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THE FUTURE OF EXPERIMENTAL HIGH ENERGY PHYSICS*

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We all regret that Leon Lederman was not able to be here in Singapore. But no one regrets it more than I do. While it is impossible for me to adequately replace him, I have chosen not to change the title of his contribution. My reason is simple. The theoretical situation has been fully covered, and the beautiful and eloquent talk preceding this by Lev Okun would be an impossible act to follow. Besides, the heart of our field is in the machines and experiments, and it is fit and proper that they get adequate attention here.

It has been commented upon that it is a quiet time for the field. The 1980s have little of the revolutionary character possessed by the 1970s. That does not mean, however, that nothing has happened experimentally. This meeting has exhibited the products of a decade of extraordinary progress in the building of the large machines such as the $SP\overline{P}S$, TeVatron, SLC, and LEP. The experiments are impressive and beautiful, and the central output is the acquisition of solid confidence in the standard model. I never would have dreamed in the l960s that things would have progressed to the highly orderly state we now have, where a theorist working within the standard model feels like an engineer, and one working beyond it feels like a crackpot.

Okun and Cecilia Jarlskog¹ have heaped deserved praise on the standard model description of nature. Cecilia even gave the Higgs sector an orchid. I wouldn't go that far. Maybe a durian² would be enough.

Anyway, the 1980s have been themselves quietly revolutionary, and I here want to discuss some of these revolutions, namely industrial, sociological, and technological. We expect these to be leading us to another physics revolution. The question is not whether, but when and where.

Machines and The Industrial Revolution

At the present time we see a new kind of industrial revolution in accelerator physics: the proliferation of factories. The standard model territory has been rather well explored, and the rich physics lodes identified. What remains is to manufacture useful output from these raw materials. Name a particle and there is a factory either in existence or proposed for producing a vast number of them. Every quark (not counting those from which we are made) has its factory or factories. Kaon factories have been around for some time, yielding the beautiful limits on rare and forbidden decays as reported here.³ New ones, such as KAON in Canada, are hopefully on the way. There is renewed interest in phi factories, *i.e.* e^+e^- colliders of 1 GeV cms energy, with the central motivation to produce $K_L - K_S$ pairs for CP violation studies. A proposal for such a machine at Frascati is approved, with the goal of attaining a luminosity of 10^{33} cm⁻² sec⁻¹. Not only is this a daunting challenge, but so also is the CP experiment itself, which requires very good electromagnetic calorimetry in both energy and spatial resolution.

It is a pleasure to see the Beijing charm factory entering the field at this meeting,⁴ and we all, I am sure, wish them a productive future. But already there is serious discussion in a number of places, notably Spain, on going to much higher luminosity at charm threshold. Not only are such machines factories for charm, but also for tau leptons, for which very beautiful precision measurements can be carried out.

The e^+e^- colliders are not the only sources of charm. High energy fixed target experiments using photon and/or hadron beams have been and will continue to be a serious alternative. For example, it is expected that the next generations of Fermilab fixed target experiments will reach a level of a million <u>reconstructed</u> charm particles per experiment. And maybe in the long run hadron-hadron colliders will go well beyond that—although at present I know of little activity aimed in that direction.

By far the most popular factories now under discussion are the bottom factories. This is right and proper, given that the bottom quark promises to teach us much about the electroweak mixing parameters and thus ultimately about the origin of quark mass and perhaps electroweak symmetry breaking. The criteria for e^+e^- bottom factories, set by the desire to observe CP violating processes, are that they have luminosities well in excess of 10^{33} cm⁻² sec⁻¹ and preferably that they have asymmetric beam energies, say 9 GeV against 3 GeV. Then the moving center of mass of the upsilon parent, together with high precision vertex detection, allows resolution of the decay vertices of the *B* mesons and therewith highly improved sensitivity to mixing phenomena and the CP physics. There is discussion in Japan, the USA, western Europe, and the Soviet Union. At least one such machine, if technically feasible, must be built somewhere.

A complementary approach utilizes hadron-hadron collisions. Already at the TeVatron collider rare B decays have been observed without the help of microvertex detectors. There has been a lot of study on what may be reachable with an optimized collider detector dedicated to the study of B physics for its own sake. The CP violation level looks attainable, although the technical requirements on the detector and data acquisition system are very severe. And at CERN⁵ (and Fermilab⁶) we shall soon see how well microvertex detection can be implemented in the relatively hostile collider environment.

