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Code, drawings and causality: early computer-aided data analysis in high energy physics

Cross-section of two DFG-Projects "History of Early Particle Physics" + "Media Cultures of Computer Simulation"

overarching research question: Interplay of theoretical and experimental practices in (early) particle physics - a topic usually neglected in historiography

focus on theory --> experiment delivers data for theoretical interpretation

focus on experiment --> experiment is independent from theoretical interpretation

my thesis: both data and interpretation emerge from the close interplay of theory and experiment - in fact, the line between theory and experiment is blurred

Computing as an additional historiographical complication!

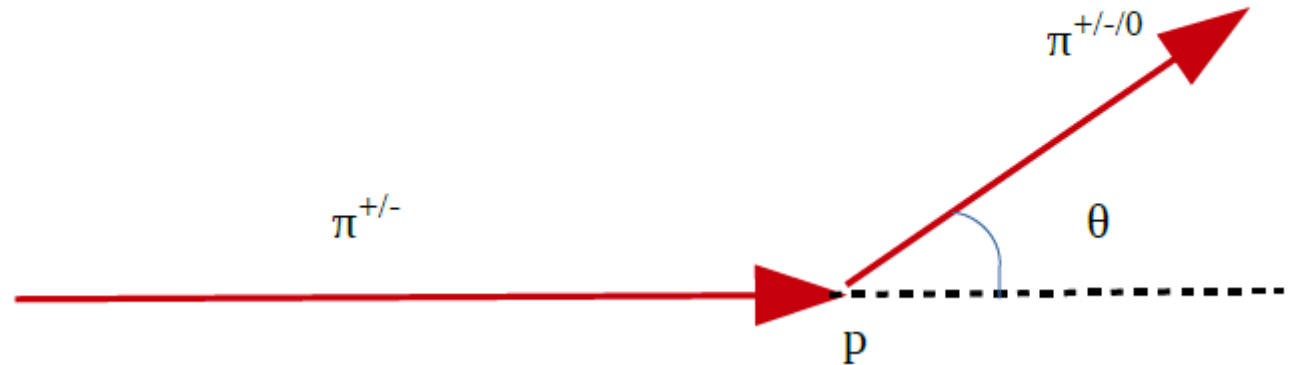
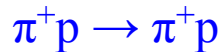
Case study: first use of an electronic, digital computer to analyse data in particle physics - code as one more tool in interplay of theory and experiment - one cannot project back to this time notions of algorithm or digital tools!

Plan of the talk

- the physics question
- from the physics question to the mathematical problem
- from the mathematical problem to its numerical solutions (!)
- mixing and choosing solutions
- the numerical and physical solution is found
- what can we conclude, as far as computing is concerned?

1951: Chicago (Enrico Fermi, Herbert Anderson) - new accelerator, new experimental set-up: pion beams (π^+ or π^-) of energy E are shot onto a target of protons (p)

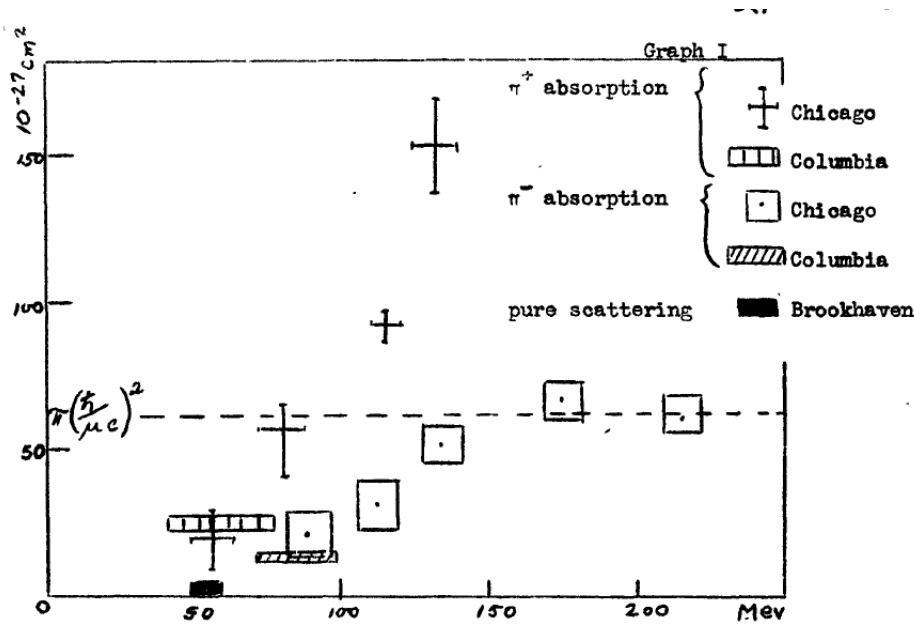
---> some pions just keep on moving in a straight line, others interact with the target through nuclear forces and are scattered (eventually with charge-exchange)



