complaint of neck, back, and shoulder pain associated with the postural/mechanical effects of the weight of the breasts. These patients experience significant activity restriction and chronic pain that is not relieved by medical management or physical therapy. Furthermore, there is voluminous actuarial data, based upon many years of longitudinal population-based study by medical insurance agencies that is used to predict who will benefit from surgery, and who will not. These physical, objective tests, based upon the actual measurement of the breasts, and the patient's overall body habitus, have known error rates that can be used to predict the likelihood that a breast reduction will relieve the orthopedic complaints of neck, back, and shoulder pain. When the tissue specimens are submitted to pathology, they are weighed in order to ensure that enough tissue has been removed so that there will be a very high likelihood that the surgery will relieve the orthopedic condition of neck, back, and shoulder pain (Accuracy of Predicted Resection Weights in Breast Reduction Surgery, Theodore A. Kung, MD, Raouf Ahmed, MBBS1 Christine O. Kang, MPH,1 Paul S. Cederna, MD, and Jeffrey H. Kozlow, MD; Plast Reconstr Surg Glob Open. 2018 Jun; 6(6): e1830.

Based upon that, adequate pre-operative consent can be obtained. The supporting data is based in very high-quality methodology. There is no quality research data, no pre-operative test or study, and no known error rates that can be used to predict the likelihood that any child suffering from gender dysphoria will benefit from the experimental procedures of mastectomy and chest "masculinization." As noted above, because of the very low quality data, transgender chest masculinization is at best experimental and at worst, should be viewed as a form of medical child abuse — it is important to note that Finland, Sweden, and the UK apparently now all agree with this analysis, as they have all retreated from such reckless surgical procedures for (See detailed citations in the "Notes" section of this report below).

It is crucial to remember that "chest masculinization-affirmation surgery" of healthy breast tissue results in a complete loss of function, that this loss is two-fold (breast feeding and erotic sensibility), and the cause of the loss is two-fold (gland removal and severing of the intercostal nerve). (See Breast Reduction with Use of the Free Nipple Graft Technique; Stephen R. Colen, MD; Aesthetic Surgery Journal, (Breast Reduction with Use of the Free Nipple Graft Technique; Stephen R. Colen, MD; Aesthetic Surgery Journal, Volume 21, Issue 3, May 2001, Pages 261–271, https://doi.org/10.1067/maj.2001.116439).

If a patient who undergoes "chest masculinization" should regret the surgery, they do have the option of breast reconstruction. However, all that will be produced is a counterfeit of a breast. The patient will have lost the function of breast feeding. Additionally, the most commonly performed "masculinization" surgery involves the removal of the nipples, and subsequent re-

attachment in the form of a nipple graft. Those nipples will have lost their native nerve connections that provoke erotic sensibility. All that can be hoped for is the eventual random ingrowth of local skin sensation, but there will never be erotic sensation because the particular branch of the fourth intercostal nerve which communicates with particular centers in the brain responsible for oxytocin release and erotic provocation will have been permanently severed. This means that breast function has been completely and irreversibly sacrificed for the sake of producing a cosmetic result (a masculine appearing chest). This is the exact opposite of the goals of any reconstructive surgery. It must therefore be understood that "chest masculinization" is a cosmetic procedure that has violated the most essential principle of cosmetic surgery: never sacrifice function for the sake of a cosmetic result.

Erroneous use of the word "Reconstructive" to describe Gender Affirmation Surgeries

The transgender treatment enterprise uses the word "reconstructive" to characterize a group of surgical treatments that seek to alter the sexed appearance of the person. It is important to understand that these procedures, because of the indications for surgery, the motivations for surgery, and the outcomes of surgery, are not reconstructive, but are to be properly understood to be cosmetic in nature.

Reconstructive surgeries are procedures that seek to establish or restore structures and their functioning that have been lost due to trauma, disease, in-utero developmental abnormalities, or surgical treatment for disease. Such reconstructive surgeries must begin with the objective characterization of the defect, including abnormalities of form, and associated loss of function. This process of defining the defect begins with a thorough understanding of normal human form and function and seeks to select, develop, and execute procedures that will restore both. In some cases function may be emphasized more than form, as when the mangled hand of a man is reconstructed. In other cases, reconstruction of form is all that is possible because as yet there are no techniques to restore function. An example of this is seen in the reconstruction of a woman's breast following cancer care. All that can be offered is the appearance of a breast; she will never be able to feed an infant through the reconstructed part.

