Golden Events and Statistics: What's Wrong with Galison's Image/Logic Distinction?

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Peter Galison has recently claimed that twentieth-century microphysics has been pursued by two distinct experimental traditions—the image tradition and the logic tradition—that have only recently merged into a hybrid tradition. According to Galison, the two traditions employ fundamentally different forms of experimental argument, with the logic tradition using statistical arguments, while the image tradition strives for non-statistical demonstrations based on compelling ("golden") single events. I show that discoveries in both traditions have employed the same statistical form of argument, even when basing discovery claims on single, golden events. Where Galison sees an epistemic divide between two communities that can only be bridged by a creole- or pidgin-like "interlanguage," there is in fact a shared commitment to a statistical form of experimental argument.

1. Introduction

In his remarkable book *Image and Logic*, Peter Galison distinguishes two different traditions in the history of experimental particle physics—an "image" tradition, and a "logic" tradition. The image tradition is centered on a class of detectors that produce visual images of particle processes—detectors such as cloud chambers, photographic emulsions, and bubble chambers. The logic tradition utilizes electronic detectors that yield much less complete reconstructions of individual particle events, such as Geiger-

Thanks to Blas Cabrera of Stanford University, and to Joe Incandela and Mike Crisler of Fermi National Accelerator Laboratory, for their perspectives on the search for magnetic monopoles. Thanks also to Peter Galison, Allan Franklin, Ron Endicott, and an anonymous referee for their helpful comments and criticisms. Portions of this paper were presented at an "Author Meets Critics" session on *Image and Logic*, organized by Davis Baird and Alfred Nordmann, at the meeting of the Society for the Social Studies of Science, in Halifax, Nova Scotia, in October 1998.

Perspectives on Science 1999, vol. 7, no. 2

Müller counters, spark chambers, and drift chambers. Each tradition is marked, according to Galison, by pedagogical, technical, and "demonstrative or epistemic" continuity. This last is described by Galison as continuity "within each tradition in the characteristic form of argumentation" that is produced by and regarded as convincing by its members. Just as there is demonstrative continuity within each of the two traditions, Galison claims, there is a discontinuity between the two traditions. This difference in argumentative forms is, in fact, crucial to distinguishing the two traditions.

I wish to scrutinize these distinctions. The two traditions that he identifies are indeed distinct, but not in every respect that Galison claims. Specifically, while there is certainly a kind of "epistemic" continuity within each tradition, there is also, I claim, an epistemic continuity *between* the two traditions. Galison distinguishes the epistemic focus of the two traditions by contrasting the use of arguments based on images in the image tradition with the use of statistical arguments in the logic tradition. My claim is that the difference between the kinds of arguments employed within the two traditions, while real, is neither as great nor as fundamental as Galison's treatment may suggest. In particular, I wish to show that the arguments used for purposes of making discovery claims in the image tradition are, at root, no less statistical in nature than the kinds of arguments one finds in the logic tradition.

In making this argument, I have a particular concern with a development in particle physics instrumentation that Galison himself discusses—the union of these two traditions that came about with the development of a new class of "hybrid" detectors operating, for the most part, on the same physical principles as logic tradition detectors, but yielding such detailed information about individual particle events as to enable physicists to construct "pictures" of the events recorded.

By comparing experimental arguments from the logic and image traditions, I will show that the two experimental traditions were using the same reasoning all along. While discovery claims emerging from both traditions were based on a shared form of reasoning, each tradition faced distinct obstacles when attempting to produce usable data and verify that their experimental arrangements were adequate for the empirical claims being investigated. Each tradition likewise brought different kinds of strengths to their shared epistemic enterprise. The hybridization of the two traditions amounted to forging a combination of the two sets of strengths in order to transcend more effectively the obstacles. The underlying unity of the arguments being produced greatly facilitated the development of the hybrid detectors, and the experimental community that developed around them. The ability to produce such arguments constituted an epistemic goal that each tradition strove to attain. In the process, each tradition aimed to incorporate the strengths of the other, ultimately succeeding only by producing machines that could not be neatly classified into either tradition.

2. The Two Traditions

Cloud chambers and bubble chambers figure as canonical instruments of the image tradition. In a cloud chamber, particle tracks consisting of droplets condense out of a gas when an abrupt increase in the gas volume lowers the temperature of the gas, resulting in supersaturation. Once these tracks are formed, a photograph is taken, which can later be analyzed. Cloud chambers were used widely in the investigation of cosmic rays, and it was in this context that Carl Anderson discovered the positron (see section 6, below). The bubble chamber was developed by Donald Glaser, who used cloud chambers while studying with Anderson (Galison 1997*a*, pp. 320-36[1]). In a bubble chamber, an energetic charged particle passes through a superheated liquid, initiating boiling along its path. The result is the formation of a line of bubbles that records the trajectory of the particle through the liquid. Achieving the superheated condition required for sensitivity to passing particles requires that the pressure of the liquid be quickly lowered, a step that must be synchronized with a pulse of particles from a particle accelerator. The tracks produced during the period of sensitivity are then recorded by means of stereoscopic photography. These photographs in turn must be analyzed (*IL*, pp. 370-431).

Galison stresses the ability of both cloud and bubble chambers to produce "detailed photographic tracks in which one can see the interaction images" (*IL*, p. 425). One element missing from Galison's discussion is an analysis of the concept of an image. I would like to propose an analysis that is incomplete, but might help to make the notion of an image more precise.[2] An image, I want to suggest, is a representation of a physical process that is *visually isomorphic* to that process itself.

It may be best to begin by explaining what the concept of a "visually isomorphic representation" is *not*. It is not "a photograph of what happened" or "what you would see if the process were directly observable." A bubble chamber photograph, for example, greatly exaggerates the width of a particle's path, an exaggeration that is necessary for making the tracks visible (a similar exaggeration can be found in road maps). Furthermore,

1. For brevity, subsequent references to Galison 1997*a* will be indicated by "*IL*".

2. The analysis that I present here is not intended to be anything like an analysis of what it is for something to be an "image" or "picture" in general. My intent is to capture what is distinctive about the kinds of data that have been produced by the image tradition.

the actual particle is at only one location along its path at any given time, whereas the bubble chamber photograph presents a static image of the entire path simultaneously. If one could "directly observe" pions, one would still not see what one sees in a bubble chamber photograph of a pion moving through the chamber (it is not even clear that one can make sense of a counterfactual such as "what one would see if one could see pions"). Likewise, if one could see through the buildings and streets of Boston, one would not see what one sees when looking at the map of the Boston subway system that is posted inside the trains. In fact, the analogy to maps can be extended. Like a map, a bubble chamber photograph (like other "images" used in particle physics) is, if read "literally," a misrepresentation of a state of affairs. Its usefulness requires that it be so. One who knows in what ways the representation is accurate, and the nature of the distortions introduced in producing that representation, can however read it to gain accurate information about specific aspects of the thing represented (on cartographic "misrepresentation" see, e.g., Monmonier 1991).

The crucial aspects of a visually isomorphic representation, at least for the purposes of characterizing the image tradition in microphysics, seem to be these: (1) *spatial fidelity*—successive positions of the particles involved are represented in the correct spatial relationship to each other (actual distances between locations at successive times, and distances between points in the image are, ideally, related to one another by some linear function, or, if a distortion is introduced, it must be a known distortion, the effects of which can be taken into account, to a satisfactorily high degree of approximation); (2) *high resolution*—one can determine, on the basis of the representation, the relative position of the particle at any given moment (within the period during which the particle is in the "active" part of the detector), to a very high degree of accuracy; (3) *completeness*—the portion of the path of the particle that is represented is typically long enough for physically relevant information to be gathered from it.[3]

Galison's characterization of the image tradition does not rest only on its use of images as data, however. Also prominent in Galison's discussion

3. Readers familiar with the debate over mental imagery may notice some similarities between my analysis of visually isomorphic representations and Stephen Kosslyn's account of "quasi-pictorial representations" or "functional pictures" (Kosslyn 1980, esp. pp. 31–35; see also Tye 1991, esp. pp. 33–41). There are, however, important and fundamental differences between Kosslyn's quasi-pictures and what I am calling visually isomorphic representations. What Kosslyn calls "abstract spatial isomorphism" is similar to, but not identical to, my "spatial fidelity." The two properties are different not least because Kosslyn's quasi-pictures are in a medium that is not literally spatial, but only "functionally equivalent to a (perhaps Euclidean) coordinate space" (Kosslyn 1980, p. 33). Furthermore, Kosslyn's "abstract surface-property isomorphism" is not a requisite for a visually isomor-

of the image tradition is the value placed on "golden events." A golden event yields an image so compelling that it is capable, all by itself, of producing conviction as to the existence of a new phenomenon. Galison writes, "The golden event was the exemplar of the image tradition: an individual instance so complete, so well defined, so 'manifestly' free of distortion and background that no further data had to be invoked" (*IL*, p. 23). Hence it seems also to be characteristic of the image tradition that it could produce arguments based on single events.

The logic tradition, as Galison tells the story, begins with the counter. An instrument such as the Geiger-Müller counter does not yield a visually isomorphic representation of a particle process, but (simplifying somewhat) a yes or no answer to the question: "did something pass through here?" Early experiments in the logic tradition relied on sometimes ingenious arrangements of counters to single out a particular kind of phenomenon to be studied.