The possibility of fixed-target experiments should not be ignored either. Interesting, sensitive experiments are being mounted at Fermilab, and there is serious

discussion of whether a fixed-target approach at the SSC is competitive with collider experimentation.⁷ In order to compete, there has to be a great instrumental advantage in the fixed-target approach to overcome the superior signal-to-noise ratio in collider mode. In particular, there is a folk-theorem that states that for a fixed detector geometry and acceptance, the signal-to-noise increases rapidly if either beam energy is raised. This is because for a fixed detector architecture, the main thing that changes with increasing energy is the parton-parton luminosity. That quantity in turn grows rapidly with increasing energy because the parton longitudinal fractions x_1 and x_2 decrease. Generic backgrounds associated with minimum bias physics do increase slowly with energy, but much less rapidly than the hard-collision signal. In comparing fixed target architectures with collider architectures, there is, however, a potential distinction coming from the necessity of a beam pipe for the circulating beams going through the center of the apparatus, something which is sometimes avoidable in fixed-target mode. But were the beam-pipe problem able to be overcome, I think that the folk-theorem implies that for hadron-hadron collisions just about any measurement based on an underlying hard collision of partons is in principle best done in collider mode at the highest possible energy, no matter what the intrinsic mass scale of the relevant physics.

The top quark has not yet been discovered, but already one can anticipate future top-factories, most likely next-generation e^+e^- linear colliders. Top threshold should be a convenient "stage-one" way station on the path to the TeV energy scale for such machines. If, for example, Nambu's idea⁸ that the Higgs condensate is in some sense made of top-antitop is right, there probably needs to be extra interactions of tops with themselves to make the dynamics work. Study of the threshold region may then be an especially sensitive probe of this idea.⁹ Beyond these quark factories lie future Z-factories, the desirability of which need no elaboration at this meeting. No matter what the luminosity, there will remain hunger for even more. And someday there will be the need for "Higgs factories", depending upon what nature has in store for us. But I would not venture a guess as to what they will look like.

All these factories, in addition to providing some superb physics, also provide opportunities for research at a scale small compared to the big machines. But they cannot be a substitute for the big ones. The future of the big ones now focuses on the next-generation proton colliders. The heritage of the SSC/LHC goes back to the 1970s and the idea of the VBA (very big accelerator) which was originally conceived as a "world machine"—too big for any region to do by itself.¹⁰ The idea—in which Leon Lederman was very involved—gave birth to ICFA, the International Committee for Future Accelerators—and provided the initial line of feasibility studies for 20 TeV proton colliders. But the politics never jelled properly, and we now find the remarkable situation in which not one but probably two such colliders will exist, each primarily supported at the regional level. By definition, this raises the possibility that some day it will be appropriate to get to the next step at the world scale. That day is a long way away, but it is appropriate to question whether the next step is technically feasible. Eventually circular proton machines suffer the same fate as electron colliders: synchrotron radiation becomes intolerable. It is much worse for the protons because superconducting magnets do not peacefully coexist with synchrotron photons. There have been some studies¹¹ as to where the barrier lies; within a factor π my guess is 200 TeV per beam. Beyond that wall is the world of radical, very-high-gradient linear acceleration techniques.

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Probably the linear acceleration line will remain in the realm of electron colliders for a long time. And the technique for the next generation of linear colliders seems to be converging on a relatively conservative extension of existing techniques to higher accelerating gradients, smaller emittance beams, and more efficient delivery of power from wall-plug to beam. At SLAC, an international collaboration will construct a facility for creating and studying the small submicron beams essential for all such future machines. And sooner or later one may expect real designs for electron colliders in the TeV class emerge.

