# Infrared fixed points in asymptotically free field theories: What do they tell us?

Jiří Chýla*
*Laboratory of Theoretical Physics, Joint Institute for Nuclear Research, Dubna, Union of Soviet Socialist Republics*
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The meaning of infrared fixed points in asymptotically free field theories is discussed in connection with a recent paper of Kubo, Sakakibara, and Stevenson. It is argued that our understanding of the infrared behavior of perturbation theory depends crucially on our ability to cope with the divergences of perturbation expansions for physical quantities. As at any finite order the presence or absence of the infrared fixed point of an associated $\beta$ function is a matter of choice of the appropriate renormalization scheme (RS), the resulting infrared behavior of truncated perturbation expansions of physical quantities is RS dependent. It is shown that under certain circumstances the principle of minimal sensitivity fails already at finite values of external momenta and cannot therefore in this case be used in investigation of the infrared behavior of physical quantities. It is furthermore explained why contrary to popular belief "critical" exponents associated with infrared fixed points, present in some renormalization conventions, have no direct physical meaning.

In their article,[1] Kubo, Sakakibara, and Stevenson (KSS) discussed perturbation expansions, with special emphasis on the associated renormalization-scheme (RS) ambiguity, for the theories which are asymptotically free in the ultraviolet region and which have an infrared fixed point $a = a^*$. (The notation used in Ref. 1 and followed here is that of Ref. 2.) The reason for this interest is obvious: if $a^*$ is small, it is tempting to conclude that perturbation theory could apply even up to $a^*$. Although the situation considered in Ref. 1 is more general let me concentrate on the physically most interesting case of massless asymptotically free non-Abelian gauge theory, such as QCD.

The physical question we are interested in is that of the infrared (IR) behavior of various physical quantities. If, for instance, $R(Q)$ is a physical quantity depending on a single external momentum variable $Q$ (and so as the external momenta of particles vanish so does $Q$), then depending on whether or not the limit

$$\lim_{Q\to 0} R(Q) \equiv R \tag{1}$$

exists, the theory is considered IR stable or not [for this particular quantity $R(Q)$]. So far, only physical concepts were involved in the formulation of the problem and IR "fixed" points have not yet appeared. They do so once we discuss $R(Q)$ in the framework of perturbation theory, where $R(Q)$ is given as a series:

$$R(Q) = a^d(\mu,c_i)[1 + r_1(Q/\mu)a(\mu,c_i) + r_2(Q/\mu,c_2)a^2(\mu,c_i) + \cdots ] . \tag{2}$$

In the following I take, as do KSS, $d=1$. The couplant $a(\mu,c_i)$ appearing in (2) obeys the familiar equation

$$\frac{da(\mu,c)}{d\ln\mu} \equiv \beta(a) = -ba^2(1 + ca + c_2a^2 + \cdots ) , \tag{3}$$

where $b,c$ are fixed (in QCD by specifying the number of quark colors and flavors), but all the higher $c_i$, $i>1$ are *completely arbitrary.* They define, together with $\mu$, the renormalization scheme (RS) of the couplant: $S_R = \{\mu, c_i\}$. Let me also recall the useful concept of the renormalization convention (RC), which is defined by specifying all the free $c_i$'s, but leaving $\mu$ undetermined: $C_R = \{c_i\}$.

For the Green's function $G$, the definition of the RS involves, in addition to the $S_R = \{\mu, c_i\}$ of the couplant, also the specification of arbitrary coefficients $\gamma_i$ appearing in the expansion of the associated anomalous dimension $\gamma(a)$: $S_R^G = \{\mu, c_i, \gamma_i\}$. Similarly for the RC, $C_R^G = \{c_i, \gamma_i\}$.

Let me for the moment consider physical quantities only. The renormalization-group (RG) invariance stipulates that *all the RS are in principle equally good* for the evaluation of $R(Q)$ according to (2) and must give the same result if (2) is summed to all orders. This uniqueness is guaranteed, on a formal level, by specific dependence of the coefficients $r_k$ on $\mu, c_i$. The internal consistency of perturbation theory does not, however, by itself help us in summing series such as (2), if, as is the case in QCD, these expansions turn out to be divergent.[3] This divergence cannot be considered a mere nuisance and is, in my view, quite essential, especially for the investigation of such a subtle question as is the IR behavior of the theory. Let me state my position on this point before moving to the content of Ref. 1.