Take a simple example: Thomas Johnson investigated cosmic rays using three Geiger-Müller tubes wired in coincidence. This arrangement allowed him to restrict his attention to occasions when all three tubes fired, thus singling out cosmic rays originating from a particular region of the sky. This allowed him to establish an east-west asymmetry in the detection rate inconsistent with the hypothesis that cosmic rays consist of photons (*IL,* pp. 444-45; Johnson 1933*a*, 1933*b*).

Galison refers to the use of this kind of coincidence counting in the logic tradition as "irreducibly statistical" (*IL*, pp. 453). An array of tubes situated linearly and wired in coincidence might fire simultaneously as a result of different particles coming from a single shower passing through each tube simultaneously, thus mimicking a single particle. A coincidence could also be produced by a spurious simultaneous discharge of the counters involved. Such coincidences from sources other than the target phenomenon constitute the "background," and any claim about a particular phenomenon had to invoke a statistical argument that the counts recorded exceeded the expectation from background alone. Galison comments,

> Any argument for the penetration of a single particle . . . or for a shower . . . had to be of the form: one hypothesis is more probable than another. Consequently, a single event . . . was meaningless in

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phic representation, as it refers to the ability of a quasi-picture to represent surface properties of objects such as texture and color. This is a feature strikingly (and inherently!) absent from image data in particle physics. There may well remain aspects to the image/logic question in particle physics that would benefit from being informed by work being done in cognitive psychology.

itself. Data in the logic tradition became persuasive only in their statistical aggregation (*IL*, p. 453).

Regarding the logic tradition in general, Galison writes that it "relied fundamentally on statistical demonstration. . . . Any single coincident firing of the two counters meant nothing" (*IL*, p. 23). Thus, a crucial part of the distinction between the logic and image traditions, as Galison draws it, seems to be the image tradition's deployment of arguments based on *single* "golden" events, as opposed to the logic tradition's appeals to *large numbers* of events beyond background predictions.

3. The Argument

Galison bases the epistemic distinction between the image and logic traditions on a difference between their characteristic arguments. What is it, though, that distinguishes these argumentative forms? I wish to consider three possible grounds for drawing the distinction:

(A) The means of representing data:

> (i) The image tradition relies on data consisting of "images" or visually isomorphic representations of the processes studied by the experiment.
>
> (ii) The logic tradition relies on data consisting of abstract (numerical or otherwise) representations not visually isomorphic to the processes studied by the experiment.

(B) The employment of quantitative statistical calculations:

> (i) The image tradition is able to produce convincing arguments without employing quantitative statistical calculations.
>
> (ii) The logic tradition is essentially dependent on quantitative statistics in order to produce convincing experimental arguments.

(C) The number of recorded events needed for experimental demonstrations:

> (i) The image tradition is able on occasion to produce (and strives to produce) a representation of a single interaction that is the basis for a convincing experimental argument for a claim about what happened in that interaction.
>
> (ii) The logic tradition requires data from a multitude of interactions in order to produce a convincing experimental argument.

I should state at the outset that only the third of these differences seems to be the one which Galison himself takes to be crucial to the epistemic divide between the two traditions. It is clear from his text that (A) and (B) do not mark the epistemic difference he has in mind. I include them, however, for the sake of completeness and in order to show just how a shared form of argumentation is compatible with a number of differences between the image and logic traditions.

I will argue that none of these aspects of the image/logic distinction supports Galison's claim that the logic tradition's dependence on statistics is in some sense more "fundamental" than that of the image tradition. Rather, both traditions have produced discoveries employing a characteristically statistical form of argumentation. That form of argumentation consists of what is sometimes called a "counting experiment."

In a counting experiment, events are recorded with certain characteristics *A* that are considered to be indicative of some new phenomenon *H* ("new" either in the sense of being predicted on the basis of existing theory but not yet experimentally detected, or in the sense of being previously unknown and entirely unexpected). Events with characteristics *A* might also be produced by other "background" sources, however, and any discovery claim has to rest on an argument that the observed events significantly exceed what could be expected on the basis of background alone. Thus, arguments based on counting experiments take the following (greatly simplified) form:

1. Based on an estimate of the probability distribution for background (non-*H*) sources, the number of background events in a sample of this size is expected to be *B*.

2. The number of events in the sample with characteristics *A* (candidate events) is *M*.

3. *M* is significantly greater than *B*.

4. Events with characteristics *A* are not expected from sources other than *H* and those background processes estimated in premise 1.

Therefore:

5. The probable source of excess events with characteristics *A* is *H*.[4]

Naturally, not all of the elements in this argument will be made explicit in every case. Hence, such arguments might not always wear their statistical nature on their sleeves. Nevertheless, I will call arguments having this

4. For a more detailed discussion of counting experiments see Staley (1997, esp. ch. 4).

form "statistical" because, whatever the details, such an argument points to a "statistical excess" in the data over the background expectation. This excess provides the basis for judging it is unlikely that the observed events were produced by background processes. However they are labeled, such arguments are employed in both the logic and image traditions, and mark a unity between the two traditions at precisely the point where Galison sees an epistemic disunity.

In the following sections I will support these claims with historical examples meant to show that none of the above differences (A, B, and C) can serve as the basis of the epistemic distinction that Galison draws. Section four will show that the use of different forms of data (difference A) does not result in different kinds of arguments. Section five will show that the use or non-use of quantitative statistical assessments (difference B) does not constitute a difference in the kinds of argument employed. Sections six and seven will show that golden event discoveries are not non-statistical (difference C).

4. Statistical Arguments Using Images (Difference A)

Galison himself does not intend to base the epistemic distinction between the image and logic traditions on the differences between their data. *Image and Logic* contains many examples of clearly statistical arguments based on images.

An excellent example of an explicitly statistical argument based on image data can be found by examining the arguments given in the discovery of the last of the "pseudoscalar mesons," the η, by means of Luis Alvarez's 72-inch bubble chamber at the Lawrence Radiation Laboratory, in 1961. A group from Johns Hopkins University, headed by Aihud Pevsner, and a group from Northwestern University, led by M. Block, collaborated to study films from the bubble chamber.[5] By producing the reaction $\pi^+ + d \rightarrow p + p + X^0$ (where π^+ is the positive π-meson (pion), d represents a deuterium nucleus, p stands for a proton, and X^0 represents the missing meson), they hoped to observe the decay of the X^0 into the

5. Galison discusses this episode from a different perspective. The analysis of the data that led to the discovery of the η took place far from the bubble chamber itself, and the authors of this paper were not directly involved in operating the bubble chamber. Galison concludes that "it is possible to author a discovery principally through involvement in analysis. At the same time, the 'site' of experimentation diffuses, now that construction, manipulation, and analysis are no longer located in the same place" (*IL,* p. 415). Galison's observations on the flexibility and historicity of the concept "experiment," which he introduced in his earlier book, *How Experiments End* (1987), are extremely helpful and important for understanding the development of experimental science. The next important step is to go beyond the question "what is an experiment?" and ask "what are experiments for?" On this, see Staley (1997, chs. 3, 4).

three-pion system $\pi^+ + \pi^- + \pi^0$. Effectively, this led them to scan the bubble chamber films for examples of the following reaction: $\pi^+ + d \rightarrow \pi^+ + \pi^- + \pi^0$. To count as a candidate event, an event had to pass certain statistical tests. First, the χ^2 fit to the hypothesis $\pi + d \rightarrow p + p + \pi^+ + \pi^- + \pi^0$ could not exceed 6. Second, the χ^2 fit to the hypothesis $\pi^+ + d \rightarrow p + p + \pi^+ + \pi^-$ could be no less than 25. Finally, for events in which protons were difficult to differentiate from positive pions (i.e., where protons have momentum in excess of 700 MeV[6]), the χ^2 fit to the hypothesis $\pi^+ + d \rightarrow p + n + \pi^+ + \pi^+ + \pi^-$ was required to exceed 15. Here we see quantitative statistics applied to individual events.

For each of 233 events matching this description, the Hopkins-Northwestern group reconstructed the effective mass of each of the three-pion final states, and plotted the results in a histogram (see figure 1). Two peaks can be identified, one of which represents the known 770 MeV ω^0 meson. In addition, a smaller peak is found near 550 MeV, and it is this "resonance" that experimenters identified as the η. One thing to note is that the experimenters were not content merely to show a peak in the histogram. They also evaluated the background: "We have calculated the Lorentz-invariant phase space for the 3-pion mass from the background reaction . . . using the experimental average of the total energy in the p-3π center-of-mass system" (Pevsner et al. 1961, p. 422). The background curve is represented by x's in figure 1. The argument that follows is clearly statistical in the sense that I am employing that term:

> Clearly, because of the presence of the ω^0 particle at 770 MeV, such a normalization of phase space yields a gross overestimate of events expected near 550 MeV. Between 540 and 600 MeV there are 36 events in the experimental distribution, whereas the overestimated phase space [background] would account for 12 (Pevsner et al. 1961, p. 422).

Although no attempt is made to assess quantitatively the statistical significance of this excess over background, the argument is clearly meant to persuade on the basis of the improbability that background sources would produce such a peak in the effective mass histogram. (See below for more on statistical arguments in the absence of quantitative statistics.)

It may be useful to notice some distinctions that are apparent in this example. Different kinds of arguments are given in experimental reasoning

6. Strictly speaking, the units of momentum used here should be MeV/*c,* where *c* is the speed of light, and the units of mass should be MeV/c^2. High energy physicists adopt the useful convention of setting c=1, so that units of eV can be used for energy, momentum, *and* mass, where context dictates the implied denominator.