But as with the proton machines, the linear electron machines eventually reach a barrier beyond which the extension of existing techniques won't work. If we go to extremes, and ask that in some future century linear colliders surpass the ultimate circular proton machine in energy, one talks about accelerating gradients of, say, 10 GeV per meter or more. This is 1 eV/Angstrom. Given that free electromagnetic fields do not efficiently accelerate beams (the E field is at right angles to the delivered momentum), this means that matter necessarily is very near or within the beams. Energy considerations demand that the transverse scale of the entire accelerating structure be very small. These requirements together make just the survivability of any solid accelerating structure a doubtful proposition. The list of basic problems is very long, and what seems clear is that the technique at such scales is probably very different from state of the art.¹² While there is still some time before these questions have to be faced, it is clearly important that the R&D proceed with considerable priority. The level of the required effort to make real progress is nontrivial. I cannot tell whether it is satisfactory at present; my guess is that there simply are not enough accelerator physicists around to do this kind of thing as well as to deal with the present menu of big machines and factories.

Before leaving this section, it is appropriate to mention another industrial revolution, namely the increasing involvement of industry in the design and construction of the biggest machines. The interaction of industry and the traditional scientific establishment is very different in different regions (compare Japan, USSR, and CERN, for example). But a particular focus is in the United States with the management of the SSC. One need only read the newspapers to appreciate the potential hazards.

Experiments: A Social Revolution

For a long time, experiments have been growing in size and complexity.¹³ But it seems to me that in just the last ten years the change has been quite extraordinary. It is now routine to have experimental groups which number in the several hundreds. With the SSC is seen further escalation in size. I am told that the SDC collaboration now charges a registration fee for its group meetings and issues proceedings.¹⁴ This collaboration, which has so far produced a document "expressing interest" in experimentation at the SSC, will eventually produce a proposal, and hopefully be approved. But already there are more than 500 physicists signed on.

In contemplating this, I am reminded of the old days when SLAC was being built. The total number of physicists and construction budget of SLAC was no larger than the SDC enterprise. And there was plenty of concern—especially locally—of the possible negative effects on the independence and creativity of individual physicists by this manifestation of Big Science. And in those days SLAC really represented the biggest of big science. There were problems, but in general I think most people would agree that both scientifically and socially things turned out very well. So it is tempting to compare contemporary individual experiments with complete laboratories of earlier times.¹⁵ The detailed organizational problems are very different,¹⁶ but the level of complexity and the potential degree of depersonalization, etc. may be considered as comparable. And with this comparison in mind I am tempted to draw a guardedly optimistic conclusion that just as a big laboratory of high energy physics can be a rewarding and stimulating environment for an individualistic scientist, in principle the same applies for a large contemporary collider experiment.

In my talk, I also drew the inference, based on rather superficial interactions with experimental colleagues, young and old, that things seem to be working out reasonably well with respect to the existing big experiments. But I was taken by surprise by the intensity of negative responses afterward from various young experimentalists. Big problems seem to remain. I am not, nor is it my intention to become, expert in the nature of the concerns. Some may well be specific to the personalities and styles of the individual experiments, and therefore are their internal business. But if there are problems of a more universal nature, now would be a good time to address them, before one gets too far along the road toward the next generation experiments. A problem that seemed to me to be a generic one is the identifiability to the outside world of the specific contributions of individual young members to the experiment. As I understand it, this is meant as formal identifiability, not the anecdotal identifiability through the grapevine. Most physicists like to see their own contributions publicly documented; this gets harder nowadays in the big experiments. I refrain here from floating proposals or reviewing the mechanisms already in existence for dealing with this;¹⁷ it is not germane to the point I offer here. Here I would only suggest that if indeed there are problems of a universal nature, perhaps the young, untenured experimentalists from the sundry collaborations should create a forum where these concerns can be discussed and identified, with remedial actions proposed. Perhaps the professional societies could in their meetings help to sponsor such an enterprise. And since the sociology of the large collaborations has already attracted the interest of professional sociologists and social anthropologists, their expertise should not be neglected. And of course the sensitivity of the senior leadership to these issues is crucial.

There are a host of other problems associated with the bigness of our big science, such as the long time scale for individual experiments and the resultant mismatch with the traditional time scales for career advancement, especially in the world of academe. That problem ought to be solvable.¹⁸ More fundamental may be what might happen a century or so from now, when the time scale for a machine and/or experiment exceeds the career lifetime of an individual scientist. Then high energy physics really becomes cathedral-building, and the mentality of the typical physicist must undergo a fundamental change. I am surprised by how many of my colleagues are motivated by the expectation that the answers to our questions really will be found within our lifetime. I cannot imagine the most serious questions ever being all resolved (although I couldn't imagine the existence of the standard model either). If cathedral-building is necessary in the future, I would hope that our great grandchildren will make the adjustment.

