

Letter to the Editor

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Author's response to “*letter to the editor comment on: ‘A unique pseudo-eligibility analysis of longitudinal laboratory performance Data from a transgender female competitive cyclist’*” by Lundberg, O’Connor, Kirk, Pollock, and Brown

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We thank the authors of the letter [1] for their interest in our paper and the opportunity for further discussion around this important topic. The letter in question [1] critiques two papers authored by our research group: “Strength, Power, and Aerobic Capacity of Transgender Athletes: A Cross-Sectional Study [2]” and “A Unique Pseudo-Eligibility Analysis of Longitudinal Laboratory Performance Data from a Transgender Female Competitive Cyclist [3]”. While this critique presents itself as scientific, it seems to be driven more by subjective opinion for the reasons we set out below.

Article note: This article addresses an important issue within the modern Olympic movement: gender equality, diversity, and inclusion in sports participation. The objective of TEB is to foster research that links theory with practice, and we are delighted to have received a Letter to the Editor from readers on this highly debated issue. We have invited the authors to respond to the Letter and have decided to publish both contributions for the benefit of our audience. Healthy and respectful scientific debates will enhance our understanding and practical solutions to this pressing issue.

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Terminology

Regarding the authors’ key argument that sex is binary, it is important to note that this idea is widely disputed in modern scientific literature. For example, King [4] has stated that sex is not a simple binary concept, citing numerous chromosomal variations and biological complexities that contribute to athletes with sex variation classification. Fausto-Sterling [5] and Massa [6] further emphasise that sex may be understood as a spectrum. By not considering these perspectives, the authors of the letter present a one-sided argument that could mislead readers and overlook the nuances needed for a balanced debate on this important topic.

In the critique of the title of our cycling paper [3], the letter [1] claims that the terminology used is confusing, particularly the distinction between “*transgender female*” and “*transgender woman*.” However, the terminology is not confusing to the readers, nor is it unclear in the paper itself. The paper clearly states that it is a pseudo-eligibility analysis of a transgender female cyclist, not competing at elite or grassroots levels. ‘*Female*’ and ‘*woman*’ are often used interchangeably, and this extended discussion of language adds little value to a scientific critique. We, also disprove the suggestion to use negative language such as “*trans-identifying male* [1]”, as this negative language represents misgendering of transgender people and can cause harm [7].

Regarding comparison groups

The opportunity to address the concerns raised regarding the comparison groups from our previous work [2] utilised in our analysis of a sub-elite transgender woman cyclist [3] is appreciated. To clarify, all transgender and cisgender

athletes included in the cross-sectional study [2] were not competing at the elite or sub-elite level and exhibited comparable average training intensities, therefore representing a heterogeneous cohort of athletes across all genders recruited using the same methodology. Given the cross-sectional nature of the original analysis, we agree no definitive causation regarding the effects of gender-affirming hormone therapy (GAHT) could be established with any confidence [2]. This limitation underscores the necessity for longitudinal studies, beginning with this case study [3], aimed at describing, rather than conclusively determining the impact of one year of GAHT on a sub-elite transgender athlete. We then used this case study data to perform a pseudo (sham) eligibility analysis to demonstrate how a case-by-case approach may work to promote academic discourse.

While we thank the authors [1] for their discussion and interest in our research, their critique argues that the transgender women participating in our cross-sectional study were not indicative of an athlete cohort and that we were guilty of comparing “*apples to oranges* [1]” and we dispute this assertion. For example, a similarly designed study in sedentary transgender women [8] found transgender women’s absolute and relative $\dot{V}O_2\text{max}$ to be 67 % (3.68 vs. 1.83 L min⁻¹) and 36 % (45.1 vs. 31.2 ml min⁻¹ kg⁻¹) less than our athlete cohort [2], while after a year of GAHT, the cyclist athlete [3] was similarly well above the sedentary cohort (i.e., absolute, 3.74 vs. 1.83 L min⁻¹, Δ -69 %; 41.9 vs. 31.2 ml min⁻¹ kg⁻¹, Δ -29 %) [8]. Results like this convey an important message that transgender athletes must be studied and compared to cisgender athletes to evaluate their performance standing in sport. As such, we argue that using the difference between cisgender women and male athletes as a proxy for transgender women athletes’ performance difference to cisgender female athletes is the true case of comparing “*apples to oranges*”, an approach frequently adopted and supported by the authors of this letter [1, 9, 10]. By using performance differences between cisgender men and women as a proxy for transgender athlete performance, Lundberg [1] and colleagues disregard that GAHT is a performance-reducing treatment and overlook the necessity of studying transgender athletes directly. The primary conclusion from both our cross-sectional [2] and longitudinal [3] papers are that transgender women or transgender men athletes do not exhibit the same performance characteristics as their cisgender peers; therefore, both should be studied as distinct cohorts. This approach is precisely what both studies [2, 3] have endeavoured to observe and accomplish. Moreover, we acknowledge and discuss the well-known limitations inherent in both papers within the published articles [2, 3].