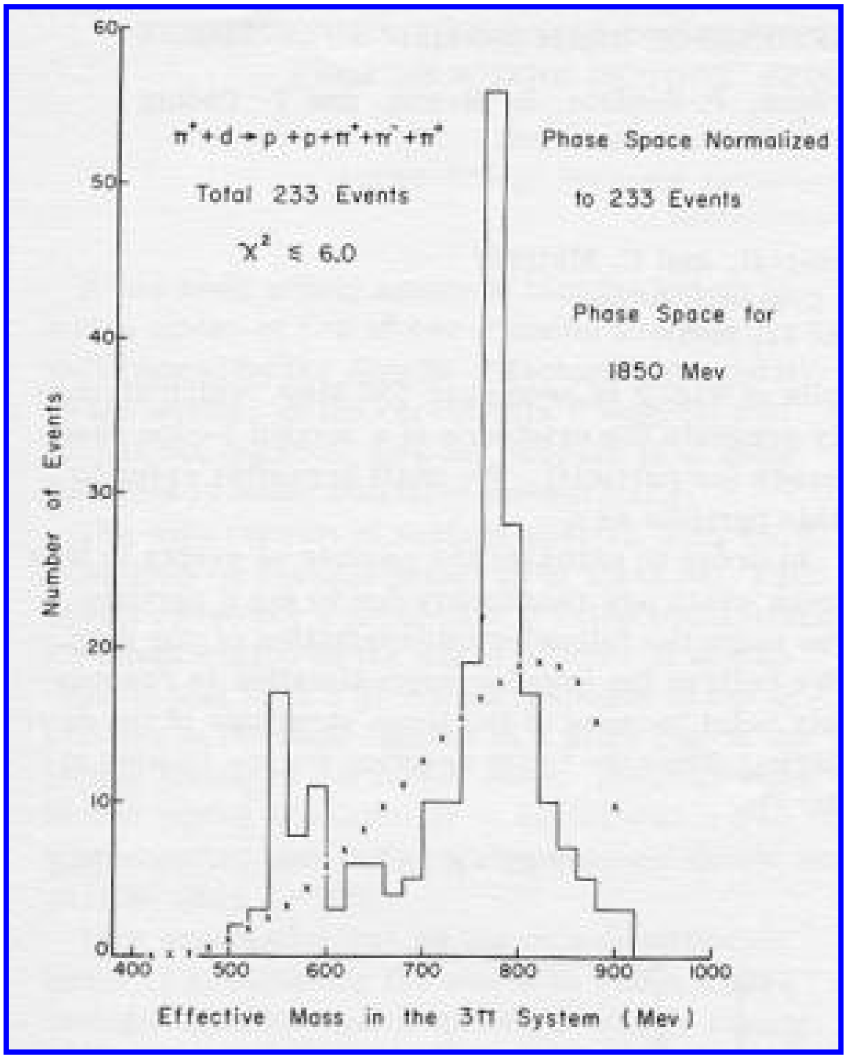

Figure 1. Identifying an effect against background. This histogram shows number of candidate events according to the effective mass of the three-pion system. The tall peak at 770 MeV is due to the ω^0. The smaller peak at 550 MeV represents a new particle identified as the η. The background curve is represented by the x's (Pevsner et al. 1961, p. 422).

for different purposes. In order to verify that their candidate events were indeed events in which the three-pion system $\pi^+ + \pi^- + \pi^0$ was produced, the experimenters used chi-squared measures of individual events. This is a separate (still statistical) argument from the counting-experiment argument used to establish the existence of the η, although the success of the counting-experiment argument depends on the success of this lower level argument. Still other kinds of arguments will be used to verify that the bubble chamber does give one the kind of information that one wants from it (such arguments *may* rest on theories—possibly false—of the physical process by which bubble chamber images are produced).

The unity that I claim we can find between the image and logic traditions is a unity in their use of statistical, counting-experiment arguments for the purpose of establishing the existence of new phenomena. Such a unity is compatible with the undeniable fact that at other levels of experimental argument the two traditions use arguments of a rather different character. But it is easy to see *why* these arguments tend to be different once it is understood what they are about. A counting-experiment argument can always be called into doubt by raising questions about "lower level" considerations. Is the experiment being performed adequate for addressing the question at hand? Is the set of characteristics being used to define a "candidate event" a sensible choice? Can we use the instrument at hand to perform the relevant measurements with the necessary accuracy and precision? Such questions have to be addressed, often on the basis of the experimental data themselves.[7] The image and logic traditions produced different kinds of arguments to settle these kinds of questions, but this should come as no surprise. These are largely questions about the experimental devices themselves, and how they were used. The relevant lower-level arguments in the two traditions are different because they are *about* different kinds of things.

There is no question that bubble chambers produced images, and yet these images could be used for statistical arguments, which Galison takes to be central to the logic tradition. Images were used for statistical

7. One useful way of thinking about experimental reasoning is to think in terms of a "hierarchy of models" that represent different aspects of the experimental situation (see Suppes 1962; Mayo 1996; I apply such an approach in analyzing a recent particle physics discovery in Staley 1997). One can then distinguish between the model of the theory being tested, the model of the data, the model of the experiment, and considerations of experimental design and ceteris paribus conditions. The differences between the image and logic traditions lie largely at the level of experimental design considerations. Another possibility is to think of this as a matter of addressing questions about the "background theory" as opposed to the "interpretation of data," as that distinction is drawn in Hon (1989).

arguments in the discovery of the η, as well as in numerous other discoveries, particularly during the bubble chamber era of particle physics. One finds similar patterns of reasoning in, for example, the discovery of the first "strange" resonance by Alvarez's own group (Alston et al. 1960), the discovery of the first meson resonance (the $K^*(890)$), found in the very same data (Alston et al. 1961), the discovery of the ρ at the Brookhaven Cosmotron (Erwin et al. 1961), the discovery of the ω, again involving Alvarez himself (Maglic et al. 1961), the discovery of the ϕ meson (Connolly et al. 1963)[8], and the discovery of the Ξ^* resonance using data from the LRL 72-inch chamber (Pjerrou et al. 1962). Indeed, if one takes seriously Galison's claim that statistical argumentation is characteristic of the logic tradition rather than the image tradition, then the prevalence of statistical argumentation based on bubble chamber images might lead one to question whether the bubble chamber should be classified as an image tradition instrument at all. And yet, if the *bubble chamber* is not to be considered an image instrument, and *Luis Alvarez* must be considered to have spent a substantial part of his career doing logic tradition physics, then how useful could this classification be?

However that may be, I do not want to suggest that Galison himself claims that arguments based on images cannot be statistical. He quite explicitly notes that they can, and describes some disputes within the bubble chamber community as conflicts between those wishing to use bubble chamber images for logic-tradition purposes and those wishing to use them for the kinds of arguments Galison takes to be central to the image tradition (he places Alvarez firmly in the latter category) (*IL*, pp. 400–401).

My primary aim in this section, then, is simply to make explicit a point that can already be found in Galison's discussion. Whatever marks the distinction between statistical and non-statistical arguments, it cannot simply be the form of the data. I do wish to add, however, that the prevalence of statistical arguments based on bubble chamber data should

8. Lest one think that bubble chamber physicists based their discovery claims only on *qualitative* statistical assessments, it should be pointed out that Connolly et al.'s argument for the existence and properties of the ϕ meson is based on such quantitative statistical statements as "the $C = -1$ is in excellent agreement with the data, while the $C = +1$ hypothesis is in disagreement by 12 standard deviations" (Connolly et al. 1963, p. 373), and "For events satisfying these criteria, their 3π effective mass spectrum . . . is examined for evidence of a peak at the ϕ mass. There is a deviation at $M(3\pi) = 1020$ MeV of about 1.5 standard deviations above background and of width consistent with the experimental resolution at this mass" (Connolly et al. 1963, p. 375). Image physicists used both quantitative and qualitative statistical arguments.

already provide grounds for wondering whether the lines are being drawn correctly.

5. Statistical Arguments in the Absence of Quantitative Statistics (B)

It cannot be denied that, in many of the experimental demonstrations that emerged from the image tradition, there was a noticeable lack of quantitative statistical analysis. Such analysis, however, plays an important role in many of the arguments produced by the logic tradition. While, by Galison's own admission, statistics did play several important roles in the image tradition, the ideal towards which image tradition physicists strove, as Galison tells the story, was the production of images so compelling that no quantitative analysis of the error characteristics of the experiment was needed. "While statistics could certainly be used within the image tradition, it was by no means necessary for most applications" (*IL*, p. 451). By contrast, experiments in particle physics involving the use of Geiger-Müller counters, for example, "were inherently and inalienably statistical. Estimation of probable errors and the statistical excess over background is not a side issue in these detectors—it is central to the possibility of any demonstration at all" (*IL*, p. 451). Galison goes on to describe the extended discussion of statistical methods in textbooks on counter devices, both for the testing of devices and the analysis of data (*IL*, p. 451).

The degree of emphasis and reliance on quantitative statistical analysis apparently does mark a distinction between the practices of the logic and image traditions. Does it, however, mark a distinction between the forms of argument employed in the two traditions? Are arguments that employ no quantitative statistics non-statistical? I wish to suggest that an argument in which no formal statistical analysis is employed may nevertheless be a statistical argument, and that such arguments are to be found in both the logic and image traditions.[9]

An excellent example of such an argument can be found in one of the great successes of the logic tradition in particle physics—the discovery of the antiproton. The experiment that led to this discovery claim was conducted by Owen Chamberlain, Emilio Segrè, Clyde Wiegand, and Thomas Ypsilantis at the Bevatron accelerator at Berkeley in 1955. Establishing the existence of the antiproton required that one establish the existence of a particle with mass equal to that of the proton, but with negative charge. The Berkeley group did this using particles created by the impact of

9. Here again it is useful to recall the differences between kinds of experimental argument. Quantitative statistical methods may have been more useful for verifying the proper functioning of counter detectors than, say, photographic emulsions. This is, however, compatible with the use of both kinds of detectors for producing statistical arguments for discovery claims.