---> early experiments: how many pions (π^+ or π^-) are scattered for a given E ?

no theories make clear-cut predictions, but in general symmetry between the two pion types is expected

an unexpected result: interactions for higher energies are stronger for π^+ than for π^-



Why?

"resonance"-hypothesis: the interaction between π^+ and p is strong ("resonant") only for energies $E \sim 200$ MeV and pion-angular momentum $J=1$ - for other E's and J's it is lower

How to test this hypothesis?

(1) look at what happens for $E > 200$ MeV? not yet technically possible!

(2) compare $J=1$ vs. $J=0, [2, \dots]$? how? the pion beam is a combination of all J's...

----> use a method from nuclear physics: "phase-shift analysis"

Phase-shift analysis:

1 - For each energy E , measure the angular distribution of all three possible processes

$$\pi^+p \rightarrow \pi^+p$$

$$\pi^-p \rightarrow \pi^-p$$

$$\pi^-p \rightarrow \pi^0n .$$



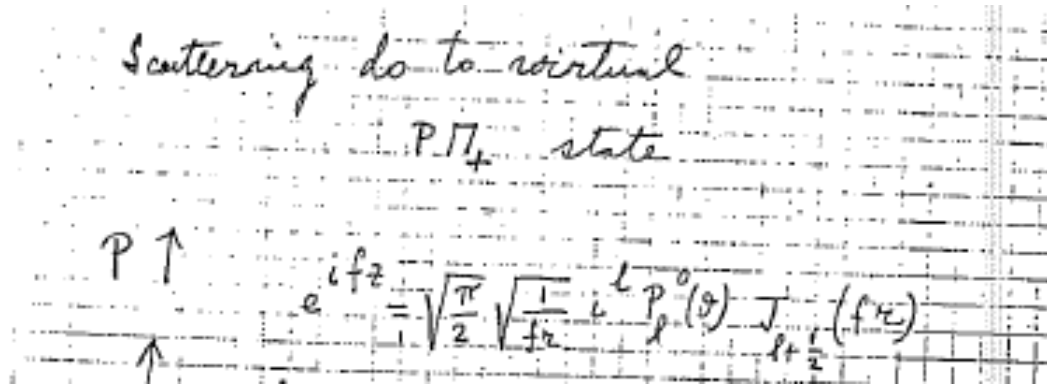
2- Parametrize the three angular distributions (for given E) using 6 parameters - 6 angles $\alpha_1 \alpha_3 \alpha_{31} \alpha_{13} \alpha_{11} \alpha_{33}$ called "phase shifts" (PS)

3 - is the "resonance" there for $E = 200$ and $J=1$? If (and only if) the phase-shift α_{33} for $E = 200$ MeV is equal to 90°

the mathematical problem: parametrizing the data in terms of the 6 phase-shifts

additional difficulty: the data are few, with large errors and not at the same exact energy values for π^+ and π^- beams

Dec. 1951/Jan 1952: Enrico Fermi makes by hand a PS-analysis of the data



(from Fermi's notebook)

March 1952 - publication (Anderson, Fermi, Nagle & Yodh): Fit of Chicago data to phase shift model - by hand, ca. 1-2 weeks to get all 6 PS --> no resonance....

