Problems, Puzzles and Prospects: A Personal Perspective on Present Particle Physics

H. David Politzer

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PROBLEMS, PUZZLES AND PROSPECTS: A
PERSONAL PERSPECTIVE ON PRESENT PARTICLE
PHYSICS

H. David Politzer *
California Institute of Technology
Pasadena, California 91125

The organizers of this meeting on novel results asked that I make some closing remarks which would put in perspective some of the issues we discussed. Perspective depends, of course, on where you stand, so what follows will be of a highly idiosyncratic nature.

A few years ago, we solved everything. The "standard model" ($SU(3)_{\text{color}} \otimes SU(2)_L \otimes U(1)_Y$) accounted for all known and virtually all doable particle physics. It could be "unified" elegantly into a single compact theory,¹ and this unification predicts the Weinberg angle to uncanny accuracy² as well as accounting for the cosmological baryon excess.³ (I register in passing my own deep suspicion of all cosmological, as opposed to astronomical, "facts." Historically, many have been based on extremely tenuous arguments rather than on observed data.) Ideas of unification also stimulated a new generation of proton decay and neutrino mass searches.

The confidence of the theory community was echoed in the April 1980 inaugural lecture of the new Lucasian Professor of Mathematics at Cambridge, Steven Hawking. (Mind you that this chair comes with no mean legacy of theoretical tradition.) Hawking's lecture was entitled "Is the end in sight for theoretical physics?" and his answer was yes.⁴ Admittedly, there was some work yet to be done on gravity, but supersymmetrars were hot on the trail.

This is very sad. The end of theoretical physics means the end of all basic physics.

In the interim, this point of view might well have been refuted by experiment. But to date, there exists no outstanding conflict between the standard model and what we know. Rather, more evidence has accumulated: jets, scaling, μ -pairs, hadrons at large p_T , τ 's, and bottom. (Even surprises such as D-decay rates in the end strengthened rather than weakened our faith.⁵)

But give theorists enough rope, and they'll hang themselves.

As people contemplated the prospect that the standard model was actually part of a genuinely fundamental theory of the universe, they realized that it is absolutely unsatisfactory on many scores and necessarily incomplete. Depending on personal preference as to which of the model's problems seems most pressing, people go off on different tracks. But not one of these fundamental questions has been resolved to general satisfaction.

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Confusion reigns. Ask individual theorists "What are the most important theoretical or experimental issues?" And you will get a variety of contradictory answers. Here are some examples:

- 1) Are the gauge interactions that we believe we have identified necessarily fundamental⁶ or are they obviously just an effective, low energy phenomenology, whose underlying dynamics could be just about anything?⁷ (Such divergent views are held by the most respected of theorists.)
- 2) Is the supergap (the splitting, assuming supersymmetry, between the particles we know and their superpartners), necessarily a few GeV, TeV, 10^{19} GeV? Each of these possibilities is espoused by various superpeople as being necessary for supersymmetry to make any sense at all.²
- 3) Is the smallness of the observed astrophysical cosmological constant relative to the Planck or hadronic scales the most important problem facing basic physics or is it simply a non-problem? The latter could be the case if the quantum cosmological constant were truly enormous,⁸ but the parameter we observe in the Robertson-Walker metric is actually something else, e.g., the result of an averaging that we don't know how to perform because we don't understand quantum gravity.

I wish to share my confusion on a few topics to make it clear the the situation is indeed confused. But rather than attempt to straighten it all out, I will later try to address the question of what we might practically hope to do in the near future.

I. Gravity:

In the past few years, many theorists have seriously expressed the opinion that gravity is "the only thing left to work on." There are two essential problems posed by gravity: 1) It has no apparent relation to the other fundamental forces. Even if the standard model gets unified into a single, simple group, gravity remains an additional force of rather different structure. And 2) quantum gravity makes no sense --- or rather we have not understood how to make sense out of it. The symptom of this problem is non-renormalizability.

Supergravity answers problem 1).

In fact, supergravity doesn't solve problem 1), but instead relates gravity to yet other forces which, while definitely not the forces we know from particle physics, might be responsible for our familiar forces. Maybe. Regarding 2), even the most optimistic superenthusiasts expect that all known supergravities are non-renormalizable in perturbation theory⁹. Non-renormalizability means that our understanding is incomplete. Either non-perturbative effects or some new interactions must serve to give an ultraviolet cutoff. In either case, our present theory can only be a crude phenomenology, which leaves open the question of which of its properties we should take seriously. Needless to say, no one has suggested a way in which experiments (which occasionally have guided even our wisest sages), might shed light on some of these issues.