As any $C_R = \{c_i\}$ is in principle equally good for the evaluation of (2), let me define three different examples of them:

$$C_R^1 = \{c_i = 0,\ i \geq 2\},\ \text{i.e., 't Hooft scheme (Ref. 3)} , \tag{4}$$

$$C_R^2 = \{c_i = 0,\ i \geq 3,\ c_2 < 0\} , \tag{5}$$

$$C_R^{\overline{\rm MS}} = [c_i \text{ according to the usual modified minimal subtraction } (\overline{\rm MS}) \text{ prescription}] , \tag{6}$$

There is no IR fixed point in $C_R^1$ and moreover the corresponding couplant is negative for $\mu < \Lambda_1$, while for $C_R^2$

 

any $c_z < 0$ leads to a zero in the $\beta$ function, called IR fixed point $a^*(c_2)$. To specify the RS, let me, quite conventionally, set $\mu = Q$ in order to avoid large logarithms of $Q/\mu$. Then the coefficients $r_k$ are no longer functions of either $Q$ or $\mu$ but are pure numbers, all the $Q$ dependence being shifted into the couplant $a(Q,c_i)$. For $C_R^1$ the formal expansion (2) cannot be used in IR region ($a<0$ there) but in $C_R^2$ there seems to be no problem with the IR behavior of $R(Q)$. As the limit $a^*(c_2) \equiv \lim_{Q\to 0} a^*(Q,c_2)$ does exist and is finite, we find

$$R \equiv \lim_{Q\to 0} R(Q) = a^*(c_2)[1 + r_1(1)a^*(c_2) + r_2(1,c_2)a^{*2}(c_2) + \cdots ] , \quad (7)$$

where $r_k(1,c_2) \equiv r_k(Q=\mu, c_2)$. So in this $C_R^2$, the theory seems to be IR stable, the value of $R$ being given by the sum of (7).

This conclusion would be justified and consequently the IR behavior rather simple, were the series (7) convergent. For series divergent in any fixed RS, as those encountered in QCD, the situation is definitely subtler. To make reliable statements about the IR behavior of $R(Q)$ we should have some understanding of the full sum (7), or even better attempt to define (2) for finite $Q>0$ and then take the limit $Q\to 0$. Let me stress that our $C_R^2$ is as valid a choice of the $C_R$ as, say, $C_R^{\overline{\rm MS}}$ precisely because (2) is expected to diverge in both schemes. While in principle any RC can be used in (2) it might be that in some RC, say $C_R^0$, the expansion (2) is simpler than in another RC by being convergent. If that were the case for all physical quantities, not just one particular $R(Q)$, then clearly this $C_R^0$ (in fact the whole class of such RC's) would play somewhat of an exceptional role, if only from the point of view of summing (2). In QCD, however, this does not happen. There, the behavior of the coefficients $r_k$ is of the form $r_k \sim A f^k k^\delta k!$, where the factors $A, f, \delta$ are process and kinematic region-dependent parameters depending also on the chosen RS. For a given quantity $R(Q)$ we can absorb part or all of the divergence of $r_k$ into the coefficients $c_i$, making thus the expansion (2) convergent [as in Ref. 4 where $R(Q)=a(Q)$ by definition] but we cannot do this for all physical quantities *simultaneously*. By going from $C_R^{\overline{\rm MS}}$ to our $C_R^2$ we induce some additional divergence in the coefficients $r_k^2$, but this induced divergence is not worse than that of $r_k^{\overline{\rm MS}}$ and so does not make $C_R^2$ less suitable than $C_R^{\overline{\rm MS}}$.