This is to be contrasted with cosmetic, or aesthetic surgery in which the appearance of a structure is modified in order to produce a subjective (aesthetic) result for the patient. No functional restoration is addressed because no functional or structural loss exists. The object of the surgery is aesthetic. There is no lost form or function that needs to be reconstructed. It is aesthetic surgery because the motivation is aesthetic (subjective feelings about appearance). Further evidence for this is the fact that nearly the entirety of the outcome studies cited in support of these surgeries use subjective questionnaires which the patient fills out. The questions used are typical of those used to evaluate any aesthetic surgery. They are called "satisfaction surveys". Such surveys are prone to suffer from self-selection bias, confirmation bias, and high drop-out rates.

One of the key problems that the transgender treatment enterprise faces on a daily basis is the issue of third-party payment for services. No health insurance provider, including federal and state agencies will pay for cosmetic surgery. For this reason, it is necessary, in order for the business model to succeed, that providers characterize their services as reconstructive. This is doubly difficult given the intense political pressure that has been exerted upon the medical community to "de-pathologize" the condition of transgender. This is seen in the abandoning of the diagnostic nomenclature of "body dysmorphic disorder", and "gender identity disorder" in favor of the more recent DSM manual using the term "gender dysphoria". This leads transgender treatment providers into the difficult situation of claiming that transgender is not a pathology, while at the same time insisting that the services are medically necessary and describing the procedures as reconstructive without characterizing any physical/ functional defect.

As we consider the specific "gender affirming" surgical procedures we will see that comparison to medically indicated surgeries on both men and women actually serves to reinforce the evidence that these surgeries are essentially and fundamentally cosmetic.

Masculinizing and Feminizing Chest Surgeries are Not "Medically Necessary"

Supporters of "transitioning" treatments justify surgical treatment based upon "medical necessity." They claim that gender dysphoria can lead to debilitating anxiety and depression, as well as serious incidents of self-harm, including self-mutilation, suicide attempts, and suicide. Yet with only a single exception, in the studies they cite no measures are made of the effects of surgery on what is claimed to constitute the "medical necessity" for these procedures. In contrast, the Branstrom study[1] documented no reliable benefits for transgender surgery/hormonal treatments and no reduction in suicide and even an increase in serious suicide attempts requiring hospitalization in patients receiving surgery. These recent, long-term, published, peer reviewed, credible research findings are quite contrary to the claims of supporters of "transitioning treatments" — as are the National Science Reviews in this area from England-NICE, Sweden, and Finland. (See detailed citations in the Notes section in this declaration).

Scientific rigor would demand an examination of objective outcomes such as: rates of substance abuse, psychiatric hospitalization, self-harm, or suicide, and how they were changed by surgery. One paper does ask these crucial questions concerning efficacy is a very comprehensive, long term, longitudinal population cohort study which actually shows the opposite of what experts claim for these patient outcomes. When followed beyond eight years post operatively, this paper shows that patients receiving these treatments have the same alarmingly high rates of hospitalization, substance abuse, self-harm, and completed suicide as persons who have had no medical or surgical intervention.

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[1] *Correction of a key study: No evidence of "gender-affirming" surgeries improving mental health.* Home. (2020, August 30). Retrieved May 17, 2022, from https://segm.org/ajp_correction_2020

In summary, on the issue of the efficacy of these surgeries, the scientific support is very weak, while the scientific evidence rejecting the hypothesis of efficacy is remarkably strong (See Long-Term Follow-Up of Transsexual Persons Undergoing Sex Reassignment Surgery: Cohort Study in Sweden; Cecilia Dhejne, Paul Lichtenstein, Marcus Boman, Anna L. V. Johansson, Niklas Långström, Mikael Landén; PLOS One February 22, 2011 https://doi.org/10.1371/journal.pone.0016885).