II. Unification:

The standard ideas on unification, e.g., the SU(5) model, present us with the hierarchy problem: there are two vastly different fundamental scales present in a single system. Some attempted solutions try to make the existence of the two variant scales natural or at least stable to small variations of parameters¹⁰ (although neither of these criteria can be particularly well-defined). Another method of solution is to populate the enormous scale gap with many intermediate scales.¹¹ Of course, the problem exists only to the extent that one believes in both scales. What are these two scales? The lower one includes all the particles of the standard model. Few people are embarrassed that the relevant masses range over 0 for photons, a few eV for neutrinos (perhaps?), 5-10 MeV for u and d, 140 MeV for pions, 1 GeV for protons, 5 GeV for b quarks, ~ 100 GeV for W's. The higher scale of $\sim 10^{16}$ GeV exists mostly on paper.

III. Substructure:

Noting that matter is made of molecules are made of atoms are made of nuclei are made of protons are made of quarks, we need no great imagination to ask what's next. But there exists as yet no compelling evidence for nor compelling theoretical success using substructure. There are actually several obstacles confronting any model of quark substructure.¹² Furthermore, virtually no substructure models seriously address the property of the standard model that cries out for substructure, i.e., the fermion spectrum.

To get onto a more positive track, I wish to ask:

When is the soonest something dramatic must turn up in experiment?

I believe the answer to this lies within the weak interactions. Are there W's and a Z⁰ exactly as predicted by the standard model? Both minor and major variants are definitely possible, e.g., more Z's or a confining SU(2) which gives strongly interacting W's and Z's.¹³ If the standard model comes out successful, Z decay will reveal virtually all particles of mass less than $M_Z/2$. And as long as the weak standard model survives, precision measurement of its parameters is essential; from radiative corrections we can learn indirectly about yet much heavier particles.

An outstanding example is $M_W/M_Z \cos \theta_W$ and its deviation from 1, currently known to be accurate to a few percent. The value 1 reflects that before Z- γ mixing, the unmixed W^+ , W^- and W^0 form an isotriplet of what is often referred to as "custodial" SU(2). I would like to emphasize that custodial SU(2) is the weak interactions:¹³ the physical fermions are doublets of the global SU(2). They are all singlets under the gauge SU(2).¹⁴ Transitions between them are affected by W emission or absorption, which is allowed because the W's are isospin 1. This global SU(2) is in some ways one of the deepest mysteries of known particle physics because as a symmetry it is only approximate. It is broken by electromagnetism and fermion doublet mass splittings and the non-existence of right-handed neutrinos. While exact symmetries are symmetries, approximate symmetries usually arise from underlying physics.

The standard model predicts Higgs bosons. If they are light, we will see them. If they are heavy, as M_H^2 increases, so must the Higgs self-coupling increase to keep $\langle\phi_H\rangle$ fixed at ~ 300 GeV, which we know from M_W/e . Higgs bosons are required for the renormalizability and unitarity of the standard model. The Higgs particles may be much heavier than the W 's, but then unitarity requires that W - W scattering be modified near 300 GeV, if not by Higgs exchange, then by the W 's themselves becoming strongly interacting.¹⁵ While this energy is post LEP, it is not astronomical and represents a scale by which something dramatic must happen. The search for Higgs particles may turn up other scalars in addition or instead, e.g., technipions or inos.

Let us now consider a different question:

When is the soonest that something dramatic might happen?

The answer here is clearly tomorrow. The answer might even be yesterday, with the serious possibilities of magnetic monopoles,¹⁶ fractional charge,¹⁷ or both.(!)

Existing or future experiments could at any time see evidence of new particles and/or new interactions, substructure, technipions, or inos. (The latter reminds me that one achievement of QCD is that we could fairly certainly identify gluinos¹⁸ or, for that matter, any new colored object.) Rare decays (i.e., things that shouldn't happen but do or should happen but don't, typically involving neutral weak currents), should certainly be pursued further because they provide some of our sharpest constraints on speculations of new physics.

In fact, I think it is safe to say that anything that can be measured better or over a wider range must be useful in delineating alternatives.

Of course, there exist classes of experiments that are hard to interpret (usually those involving genuinely strong interactions). In such cases, unless the results show a dramatic break from earlier behavior, their significance is unclear. Yet some of these strong interaction phenomena are so conceptually simple that we must someday understand them. Examples are hadron-hadron total cross sections and hadron polarization effects. A few years ago I would have added hadron elastic scattering to this list, but today elastic processes are of clear and immediate theoretical interest because we now have some fairly explicit QCD theoretical expectations.¹⁹

Another question I would like to address, particularly because it occupies so much of so many people's efforts, is whether there is any value to the endeavor commonly known as "testing QCD."