Formally we can always talk about the IR fixed points as being present in the theory, but as long as we do not understand the sum (7) they concern the properties of the unphysical quantity $a(Q,c_2)$ in a particular RC and have no direct relation to the IR behavior of $R(Q)$ itself. Moreover, there is obviously an infinite number of fixed points, even in our restricted class of $C_R^2$, each one corresponding to different $c_2<0$. Let me stress that the arbitrariness in the presence of IR fixed points in $\beta(a)$ has nothing to do with the truncation of (2). We can simply choose our RC to be $C_R^2$ or $C_R^1$ defined above, giving it the property we want. In this sense $\beta$ functions corresponding to $C_R^1$ and $C_R^2$ are all orders results.

In other words, there are no "real" or "spurious" fixed points in the theory, there merely are, or are not, fixed points of $\beta(a)$ corresponding to a particular choice of the RC we made. To investigate the IR limit of $R(Q)$ it might appear most convenient, although by no means necessary, to work in some RC with IR fixed points, like our $C_R^2$, as there, the expansion (2) can be used *down to* $Q=0$ where it yields just (7).

Let me now recall the essence of Ref. 1. To each order the procedure for optimization (directly at $Q=0$) suggested therein amounts to choosing $S_R=\{\mu,c_i\}$ by setting $\mu=0$ and finding the free $c_i$'s by means of the original principle of minimal sensitivity (PMS) criterion. The basic result (restricted for concreteness to QCD) is the establishment of a criterion for the presence or absence of IR fixed points at third (and higher) orders and so for the IR stability of physical quantities at that order. According to this criterion QCD (for $n_f \le 8$, so that $c>0$) is IR stable if the RS invariant

$$\rho_2 \equiv r_2 + c_2 - (r_1 + c/2)^2 \quad (8)$$

is negative, while for $\rho_2>0$ $R(Q)$ is IR unstable. The first part of this statement is definitely correct, but for $\rho_2>0$ the situation is more complicated. To understand the difference between the cases $\rho_2>0$ and $\rho_2<0$ it is useful to find first the optimized $R_{\rm PMS}(Q)$ at finite $Q>0$ and then see how the optimal $\bar a(Q)$, $\bar c(Q)$, and $R_{\rm PMS}(Q)$ behave as functions of $Q$ when the latter vanishes.

If we do this for $\rho_2<0$ we find a saddle point at $\bar a(Q)$, $\bar c_2(Q)$ for any $Q>0$. As $Q\to 0$ this saddle point touches the line $1+ca+c_2a^2=0$, defining for negative $c_2$ the physically accessible region of $a$, $c_2$, at finite values of $\bar c_2 \equiv \bar c_2(0)$, $a^*(\bar c_2) \equiv a(0,\bar c_2)$ given in Eqs. (28) and (29) of Ref. 1. This saddle point has a finite IR limit in spite of the fact that it lies in a valley (running along the mentioned borderline) which grows ever narrower and steeper as $Q$ vanishes.

For $\rho_2>0$ there also is a saddle point at some $\bar a(Q), \bar c_2(Q)$ but its behavior as $Q\to 0$ is completely different. This behavior can be analyzed very easily for $c=0$ when we find that the saddle point does exist *only* for

$$\rho_1 \equiv b \ln(Q/\Lambda) > \rho_1^{\min} = \sqrt{(\pi^2-4)\rho_2/4} = 1.21\sqrt{\rho_2} .$$

While $\bar c_2(Q)$ is rather stable with respect to changes of $Q\to 0$ and has a finite limit $\bar c_2(0)=[\pi^2/(\pi^2-4)]\rho_2$, $\bar a(Q)$ increases with decreasing $\rho_1$ *until it diverges at finite* $\rho_1^{\min}$, thereby causing also the divergence of $R^{(3)}(Q)$ at that $\rho_1^{\min}$. Similarly, as for $\rho_2<0$, the saddle becomes progressively narrower and steeper as $\rho_1$ decreases to $\rho_1^{\min}$ but contrary to the latter case rises to infinity already at that finite $\rho_1^{\min}$. For $\rho_1<\rho_1^{\min}$, $R^{(a)}(Q,a,c_2)$ is a monotonous function of the couplant (inside the physically accessible region $1+ca+c_2a^2 \ge 0$) for any $c_2$. In fact, this phenomenon happens already at the second order, where the general form of $R^{(2)}(Q)$,