The use of absolute and relative values

In their letter, we are accused of “*deception* [1]” given our use of relative performance. Our study employs both absolute and relative measures and in sports like cycling, understanding both metrics is crucial for a comprehensive evaluation of athletic capabilities. Absolute performance refers to metrics like raw power output or $\dot{V}O_2\text{max}$ while relative performance (e.g., Average Power, W Kg⁻¹; 1-rep max, kg kg⁻¹, relative $\dot{V}O_2\text{max}$, mL min⁻¹ kg⁻¹) considers factors such as body mass. Contrary to cycling on flat terrain, the most proficient climbers in cycling are those who exhibit both high power output and low body mass, which is essential in cycling events with elevation changes (Figure 1). A commonly used metric to quantify these attributes is the power-to-mass ratio, calculated by dividing a cyclist’s power output in watts by their body mass in kilograms (W kg⁻¹). This metric provides a useful comparison between riders of different body sizes. For instance, the cyclist produced 4160 W of power with a body weight of 89.4 kg (47 W kg⁻¹) will have the same power-to-mass ratio as a rider with a 44.7 kg body mass and an output of 2,080 W (47 W kg⁻¹). On flat terrain (Figure 1), where weight is less influential, the heavier rider with greater absolute power will hold a distinct advantage over the lighter rider. However, when the gradient increases, the significance of absolute power diminishes, and the power-to-mass ratio becomes the critical determinant of performance (Figure 1).

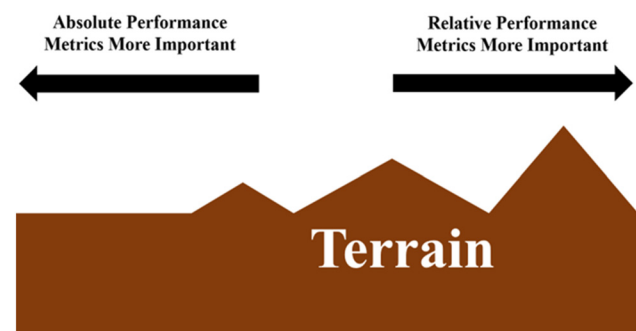


Figure 1: Importance of absolute and relative performance metrics in cycling adapted from Abram [11].

The authors of the critique [1] attempt to construct evidence against the use of relative performance and defend absolute performance using a simulated laboratory cycling time trial with no incline [12] (Figure 1). This evidence [12] employed multivariate models that predicted simulated time trial performance and found absolute peak power output, ventilatory threshold and respiratory compensation

point explain 92 % (R^2 0.920, [adjusted R^2 0.917]) of the variance in non-inclined time trial performance. However, a second model in this same paper [12] found that relative peak power output, ventilatory threshold and respiratory compensation to mass were just as successful (R^2 0.891, [adjusted R^2 0.887]) at predicting variance in time trial performance. Therefore, we are surprised that the critique [1] considers this common use of both absolute and relative measures as “*deception* [1]”. We consider using relative metrics as a well-rounded analysis. Both absolute and relative performance must be considered to accurately assess an athlete’s performance capabilities and provide meaningful comparisons and to claim that relative performance adjustments “*has no place in research related to transgender women in sports* [1]” is taking an overly simplistic view on an undoubtedly complicated topic. Such adjustments are necessary to account for body morphology and composition differences that significantly affect outcomes, especially in endurance sports like cycling. Dismissing these methods as inappropriate undermines the nuanced approach needed for performance evaluation in diverse athlete populations.

The authors remark on the “*lack of specificity* [1]” to cycling is also unfounded as both studies measured peak power output [2, 3] and the cross-sectional study reported an anaerobic threshold as % $\dot{V}O_{2max}$ (Table 1 [2]). However, we acknowledge the lack of the respiratory compensation point and absolute anaerobic threshold measures in the case study. Therefore, to be complete, we now report anaerobic threshold respiratory compensation point comparisons from both studies [2, 3] in Table 1. The main findings are that transgender women had a higher absolute respiratory compensation point and a lower relative anaerobic threshold than cisgender women in the cross-sectional study (Table 1) and that the longitudinal athlete had a reduction in all measures after 1 year of GAHT (Table 1).

