Social currents in weak interactions

The controversy surrounding possible violation of the $\Delta S = \Delta Q$ selection rule yields useful insights into the reward system of a scientific field.

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At the end of his concluding remarks, summarizing the reports at the 1962 International Conference on High Energy Physics at CERN, Victor Weisskopf referred to the excitement caused by experimental claims that the selection rule known as the $\Delta S = \Delta Q$ rule did not hold and showed the New Yorker cartoon reprinted opposite.[1]

It was not, as many readers surely know, the discovery of the century, and yet the $\Delta S = \Delta Q$ selection rule controversy holds a certain fascination for those interested in understanding the development of an important branch of particle physics, that of weak interactions. The possibility that the $\Delta S = \Delta Q$ selection rule was false, claimed by several groups of experimenters in 1962,[3] threatened to topple a highly successful theory of weak interactions,[4] and reconstruction would probably require a significant effort. While anomalous experimental data seem frequently to be published without creating much of a stew, the experimental claims in this instance were taken seriously by those working in weak interactions at the time. The fact that the experiments were shown later to be invalid gives this chapter in the history of weak interactions an illuminating character of its own, and that would be reason enough to try to reconstruct its history.

Cliometrics

Relating the $\Delta S = \Delta Q$ story is not our only purpose in this paper. Traditionally, historians of science have been able to analyze not only the published work of an individual scientist or group of scientists working in a particular field, but also their unpublished manuscripts, notes, letters, and other materials. Two facts make such an approach to the history of modern science increasingly difficult. First, the size of the science has grown tremendously. But perhaps the second fact is more important in the long run, namely that contemporary scientists generate less of the traditional sources of historical data such as letters, notes, and so on. The telephone conversation and face-to-face communication at frequent conferences or in the hall, unrecorded and unwritten, leave much of the making of current science unavailable to the historian. To replace the traditional sources, investigators are developing quantitative techniques[5] to help unravel the history of more recent science. A second purpose of our paper is to illustrate some of these techniques for the case of the $\Delta S = \Delta Q$ controversy.

The analysis of quantitative historical data, or "cliometrics" (from Clio, the muse of history) can range, in the case of a field in science, from the simple counting of papers and people to the more elaborate techniques of citation analysis. Increasingly, citation analysis has come to be regarded as a useful tool for sociologists and historians of science, replacing the earlier, rather futile notion that a count of the output or citations could serve as a measure of the quality of the scientific contributions made by a scientist, a group, a lab, or a university.

Our data consist primarily of what we believe to be an exhaustive bibliography of serial articles in the physics of weak interactions published from 1950 to 1972. The 4691 articles in our bibliography were written by 3949 authors from 50 countries. In addition to listing the articles, we coded standard bibliographic information for the references listed at the end of each article. This gave us a list of over 80 000 references, 64 000 of which were to other serial articles, the rest to reviews, conference proceedings and the like. Of the serial articles, half were to other articles in our bibliography. We read each of the 1021 experimental articles and classified it with respect to subject area, accelerator location, types of particle detector, etc. We read the abstract of each theoretical article (3571 of them) and classified it by subject. Numerical analyses of these data together with extensive reading of additional sources such as reviews (99 were in our bibliography), proceedings of conferences and published memoirs constitute the main evidence used in our study.

Serendipity

We came on the $\Delta S = \Delta Q$ question by accident. In fact, it was the kind of serendipity to which experimental particle physicists have become accustomed: we saw an unexpected peak in a graph. We had, from the first, been interested in the interdependence of theory and experiment in weak interactions, so we made a graph plotting the extent to which theoretical papers cited experimental papers, allowing for the effect of the relative sizes of the theoretical and experimental literatures. In that graph (figure 1) we saw a large peak for 1954, due to the τ–θ problem, a little one in 1957 (parity), and a bump in 1962. The 1962 bump was not caused by the interest of theorists in the experiments demonstrating CP violation, since the date of the enhancement preceded the discovery of CP violation by two

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years.[6] The subject of interest to the theorists was not immediately obvious.

Not much later, however, our attention was again drawn to the years 1961–63 by the data exhibited in figure 2. As part of our continuing attempt to write an overall intellectual history of weak interactions, we did what has become known as a co-citation analysis of our data for each year from 1956–72. The technique was developed primarily by Henry G. Small and Belver Griffith,[7] and is described in the box on page 46. Without going into too many details regarding the information to be gained from these particular plots, let us just say that we were drawn immediately, especially given the peak we had seen in our earlier graph, to a new hill that emerged in the 1962 plot (see figure 2b). In the 1963 plot (figure 2c) this hill had grown substantially, to a volume slightly greater than that of the hill representing the main theoretical research program in weak interactions. The special interest in experiments shown by theorists in their 1962 papers and the new activity represented by the second hill in our 1962 and 1963 co-citation diagrams told us that something important had happened during that period which we had to understand. On examining the papers involved, we found that the flurry of activity we found centered on the controversy involving the $\Delta S = \Delta Q$ and $\Delta I = \frac{1}{2}$ selection rules. Such unexpected discoveries are the most obvious benefits of the quantitative techniques we have employed. When a field is as large as this one, even a former participant may find it difficult to identify the issues that those working in the field at the time saw as significant.