$$R^{(2)}(Q) = a\{2 + c\ln[ca/(1+ca)] - \rho_1 a\} , \quad (9)$$

has a stationary point only for $\rho_1(Q)>0$, the optimized result diverging as $1/\rho_1$ as $\rho_1\to 0$. As $r_1(\overline{\rm MS})=1.411$ (for five flavors), Eq. (9) blows up at $Q=1.45\Lambda_{\overline{\rm MS}}$, where, on the other hand, $R^{(2)}_{\overline{\rm MS}}(Q)$ is still finite.

Now, how to interpret the fact that $R^{(3)}_{\rm PMS}\to\infty$ already at finite $\rho_1$? As there is no obvious reason to expect $R(Q)$ to diverge at that or any other, finite $\rho_1>-\infty$ ($Q=0$ corresponds to $\rho_1=-\infty$), we should be cautious to interpret this divergence as an indication of IR instability of $R(Q)$. In my view it is rather a warning that the PMS criterion itself is (at the third order) inappropriate near the IR limit for quantities characterized by $\rho_2>0$. Let me recall that the only third-order calculation of a physical quantity available so far, namely, that of the ratio

$$R=\sigma(e^+e^-\to\text{hadron})/\sigma(e^+e^-\to\mu^+\mu^-)$$

(Ref. 5), yields *positive* and large $\rho_2\simeq 62$ (for $n_f=5$). So the situation when the PMS criterion is of little use in studying the IR limit is not only of academic interest.

At the third order, in contrast to the second order, on the other hand, exist fixed renormalization conventions such as $C_R^2$ of (5), in which (2) truncated to finite order makes sense *directly at $Q=0$ for any $\rho_2$*. I stressed above that I do not think this means we do understand the IR behavior of $R(Q)$, but only that in these RC we can write down mathematically well-defined expressions for $R^{(3)}(Q)$ even at $Q=0$.

So far, I have discussed the main aim of Ref. 1 and indicated (i) why the summation of perturbation expansions such as (2) is in my view essential especially for the investigation of the IR behavior of physical quantities and (ii) when and why the PMS approach fails already at finite $Q$. Let me now comment on two specific claims made in Ref. 1. First, one should be very careful when discussing the RS dependence of the so-called "critical exponent" $\gamma^*$. While the anomalous dimension

$$\gamma(a)\equiv\frac{d\ln Z^G(a)}{d\ln\mu}=\gamma_1 a+\gamma_2 a^2+\cdots\ , \qquad (10)$$

associated with certain renormalized, amputated Green's function,

$$G(Q/\mu,a(\mu))=Z^G(\mu/\Lambda,a(\mu))G_{\rm bare}(Q/\mu,\mu/\Lambda,a(\mu))\ , \qquad (11)$$

obviously depends on the $S_R^G=\{\mu,c_i,\gamma_i,i\geq 2\}$, it is often claimed[6] that its value at the fixed point of $\beta(a)$ corresponding to $C_R\{c_i\}$,

$$\gamma^*\equiv\gamma(a^*),\quad \beta(a^*)=0\ , \qquad (12)$$

is independent of this choice and represents therefore a physical quantity. There is indeed some invariance of $\gamma^*$ but only under a very limited subset of RG transformations. The arguments underlying the above claim go back to the original paper[6] of Gross and were repeated in many reviews on the renormalization group.[7] Nevertheless, as I want to show their questionable validity, I will be rather detailed here.