The answer, I think, is definitely yes, but we must pay attention to precisely what we're doing. At present it seems unlikely but it is always possible that we will see a violation of our expectations that is so gross as to shake our confidence in the assumed underlying principles. More likely, by pursuing this question we can sharpen our theoretical and experimental understanding. This in principle would allow us to identify the first

deviations that are a signal of new physics. We also need more guidance from experiment to understand the dynamics of strong interactions. An essential element in almost all speculative thinking, this subject remains largely a mystery.

A propos "testing QCD", I would like to give a plug for lattice Monte Carlo calculations.²⁰ There are many serious criticisms that can be leveled against present efforts.²¹ However, I am optimistic that we will have in the near future a vast variety of predictions from this method that should be good to the 10% level. Some of the quantities that could thus be computed include magnetic moments, g_A 's, glueball-quark-meson mixing, and inelastic structure functions (including higher twist).

There are, to date, no "clean tests" of QCD. I am as guilty as many others for having written a paper (or many), which implied as much in its title.²² At the time we thought we had identified a particularly characteristic interference effect that manifested itself in angular distributions in lepton production. But, as with every other "clean test", there are competing effects of comparable magnitude in present experiments²³ (but which asymptotically go like $1/Q^2$). What we really need is an honest assessment of the uncertainties, theoretical and experimental --- which both tend to be quite large for the quantities of interest. At present, many people get needlessly worked up over dramatic successes or failures of QCD which are in fact neither.

If QCD perturbation theory were "exact," i.e., if there were no further corrections to worry about, the as yet uncomputed $O(\alpha_s^3)$ effects in the theory lead to an uncertainty in the measured Λ which even optimists would have to admit to $\pm 40\%$.

Of course, perturbation theory (especially as we use it), isn't the whole story. There are several possible areas of possible problems:

- 1) Even for $Q^2 \rightarrow \infty$, it is still debated whether soft exchanges between spectators render the structure functions in hadron-hadron scattering different from those measured in lepton-hadron scattering.²⁴
- 2) Do there exist strong corrections to the perturbative picture of hard scatterings that don't go away like some power of $1/Q^2$? Gupta and Quinn argue yes,²⁵ but I do not find their logic compelling, even for the hypothetical, simplified model they discuss. But the challenge will exist until we really understand strong interactions.
- 3) There is an orthodox theory of $1/Q^2$ corrections to the scaling limit. But does it make any sense to talk about power corrections without first summing all logs? The latter are each asymptotically more important than any inverse power of Q^2 . Furthermore, strictly speaking, our present calculations are only valid for the asymptotic behavior of amplitudes expanded about $Q=\infty$. I personally believe that power corrections can be defined operationally, and it is worth studying their systematics. However, the whole area is subject to this fundamental criticism.
  - a) This orthodox theory is fairly well developed for the problem of inclusive lepton production. The experimental situation is

encouraging in that we are beginning to be in a situation where we can attempt a statistically significant separation of powers from logs.²⁶ This is not an easy task because the logs come with a great number of adjustable parameters and functions, while virtually nothing is known a priori about the structure of the powers. The theory of power corrections to lepton production is given by the operator product expansion. While it is understood in principle, no serious phenomenological analysis has been attempted. A popular and thoroughly misleading model of these power corrections attributes them all to an initial parton k_1 distribution. This may well give the correct order of magnitude, but it ignores such comparable effects as off-shellness, final state interactions, and quantum interference. This naive model also gives the wrong sign for scaling violations for large x .

b) We give considerable attention to final state hadrons, e.g., in e^+e^- annihilation. Power corrections to QCD perturbation theory are invariably implemented using a "hadronization" Monte Carlo, typically of the Feynman-Field or the Lund-string varieties.²⁷ These models are crucial to present attempts to extract underlying QCD parameters from the data, and most people seem confident of their reliability. After all, if one varies the parameters of the model by $\pm 100\%$ or even goes to a different Monte Carlo and one still gets the same range of values for the underlying parameters, what more could one ask? A lot. These schemes are woefully naive and classical. There are many phenomena of comparable importance that are simply not included. So scanning the limited possibilities as described above in no way explores the whole possible range of $1/Q^2$ effects.

Here is just one example of such an effect: Consider "three jet" events. We normally think of this as arising to lowest order via a scaling amplitude in which one of the two initial quarks radiates a hard gluon. The standard Monte Carlo hadronization routines give $1/Q^2$ corrections to the energy flow. However, there are also several independent three-jet amplitudes, with their own energy and angular dependence, that go like $1/Q^2$ relative to the scaling process. Though we do not yet know how to compute them explicitly, pictorially they involve (among other things), the radiation of "diquarks" and "digluons" (neither of which are necessarily color singlets). Since these amplitudes have their own angular dependence, they are in no way simply a dressing of the individual outgoing partons of the scaling amplitude. That is to say that these corrections are process-dependent in a way that can't be mocked up by the existing hadronization algorithms.