BUT: Chen Nin Yang (theorist, Fermi's former student) writes to Fermi in May 1952: a mathematical analysis of the problem shows that, for each energy value, there are always at least two exquivalent sets of phase shifts - probably also more!

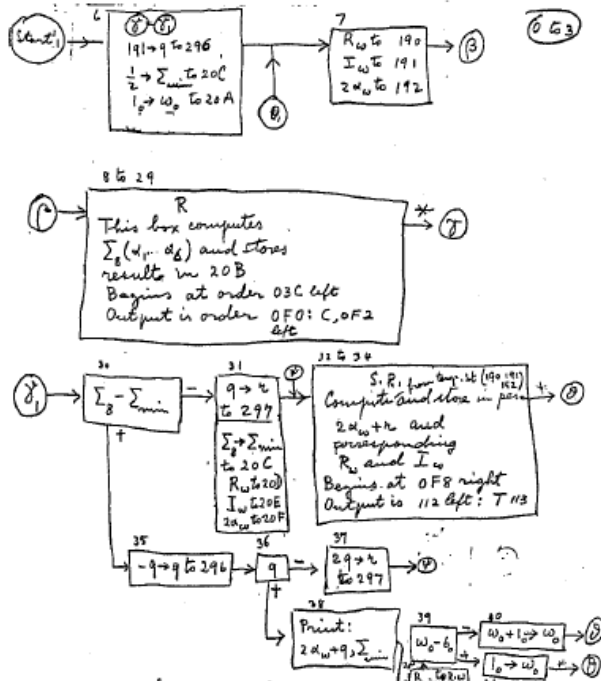
for each energy one has to derive more than one set of PS-value - and new experimental results keep coming in - how to keep up?

-----> Fermi's idea : let us use the MANIAC!

Summer 1952: Fermi is in Los Alamos, where the EDVAC-type machine MANIAC (Mathematical Analyzer, Integrator And Computer) - an- has been built by Nicholas Metropolis's team - Metropolis is ready to test it with physics problems....



Fermi performs on the MANIAC a PS-analysis ("it only takes five minutes") - he also partly writes the code (left) and writes up method and results in a Los Alamos Report (right)



Stop manually when printed \sum_{min} reports at least 6 times.
 Set break point switch. Start \rightarrow Stop
 On to Start₂

LOS ALAMOS SCIENTIFIC LABORATORY
 of the
 UNIVERSITY OF CALIFORNIA

Report written:
 November 19, 1952
 Report distributed: DEC 3 1952

LA-1492

NUMERICAL SOLUTION OF A MINIMUM PROBLEM

Work done by:
 E. Fermi
 N. Metropolis

Report written by:
 E. Fermi

Fermi-Metropolis (Los Alamos Report 1952) - "brute force"-approach:

One searches for the

set of angles that minimizes the following expression,

$$(2) \quad M(\alpha_1, \alpha_2, \dots, \alpha_6) = \sum_1^9 \left(\frac{\sigma_n - f_n(\alpha_1, \dots, \alpha_6)}{\epsilon_n} \right)^2 = \text{minimum}$$

in which ϵ_n is the experimental error of the quantity σ_n .