Selection rules and *V–A* theory

That is how we came to focus on the $\Delta S = \Delta Q$ story, but the story itself is, of course, much more interesting. Up to 1957 the $\Delta S = \Delta Q$ selection rule was merely a useful, empirical rule with no theoretical foundation. Then, in 1958, Richard Feynman and Murray Gell-Mann formulated a theory of weak interactions, the (V–A) theory, an intellectual tour de force whose impact was enormous. (The V refers to vector, A to axial vector.) The V–A theory demanded $\Delta S = \Delta Q$ as a consequence of the favored form of the interaction. Feynman and Gell-Mann themselves referred to the $\Delta S = \Delta Q$ selection rule explicity in their remarkable paper:[4]

> It should be noted that decays $\Sigma^+ \rightarrow n + e^+ + \nu$ are forbidden if we add to the current only terms for which $\Delta S = +1$ when $\Delta Q = +1$. In order to cause such a decay, the current would have to contain a term with $\Delta S = -1$ when $\Delta Q = +1$, for example $(\bar{\Sigma}^+ n)$. Such a term would then be coupled not only to $(\bar{\nu}e)$ but also to all the others, including one like $(\bar{p}\Lambda^0)$. But a coupling of the form $(\bar{\Sigma}^+ n)(\bar{\Lambda}^0 p)$ leads to

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"This could be the discovery of the century. Depending, of course, on how far down it goes."

The meaning of the $\Delta S = \Delta Q$ selection rule

The $\Delta S = \Delta Q$ selection role is simply this: when, in a weak decay, the strangeness quantum number changes by one unit, this is always accompanied by a change of one unit of charge in the strongly interacting particles participating in the reaction. It is not easy to find examples of experiments where the rule can be applied, but an important example of the effect of the rule occurs in K-meson decay. The K meson has isotopic spin $I = \frac{1}{2}$, which implies that it comes in two forms, the K^+ with $I_3 = +\frac{1}{2}$, and the K^0 with $I_3 = -\frac{1}{2}$. Both kaons have strangeness $S = 1$, and the two quantum numbers are connected by the Gell-Mann–Nishijima relation

$$Q = I_3 + S/2$$

In the weak decay of K mesons, the strangeness must change by one unit, or $\Delta S = 1$, because the lighter pions that they decay into have $S = 0$. Two examples of the reactions that we are concerned with are

$$K^0 \rightarrow \pi^+ + e^- + \bar{\nu} \qquad (1)$$

$$K^0 \rightarrow \pi^- + e^+ + \nu \qquad (2)$$

The $\Delta S = \Delta Q$ rule refers to the kaon and pion only, so in decay 1 $\Delta S = -1$, $\Delta Q = +1$ and the reaction is forbidden; decay 2, however, is allowed. The antiparticles of the K^0 have the same possible decays; in this case the only reaction allowed by $\Delta S = \Delta Q$ is

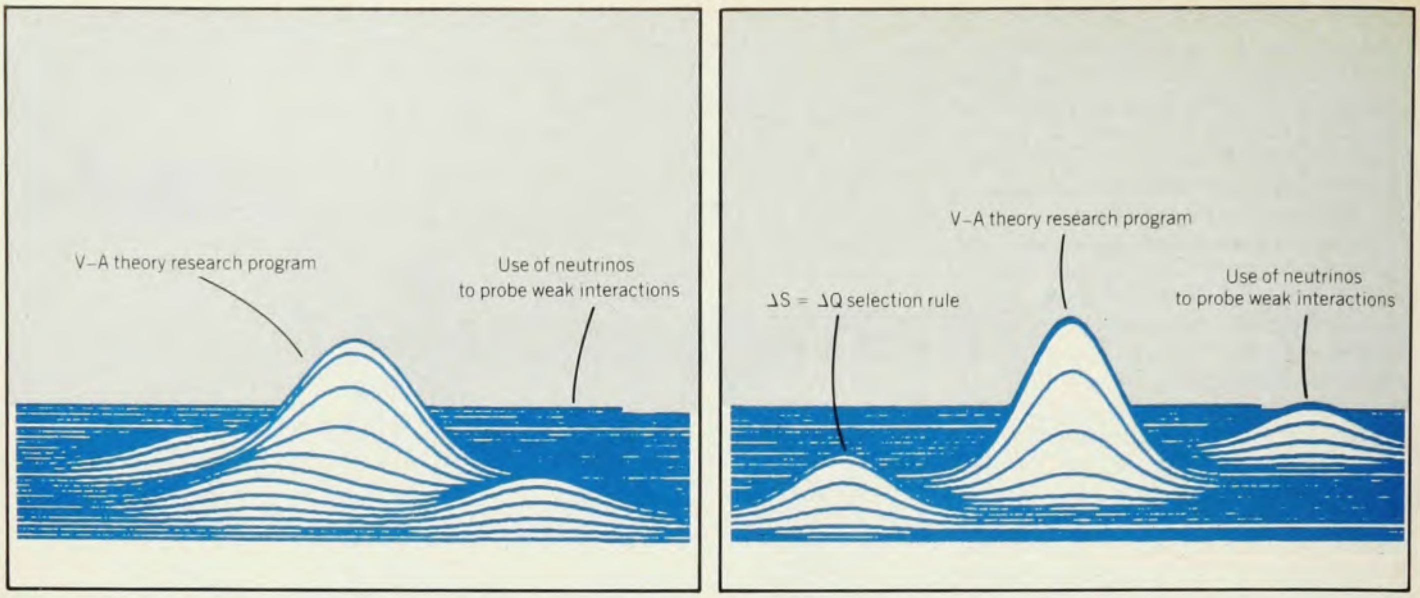
$$\bar{K}^0 \rightarrow \pi^+ + e^- + \bar{\nu}$$

This would seem like an easy possibility to test experimentally except that it is hard to be sure that one has a K^0 or $\bar{K}^0$ due to the fact that they are usually mixed in beams. The experiments are very difficult. Notice that for the K^+ meson

$$K^+ \rightarrow \pi^0 + e^+ + \nu$$

is the only possibility allowed by charge conservation; so K^+ decays cannot provide a test of the $\Delta S = \Delta Q$ rule.