Bevatron-accelerated protons on a target. The products of these collisions were then guided through a series of detectors (see figure 2). This detector array included deflecting magnets (M1, M2), focusing quadrupole magnets (Q1, Q2), scintillation counters (S1, S2, S3), and Cerenkov counters (C1, C2). Particles arriving at detector S2 were known to be negatively charged and to have "the same momentum within 2 percent" (Chamberlain et al. 1955, p. 947). To determine whether any of these particles had a mass equal to that of the proton required the measurement of the velocity of the particles. From the momentum and the velocity, the mass could easily be deduced. The experimenters could not expect to see very many antiprotons in the midst of a "heavy background" of pions, so three different methods were used to make this measurement.

The experimenters used one Cerenkov detector, C1, that detected all charged particles with a velocity $\beta > 0.79$ ($\beta = v/c$), while C2 counted only those particles with a velocity in the interval $0.75 < \beta < 0.78$. Hence, requiring antiproton candidate particles to register in S1, S2, S3, and C2, but not C1, was expected to rule out most of the mesons in the beam, which at that momentum would have a much greater velocity than the more massive antiprotons. Sixty particles passed these cuts.

A check on this selection was carried out by measuring the "time of flight" between two scintillation counters, reading out the pulse from these detectors into an oscilloscope, and then measuring the separation between the pulses from the two detectors, S1 and S2. The experimenters created histograms of the flight times of antiproton candidates and compared these to histograms showing the flight times of mesons used in calibration and the apparent flight times produced by "accidental coincidences." These histograms are displayed (figure 3), and the differences between them are apparent. The authors comment, "It will be noticed that the accidental coincidences do not show the close grouping of flight times characteristic of the antiproton or meson flight times" (Chamberlain et al. 1955, p. 949). No statistical analysis of the difference between antiproton candidates and "accidental coincidences" or between antiproton candidates and mesons is provided.

In another technique of confirming the interpretation of these events as antiprotons, the experimenters altered the magnetic field values of the magnets, while leaving the velocity selection unchanged. Varying the magnetic field amounted to varying the momenta of the particles in the beam. This arrangement in effect gave them a detector for negatively charged particles with an adjustable mass setting. If they really were looking at antiprotons, then upon changing the mass setting, they reasoned, they should see the numbers of events drop sharply as they tuned the device away from the proton mass. If, on the other hand, they were

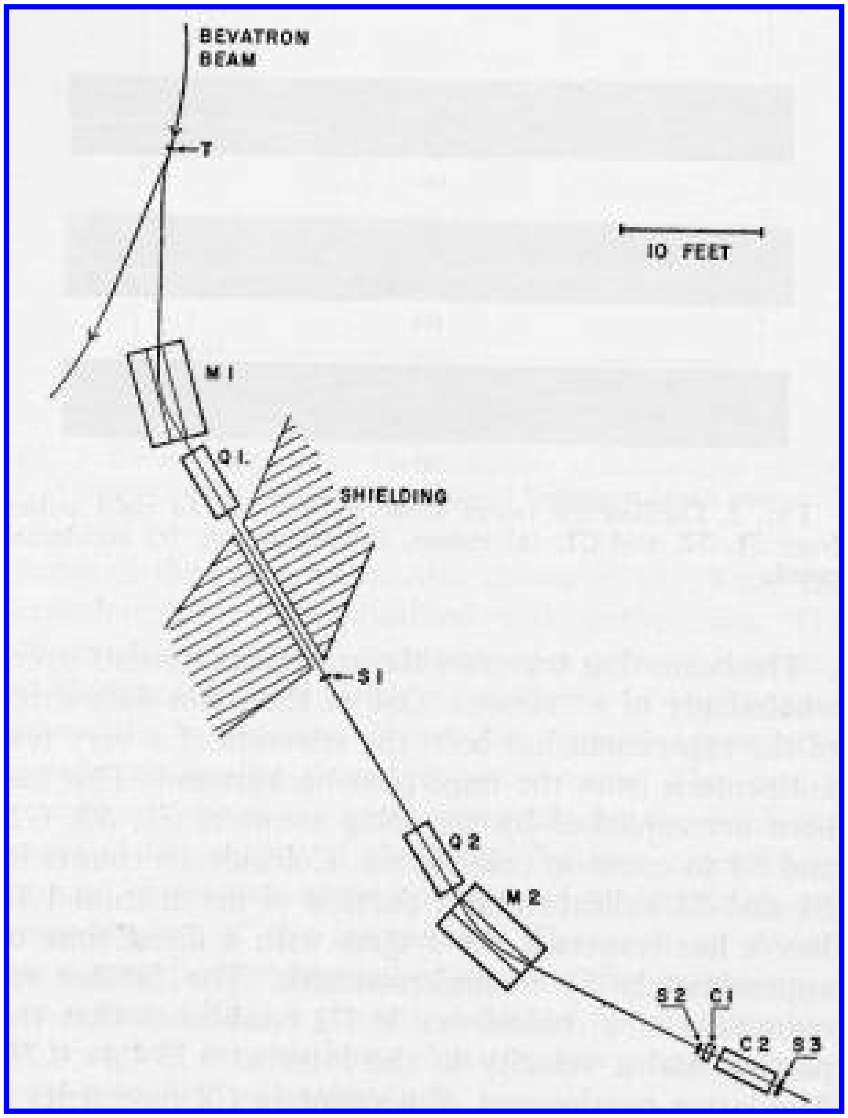

Figure 2. Apparatus used by Chamberlain, Segrè, Wiegand, and Ypsilantis to find the antiproton. M1 and M2 are deflecting magnets. Q1 and Q2 are focusing (quadrupole) magnets. S1, S2, and S3 are scintillation counters. C1 and C2 are Cerenkov counters (Chamberlain et al. 1955, p. 947).

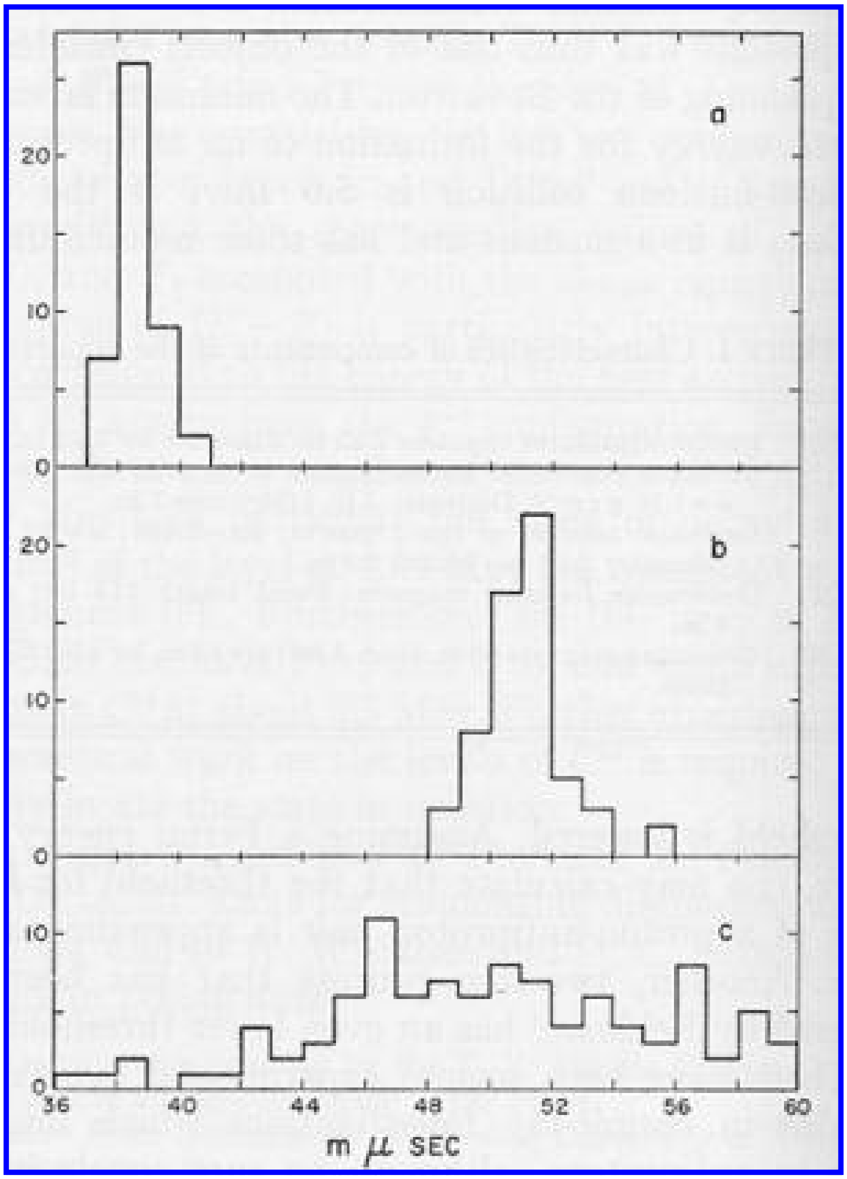

Figure 3. Comparison of flight time histograms: (a) meson flight times used for calibration (peak should be located at 40 x 10^{-9}s); (b) antiproton flight times; (c) apparent flight times for sample of accidental coincidences. The ordinates show the number of events in each 10^{-9} sec interval (Chamberlain et al. 1955, p. 948, corrected according to Cahn and Goldhaber 1989, p. 95).