Case by case assessment

The letter [1] also focuses on the necessity of a protected female category, advocating for the exclusion of marginalised athletes from female sports categories. They argue that the studies [2, 3] “*provide further support to exclude those with male advantage from the female category* [1]” suggesting that GAHT in transgender women does not entirely eliminate assigned male at-birth performance advantages [1]. However, they contradict themselves by stating that the methodological rigour and specificity of the work [2, 3], do not support concluding the effects of GAHT on athletic performance [1]. Most researchers, regardless of their position on this topic, can agree on one point: GAHT is a performance-reducing treatment for transgender women [3, 9, 13] and the magnitude of this reduction and its implications for competitive fairness are the questions needing answering. Moreover, the authors of the letter [1] criticise the concept of case-by-case assessments for transgender athletes, arguing that such a practice is impractical without considering or analysing evidence of a case-by-case policy in play [14]. Their argument is also inconsistent with their previous stance, where they seemed to support case-by-case approaches, such as the cheek swab test for sex chromosome determination [15]. Incidentally, we have recently joined efforts with other authors to oppose this view [16].

Misrepresentation

The ethical concerns raised by the authors regarding GAHT for transgender athletes also appear misguided. GAHT is aimed at aligning the physiology of transgender women with that of cisgender women, promoting both health and

Table 1: Anaerobic threshold and respiratory compensation point Measures.

Metric	Cross-sectional				Longitudinal case study			
	CW	TW	<i>t</i>	<i>P</i>	Baseline	3 Months	6 months	12 months
AT, mL min ⁻¹	2,824 ± 490	3,122 ± 434	1.7	0.56	3,895	3,987	3,339	3,271
AT, mL min ⁻¹ kg ⁻¹	47.2 ± 6.1	38.3 ± 6.6	−3.4	0.01^a	50.7	50.0	40.2	36.6
AT, % $\dot{V}O_{2max}$	87.3 ± 6.3	85.1 ± 6.2	−0.9	1.00	90	92	90	87
RCP, mL min ⁻¹	3,002 ± 462	3,520 ± 403	2.9	0.04^a	4,215	4,282	3,553	3,593
RCP, mL min ⁻¹ kg ⁻¹	50.2 ± 5.4	43.5 ± 8.4	−2.5	0.09	52.9	53.7	42.8	40.2
RCP, % $\dot{V}O_{2max}$	93.2 ± 5.6	96 ± 6.5	1.7	0.74	97	97	96	96

AT, anaerobic threshold or ventilatory threshold; RCP, respiratory compensation point or VT2; mL min⁻¹, oxygen consumption milli-litres per minute; % $\dot{V}O_{2max}$, percentage of maximal O_2 uptake that the metric occurred. Data Analysis Method: a one-way analysis of variance (ANOVA) along with Bonferroni post hoc corrections for pairwise comparisons. ^aindicates α -value is <0.05.

bodily autonomy in line with the IOC framework [17]. Misinterpreting this key point weakens the validity of the critique. Additionally, the letter [1] misrepresents our previous work by selectively quoting and omitting critical context. For example, they cite: “*Transgender women have the right to compete in sports. However, cisgender women have the right to compete in a protected category* [18]”, and claim that the advocacy for GAHT as a means for some transgender women to participate in female sports raises ethical concerns regarding the primacy of health and bodily autonomy, proposing that we as a group are pressurising athletes to undergo medical procedures [1]. However, the critique [1] failed to include the subsequent statements where we stressed the importance of informed decision-making around GAHT. Specifically, we wrote: “*Transgender women athletes should be fully informed by medical personnel of the risks and consequences of testosterone suppression treatment and must never be coerced or forced into testosterone suppression. The athletes must be free to make the decision that is best for them* [18]”. Additionally, “*If transgender women athletes choose not to suppress testosterone, as is their right, they cannot compete in the restricted female category with high testosterone concentrations. Instead, they should be offered the opportunity to compete in the male category* [18]”. These omitted statements clarify that our position does not imply a category “*completely free from male advantages* [1]” as the authors claim. Rather, we support the autonomy of transgender athletes and offer fair alternatives for competition based on their informed choices.

Conclusions

In conclusion, we appreciate the opportunity to engage in further discussion on this important topic and to clarify points raised in the critique. The authors of the letter raise concerns about scientific rigour and methodology, but their arguments are based on subjective interpretations and inconsistent positions. We have addressed their key points by emphasising the necessity of both cross-sectional and longitudinal studies in understanding the effects of GAHT on transgender athletes, rejecting their claims of inappropriate comparison groups and methodological flaws. The work seeks to observe the distinct physiological differences between transgender and cisgender athletes and stresses the importance of studying transgender athletes directly, rather than relying on indirect proxies that fail to capture the complexity of the issue. We also contend

that both absolute and relative performance metrics are essential for a well-rounded understanding of athletic capabilities, particularly in endurance sports like cycling. The critique's dismissal of these measures is an overreach that undermines the nuanced analysis required in performance research. Furthermore, we reaffirm that GAHT is widely recognised as a performance-reducing treatment for transgender women, with the debate centring on the magnitude of this reduction rather than its existence. Finally, we stand by the ethical considerations underlying our approach, which prioritises informed consent and bodily autonomy for transgender athletes while offering fair competition opportunities under current scientific understanding and international guidelines [19].

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