The existence of a $\Delta S = \Delta Q$ selection rule was first suggested about 1954 after several experiments seemed to indicate that isotopic spin (I) always changed by $\frac{1}{2}$ in the weak decays of strongly interacting particles into other strongly interacting particles after certain electromagnetic corrections were taken into account.[2] Given the Gell-Mann–Nishijima relation if $\Delta I = \frac{1}{2}$, then $\Delta S = \Delta Q$.



strange particle decays with $\Delta S = \pm 2$, violating the proposed rule $\Delta S = \pm 1$. The validity of the selection rule now took on a major importance. It was no longer merely a convenient rule, seemingly good empirically, which if shown to be false would not cause real dismay; it became a test for the new theory. If the experimental data showed disagreements with $\Delta S = \Delta Q$, the $(V\text{–}A)$ theory, which had been scoring one success after another since it was proposed in 1958, might have to be abandoned or at least be significantly modified.[8]

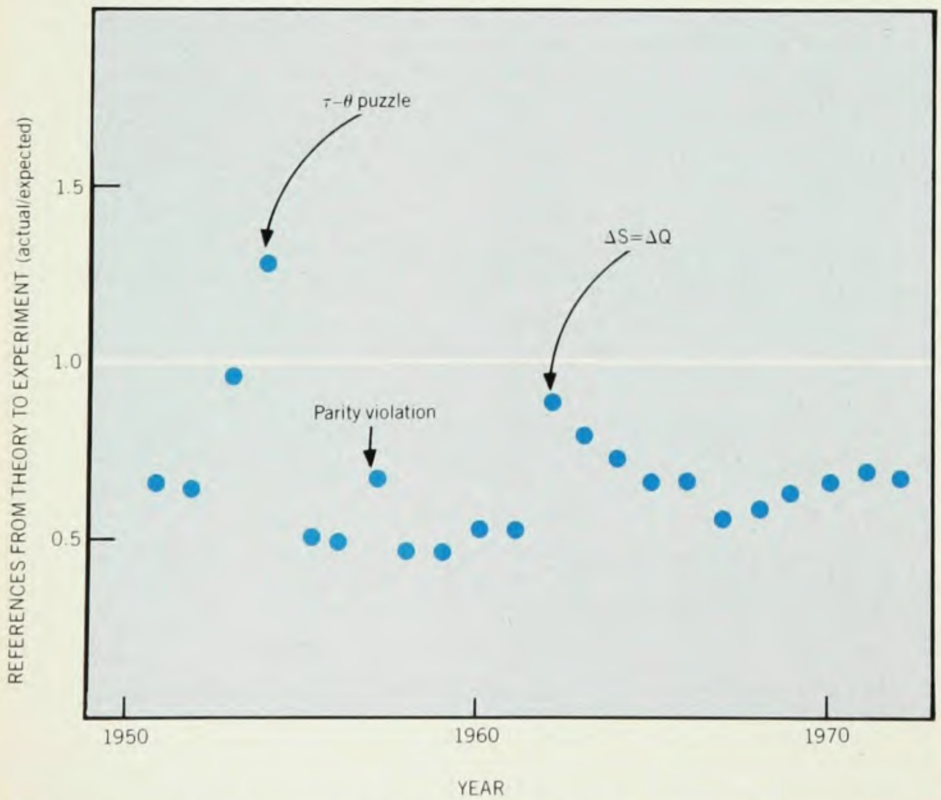
The $\Delta S = \Delta Q$ rule did not, however, really begin to receive significant experimental attention until 1961. In early 1962 the Fry–Camerini group, working at Berkeley, published experimental data that they interpreted to mean that the $\Delta S = \Delta Q$ rule had been violated. R. D. Tripp, M. B. Watson and M. Ferro-Luzzi, also in 1962, claimed in addition that the $\Delta I = \frac{1}{2}$ rule was violated to a degree that could not be explained by the expected electromagnetic correction.[9]

Further fuel was added to the fire at the 1962 International Conference on the High Energy Physics, held at CERN from 4–11 July, where Frank Crawford summarized the work done by his group as well as that of the Barkas group. Both groups claimed to have found violations of the rule.[10] L. B. Okun, in his summary of the state of weak-interactions theory[11] said:

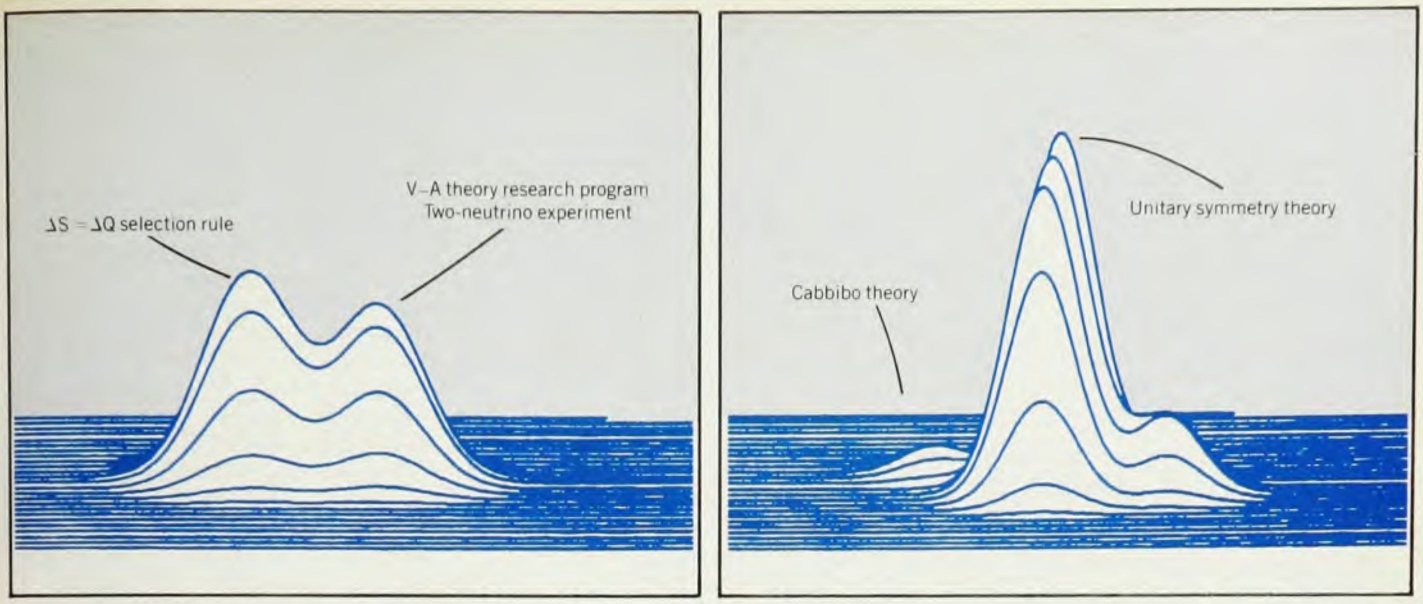
> I should like once again to stress the importance of the problem of the $\Delta S = \Delta Q$ rule and to call on experimentalists to investigate this matter whatever the effort involved. Experimental clarification of this problem will greatly stimulate progress of the weak interaction theory.

By 1963 several new experimental groups had become involved. They reported their results at the International Conference on Fundamental Aspects of the Weak Interactions, held 9–11 September at Brookhaven, and the Siena Conference on Elementary Particles, held 30 September to 5 October. At Brookhaven Jack Steinberger and Dick Plano summarized the work of their group checking the earlier results of the Fry–Camerini group and the Barkas group.[12] They concluded that, while the new results were "somewhat muddy," they had "no positive evidence that $\Delta S = \Delta Q$ is violated." They reported their claims again at Siena a few days later[13] and were joined by the Ecole Polytechnique group who felt that the results were "certainly in contradiction with a strong violation of the $\Delta S = \Delta Q$ rule."[14] But an Italian group from Padova, which had earlier participated in the Fry–Camerini experiment at Berkeley, reported new results from their experiment at CERN which were consistent with a violation of $\Delta S = \Delta Q$.[15] They were cautious however, and preferred "to avoid any anticipation and take the experiment to the end." Abdus Salam, the theoretical rapporteur at Siena for weak interactions, called the $\Delta S = \Delta Q$ controversy an "epic confusion."[16]

With time the confusion abated. It gradually became evident that the selection rule is, in fact, not violated in a major



References from theory to experiment. We show the ratio of the actual number of citations in each paper to the number expected if the citations were randomly distributed among theoretical and experimental papers, taking into account the relative sizes of the two literatures. Figure 1



Interrelations and visibility of important papers in the weak interactions. The height of each hill is proportional to the number of times each paper is cited and the distance between any two hills is determined by the number of times the two papers are cited in the same article, as discussed in the box on page 46. We show the results for the years 1961–4. A list of the papers is available from the authors. Figure 2

way, and that various experimental errors produced the apparent violations seen earlier. Our bibliography includes over twenty experimental papers from 1964–72 reporting specific tests of $\Delta S = \Delta Q$ and showing no violation of the rule. The earlier results did not mark the discovery of the century.

The story still has some interest, it seems to us, because it shows clearly the patterns of behavior of the different actors involved in the controversy. We feel that these patterns, clarified by the excitement, occur normally in particle physics, and that examining them for this case might reveal some important aspects of the social system of particle physicists.

Patterns of behavior

Patterns of article production for theory and experiment in weak interactions are, of course, markedly different. In figure 3 we show the number of theoretical and experimental articles produced in weak interactions per year during 1950–72. The rate of theoretical article production is largely dependent on the number of theorists working in the field, except for the two bumps visible in 1957–58 and in 1965–68, where the excitement of parity violation and CP violation caused significant increases in *per capita* article production among theorists. The rate of experimental-article production is fairly flat after 1957, even though the ratio of numbers of experimentalists to theorists interested in weak interactions hovers around 1.0 after 1957. Experimental-article production is constrained by technology, and the increasingly complex experiments required larger groups to produce the same number of articles as time went on.