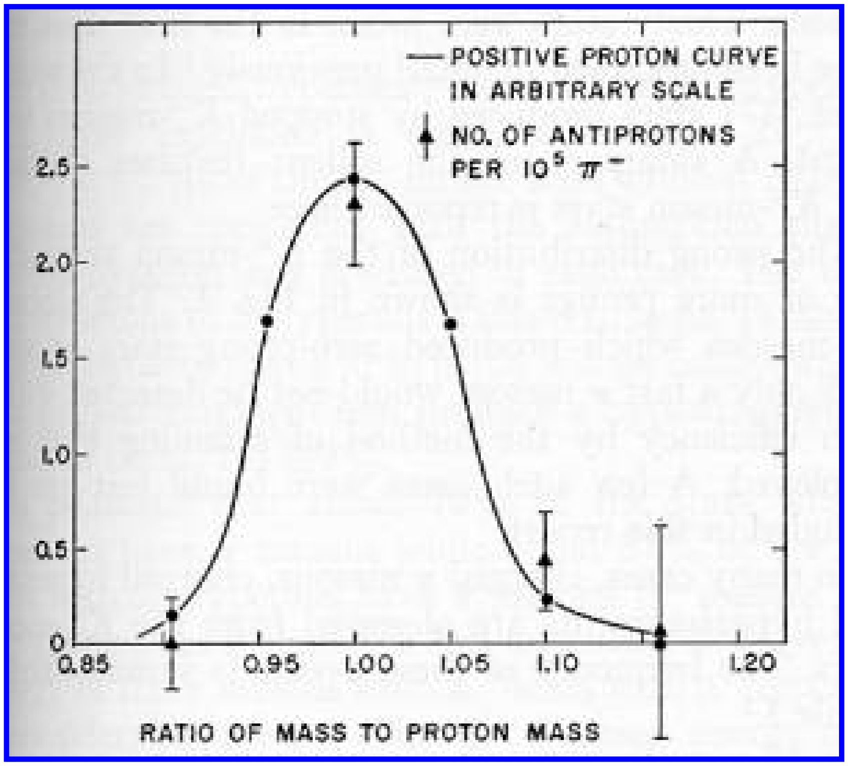

Figure 4. The number of antiproton candidate events obtained as the "mass setting" of the apparatus (figure 2) is varied (triangular data points). Note how the numbers of candidate events fall off as the mass setting is tuned away from the mass of the proton. The solid curve represents the mass resolution of the apparatus as determined using protons (Chamberlain et al. 1955, p. 949).

looking at some sort of background process, then they would not see significant changes at adjacent mass settings. They plotted the results, and compared them with the mass resolution of the instrument as determined by measurements using ordinary positive protons (see figure 4). The authors comment that "[t]he observations show the existence of a peak of intensity at the proton mass, with no evidence of background when the instrument is set for masses appreciably greater or smaller than the proton mass. This test is considered one of the most important for the establishment of the reality of these observations, since background, if present, could be expected to appear at any setting of the instrument" (Chamberlain et al., 1955, p. 949). Again, no quantitative statistical analysis of this curve is provided.

In further illustration of this point, the authors consider "possible spurious effects" as the source of the data they obtained, including the possibility that they are looking at negative hydrogen ions rather than antiprotons. They rule this out as follows: "It is extremely improbable that such an ion should pass through all the counters without the stripping of its electrons" (Chamberlain et al. 1955, p. 950). There is no attempt to evaluate *how* improbable this is by means of statistical theory.

I do not claim that the Chamberlain group made no use of quantitative statistics at all. They would certainly have determined the probable error on individual measurements. But this is no less true in the image-tradition practice of measuring particle momenta on the basis of individual tracks in bubble chamber photographs. (In any case, this is a matter, not of the demonstrative argument itself, but of the "lower level" considerations mentioned above.) The important thing to note here is that the system of coincidences used in the experimental setup employed by Chamberlain, Segrè, and company is considered by its nature sufficient guarantee against certain kinds of error, *without working out the precise probabilities by statistical methods.* This willingness to forego quantitative statistics is seen in several different aspects of this experimental argument. Yet this argument is clearly quite characteristic of the logic tradition. Certainly this is how Galison sees it, going so far as to call it a "paradigmatic application of the new counters within the logic tradition." Galison comments that "[n]o single count meant anything; the existence argument was irreducibly statistical" (*IL*, p. 459).

In the logic tradition, many experimental discoveries employing "counting experiment" logic did produce an assessment of the strength of the evidence based on quantitative statistical analysis. The instruments and the data structures produced by the tradition lent themselves to the use of statistical analysis, even in those instances when a statistical analysis hardly seemed "necessary" because of the obviously great difference between the observed number of candidate events and the expected background. In the image tradition, on the other hand, background estimates were often exceedingly difficult to produce in any reliable way. However, it was often the case that one could show that the background for a particular experiment was negligibly small without using quantitative statistics, particularly in those cases in which a discovery claim was based on an image of a single event. I will now to turn to a case of that sort.

6. Statistical Arguments Using Single Events, Part 1: Statistical Reasoning from a Single Image (Difference C)

There remains one important candidate for distinguishing image-tradition arguments from logic-tradition arguments. In the image tradition, several important discoveries came about on the basis of a single, convincing particle event recorded on film—a "golden event." It might be claimed that arguments based on a single event are not statistical arguments, and it is this distinction that marks the epistemic discontinuity between the image and logic traditions.

This, ultimately, appears to be Galison's position. He stresses the importance of golden events in numerous places throughout his book, and seems to regard the fact that an argument is based on a multitude of events as the essential characteristic of a statistical argument. His comment on the antiproton discovery is telling: "No single count meant anything; the existence argument was irreducibly statistical" (*IL*, p. 459).

Furthermore, the image tradition's emphasis on golden events is the first point of epistemic continuity within that tradition that Galison mentions: "On the image side resides a deep-seated commitment to the production of the 'golden event': the single picture of such clarity and distinctness that it commands acceptance" (*IL*, p. 22). He calls the golden event "the exemplar of the image tradition," whereas "[i]n contrast, the logic tradition relied fundamentally on statistical demonstrations" (*IL*, p. 23). In the logic tradition, "[a]ny argument for the penetration of a single particle . . . had to be of the form: one hypothesis is more probable than another. . . . Data in the logic tradition became persuasive only in their statistical aggregation" (*IL*, p. 453). While the image tradition did often make use of statistics, "throughout the history of the image tradition, from Wilson's first golden event to the discovery of the omega minus, there stood an abiding faith in the power of the individual image" (*IL*, p. 453).

Here there can be no doubt: the "statistical demonstrations" of the logic tradition are "in contrast" to the golden events of the image tradition, according to Galison. The two distinct forms of demonstration mark continuities within their respective traditions, and a discontinuity between the two traditions.

To the contrary, I will argue that experimental reasoning based on images of single events is no less statistical in nature than that based on "hits" from an array of counters. The difference lies *only* in the numbers, the form of the data, and the specific background information required for the argument to be seen as convincing. The case can be made by looking at a famous golden event: the discovery of the positive electron (positron) by Carl Anderson in 1932.

First of all, it is worth noting that, although the discovery of the positron is widely interpreted as an instance of a "golden event" discovery, none of Anderson's major announcements concerning his discovery were content to describe only a single event. Anderson's first announcement, a somewhat tentative short piece appearing in September 1932 in *Science,* describes three different events (Anderson 1932), while the more confident and detailed discussion in the *Physical Review* in March 1933 shows photographs of four events. Furthermore, in that piece, Anderson notes that

> [t]o date, out of a group of 1300 photographs of cosmic-ray tracks 15 of these show positive particles penetrating the lead, none of which can be ascribed to particles with a mass as large as that of a proton, thus establishing the existence of positive particles of unit charge and of mass small compared to that of a proton (Anderson 1933, p. 493).

Nevertheless, in both pieces, Anderson does emphasize one single event, a single set of tracks which "seemed to be interpretable only on the basis of the existence in this case of a particle carrying a positive charge but having a mass of the same order of magnitude as that normally possessed by a free negative electron" (Anderson 1933, p. 491; see figure 5), and he proceeds to discuss details of that one photograph that made such an interpretation "inevitable." So I will proceed, with some reservations, on the assumption that this is in fact a "golden event" discovery. Nevertheless, the argument given is a statistical argument, although qualitative and based on an image.

The image in question is a cloud chamber photograph that shows two tracks, one on either side of a 6mm lead plate inserted into the chamber. The two tracks match up very closely, suggesting a single particle passing through the lead. Differences in the curvatures of the tracks above and below indicate a higher energy below the lead than above, which entails, on the assumption that it is indeed a single particle and that particles do not *gain* energy when passing through lead, that the particle was traveling from the lower to the upper region of the space in the photograph. Knowing the direction and curvature of the path, as well as the magnetic field, Anderson concludes that the particle has a positive charge. But based on the length of the track and the energy indicated by the curvature, it cannot have been a proton, which would have a much shorter range. The particle, then, must have much lighter mass, on the same order of magnitude as that of a free negative electron.