Consider therefore two couplants $a_1,a_2$ corresponding to two different renormalization conventions $C_R^1,C_R^2$ and connected by perturbation expansion of the type

$$a_2=F(a_1)=a_1(1+d_1a_1+d_2a_1^2+\cdots)\ . \qquad (13)$$

We immediately find that

$$\beta_2(a_2)=\frac{da_2(\mu,c_i^{(2)})}{d\ln\mu}=\frac{da_1(\mu,c_i^{(1)})}{d\ln\mu}\frac{dF(a_1)}{da_1}=\beta_1(a_1)\frac{dF(a_1)}{da_1} \qquad (14)$$

and if $Z_2^G(a_2)=F^G(a_1)Z^G(a_1)$ then also

$$\gamma^{(2)}(a_2)=\gamma^{(1)}(a_1)+\beta_1(a_1)\frac{d\ln F^G(a_1)}{da_1}\ . \qquad (15)$$

The second term in (15) is indeed proportional to $\beta_1(a_1)$ and so at first sight vanishes at zero of $\beta_1(a_1)$, i.e., at the IR fixed point $a_1^*$, yielding

$$\gamma^{(2)}(a_2^*)=\gamma^{(1)}(a_1^*)\ \text{ at } a_2^*=F(a_1^*)\ . \qquad (16)$$

On the other hand, we know that the coefficients $\gamma_j^{(i)}$, $j\geq 2$ are for both $i=1,2$ completely arbitrary and independent of $c_j^{(i)}$ which specify $C_R^1,C_R^2$. We can therefore work in the following two $C_R^G$:

$$C_{R1}^G=\{c_i^{(1)}=0,\ i\geq 3,\ c_2^{(1)}<0,\ \gamma_j^{(1)}=0,\ j\geq 2\}$$
$$\Longrightarrow \gamma^{(1)}(a_1)=\gamma a_1\ , \qquad (17)$$

$$C_{R2}^G=(\text{as in } C_{R1}^G \text{ except for } c_2^{(2)}\neq c_2^{(2)}\neq c_2^{(1)})$$
$$\Longrightarrow \gamma^{(2)}(a_2)=\gamma a_2\ , \qquad (18)$$

where $\gamma=\gamma_1^{(1)}=\gamma_2^{(2)}$ is unique. In this situation (16) is obviously violated due simply to the fact that $a_2^*\neq a_1^*$ and contrary to Refs. 6 and 7.

The reasoning leading to (16) is valid provided the derivative $d\ln F^G(a_1)/da_1$ is not singular at $a_1^*$ and does not behave there as $1/\beta_1(a_1)$. This provision is, of course, explicitly mentioned in both Refs. 6 and 7, but obviously considered as a quite natural condition to be imposed on the acceptable RG transformations. But, is it really so? I think it was only after the 't Hooft observation[3] that the dependence of both $\beta$ and $\gamma$ functions appearing in (14) and (15) on the renormalization conventions implies that we may work in what is today called the 't Hooft RC, (4) (and by extension in any RC where all but a finite number of coefficients $c_i$ or $\gamma_i$ are set to zero) that the implications of the RG freedom were fully realized. This freedom gives us the right to also use the RC, (17) and (18), defined above which by definition possess an IR fixed point at a $a^*=1/\sqrt{-c_2}$. In these RC we easily find (for reasons of technical simplicitly let me assume $c=0$ in the following considerations) that the derivative of the $\beta$ functions at the IR fixed point is a function of $c_2$:

$$\left.\frac{d\beta(a)}{da}\right|_{a=a^*}=2ba^*(c_2) \qquad (19)$$

violating another of the statements contained in Refs. 6 and 7 and, namely, that (19) is independent of the chosen

$C_R=\{c_i\}$. This conclusion again holds only provided the logarithmic derivative appearing in

$$\left.\frac{d\beta_2(a_2)}{da_2}\right|_{a_2^*}=\left.\frac{d\ln[dF(a_1)/da_1]}{da_1}\right|_{a_1^*}\beta_1(a_1^*)+\left.\frac{d\beta_1(a_1)}{da_1}\right|_{a_1^*} \quad (20)$$

is not singular at $a_1^*$. Indeed straightforward evaluation of $F(a_1)$ near the fixed point $a_1^*$ shows that it behaves there as

$$F(a_1)\simeq a_2^*[1-\kappa_{12}(1-a_1/a_1^*)^{a_2^*/a_1^*}], \quad \kappa_{12}=\left[\frac{e^2}{2}\right]^{(a_2^*-a_1^*)/a_1^*}. \quad (21)$$