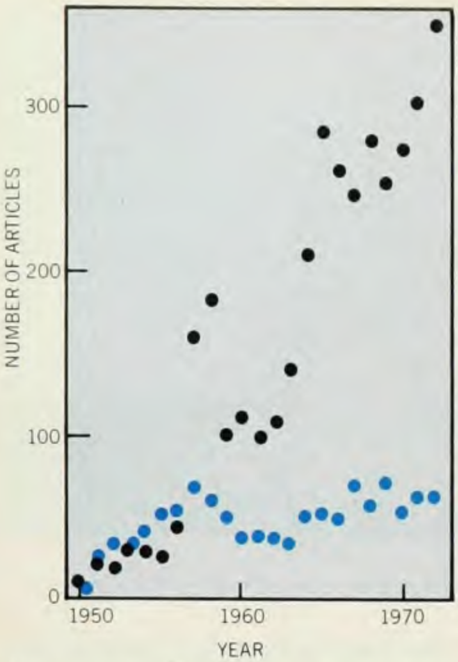
Big blocks of theoretical articles focussed on a small number of specific topics during 1950–72, and each of them should be familiar to anyone even casually acquainted with the field. The breakdown is shown in figure 4. Theoretical high-lights include the τ–θ puzzle, parity violation, the seemingly endless analysis of K-decay form factors, and, just visible near the end of the time scale, the emerging efforts of theorists working on weak–electromagnetic unification (the Weinberg–Salam theory) after t'Hooft showed how it could be renormalized. A similar plot of experimental activity would show a similar rising and waning of interest in a small number of categories.

Compared with the number of articles devoted to such excitements as parity and CP the $\Delta S = \Delta Q$ controversy was only a small blip. We found only 33 experimental articles in all that were devoted to $\Delta S = \Delta Q$, and a total of only 37 theoretical articles. Clearly, the importance to the field of the $\Delta S = \Delta Q$ controversy does not emerge from article production data.

The small number of theoretical and experimental articles devoted to the $\Delta S = \Delta Q$ selection rule were the object of intense interest on the part of both theorists and experimenters, however, as figure 5 shows. Experimental articles concerned with $\Delta S = \Delta Q$ were cited over 23 times more often by theoretical articles in 1963 than one would expect given their cumulative proportion of all weak interactions articles published up to that point, and they were cited over 13 times more often than expected in 1964 by experimental articles. Theory articles on $\Delta S = \Delta Q$ were cited by experimental articles over 7 times more often than expected in 1962, and about 4.5 times more often than expected by theory articles in 1962.

The differences in timing of the peaks in figure 5 reveal some of the internal publication dynamics in a field such as weak interactions, where theorists are able to publish very quickly but experimenters quite often must design and build apparatus, take data, and analyse it before being able to publish. At least two theorists published papers before 1962 speculating on the consequences to $(V–A)$ theory of a violation of $\Delta S = \Delta Q$ and proposing various fix-ups.[17] So, when news of the Fry–Camerini data showing a violation of $\Delta S = \Delta Q$ became known in late 1961, both theorists and experimenters who wanted to take note of the question in 1962 could refer to at least two theoretical papers but had only the Fry–Camerini experimental paper (published in February, 1962) to cite until others were published much later in the year. These first experimental papers reporting violations of $\Delta S = \Delta Q$ were picked up very rapidly by theorists publishing in 1963, whereas for experimental references to experimental tests of $\Delta S = \Delta Q$ one has to look for the publication of results from second-generation experiments during 1963–64. Thus it is quite natural that the peaks for references by both theorists and experimenters to theoretical articles on $\Delta S = \Delta Q$ fall in 1962, the peak for theoretical citations of experimental $\Delta S = \Delta Q$ articles appears in 1963, and the peak for experimental references to other experimental papers comes in 1964.

But some of the finer details of figure 5 should also command our attention. First, they show that while theoretical interest in $\Delta S = \Delta Q$ was intense early on, there was a rapid drop in that interest after 1963. Experimental interest dropped also (especially experimental interest in theoretical $\Delta S = \Delta Q$ papers), but the level of experimental citation of experimental $\Delta S = \Delta Q$ papers never declined below five times the expected value. We will come back to these data



Number of theoretical and experimental papers on weak interactions from 1950 to 1972. Note that the number of theoretical articles (black), which depends mainly on the number of theorists interested in the field, shows large fluctuations as well as a rapid increase, while the number of experimental articles is apparently subject to stronger constraints. Figure 3

below. Each of the four graphs also shows two secondary peaks, one about 1965–66, the other around 1968–70. Both renewals of interest in $\Delta S = \Delta Q$ were due to speculation that a small violation of the selection rule in K decays might explain, or somehow be associated with, a violation of CP invariance that had been found in K^0_L decays by the Fitch–Cronin group in 1964.[6]

One final item is crucial for the interpretation we offer below for why people behaved the way they did during the $\Delta S = \Delta Q$ controversy. Theorists who published on the subject in 1962–4 were on the whole not those who had assumed a leadership role in the evolving theoretical research program in weak interactions, which at that time involved activity directed toward an integration of developments in unitary symmetry theory with the newly achieved understanding of the weak interaction provided by the V–A theory. The theorists who responded so rapidly to the experimental claims of a violation of $\Delta S = \Delta Q$, proposing various theoretical fixes, were largely theorists whose ideas were not deeply implicated in the current theoretical apparatus.