To show that Anderson is here making a statistical argument, as I have been using the term, I will need to show that the argument is premised on the claim that what has been observed is significantly in excess of the expected background. For such an argument to work in the case of a single observed event, of course, the expected background has to be shown to be so negligibly small that, while it is not literally impossible for an event of this type to be background, such an outcome has a probability that is nearly, or "for practical purposes," equal to zero.

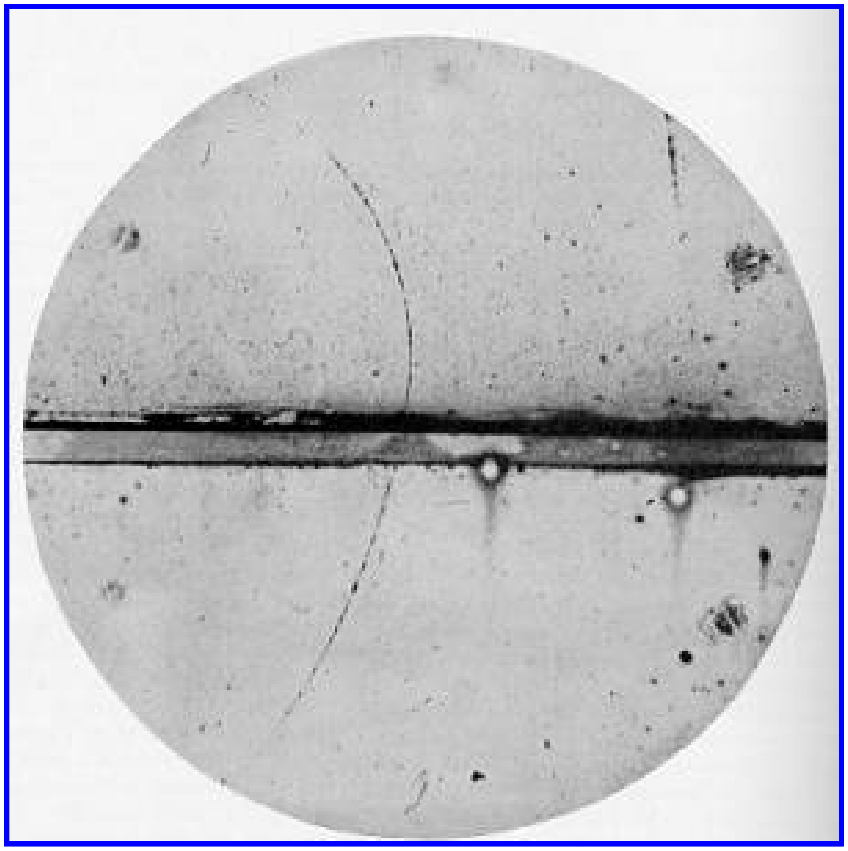

Figure 5. Anderson's "golden event." Anderson's caption for this figure reads: "A 63 Million volt positron ($H\rho$ = 2.1 x 10^5 gauss-cm) passing through a 6 mm lead plate and emerging as a 23 million volt positron ($H\rho$ = 7.5 x 10^4 gauss-cm). The length of this latter path is at least ten times greater than the possible length of a proton path of this curvature" (Anderson 1933, p. 492).

Anderson gives precisely this kind of argument in both of his early published announcements. The very brief 1932 discussion concludes,

> For the interpretation of these effects it seems necessary to call upon a positively charged particle having a mass comparable with that of an electron, or else admit the chance occurrence of independent tracks on the same photograph so placed as to indicate a common point of origin of two particles. *The latter possibility on a probability basis is exceedingly unlikely* (Anderson 1932, p. 239; emphasis added).

The discussion in the *Physical Review* is more explicit. All other sources of background having been ruled out, the "only escape" from interpreting this event as a very light, positively charged particle passing through the lead and losing energy would be

> to assume that at exactly the same instant (and the sharpness of the tracks determines that instant to within a fiftieth of a second) two independent electrons happened to produce two tracks so placed as to give the impression of a single particle shooting through the lead plate (Anderson 1933, p. 491).

The potential background source is thus explicitly identified: an accidental coincidence of two negative electrons. This source of background, Anderson continues,

> *was dismissed on a probability basis,* since a sharp track of this order of curvature under the experimental conditions prevailing occurred in the chamber only once in some 500 exposures, and since *there was practically no chance at all* that two such tracks should line up in this way (Anderson 1933, p. 491; emphasis added).

Here Anderson explicitly evaluates the probable expectation from this source of background, although very informally. Only one in 500 photographs have a track with such curvature. This gives one a rough idea of the probability of two such tracks in a single photograph. (Assuming a one in 500 chance for a single track, and assuming statistical independence, this would happen once in 250,000 photographs—Anderson had a total of 1300 photographs.) Once one adds the requirement that the two tracks line up perfectly as they do here, the probability becomes incalculably small: "there was practically no chance at all."

Here the form of the argument is clearly statistical, in exactly the same way that the argument given by Chamberlain, Segrè, Wiegand, and Ypsilantis is statistical: while it is in principle possible for this effect to be produced by background, the probability of that happening is so small that the only reasonable conclusion is that the data (datum) in hand were (was) not produced by background. Or, borrowing Galison's description of the reasoning used in logic tradition experiments, "one hypothesis is more probable than another" (*IL*, p. 453), and so much more probable that the acceptance of Anderson's discovery claim is inescapable. Note that Anderson relies crucially on a coincidence requirement: the requirement that there be sharp tracks above and below the lead plate, and that the upper and lower tracks are aligned properly. Galison, however, regards reasoning based on coincidence requirements as a hallmark of the logic tradition (*IL*, pp. 438–54).

7. Statistical Arguments Using Single Events, Part 2: Statistical Reasoning from a Single Count (Difference C)

The preceding example is sufficient to make the point that an argument based on a single event may nevertheless be statistical in nature. But there is another possibility worth exploring here, and that is the possibility of a *logic* tradition discovery based on a single event. According to Galison's analysis, we should not find any such discoveries. In fact, I do not claim that the logic tradition has produced any "golden event" discoveries, but it has come quite close, and the details of the case make clear exactly what the nature of a "golden event" discovery is: It is a discovery in which a single event (whether recorded as an image or as a count) has a great deal of statistical significance on account of the exceedingly low background in that experiment. Any discovery claim based on such an event has to be able to produce a convincing argument that this condition is satisfied.

P.A.M. Dirac first introduced the concept of a magnetic monopole in 1931, as a possible explanation for charge quantization, then developed a theory of magnetic monopoles in 1948 (Dirac 1931, 1948).[10] These particles would, if they exist, have very large masses, far beyond what can be produced by particle accelerators. Ordinary particle detection methods pose enormous difficulties in searching for magnetic monopoles.

Consequently, researchers have looked for cosmic-ray-induced changes in the current passing through a superconducting loop, as a result of Faraday induction, as a possible source of evidence for the existence of magnetic monopoles. Faraday's law of induction entails that a magnetic monopole with magnetic charge g will induce a current $I = 4\pi g/L$ through a closed superconducting loop, where L is the inductance of the loop. The magnetic flux from a magnetic monopole should be, according to Dirac's theory, $4\pi g = hc/e$, while the flux quantum of superconductivity is $\phi_0 = hc/2e$. Thus a magnetic monopole passing through a superconducting loop consisting of a single turn should cause a change in the magnetic flux through the loop equal to $2\phi_0$.

Blas Cabrera of Stanford University pioneered this approach to detecting magnetic monopoles. His first such detector consisted of a four-turn superconducting ring with a diameter of five centimeters, monitored by a superconducting quantum interference device (SQUID) magnetometer. Based on the above calculations, he expected a single magnetic monopole passing through his four-loop detector to result in a magnetic flux change equal to $8\phi_0$. Furthermore, this effect should be independent of the "veloc-

10. It is interesting to note that Dirac also predicted the existence of the positron in his 1931 paper. It is no ordinary paper that nearly gives rise to two golden event discoveries 50 years apart.

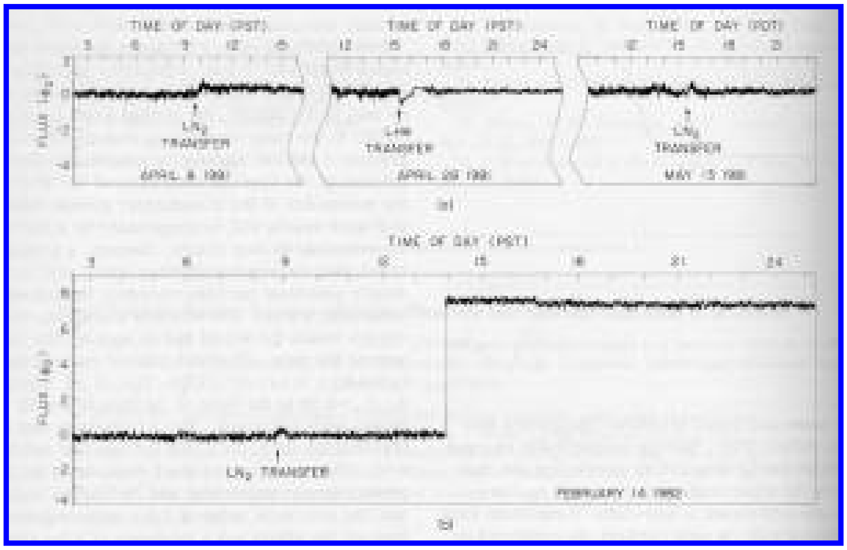

Figure 6. (a) Output from Blas Cabrera's single-loop monopole detector during typically stable operation, showing small ($\approx 1\phi_0$) offsets due to liquid helium and liquid nitrogen transfers. (b) Output from the same detector showing the candidate monopole event (Cabrera 1982, p. 1380).

ity, mass, electric charge, or magnetic dipole moment" of the particle in question (Cabrera 1982, p. 1378).