The search for the minimum involves a sequence of successive computations of M for different values of the angles. Each time that new angles yield a value of M smaller than any of the preceding ones, this value is stored as a temporary minimum. The procedure stops when a set of angles, $\alpha_1, \alpha_2, \dots, \alpha_6$, is found such that the values of M for this set are smaller than the 12 values of M obtained when one of the six angles is either increased or decreased by a specified small step. The smaller is the step, the higher is the accuracy of the minimum values found.

results: for each energy the code finds 2 Minima - but no trace of a resonance! (the relevant PS α_{33} is never $\sim 90^\circ$)

December 1952 – 3rd Rochester Conference: Fermi presents these results

Fermi then presented the new data for which the conference had been waiting, first remarking that the courier when he got here handed him a small piece of paper on which there were written, in a cryptic fashion as is proper for something that comes from Las Alamos, certain numbers which then had to be decoded. Fermi then presented to the conference certain essential results which he had converted from duo to decimal notation. He has since supplied us with the full and correct data, and these are given in the table below:

Mev	Type	η	Phase Angles (degrees)						
			α_3	α_1	α_{33}	α_{31}	α_{13}	α_{11}	
53	Conv.	0.78	0		-9	-2			Brookhaven
78	Conv.	0.97	6		-13	3			
113	Conv.	1.20	13	-7	-27	-1	12	14	Old Data
113	Yang	1.20	13	-7	-10	-36	14	10	Old Data
135	Conv.	1.325	21	-3	-38	-11	17	4	Old Data
135	Yang	1.325	20	-2	-21	-49	15	9	Old Data
									New Data
									Least Squares

Prominent, elder theorist Hans Bethe bring physics arguments why these results cannot be correct - he says that one might also fit well using only 3 PS

6. April 1953: Letter from Fermi to Metropolis:

"thank you for sending the results of the calculations. They are quite interesting [...] the results do not show any evidence for a resonance in the state 33 [...] In the attached sheets you will find the program for a new calculation on the same experimental material. The new calculation differs from the old one only because the smoothing out of the experimental data has been done differently."

Fermi and Metropolis have the MANIAC, but other physicists find alternative fitting procedures...

Dr. Nicholas Metropolis
Los Alamos Scientific Laboratory
P.O. Box 1663
Los Alamos, New Mexico

Dear Nick:

Thank you for sending the results of the calculation. They are quite interesting in that they show an energy dependence of the phase shifts rather different from the one that had been anticipated by most. In particular, the results do not show any evidence for a resonance in the state 33. A resonance should be indicated by the fact that the corresponding phase shift crosses 90° . As you may have noticed, this is not at all the case. In fact, it appears that the phase shift in question reaches a maximum of about 50° , and then begins to decrease. The calculation seems to be correct in the general lines.

I have been somewhat puzzled by the fact that you noticed that for some energies the cross sections are not obtained quite accurately, but show some errors of a few per cent. You may remember that we had had this trouble in other problems, and I am still somewhat uncertain as to what is the reason for it. In any case the cross sections are known with an accuracy worse than a few per cent, so that we should not worry about a discrepancy of this order of magnitude.

In the attached sheets you will find the program for a new calculation on the same experimental material. The new calculation differs from the old one only because the smoothing out of the experimental data has been done differently. I would like to see how much the results that were obtained on the previous calculation depend on the way the adjusting is made.

Thank you again for your help.

Sincerely yours,


Enrico Fermi

March 1953 paper by Ashkin & Vosko (Pittsburgh): Graphical method for determining various sets of PS - not as precise as the MANIAC, but allows visualisation of symmetries!