An interpretation

We have identified several patterns of behavior during the $\Delta S = \Delta Q$ controversy for which explanations are not immediately obvious:

- the fact that the great surge of interest by theorists in the $\Delta S = \Delta Q$ rule ended so quickly;
- the fact that experimental interest in $\Delta S = \Delta Q$ experiments continued to be high long after the excitement died down; and
- the fact that the theorists who published on $\Delta S = \Delta Q$ during this period were in general ones who were not centrally involved in the main line of theoretical development in weak interactions (which says nothing, of course, about their level of activity in other areas of particle theory, such as strong and electromagnetic interactions).

No explanation flows directly from our data, but we offer a series of conjectures that are consistent both with the data reported here and with ideas about scientific competition that have evolved and become accepted within the sociology of science.

Theorists seem to fall easily into four categories with respect to the $\Delta S = \Delta Q$ controversy. First, there were those who could be expected to be most committed to the current theory. Their inclination right from the start was that the experimental evidence was wrong and should probably be ignored. If Steven Weinberg's later recollections[18] are accurate, Gell-Mann held this view:

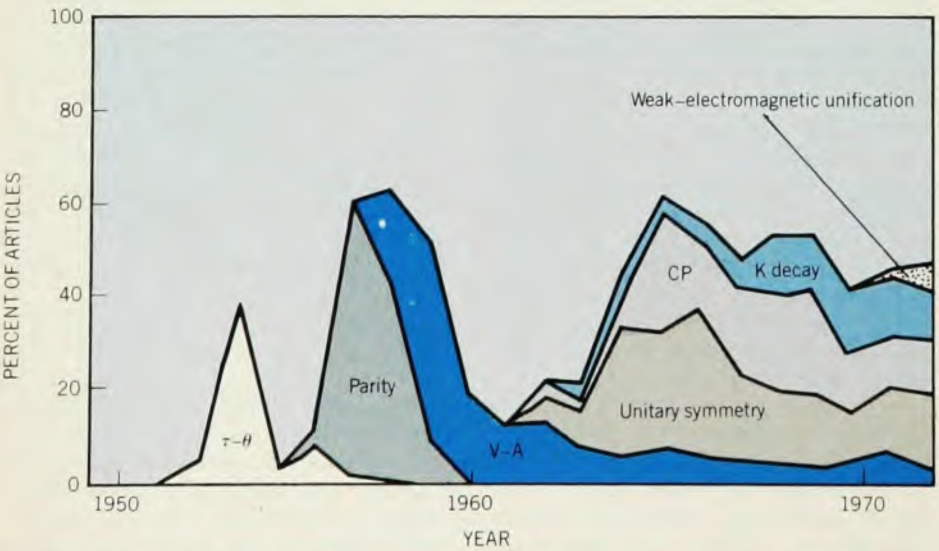
> At a high energy physics conference in 1962, data were reported to the effect that neutral K-mesons and their antiparticles can both decay into a positive pi-meson, an electron and a neutrino. If true this would have overturned a theory of weak interactions, the "current–current model," which had served as a basis of a great number of successes in other contexts. I remember Murray Gell-Mann rising and suggesting to the meeting that since the experiments did not agree with the theory, the experiments were probably wrong.

A second theoretical role might be called the "statesman." Several important theorists, not really a part of the action, took great pains as rapporteurs to point out to the community the gravity of the experimental claims regarding $\Delta S = \Delta Q$. Weisskopf, in his summative evaluation of the 1962 CERN conference, played this role, as did Okun at the same conference.