The surgical removal of the breasts, and the re-contouring of the chest through liposuction is a common procedure for women who seek to present as men. These operations are performed in both men and women, for a variety of reasons. They are generally very safe, and typically performed in the outpatient setting. It is important to understand that the only way of distinguishing cosmetic breast surgery from "medically necessary" surgery is based upon the diagnosis of underlying pathology. For example, breast reduction may be cosmetic, or it may be medically indicated. In both cases, the patient presents with a complaint that her breasts are too big. The distinction between cosmetic breast reduction and medically indicated breast reduction is based upon the presenting symptoms of orthopedic problems when working, such as chronic neck back and shoulder pain caused by the weight of the breasts. But even then, the weight of the removed tissue is factored into the objective verification that the surgery was "medically necessary." There is a vast body of medical and actuarial data that demonstrates the relationship between the weight of the breast tissue removed and the probability that back pain will be cured by performing a breast reduction.

The same issues are at stake in breast enhancement for men seeking to present as women. Cross-sex hormones will have caused varying degrees of gynecomastia (breast enlargement in men). Surgical enhancement procedures are exactly the same in both men and women.

Medically necessary surgery in women is based upon the diagnosis of an objective medical condition, such as Poland's syndrome (congenital absence of a breast), surgical absence of the breast following cancer care. In men, the objective diagnosis of gynecomastia might warrant surgery based upon medical necessity, but it would be the removal of tissue that has objective pathological features (breast gland proliferation in a man). A rare diagnosis of breast cancer in a man might warrant chest wall reconstruction after cancer care. On the other hand, cosmetic surgery of the breast is entirely about the subjective feelings of the patient, and that is all that we find in the case of the self-identified transgender patient.

In the case of transgender chest surgery, the diagnosis is based on the patient's subjective report of dysphoria, but the medical necessity is based on the expectation that surgery will relieve the patient of the risk of, among other things, major depression, self-harm behaviors, and suicide. None among the many papers typically cited by supporters of "transitioning treatments" address themselves to the question of medical necessity for either masculinizing surgery, or feminizing surgery. They only address technical issues, management of complications, and subjective outcomes that employ precisely the same language that is used to assess every

other cosmetic surgery of the breast. Such papers often begin with standard language about the suffering of self-identified transgender adolescents, and their risk of self-harm. They will claim that the reported surgeries somehow reduce the risk of suicide, or the frequency or severity of self-harm, but they never report actual results of improvement in the risk of suicide, or substance abuse, or cutting, or sexual risk taking. The claim of benefit is unsupported in the scientific literature.

In summary, the medical necessity of transgender chest surgery is not supported by scientific evidence and appears to be firmly in the category of cosmetic surgery. What is more, the surgeries when performed on natal females causes a life-long loss of function, placing those surgeries in the category of malpractice. No other cosmetic procedure is expected to produce major functional loss. Such a result would only be the result of a complication, or other surgical misadventure. To actually have a 100% certainty of loss when surgical consent is being obtained constitutes a complete neglect of one of the foundational principles in plastic surgery: Never sacrifice function for the sake of a cosmetic result.

About the Author

Education and Training: I received my Bachelor of Arts in Biological Sciences at the University of California, Santa Barbara, 1979. There I was engaged in research in cell membrane physiology with Dr. Philip C. Laris, studying stoichiometry of the sodium: potassium ATPase pump. I received my M.D., Doctor of Medicine degree at the Uniformed Services University of the Health Sciences, 1983 at Bethesda, Md. I served my General Surgery Residency at the Naval Hospital Oakland/UC Davis East Bay Consortium, 1987-1991 and served as Chief Resident, Department of Surgery, Naval Hospital Oakland, 1990-1991. I also served a Plastic Surgery Residency at the University of Tennessee-Memphis, 1992-1994. My professional background, experience, and publications are described in more detail in my curriculum vitae, which is attached as Exhibit A to this declaration.

Board Certifications in Medicine: I have been Board Certified in Surgery (American Board of Surgery, 1992), in Plastic Surgery (American Board of Plastic Surgery, 1997; American Board of Plastic Surgery, 2008).