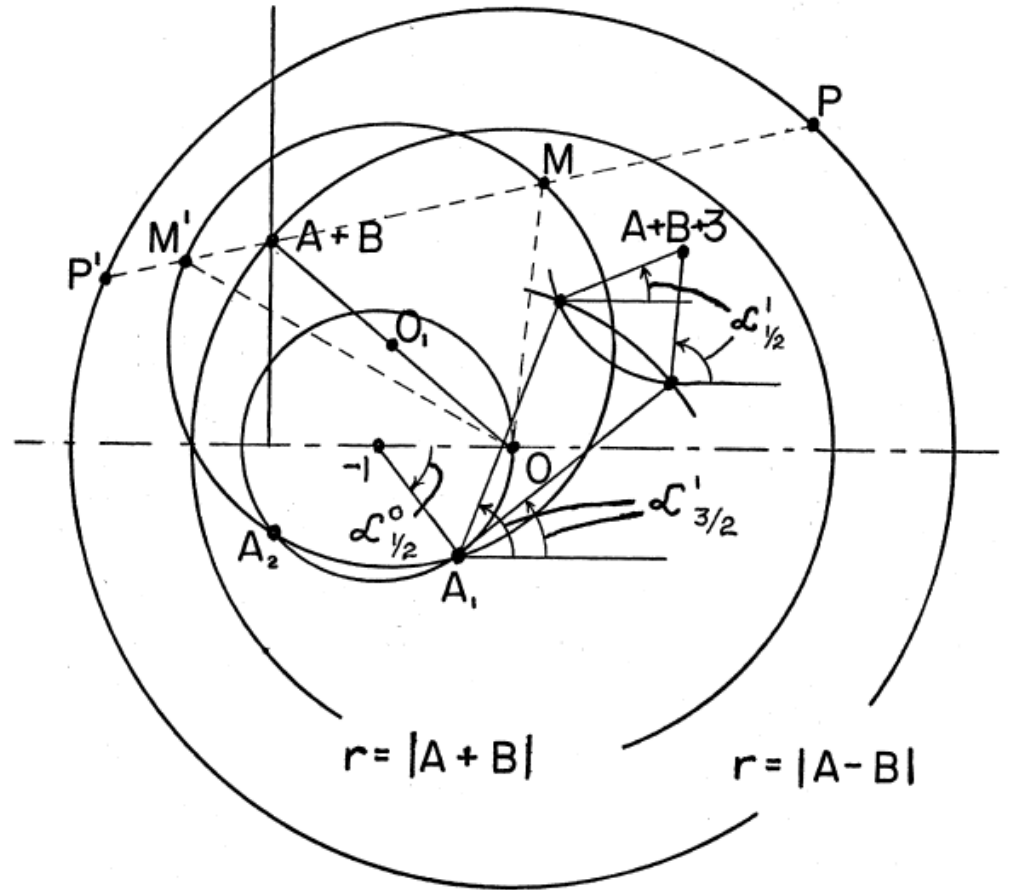
"a less accurate, but convenient method [...] it may give some insights into the structure of the phase fitting problem and be useful for rough estimates"

yet the resonance is not there...

Maurice Glicksmann (Chicago) and Ronald Martin (Cornell) use the graphical method to fit with only 4 (Glicksmann) or 3 (Martin) phase shifts (Bethe's idea)

finally $\alpha_{33} = 90^\circ$ ----> *the resonance is there!*

these results circulate unofficially - and of course Fermi soon learns about them...



December 1953 - Fermi writes to Metropolis - reports the (still unpublished) results by Glicksman and Martin

in both cases parameter α_{33} passes through 90° - the resonance is there!

Fermi's conclusion: *"All this tends to confirm that there are many acceptable solutions in the high-energy region and the ultimate result probably will be that no decision is possible unless the experimental accuracy of the data is considerably improved."*

but Hans Bethe is not convinced, and he, too, decides to employ the MANIAC

obtained by using a graphical method and the assumption that α_{13} and α_{11} are both equal to zero. On these assumptions he claims that a solution that fits as far as they are known, both the positive and negative data is the following:

MeV	α_{33}	α_{31}
80	14°	0
101	20	0
125	32	2
150	47	5
177	72	10
190	90.5	14
205	117	48

$$\alpha_3 = -10^\circ$$

$$\alpha_1 = 10^\circ, \text{ independent of energy.}$$

Another attempt to fit the data with a set of phase shifts has been made here by M. Glicksman. He makes even more drastic assumptions, namely, that of the p-phase shifts, only α_{33} is different from 0. On these assumptions, he obtains a reasonable fit with the following phase shifts.