Before leaving this subject, there is another quite fundamental question associated with the large size of experiments, and that is whether it affects the nature of the scientific results. An interesting example appeared in Dydak's beautiful summary¹⁹ of the results from LEP. The χ^2 of the combined measurements was typically well below one per degree of freedom, even when systematic errors were removed as much as possible. My immediate reaction to this was what I call "social pressure". By this I mean that any analysis procedure must be terminated at some point, and the choice of time when it is terminated is biased toward when the data agrees with expectations. I hasten to add that I am not questioning the integrity of the experiments; they are fully aware of this bias and create, I am sure, a variety of protective mechanisms against it. But the history of science is full of examples of social pressure at work at a very subtle, essentially subconscious level. In the corridors afterward, I heard another hypothesis, namely that conservatism led to overestimate of systematic errors, some of which remained in Dydak's compilation. While "social pressure" suggests that the quoted error is too small, the latter "conservatism" interpretation suggests the opposite. Thus you are free to draw whatever conclusion you prefer. The result to me is simple: we have evidence that these monster experiments have not become gigantic automata, but that they are still human. I see nothing wrong with that.

A possible effect of bigness of experiments which worries me more is an inherent conservatism which tends to repress the reporting of marginal phenomena. Suppose experiment A sees a three or four sigma effect of potential importance but marginal significance. Even if it is reported in an appropriately tentative manner, the glare of publicity, as well as overenthusiasm within the theoretical community, can inflate its significance and credibility. Then when the effect turns out to be a fluctuation, the reputation of the collaboration is negatively affected. So such phenomena, I believe, tend nowadays not to be reported as freely as in the past. If that were all there were to it, it would appear to be a better situation than before. But what I worry about is whether some cross-fertilization is lost. For example Experiment B, unrelated to A, might have in its data a marginal signal which could be related to A's. Then theorist C builds a working hypothesis which suggests searches in Experiments D,E and so on. Now nine times out of ten these leads go nowhere. But what about that tenth time?

Apparatus: Technological Revolutions

The 1980s have seen a variety of technological revolutions. I guess I put the superconducting magnet developments, the linear-collider technology, and the revolution in data acquisition and processing at the top of the list. What about the future? In electron-positron colliders it is the machine technology, about which we already commented, which is most demanding, while the detector design seems straightforward. There is no sensible alternative to just measuring every event as completely as possible with the standard 4π detector. In the case of the proton colliders, the machine technology, while hardly trivial, is regarded as less of a fundamental problem than that faced by the detectors. The 4π open geometry detectors are supposed to perform at a luminosity at least a hundred times higher than has been the practice in open-geometry fixed target hadron physics in the past. There the scientific opportunities for higher luminosity experiments have been great for a long time; yet there are precious few examples of open geometry experiments running at the rates proposed for LHC/SSC. I can think of no harder evidence for the challenge facing the designers of these next-generation detectors. Of course the scale of effort—and the funds—expended on this problem dwarfs what has been done in the past, and some of the technologies are new. What are the problems? The first is simple survivability of the detector: the singles rates in individual channels must be acceptably low, and the detection elements must be radiation hard. Next, the on-line data processing system is massive and must operate at great speed. And the event-selection strategies are multilevel and complex. Finally there is a premium on resolution. Much effort goes into the resolution in calorimetry. I would guess that there will be a big premium in the long run on resolution in tracking as well, especially in the microvertex tracking needed to isolate heavy-flavor hadrons. Most everyone puts highest priority on good lepton/photon detection, with quark/gluon jet detection close behind, in addressing the physics of the "TeV mass scale". To me the next priority lies in identifying the jets produced by heavy quarks. Gluon jets are an accursed background to much new physics, and the decay products of objects within the Higgs sector or other "new-physics" sectors are likely to be very rich in heavy-quark jets.