Depending on whether $a_2^*>a_1^*$ or vice versa, (21) is described by two different curves depicted in Fig. 1. Evaluating the logarithmic derivative in (20) near $a_1^*$,

$$H(a_1)\equiv\frac{d\ln[dF(a_1)/da_1]}{da_1}=\frac{a_1^*-a_2^*}{a_1^*(a_1^*-a_1)}, \quad (22)$$

we see that indeed it is singular as required to cancel the zero of $\beta_1(a_1)$ at $a_1^*$. Let me, however, stress that while (22) is singular there, the function $F(a_1)$ itself *is not.* In fact for any values of $a_1^*,a_2^*$, $F(a_1)$ is given by a convergent expansion (13) in the whole interval $\langle 0,a_1^*\rangle$. *There is no good reason to reject this RG transformation* and in fact we cannot even think of rejecting it if we do not want to run into serious difficulties elsewhere. In general, we can break all the transformation functions $F(a_1)$ into two subsets, one obeying the condition $|H(a_1)|<\infty$, the other not. Analogously we can split the set of all $\beta$ functions with IR fixed point at some finite $a_1^*$ into distinct subsets $S_t$, each of them being characterized by a different tangent $t$ at that fixed point $a_1^*$. Transformations connecting two couplants whose $\beta$ functions belong to the same subset $S_t$ will be described by the functions $F(a_1)$ coming from the first subset, those connecting couplants from two different subsets $S_t,S_{t'}$, from the second one. In the latter case, the shape of the corresponding function $F(a_1)$ will look near $a_1^*$ similarly as in Fig. 1.

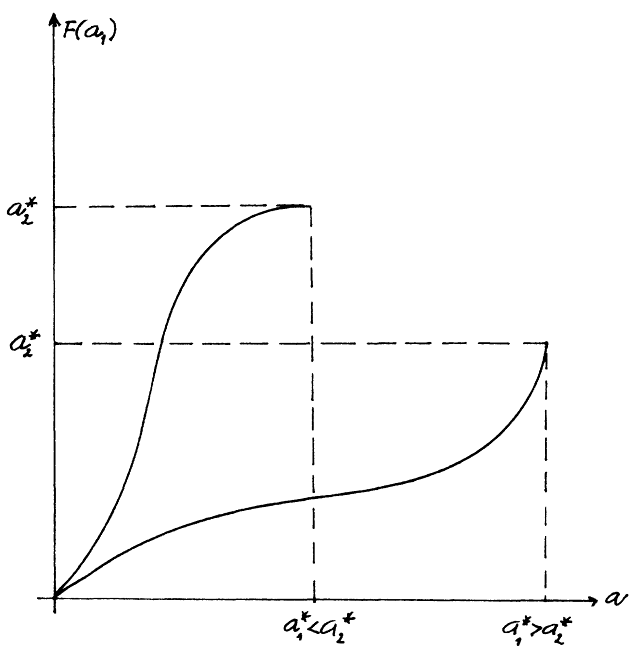


FIG. 1. The shape of the function $F(a_1)$ of Eq. (19) for two possible orderings of $a_1^*,a_2^*$.

We cannot simply reject the second subset of functions $F(a)$ in (13), for which $|H(a)|=\infty$, because in doing so we would lose the bridge between different subsets $S_t$ of the RC. This would make sense only if we would be able to decide *which of them* is the "correct" one [for our restricted class of RC in (5) this would require us to choose one and only one of all the possible $c_2$]. In my view there is no reason for preferring one such subset $S_t$ to another and no one has ever come forward with any suggestion in this regard. The derivative $H(a)$ of the $\beta$ function at the fixed point is therefore as unphysical quantity as $d\beta(a)/da$ at general $a$.