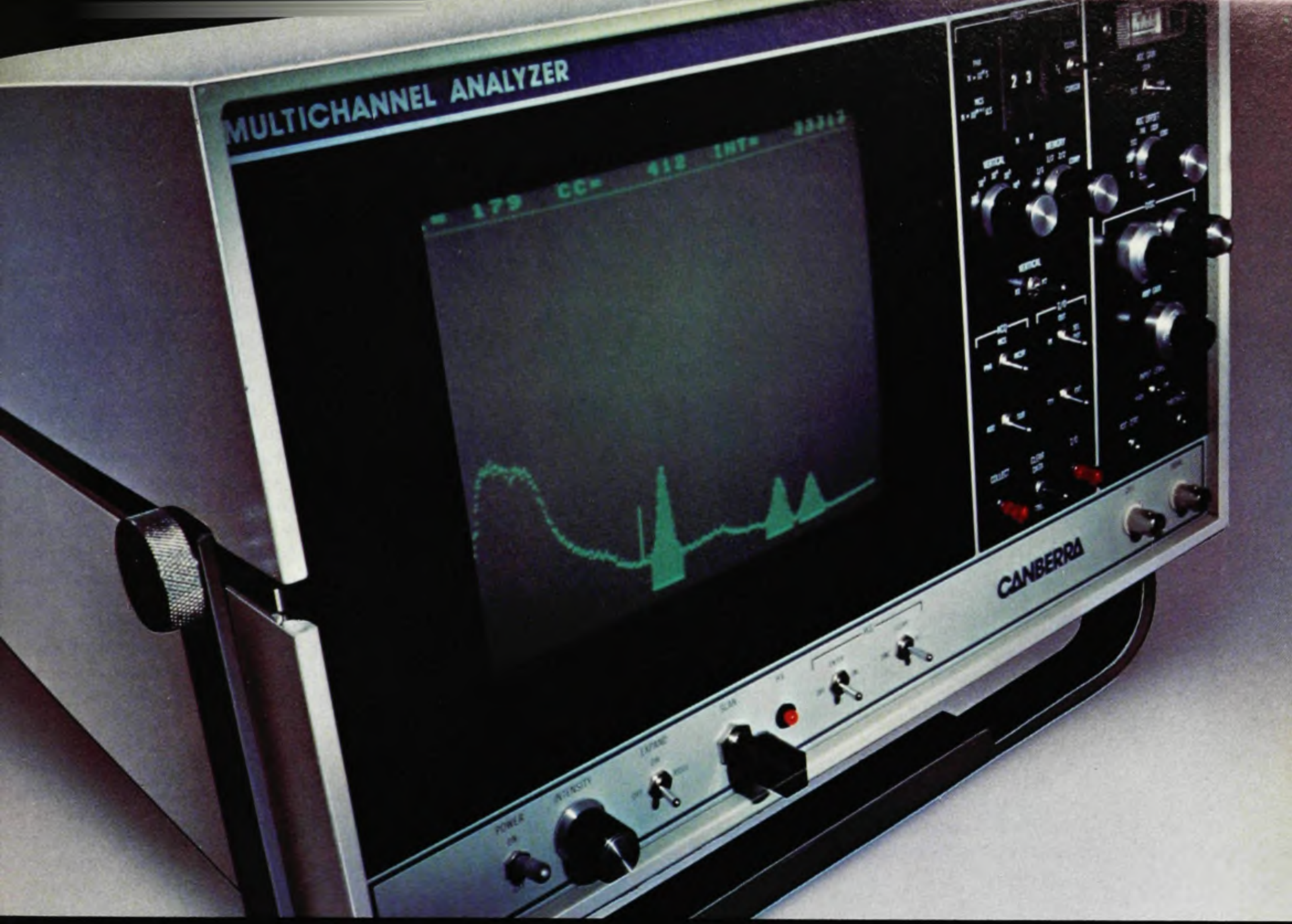
A third group are those who want to join the game. In this group were a large number of theorists who competed with one another in producing schemes that would alter or fix up the general theory so that a violation of $\Delta S = \Delta Q$ of a magnitude consistent with experiment would be allowed, in case the experiments were proved right after all. They also seemed to prefer that the current theory be valid, but in case it wasn't, they wanted to be in on the next theoretical movement, preferably leading the parade.

A final group is made up of some fraction of the remainder of the active theorists, who serve as an audience before whom those in the first three categories perform. This group is not just a residual category in disputes such as this, but is essential if the others are to play their roles. By granting or withholding its approval the audience conveys to the other participants in the drama an increased sense of the importance of their actions. At most times, of course, the audience is present only in the minds of the actors in scientific dramas, as researchers anticipate how the audience would react to this or that action. But in physics, perhaps more than in other fields, there are several conferences each year where important scientific results are communicated and where the audience is physically present and therefore able to heighten the drama.

We believe that the behavior of these groups of theorists during the controversy can be explained largely by knowing what each stood to gain or lose, in the eyes of each other and of the audience, depending



Fraction of theoretical articles on weak interactions devoted to various topics as a function of time. The $\Delta S = \Delta Q$ controversy, represented by 37 theoretical articles (and 33 experimental ones) occupies too small an area to show up on this graph. Figure 4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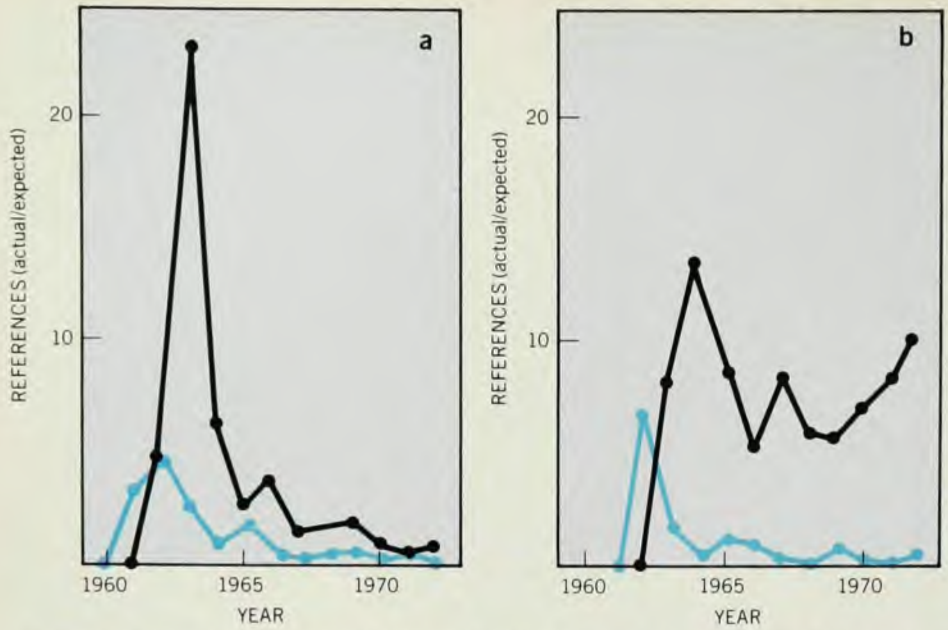