What to do? I suggest a very phenomenological approach: collect data over a broad enough range to attempt a statistically significant separation of the scaling piece from $1/Q^2$ corrections. If that is not yet possible, I would simply use existing hadronization routines as an estimate of the relevant uncertainty, i.e., compare with no hadronization to see the magnitude of the effect in the quantity under study.

I'd like to point out that even in quantities where direct comparison with perturbation theory is possible, (i.e., "infrared safe,") hadronization or $1/Q^2$ effects may be important. They are certainly important in present-day analyses of energy flow in e^+e^- annihilation.

The total e^+e^- annihilation cross-section is reputed to be one of the best understood QCD predictions. There, the operator product expansion can be used to estimate the first power correction to straightforward perturbation theory. Aside from calculable heavy quark mass effects, the first such correction to R is of order $(400\text{MeV}/Q)^4$. In fact, using ITEP operator matrix elements,²⁸ that is probably the value (with a plus sign), to within a factor of two, making it, in practice, rather small. However, one is always near thresholds--- light quark thresholds if not heavy ones. This necessitates smearing in energy-squared by an amount Δ .²⁹ A safe Δ is presumably some small fraction of Q^2 , but no one really knows. This uncertainty, which must also be present in shape predictions, undermines the predictive power of perturbative QCD.

QCD phenomenological analyses are sufficiently complex that it is tempting to lie and/or be lazy about the inclusion of some effects. Things that we already understand in principle are often ignored in practice. Common examples are quark mass effects and the uncertainty of the glue distribution. And sometimes silliness prevails, as in the attempt to add up the electric charge of a jet. If we are to be serious about testing QCD, we must raise our standards.

Old references are not always models of careful analyses. Consider exhibit A.³⁰ The solid lines show QCD predictions for scaling violations in electroproduction, with the uncertainty reflecting the uncertainty of the glue distribution. No one today would take those 1974 fits seriously. Too many relevant phenomena were ignored. But have we really learned our lessons? Consider exhibit B.³¹ (Let me emphasize that I think the experimental work and the method of presentation of ref. 31 are excellent.) The observation of the running or Q^2 -dependence of α_s is subject to the same criticisms (and then some), as my 1974 scaling fits. This analysis of $\alpha_s(Q^2)$ came from studying energy moments of the cross section into a cone of variable opening angle. While the data fits the "theory" nicely, it is noted in the top figure that α_s must be varied from 0.16 to 0.18 to fit different moments. Hence one might conclude there is a 10%, i.e., $O(\alpha_s^2)$, uncertainty in α_s . However, the running of α_s , supposedly determined from the same analysis, is itself $O(\alpha_s^2)$. So I would say this is yet another situation where a variety of low Q^2 phenomena came to be described over a limited range by simply readjusting the perturbative QCD parameters.

To summarize briefly all that I've been trying to say: I think the experimental prospects are wide open. All we have to do is try.

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Fig. 9. Plots of $\int_0^1 x W_2^{proton} x^{n-2} dx$ for $n = 2, 4, 6, 8$; normalized to one at $-q^2 = 4 \text{ GeV}^2$. The solid lines are the maximum and minimum variations for $\bar{g}^2/4\pi = 0.1$.

led predictions for the violations of scaling. In fig. 9 the predictions for the first few moments are sketched for the $SU(3)_{\text{physical}} \times SU(3)_{\text{color}}$ model. The only free parameter is that $\bar{g}^2/4\pi = 0.1$ at $-q^2 = 4 m_p^2$. Because the A^n are not calculable, they are varied to one at $4 m_p^2$, and for each moment, there is a minimum and a maximum corresponding to the smallest and largest eigenvalues of γ^n . The credibility of these predictions is high because they represent a particular case of the x dependence of the parton distribution functions. Clearly, over a wide range in Q^2 , the predictions are not significantly altered by an $x' = x + \delta x$ or by a change in $\bar{g}$ and γ of order $1/q^2$. Fixing the value of $\bar{g}$ at $4 m_p^2$ is a reasonable assumption.

EXHIBIT A

Fig. 37. $C_2(s)$ and $C_8(s)$ with jet calculus predictions.

previous determinations of α_s and with the correlation method. In addition to using the $\alpha_s(Q^2)$ for $Q^2 = (29 \text{ GeV})^2$, we can invert the variation of α_s with q^2 . Using the formula for

$$\frac{\alpha_s(x^2 s^2 Q^2)}{\alpha_s(Q^2)} = f(C_N(s),$$

The behavior of α_s deduced from the moment C_2 is shown. The data agree with the arithmetic variation predicted by QCD which is shown. $\alpha_s(29 \text{ GeV}) = 0.16$. The 4π conversion for each

Fig. 38. Q^2 behavior of α_s deduced from $C_2(s)$.

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R. Hollebeek
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EXHIBIT B