In a paper submitted to *Physical Review Letters* in April 1982, Cabrera reported on 151 days' worth of collected data. The abstract reported that "[a] single candidate event, consistent with one Dirac unit of magnetic charge, has been detected during five runs totaling 151 days" (Cabrera 1982, p. 1378). Cabrera noted that this "single large event" was "consistent with the passage of a single Dirac charge within a combined uncertainty of 5%. . . . It is the largest event of any kind in the record" (Cabrera 1982, p. 1379; see figure 6). The event was recorded on February 14, 1982. Cabrera compared this event to the other 26 events exceeding $0.2\phi_0$; the single large event is clearly isolated in magnitude from background events (see figure 7).

Cabrera made no discovery claim based on this one event. Rather, he listed possible sources of background, and gave arguments why each was highly unlikely to be the cause of this one event. These potential backgrounds included line voltage fluctuations ("failed to cause detectable offsets"), radio-frequency interference from the motor brushes of a heat gun ("failed to produce any offsets"), external magnetic field changes ("are attenuated by 180dB"), ferromagnetic contamination ("minimized using

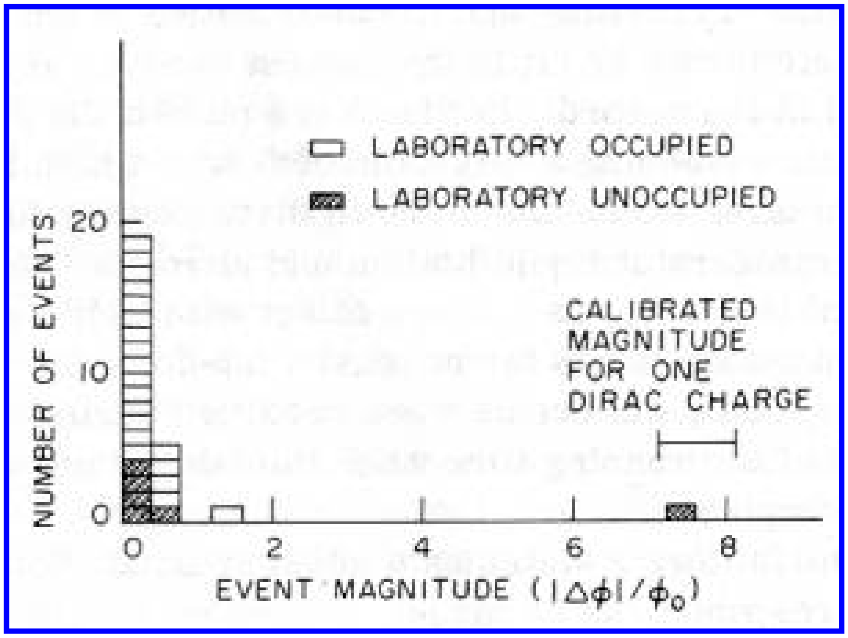

Figure 7. Histogram of event magnitudes from Cabrera's first 151 days' worth of data. The Valentine's day, 1982 event stands by itself on the far right (Cabrera 1982, p. 1380).

clean-bench assembly techniques and checked with magnetometer measurements"), the superconductor in the loop going "critical" ("critical current . . . not reached for currents a thousand times greater than $8\phi_0/L$"), seismic disturbance (none occurred on the day of the event), and energetic cosmic rays (those depositing ≤ 1 GeV/cm in passing through the superconducting wire would raise the temperature of the wire only a minuscule fraction of the amount needed to reach the critical temperature of the superconductor). Only one possible background source remained. The possibility of *mechanically induced* offsets was investigated by means of "sharp raps with a screwdriver handle against the detector assembly." On 2 out of 25 attempts such blows produced offsets in excess of $6\phi_0$ (these offsets were followed by "drifts" in the detector output, which were not seen following the February 14 offset). While such an externally produced impact was "not seen as a possible cause for the event," Cabrera admitted that he could not rule out "the possibility of a spontaneous internal stress mechanism" (Cabrera 1982, pp. 1379-80).

In fact, it was precisely this possible background source that prevented Cabrera from making any discovery claim in this paper. He later recalled, "It was a striking event, because it was exactly the right size step. I thought that there was a good chance it was caused by magnetic charge, but I was not convinced because of the other possible although improbable mechanism" (Cabrera 1998).

Cabrera was not the only member of the physics community who believed that this event was probably a magnetic monopole. One physicist working on a monopole experiment at the time recalled that Cabrera's Valentine's day event "really shook the world. . . . People immediately thought monopoles existed" (Incandela 1998).

Cabrera's experiment was clearly a logic tradition experiment, in Galison's sense. His detector was essentially a counter designed for one specific kind of particle, in which a single count was *almost* sufficient to claim a discovery. Although no discovery claim was made, the reason why is quite telling. The background for this experiment was low, but not quite low enough. When Anderson argued for the existence of the positron on the basis of a single event, he was able to argue successfully that the background for his experiment was, for practical purposes, zero. Cabrera could not make this claim, and so he could not be sufficiently confident in his interpretation of this event to make a discovery claim.

In other words, Anderson's and Cabrera's papers report on two episodes, one an experiment clearly in the image tradition, and the other just as obviously a counter-based, logic-tradition experiment. Both involve single events, and both pursue the same argumentative strategy. What makes the one a discovery and the other not quite a discovery is not a difference in the form of the data. Rather, what prevented Blas Cabrera from being able to make a convincing discovery claim was the possibility of a background that, while small, could not be ignored.

It was precisely the problem of background, and a suspicion that the single event might really be a monopole event, that drove Cabrera and his collaborators on to build another device with three rings. Cabrera later recalled, "after this event we quickly realized that a coincident detector where more than one detector loop and SQUID system would simultaneously see the event would allow a claim if more events were seen" (Cabrera 1998). Requiring coincidences is a standard logic-tradition approach to reducing background, but Anderson's discovery shows that it works just as well in image-tradition experiments.

The new, three-loop detector, which had a much greater sensing area (Cabrera et al. 1985), did not yield any candidate events (Cabrera et al. 1983), but neither did it yield any single-count events similar to the February 1982 event. "No large spurious or real signals were seen, casting no light on the origin of the previously reported candidate" (Cabrera et al. 1983, p. 1936). Cabrera recalled,

> When no events were seen after a factor of 50 increase in exposure, I further doubted that the event in the prototype detector had been caused by a magnetic charge, but we were still driven by the fact that no single loop events had been seen in this second experiment of the type in the original experiment (Cabrera 1998).

In other words, if the original event was due to some background source, it was a source that was not producing similar effects in the follow-up experiment.

Meanwhile, a Chicago-Fermilab-Michigan experiment had built a pair of detectors with larger areas, and required a coincidence in signals from the pair (Incandela et al. 1984). They also did not see any candidate events.[11] Joe Incandela, whose Ph.D. dissertation at Chicago was based on this experiment, recalled that "when we built our detector we found that these sorts of flux jumps . . . happen quite naturally in response to other things." By imposing a coincidence requirement from two detectors, the rate of background dropped, Incandela recalled, to "something like once in a hundred thousand years" (Incandela 1998).

In addition, Cabrera's Stanford group continued to run the three-loop detector, and built an eight-loop device using an octagonal configuration, both of which failed to find any monopole candidates, thus driving the upper limit on the magnetic monopole flux ever lower (Gardner et al. 1991; Huber et al. 1990, 1991). Cabrera was by then convinced that the original event had not, after all, been a monopole: "It was only after running the third generation of detector some 1500 times more exposure and still getting a null result, that I was willing to believe that the original event was likely to have been spurious and most probably caused by the flux motion mechanism [described in the first paper]" (Cabrera 1998). An initially promising event looks less and less "golden" as the statistics accumulate against it. As Joe Incandela noted, "You can never say that that wasn't a monopole, what they saw, but the odds are very low" (Incandela 1998).

The fate of that single monopole candidate potentially awaits every "golden event." An event that seems compelling at first can always turn out later to have been background. The lower the background is, the less likely this is to happen, and so both the image and logic traditions base their arguments on a comparison between the number of candidate events

11. One event that turned up in this experiment actually far exceeded the magnitude for a single Dirac charge, and was also not consistent with an integral number of Dirac charges. This event "occurred 1 h after recovering from a catastrophe in which the power cord to the computer caught fire." Radio-frequency interference was suspected to be the cause (Incandela et al. 1984, p. 2070).

and the expected background. Such discoveries, whether based on counts or images, whether based on many events or a single event, depend entirely on a finding that the background is very low relative to the candidate event sample. And, as the monopole search indicates, discoveries that live by statistics can also die by statistics.

The examples I have examined here strongly suggest that there is a unity of argumentative form in particle physics. But unities, like disunities, might exist and yet fail to be interesting or important. Next I will address whether this is a trivial unity.

8. Image, Logic, Hybrid: One Physics under a (Statistical) Groove

It will come as no surprise that I do not consider this unity within particle physics to be trivial. I propose that this unity can help to explain the course of development of experimental particle physics in this century, a development that Galison rightly characterizes as a process of hybridization between the logic and image traditions. This hybridization occurred with the advent of machines, such as drift chambers and time projection chambers (Fernow 1986, pp. 234–57; *IL*, pp. 553–688), which were entirely electronic and triggerable, like the Geiger-Müller counters of the logic tradition, but were also able to produce visually isomorphic representations of the physical processes being detected, as could the bubble chambers of the image tradition.