Coming now to our two RC's (17) and (18) for the Green's function (11),

$$F^G(a_1)=\left[\frac{F(a_1)}{a_1}\right]^{-\gamma/b}\left[\frac{1-c_2^{(2)}[F(a_1)]^2}{1-c_2^{(1)}a_1^2}\right]^{\gamma/2b}, \quad (23)$$

we find using (21) that near the fixed point

$$F^{\alpha}(a_1)\sim(a_1^*-a_1)^{\gamma(a_2^*-a_1^*)/(2ba_1^*)} \quad (24)$$

which leads, as expected, to singular logarithmic derivative

$$\frac{d\ln F^G(a_1)}{da_1}=\frac{\gamma}{2ba_1^*}\frac{a_2^*-a_1^*}{a_1-a_1^*}. \quad (25)$$

Depending on whether the product $\gamma(a_2^*-a_1^*)$ is positive or negative $F^G(a_1)$ vanishes or diverges at $a_1^*$. It is clear that to violate the equality $\gamma^{(2)}(a_2^*)=\gamma^{(1)}(a_1^*)$ for our two $C_R^G$ (17) and (18) it suffices when $F(a)\neq a$, the couplants $a_1,a_2$ being even from the same subset $S_t$. To reject RG transformations described by such $F^G(a)$ would imply that the change of the $C_R=\{c_i\}$ of the couplant is *correlated* with that of the Green's function [i.e., $\gamma_i$ in (10)]. Moreover, as in the case of $F(a)$ we would again have to specify which $C_R=\{c_i\}$ is to be associated with a given set $\{\gamma_i,\ i\geq 2\}$. I do not think there are any reasons to expect such a correlation or ideas on how to do it.

Let me stress that in both cases discussed above the transformations violating the statements in Refs. 1, 6, and 7 do not form in any sense a "small" class, but are on the contrary absolutely indispensable for the group structure of the set of renormalization transformations. The complete arbitrariness in the possible values of $\mu$, $c_i$, and $\gamma_i$ is in fact also at the heart of the PMS approach.

The same kind of reservations must also be borne in mind when discussing the relation of the anomalous dimension (7) to the truly RS invariant, physical quantity,

$$R^{G}(Q) \equiv \frac{d \ln G(Q,\mu,c_i,\gamma_i)}{d \ln Q} \tag{26}$$

which in virtue of the fact that $Q$ enters always in the ratio $Q/\mu$ is equal to

$$R^{G}(Q) = \frac{d \ln G(Q/\mu,c_i,\gamma_i)}{d \ln \mu} \ . \tag{27}$$

Again, the fact that

$$R^{G}(Q) = \gamma(a) - \beta(a) \frac{\partial G(Q/\mu, a(\mu))}{\partial a} \tag{28}$$

does not imply the relation $\lim_{Q\to 0} R^{G}(Q) = \gamma(a^*)$ at the fixed point $a^*$. As shown above $\gamma(a^*)$ *does depend* on the choice of the $C_R^G = \{c_i, \gamma_i\}$ chosen to renormalize $G$, while $R^{G}(Q)$ manifestly does not. The second term in (24) again compensates the RC dependence of $\gamma(a^*)$ to yield the RS-independent quantity $R^{G}(Q)$, even for any finite $Q$.

In summary the PMS criterion for fixing the RS is a quite reasonable procedure provided, of course, that it works. The IR limit of $R^{(3)}(Q)$ is, for positive $\rho_2$, just one case where it does not. The PMS runs into problems also at arbitrarily large $Q$ in the physically most interesting cases such as QCD where perturbation expansions are (in fixed RS) factorially divergent but of asymptotically constant sign. Should one conclude that because of the failure of PMS in such a circumstances theories such as QCD cannot be sensibly defined at high orders at all? The PMS, at least in its original form,[1] is of no help here, but we can still use the renormalization conventions, such as $C_R^2$ of (5), in which the fixed point appears by definition. In such a RC the question of the IR behavior of physical quantities reduces to the same basic problem as that for finite $Q$: finding a way of handling divergent series such as (7).

*Present address: High Energy Physics Department, Institute of Physics, CSAV, 180 40 Prague 8, Na Slovance 2, Czechoslovakia.

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