It seems to me that one of the challenges for the new, big SSC detectors is to create a data acquisition system which can evolve with time gracefully, and exploit concurrent developments in the information industry outside high energy physics. The problems of fast processing of massive volumes of data, and use of pattern recognition strategies, are important to the outside world too. The example of neural networks comes to mind. There exists, for example, some recent work by Peterson and collaborators in the Lund group on training the computer to distinguish gluon jets from quark jets. The claim²⁰ is that with Monte Carlo data an 85% success rate was achieved. Whatever the fate of this line of work is, it is a reminder that the event selection strategies of today may look very obsolete a few years from now.

But despite the enormous challenges of meeting the demands of high luminosity, there is the reassuring feature that if the goals are not initially met, there is a "soft landing". The folk-theorem I discussed in the first section provides assurance that even were the SSC run at contemporary luminosity, with a contemporary detector, physics would be greatly advanced beyond where we are now. There would be an order of magnitude more W's and Z's, and probably the top quark could be seen. This is evidently an extremely minimal kind of "existence theorem," and without a very aggressive attitude toward new technology one could surely go much further than that.

Closely related to this is the opportunity for doing physics of a specialized nature at hadron-hadron colliders. There is in principle just about as much diversity possible in experiments and detector architectures at the SSC as there is in the entire fixed-target plus collider program at Fermilab or CERN. There is a place for limited-aperture spectrometers, especially for coverage of the high-rapidity, smallangle regions of phase space. There is already the example of detectors dedicated to generic B-physics. I think there is a place for 20π physics, namely a spectrometer with full coverage of all rapidities with uniform sensitivity. No one has seen a single event at collider energies with the information content (per event) of a bubble-chamber picture. Maybe if they existed one would learn something new and unexpected.²¹ At the SSC it may be anticipated that there will be considerably more collision regions than can be instrumented with the big expensive SDC-style 4π generic detectors. With an investment which is a small fraction of the cost of that, a variety of very interesting, albeit lower priority, physics can be carried out. I suspect these areas may be an arena for innovations in physics direction as well as in experimental technique. Taking this kind of development into account, I would expect a facility like the SSC to have great longevity. I give it at least a half century of extremely productive research.

So far, we have discussed mainly the technologies associated with the big new machines. With respect to the "factories", the technical problems focus in most cases upon the machines more than the detectors. There is a great challenge in attaining the very high beam currents and luminosities which are needed for the physics. One cannot expect those gains to come easily.

Physics: Where and When will the Revolution Occur?

Sooner or later the standard model will break down. Since at this meeting we heard the results of various polls, I started my own poll and asked various people the question in the above heading. I didn't have enough time to get good statistics, but the answers were not very helpful. There was a very strong correlation of the answer with the direct research interest of the interviewee, so that before long I could predict the responses in advance. So I guess a real poll would show LEP as the best candidate. But I am not sure I agree with that. I think my own opinion is rather mainstream, and it goes as follows:

- 1. Highest priority must be the exploration of the TeV mass scale with the next-generation big machines. The general arguments that the high energy scattering of longitudinal W's and Z's will be revelatory are convincing but in my view conservative. The analogy in strong interactions is that the way to understanding the strong force is via $\pi \pi$ scattering. That viewpoint was once very prevalent,²² but one cannot even find the results of the experiments in the Particle Data Group compilations. In the case of the electroweak theory, nature may well provide an answer even more novel than technicolor. The message is that searches should be as broad-based as possible. The breakthrough may be as unanticipated as was strangeness, quarks, and color.
- 2. The physics of the twenty fundamental parameters of the standard model should continue to be studied as incisively as possible. The LEP activity centering around the gauge couplings may provide indications of new physics

via radiative corrections. And of very high priority is the study of the CKM mixing parameters, especially via B-decays. I can think of no scenario which renders that physics dull and uninteresting, including all kinds of revolutionary discoveries at the TeV mass scale.

