Comment on "Asymptotic Estimate of the n-Loop QCD Contribution to the Total e^+e^- Annihilation Cross Section"

Recently West [1] claims to have obtained an asymptotic estimate, (22) in Ref. [1], of $r_n(1)$, the nth-order perturbative coefficient of the normalized total e^+e^- annihilation cross section, which with $N_f=5$ flavors yields $r_3^{\rm est}(1)\simeq -13.4$. This estimate effectively sets the β-function coefficients $b_3=b_4=\cdots=0$, but since $b_3\simeq 4.6\times 10^{-5}$ in the modified minimal subtraction $(\overline{\rm MS})$ scheme one may compare $r_3^{\rm est}(1)$ with the exact $\overline{\rm MS}$ result [2] of $r_3^{\rm ex}(1)=-12.8$. West claims [1] that this remarkable agreement is rather better than one might have expected since one can anticipate $o(b_2/nb_1^2)$ corrections to $r_n^{\rm est}$, giving a $\sim 20\%$ correction for $n=3$. Unfortunately examination of the N_f dependence of $r_3^{\rm est}$ and $r_3^{\rm ex}$ reveals that the agreement is accidental. The exact result is [3]

$$r_3^{\rm ex}(1)=-6.637-1.200N_f-0.005N_f^2\,, \qquad (1)$$

omitting the "light-by-light" term which makes only a small extra contribution. As can be seen in Fig. 1 the agreement for $N_f=5$ is fortuitous—the curves just happen to intersect near to $N_f=5$. In fact, West has underestimated the possible corrections at low n. One of the final steps in the argument involves approximating ${\rm Im}D(1,1/k_1)$ by the first term in its perturbative expansion in $1/k_1$ on the grounds that $k_1\sim b_1(n-1)$ is large. However, when $n=3$, $1/k_1\sim 8$ and even just retaining the hitherto neglected $o(1/k_1^3)$ term gives an 80% correction, significantly larger than the overall uncertainty claimed in [1].

Of course it might still be that the estimate is a valid asymptotic result. However, the argument leading to it is invalid as can be seen by evaluating (16) in Ref. [1] exactly for some special cases. Trivially, when s is a positive integer, the cut along the positive real axis vanishes, allowing C' to be closed at infinity and making the integral vanish, a key behavior not represented by the approximation. In fact, when $b_2=0$ the integral can be evaluated for a general s in terms of a generalized Riemann zeta function [3] and the answer is proportional to $\sin\pi s$ instead of the claimed $\cos\pi s$. Although $b_2=0$ is an unphysical assumption, it is an unremarkable one in that the original argument shows no indication of a breakdown as $b_2\to 0$.

Clearly the saddle-point method gives the wrong answer for this example, presumably due to the saddle point lying on the positive axis with the path of steepest descent perpendicular to the cut along it, so the contour cannot pass over the saddle point as required. The alternative of a contour which doubles back is at odds with the entire motivation for the saddle-point technique.

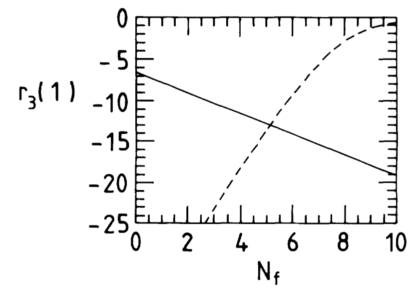

FIG. 1. The N_f dependences of $r_3^{\rm est}(1)$ (dashed line) and $r_3^{\rm ex}(1)$ (solid line).

However, the difficulty lies deeper than any possible problem in estimating the integral, since the exact form of (16) in Ref. [1] is evidently nonsensical in implying that $d(s)$ vanishes for integer s. Our suspicions fall on the interchange of the order of integrations involved in deriving it. Consider the much simpler example where

$$D(g^2)=\int_0^\infty e^{-z/g^2}f(z)\,dz \qquad (2)$$

replaces Eq. (9) in Ref. [1] and clearly $D(g^2)$ is arbitrary. Then interchange the integrations

$$d(s)=\int_0^\infty f(z)\,dz\int_C\frac{dg^2}{2\pi i}e^{-z/g^2}(-g^2)^{1-s}. \qquad (3)$$

Now, for s a positive integer, $d(s)=0$ since then the contour integral has zero residue from the essential singularity at $g^2=0$.

We conclude that, unfortunately, the estimate of Eq. (1) and the argument which led to it are incorrect.

D. T. Barclay and C. J. Maxwell
Centre for Particle Theory
University of Durham
Durham, DH1 3LE, England

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