MeV	α_3	α_1	α_{33}
120	-13	9	32
135	-11	11	40
169	-15	10	65
194	-9	0	81
217	-23	-4	100

All this tends to confirm that there are many acceptable solutions in the high energy region and the ultimate result probably will be that no decision is possible unless the experimental accuracy of the data is considerably improved.

in 1954 two papers are published back-to-back in Physical Review - both submitted on May 6th - both present phase-shift analyses of the same data obtained with the help of the MANIAC:

Paper A: Fermi, Metropolis & Alei: presentation and use of various methods for searching for a minimum - using different ways of "smoothing out" the data - a number of solutions (PS-sets) are found, but it's unclear which is "the best one" - and anyway not resonance !

"A decision can be obtained only by introducing theoretical arguments, as has been done, for example, in the following paper"

Paper B: de Hoffmann, Metropolis, Alei, Bethe: *"There is a multiplicity of [solutions] in our high-energy region. We shall find that some decision between solutions is possible on experimental grounds, but to a large extent we have to rely on theoretical discussions concerned with the physics of the situation. [...] we naturally prefer a solution which shows the smoothest and least steep variation of phase shifts.*

Paper B: de Hoffmann, Metropolis, Alei, Bethe (PR 95, 1586)

Search for minima with the MANIAC, then using these results as a basis, the graphic method is used to construct "better" solutions - *"the question arises why the solution of opposite sign is not found by the MANIAC. The explanation may be given most easily in terms of [...] an Ashkin graph"*

various solutions are rejected, and Track I is chosen - *"Track I has no such faults and we therefore believe it to be the correct solution"* - Track I has a resonance!

TABLE VII. Track I.

Energy (Mev)	α_3	α_1	α_{33}	α_{31}	α_{13}	α_{11}
217	-20	-4	107	-14	7	-7
194	-13	-14	90	-16	0	5
169	-4	7	64	3	-1	7
144	-13	14	46	5	3	-5
120	-12	8	30	6	2	-4

January 1954 - 4th Rochester conference: Bethe presents the solution from Paper B as *"a solution which satisfies the demands of theoretical intuition"*

Meanwhile, in largely unrelated theoretical development, theorists are trying to find a formal mathematical expression for the principle of causality in particle physics (quantum field theory)

Question: how to formally impose the condition that the cause (e.g. a collision) always precedes the effect (e.g. scattering) in quantum field theory?

Problem: in quantum field theory, you only have particle fields that spread over all space time - moreover, computations are usually performed in energy-momentum space (i.e. Fourier-transform of space-time)

therefore, establishing what came before or after is not easy....

...the solution proposed in 1954-55 has the form of what is usually called "dispersion relations" between quantum fields

---> because of causality pion-proton scattering has to fulfil dispersion relations

---> *causality can help to select the correct set of phase shifts!*

Mai 1955: Anderson, Davidon & Kruse (Chicago) "Causality in the pion-proton scattering"

"Dispersion relations [...] have been used for analysing pion-proton scattering data. [...] With suitable choice of the pion-proton coupling constant, good agreement can be obtained for the phase-shift solutions with a resonant behaviour. [...] The choice [of phase shifts] coincides with that selected by de Hoffmann, Metropolis, Alei and Bethe [paper B] on other grounds. This is the solution in which a_{33} passes through 90° . A non-resonant behaviour for a_{33} seems excluded by causality."

- the "physical" reason for the "correct" choice of phase-shifts is now found, and can become part of the numerical fitting method, by excluding solution that do not fit "causality" already in the code
- at the same time, the success of the formal causality relationship in helping interpret experimental results appears as a proof of its physical significance

What can we conclude, as far as computing is concerned?

in early particle physics, the computer is just one more tool in the complex interplay of theory and experiment...

..it is not the obvious best means to solve numerical problems and has no special authority

one should not project back to this time notions of algorithm, digital tools or black-boxes from later times

on the contrary, early examples of these hybrid practices can help become aware of factors that later became black-boxed