Citations of theoretical and experimental articles on the $\Delta S = \Delta Q$ selection rule: (**a**) references from theoretical to theoretical (color) and experimental (black) papers, and (**b**) references from experimental to theoretical (color) and experimental (black) papers. In both graphs we plot the ratio of the actual number of citations to the expected number. Figure 5

on the outcome. The leading developers and elaborators of the *V–A* theory are clearly best off if the experimental results prove wrong, and they gain in the eyes of their peers if they can claim to have known it all along. The statesmen have recognition by just taking a ceremonial role, and the visibility can make them influential with the group as a whole. This function appears to have become associated with a strong tradition of dispassionate comment.

Those who wish to join the game, the third group mentioned above, are caught in an interesting dilemma. If the experimentalists are wrong, the time spent proposing "fix-ups" for significant new theoretical departures is clearly wasted. But, if they turn out to be right, their reputations can be made or enhanced by proposing the most suitable new direction. They have to avoid looking like rats leaving the sinking ship because such opportunism lacks style, but they must advocate new possibilities. Their behavior illustrates an interesting kind of ambivalence[19] that may occur frequently in fields with solidly formulated theory.

The technique of co-citation analysis

Co-citation analysis is a technique for producing a graphic summary of the dominant intellectual foci in a scientific field in a particular year. The computer plots shown in figure 2 were produced by the following process. We identified the serial articles, whether on weak interactions or not, that were cited five or more times by articles published in a given year, for each year during 1962–64, and constructed the matrices of all co-citations among the articles for each year. Two articles are "co-cited" when they are both cited in the same paper; the matrices contain the number of times each pair of highly cited documents was cited together in that year. Next, we inspected the cells of each co-citation matrix to identify those highly cited articles which were co-cited with at least one other highly cited article seven or more times for 1962 and ten or more times for 1963–64. We eliminated the articles falling below these somewhat arbitrary thresholds from the matrix. (The change in thresholds was intended to account, in a preliminary way, for the dramatic increase in the number of papers after 1962.)

One way to think of a co-citation matrix is as a matrix of similarity coefficients. The more often two articles are cited together, the higher the probability that they are seen by researchers in the field as *similar* to each other in intellectual focus. Such similarity matrices can be analyzed by a technique known as multidimensional scaling, an iterative procedure that can produce a plot of the set of highly cited papers in two dimensions such that the highly co-cited papers are placed near to each other and the little co-cited papers are placed farther apart. The relative distances between papers in the plane should then be an indication of the degree of their intellectual relatedness. To produce diagrams of the kind shown in figure 2, each paper is treated as if it were a Gaussian hill whose height is the number of times it was cited in that year (its visibility) and whose width is arbitrarily set at a value convenient for producing attractive pictures. The Gaussians are added together and the resulting plot of hills and valleys gives a sense of how much activity is going on in different parts of the "subject space."

Notice that throughout all of this the experimenters play a peripheral role. That is because theorists in this field largely, though obviously not completely, run their own enterprise. While it touches from time to time on the results of experiments, the theoretical program has a coherence and thrust of its own. This widespread attitude of theorists toward experiment leads experimentalists to take some, but always publicly understated, pleasure in deflating the edifices constructed by the theorists. To discover a violation of the $\Delta S = \Delta Q$ rule, thereby bringing down one of the major successful theories in the history of particle physics, would be a joy few experimenters could pass up. To avoid losing face all around, however, it is important that if such a challenge is to be made, the data and analysis had better be solid. Here we have another example of institutionalized ambivalence in science where strong norms of one kind (an important impact you can make is to bring down an established theory, so you should try to do that) are counterbalanced by other strong norms (but, you have to be right because to be so visibly wrong causes a loss of community respect) and keeps the system from oscillating too widely.

These two conflicting motives tugged experimenters back and forth for several years with respect to the $\Delta S = \Delta Q$ question. Within a year after several groups announced that a violation had been seen, several other groups with different apparatus and more data reported no violation, thus seriously undermining the case made in the earlier, reports. It was this state of affairs that had prompted Salam to call the whole thing an "epic confusion." We do not mean to underestimate the excitement of a supportive discovery, but we might say that, next to bringing down an accepted theory, experimenters most enjoy showing up other experimenters who have made invalid claims to have data in disagreement with major theories. Experimenters seem largely to want to display the facts of nature a way that changes the point of view of the entire community.

After the excitement over a possible dramatic violation was over, the experimental community recognized that the experimental limit on the validity of the selection rule was a fundamental number that should be measured as well as possible. This widely held view caused experimental groups to return to the $\Delta S = \Delta Q$ rule over and over again in succeeding years for one more check as new technology made more and more accurate tests possible. That view explains, we believe, the continued high ratio of actual to expected references to experimental $\Delta S = \Delta Q$ papers that is seen in figure 5b.

Some might argue that we have taken liberal poetic license in our description of the motives of theorists and experimenters in particle physics. Our description is impressionistic and oversimplified, but we feel, as do other sociologists of science,[20] that the reward system in a scientific field is the place to look for keys to understanding the behavior of scientists. We hope also that our analysis has illuminated some interesting features of the $\Delta S = \Delta Q$ controversy.

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