As Galison tells the story, the image and logic traditions, while sharing much, were divided into distinct subcultures within physics by, among other discontinuities, allegiance to entirely distinct forms of argument. Hence, the development of such hybrid machines required that physicists from distinct traditions *overcome* this divide. Such processes of exchange between distinct subcultures within physics take place in what Galison calls a "trading zone." In a trading zone, communication takes place via an "interlanguage," a concept that Galison imports from anthropological linguistics. Examples of interlanguages include pidgins and creoles. By means of such interlanguages, distinct communities can trade with one another on a "local" basis in spite of "global" differences. Galison comments, "All of these [interlanguages], to one degree or another, facilitate local communication between communities of what would otherwise be mutually incompatible languages while preserving the separateness of the parent languages" (*IL*, p. 48). While the definitions of terms such as "pidgin" remain disputed within anthropological linguistics, it is characteristic of such languages that they arise to facilitate communication between distinct communities in the absence of a shared language (cf. Romaine 1988, esp. chap. 2). In discussing a particular example of such an exchange, Galison comments that while we focus on what appears to be a simple exchange of goods between two people, "[o]ut of our narrow view . . . are two vastly different symbolic and cultural systems, embedding two incompatible valuations and understandings of the objects exchanged" (*IL*, p. 804). (Galison discusses the hybridization of the image and logic traditions also in his 1997*b*.)

For such an analysis to be appropriate for examining the exchange between the logic and image subcultures in physics, therefore, there must be a substantial linguistic divide between the two groups. Otherwise, no interlanguage is needed. While Galison discusses many other types of discontinuities, the epistemic divide is an important part of his case for employing notions such as "pidgin" and "creole" for his account.

In the case of the logic and image traditions, as Galison tells it, part of what separated the two subcultures was a disagreement over the meaning and relative worth of different kinds of evidence. Image physicists were more likely to be persuaded by an image of a single "golden event" and to be skeptical of abstract statistical arguments, as when P.M.S. Blackett quoted Lord Rutherford's outburst to a verbose research student: "Do, goodness sake, forget about the theory of error and go back into the laboratory and do the experiment again!" (quoted in *IL*, p. 217). Logic physicists, on the other hand, were more likely to be confident of their statistical analyses and to dismiss allegedly "golden" events by noting "anything can happen once." Epistemic discontinuity, then, divides the logic and image traditions into distinct communities that place entirely different meanings on arguments. Cooperation between the two subcultures required them to arrive at an accommodation so that they could collaboratively produce arguments that all would find convincing. This accommodation could only be achieved through an interlanguage that enabled members of both traditions to work with and talk about the same data and instruments, in spite of underlying disagreements about their significance, so as to produce coordinated actions and beliefs. Eventually, through a continuous process of such exchanges, the two communities came together into a (somewhat) unified group with a (partially) shared understanding of their instruments, data, and arguments.

In the present context, I can neither do justice to Galison's account of this historical development in physics, nor produce anything like an adequate alternative account of my own. But the shared commitment to a form of statistical argument for discovery claims can serve as a point of departure for outlining a different possibility. The epistemic divide between these two traditions was not so great as to make an interlanguage necessary for them to communicate, cooperate, and, eventually, unite in a single experimental pursuit.

How can we understand the difference of opinion between image and logic tradition physicists concerning particular experimental claims, and how can we understand how those differences were overcome? If the two traditions were employing the same form of argument all along, what changed during the hybridization process?

The first step towards understanding this story is to re-examine the precise nature of the disagreements between logic and image tradition physicists. Galison in fact does not provide any evidence that physicists in either tradition systematically doubted the specific results produced by the other tradition. There is no evidence that logic physicists doubted, for example, that Anderson had in fact discovered the positron or that Cecil Powell's use of nuclear emulsions uncovered the first evidence of the pion's existence. Nor is there evidence that image tradition physicists doubted that Chamberlain, Segrè, Wiegand, and Ypsilantis had made a strong experimental argument for the existence of the antiproton. In general, the disagreements that Galison describes between the two traditions were prospective, rather than retrospective: Logic physicists thought that their approach to discovering new phenomena was more likely to succeed, while image physicists considered their techniques to hold more promise.

While it is true that logic physicists may have from time to time used the slogan "anything can happen once," this phrase has to be taken with a grain of salt. The statistically-minded physicist knows that it is no less true that anything can happen twice, or a thousand times. The important question is always how probable such an outcome is.

In both traditions, physicists were pursuing the same kind of argument: "this finding represents a significant statistical excess over background." Image tradition physicists could make a "golden event" discovery claim only when they convinced themselves that the background for that kind of event was so low that a single event constituted a significant statistical excess. But these arguments generally had to be made informally.

Logic tradition physicists, of course, had to satisfy the same requirement when they made a discovery claim. They had to show that their outcome represented a significant excess over the expected background. But these evaluations rest on assumptions, which can be questioned by the skeptical. Being able to address these "lower level" considerations, or even knowing what kinds of questions to ask at this level, may require considerable background knowledge about particular detector systems and how they can go wrong. Certainly members of the two traditions differed in their ability to address such questions, but their discovery claims still rested ultimately on one kind of statistical argument that they both employed.

Galison notes that

> the coming together of the two traditions was a halting coordinative effort that frequently ran aground on the technical obstacles. While casting aspersions on the other, each side insistently tried to acquire the virtues of its rival: logic had statistics and experimental control and wanted persuasive detail; image had the virtues of being fine-grained, visible, and inclusive but wanted the force of statistics and control over experimentation (*IL*, p. 807).

I agree with all of this, but do not see how the notion of a creole or pidgin or other interlanguage helps us to understand the process. On the contrary, recognizing the epistemic unity between these two traditions, their shared commitment to a particular form of argument, helps to make sense of *why* these alleged competitors would see these "virtues of [their] rival[s]" *as* virtues. The logic tradition wanted persuasive detail because more detail generally means more manageable (lower) backgrounds and hence greater sensitivity to new phenomena. The image tradition wanted "the force of statistics and control over experimentation" at least in part because these virtues would make it possible to present more precise and convincing evaluations of backgrounds.

There was nothing inevitable about the coming together of the image and logic traditions. Galison gives a rich and fascinating view into the many obstacles—both technical and social—that had to be overcome during the process. There does not seem to have been the particular kind of *epistemic* obstacle, however, that Galison claims, and hence there does not seem to have been any need for an interlanguage—at least for the purposes of coordinating exchanges concerning the significance of experimental results.

9. Concluding Suggestion: Unity of Methods as a Working Hypothesis?

The trend in science studies is towards identifying disunities in the sciences, and Galison's book is a valuable and original contribution to that trend. Galison writes, "I will argue this: science is disunified, and—against our intuitions—it is precisely the disunification of science that brings strength and stability" (*IL*, p. 781). While there is much truth in this claim, I think that it would be a mistake to be taken up so much in the quest for disunities as to overlook the important unities within the sciences, as these may also be important in giving science "strength and stability." I have attempted here to identify one such unity, at least within the field of particle physics. The unity in question is a unity of methods.

The idea of the methodological unity of science is certainly not new, and the sense in which I am using the term needs some clarification. When Paul Oppenheim and Hilary Putnam published "Unity of Science as a Working Hypothesis" in 1958, they were arguing for the plausibility of the hypothesis that science is engaged in a process of becoming, and ultimately can become, unified with respect to the language employed in scientific theories, and with respect to the laws set forth within those theories. They contrasted this with the "unity of methods in science" (Oppenheim and Putnam 1958, p. 5). Obviously the object of my discussion here is not at all related to Oppenheim and Putnam's claim concerning the unity of science with respect to language or laws, a claim that has run into powerful opposing arguments.

Yet I think that we should not too quickly abandon the possibility of a unity of methods in the sciences. We might entertain the following version of the "unity of methods" thesis: there are a small number of forms of argument that are shared among otherwise diverse areas of investigation, or that are employed in common during otherwise distinct historical periods of scientific endeavor.[12]

Here I have identified a quite local instance of such a unity. My finding is modest: For purposes of discovering new phenomena, both the image and logic traditions employed the same statistical form of argument that I have characterized above.

As did Oppenheim and Putnam with their flavor of unity, I consider it to be an empirical question whether, and to what extent, this kind of unity exists within the sciences. I suspect that other scientific disciplines do employ the form of argument that I have discussed here, but there will probably be some fields of research that do not. For example, this particular statistical form of argument makes use of an ontology of "events," which will not be shared by every field of empirical inquiry. (Although one might articulate a more general form that might be found among a yet wider variety of scientific disciplines.) Furthermore, there are different kinds of phenomenal claims that rest on different kinds of evidential arguments. The kind of argument discussed here is appropriate for establishing the existence of certain kinds of phenomena, but it is different from the kind of argument given, for example, when one is presenting evidence that two quantities are related to one another according to a given law.

12. Putnam and Oppenheim describe the unity of methods differently, by means of the thesis that "all the empirical sciences employ the same standards of explanation, of significance, of evidence, etc." (Oppenheim and Putnam 1958, p. 5).

The identification of forms of argument that are shared among different scientific enterprises is an important part of our understanding of the development of the sciences. Furthermore, such an endeavor is no less important than the identification of disunities. Looking at forms of argument may well uncover an important source of strength from unity in the sciences to complement Galison's important stress on strength from disunity.

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