- 3. Dedicated, albeit speculative, beyond-the-standard-model searches should continue to be pursued. In speculative experimentation there is often a costeffectiveness, or risk-factor, question to be addressed. How does one decide whether an expensive search with the likelihood of a null result is worth the effort? Very few would question the investment in proton decay experiments, especially since they set a new standard of quality for nonaccelerator experimentation and paid off handsomely with respect to SN1987a. Another noncontroversial area is the search for forbidden kaon decays; there any improvement in sensitivity by say two orders of magnitude is deserving of a major effort. And the search for neutrino masses and mixing, I believe, is worth a major effort if a new square order of magnitude in the usual exclusion plot of δm^2 versus $\sin^2 2\theta$ can be explored. This list can certainly be extended. But in any case the searches should go on everywhere possible.
- 4. There are many open questions in strong-interaction dynamics requiring a lot of study. Since strong processes, if nothing else, represent background for the new physics, it is essential to gain a solid data base and understanding of them. And in the case of B-decays, the quality of the output CKM and CP physics will be in direct proportion to the quality of the understanding of the spectroscopy and decay properties of heavy-flavored hadrons. Some favorite areas to me are
 - (a) Properties of the Pomeron in QCD. This includes study of "multiple

production of rapidity gaps" in minimum bias events, as well as study of the parton structure of the pomeron via deep inelastic scattering (here HERA promises²³ to break important new ground) and dijet production in single-diffraction hadron-hadron collisions. The subject theoretically is difficult and interesting and needs stimulation from experiments. Aside from its own intrinsic interest, it might lead to new ways of approaching new physics in a more background-free way.

- (b) The small-x region, say $x < 10^{-5}$, is of special theoretical interest because gluon densities become extremely large, and nonperturbative effects can be expected.²⁴ Again there ought to be a variety of experiments which look at this. The smallest attainable x will in principle occur at the SSC in production of low-mass (say 5-10 GeV) systems in the forward direction, where $x < 10^{-7}$.
- (c) The study of high-multiplicity final states and the search for quark-gluon plasma, both with nucleons and heavy ions, may lead to new insights. If the quark-gluon phase transition temperature were to be measured, it might become the most accurate measure of $\Lambda_{\rm QCD}$.
- (d) Study of QCD effects at top-quark threshold is clean and quite interesting. It will be a very nice program at any future threshold $e^+e^$ top-factories.

This list could go on and on. The bottom line is that there are plenty of directions to pursue, and there still is potential in most all of them for big surprises. Somewhere in all these possibilities will lie the expected breakout from the standard model. We need to be patient and never give up.

Acknowledgement

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- 10. For example, cf. Proceedings of the ICFA Workshop on the Possibilities and Limitations of Accelerators and Detectors, Fermilab, 1978.
- For example, cf. M. Tigner, Proceedings of the 23rd International Conference on High Energy Physics, July 1986, Berkeley, California, ed. S. Loken (World Scientific), Vol. 2, p. 1521. An amateurish try of my own is in "Techniques and Concepts of High Energy Physics II," ed. T. Ferbel (Plenum, New York, 1982), p. 233.
- For a recent discussion, cf. E.J.N. Wilson, CERN report CERN 90-05 (1990), and references therein.

- 13. An excellent recent study for the U.S. Department of Energy on the topic of this subsection was carried out by a HEPAP subpanel chaired by Sam Treiman: "Report of the HEPAP Subpanel on Future Modes of Experimental Research in High Energy Physics;" DOE/ER-0380 (1988).
- 14. I am assured that this fee is merely for the amenities, the overhead borne by the local host, and the cost of distributing transparency copies (the "proceedings") to collaborating institutions. But for the organizers it must look very much like a workshop.
- 15. Peter Galison, Ref. 13, has expounded in considerable detail this point of view.
- 16. One big difference is the geographic dispersal of the leadership, something for which modern communication techniques can help. Another difference has been eloquently expressed to me by Sam Treiman (private communication): "During the design and construction phases there is considerable resemblance to what went on in the olden days in the laboratories, with the large team breaking down into little subgroups, with all the individuality that entails. But in that ancient era, when it came time for the actual experiments—for the *physics*—the little groups continued on their separate ways doing their own experiments. With the modern project, when it finally comes to the physics—the papers, the glory, or the blame—everybody is thrown into the same pot, at least in the most important early-physics phases. Moreover, the prospect that that is going to happen in the end reflects itself backward in time to the sociology of the earlier design and construction phases."
- 17. Again, see the Treiman panel for more discussion on this issue.

- 18. Again the report of the Treiman panel addresses this issue in some detail. And it has been pointed out to me that an interesting positive (?) effect of large multi-institutional collaborations comes from the visibility of young members within that group. A job candidate can have an advantage in getting a position from some other institution in his collaboration because his qualifications are well known relative to competitors from outside.
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