Medical Staff Appointments: I served as the Staff General Surgeon at the Naval Hospital Oakland, CA 1991-1992 and as Associate Professor of Surgery, UC Davis-East Bay, 1991-1992. I also served as a Plastic and Reconstructive Surgeon, Naval Medical Center, Portsmouth, Virginia, 1994-2002 and as Chairman, Department of Plastic and Reconstructive Surgery, Naval Hospital Portsmouth, Virginia, 1996-2002. I later served as Clinical Assistant Professor, Department of Surgery, Uniformed Services University of the Health Sciences, 1995-2002 and as Founding Director, Pediatric Cleft Palate and Craniofacial Deformities Clinic, Naval Hospital Portsmouth, Virginia, 1996-2002 also as the Founding Director, Wound Care Center, Naval Hospital Portsmouth, Virginia, 1995-2002. I have also served as a Staff Plastic Surgeon in Nebraska and Alabama.

U.S. Surgeon General Service: I served as a Specialty Leader, Plastic and Reconstructive Surgery, Office of the Surgeon General-USN, 1997-2002.

Faculty Appointments: I served as Teaching Faculty at Eastern Virginia Medical School, Division of Plastic Surgery, 1995-2002. I also served on the teaching faculty of the Via College of Osteopathic Medicine, 2017-2020.

Military Service: I served as an Aviation Officer Candidate, Naval Aviation Schools Command, NAS Pensacola, 1978 and was Commissioned an Ensign, MC, USNR 1979 and Commissioned as a Lieutenant, MC, USN 1983. I served as a Designated Naval Flight Surgeon, Naval Aerospace Medical Institute, 1985, and I was Assigned Marine Fighter/Attack Squadron-451, serving as Flight Surgeon, and serving as Radar Intercept Officer in the Marine F4S Phantom, accumulating 235 flight hours, and trained for qualification as an Air Combat Tactics Instructor. I was deployed to the Western Pacific as UDP forward deployed fighter squadron in Korea, Japan, and the Philippines. I served in the US Navy for 24 years, and I served in the USMC for 3 years. I retired with the rank of Captain, USN in 2002.

Publications - Peer Reviewed Medical Journals: Lappert PW. Peritoneal Fluid in Human Acute Pancreatitis. Surgery. 1987 Sep; 102(3):553-4; Toth B, Lappert P. Modified Skin Incisions for Mastectomy: The Need for Plastic Surgical Input in Preoperative Planning. J Plastic and Reconstructive Surgery. 1991; 87 (6): 1048-53; Lappert P. Patch Esophagoplasty. J Plastic and Reconstructive Surgery. 1993; 91 (5): 967-8; Smoot E C III, Bowen D G, Lappert P, Ruiz J A. Delayed development of an ectopic frontal sinus mucocele after pediatric cranial trauma. J Craniofacial Surg. 1995;6(4):327–331; Lappert PW. Scarless Fetal Skin Repair: "Unborn Patients" and "Fetal Material". J Plastic and Reconstructive Surgery. 1996 Nov; 98(6): 1125; Lappert PW, Lee JW. Treatment of an isolated outer table frontal sinus fracture using endoscopic reduction and fixation. Plastic and Reconstructive Surgery 1998; 102(5): 1642-5.

Publications - Medical Textbooks: Wound Management in the Military. Lappert PW, Weiss DD, Eriksson E. Plastic Surgery: Indications, Operations, and Outcomes, Vol. 1; 53-63. Mosby. St. Louis, MO 2000.

Operations and Clinical Experience: Consultations and Discussions: As a physician and surgeon, I have treated many thousands of patients in 7 states and 4 foreign nations. My practice has included Primary Care, Family Medicine, Aerospace Medicine, General Surgery, Reconstructive Surgery for combat injured, cancer reconstructive surgeries including extensive experience with microvascular surgery, Pediatric Congenital Deformity, and the care of chronic wounds. I have practiced in rural medicine, urban trauma centers, military field hospitals, university teaching hospitals, and as a solo private practitioner. In my private practice I have had occasion to treat many self-identified transgender patients for skin pathologies related to their use of high dose sex steroids, laser therapies for management of facial hair both in transitioners and detransitioners. I have performed breast reversal surgeries for detransitioning patients. My practice is rated as "LGBTQ friendly" on social media. I have consulted with families with children who are experiencing gender discordance. I have given many presentations to professional meetings of educators and counselors on the subject of transgender, and the present state of the science and treatment. I have discussed the scientific issues relevant to the case with many physicians and experts over a number of years and also discussed related issues with parents and others.