THE UNIVERSITY OF CHICAGO
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THE DEPARTMENT OF EDUCATION

Danish

November 2, 1964

Dr. George Rasch
Statistical Institute of University
of Copenhagen
19, 1. Sankt Pedersstraede
Copenhagen k., Denmark

Dear George:

I am really having fun teaching your mathematical psychology to my students. I was just now rereading Chapter 10 of your book, particularly the section on the separation of the parameters and even though by now I am somewhat familar with the ideas, I cannot suppress a spontaneous sense of delight as I think over what you are saying, particularly on Page 175, and what it means for the analysis of educational research data.

In considering how to deal with the case of many categories, I am still turning over in my mind the generalization of what you discuss on Page 171 where you compare items 2 at a time. I wonder if that would not be a good way to proceed now that we have high speed computing machinery to do all the dirty work. My problem here has to do with the best way to combine the separate estimates while at the same time using their discrepancies as tests of the applicability of the model.

Another idea I have is probably simpleminded. But I cannot help wondering if one approach to the many-category problem might be to estimate parameters for the categories one at a time as though you had as many tests as categories. I cannot see at the moment what assumption of the model this violates and so find myself wondering if I did it that way how I would then bring together and organize the many estimates; that is, the separate sets of estimates for each category.

The above remarks, of course, apply to the situation where the categories can be lined up so we know which are comparable categories for a set of items. When this cannot be done, I cannot see how conformability can be achieved for a set of items. What this may mean is that the simple 2-category or dichotomous model is the more general one from which all subsets should be derived smoothly and naturally. This raises the problem of how to view the case of more than two categories and where the categories are comparable from item to item as a special case of the dichotomous model rather than the other way around.

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March 17th, 1965.

Professor Benjamin Wright,
The University of Chicago,
Chicago 37 - Ill.,
U.S.A.

Dear Ben,

Thanks for several letters. Of course it is hard to squeeze an answer out of me, you knew that!

Take the last letter first. I am delighted to learn of the Midwest symposium on my work, and in particular that Jane Loevinger - my hardest case of convertion - is going to preside over it.

Is this the materialization of the small gathering mentioned in your letter of February 3rd? I hope that Walther Stellwagen - for a while not in Michigan, but in Logistics Department in Washington, D.C. - will attend. In 1960 he got a very good grasp of the Mathematics of my models, as far as then developed, including the Berkeley Symposium paper. Do you think you could persuade one of the people from Educational Testing Service, e.g. Fred Lord, to come. Finally I may mention Rosedith Sitgreaves of Teachers College, Columbia University, N.Y., who has given a most intelligent and critical review of my book, in particular feeling - quite correctly - the weakness of the soft part (chapters V and VI) of my presentation of the Item Analysis model. I met her in Washington or Philadelphia 1963 and got a very favorable impression of her understanding of my ideas.

Recently I have completed a paper for a UNESCO-Reader on Mathematical Methods in Social Sciences. The title is: "An Individual-centred Approach to Item Analysis with two Categories of Answers" and in this paper I give a rather detailed discussion of various points that have cropped up in my numerous conversations with psychologists. As the manuscript has been mimeographed I could send a few copies for your meeting. How many? cf. PS.

If, during your discussions, you feel that a number of questions have been left unanswered you might put them on paper and if - for some reason - I am not able to react on them immediately we might discuss them carefully when you come over and, possibly with Panos as a secretary, produce as much of an answer as we are able to.

This brings me to my next reason for dropping you a line just now, namely your own visit and, the application of your student Robert Panos, which I of course supported strongly, telling the USEC, Danish branch, that naturally I did not know Panos personally, but that his idea was splendid and his background was good. Further that I know two of the sponsors personally, one of them very well! I added that the applicant's versatility in Fortran programming would even be a great help in my own work. - I hope it works.

By the way, do you have any notion of when he may wish to arrive? In this connection I may just mention that the fall term at this University starts Sept. 1st, by which ti

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The idea of sending a good man over here for a full year is really splendid; that, I think, is the right way of spreading "the message." I do hope that others will follow your example.

And of course, you are welcome in Copenhagen in August - September. Certainly I shall spare you what time I can and it would be nice to have Panos with us.

And now to the more serious questions.

The estimation procedures have as yet ~~xaxy~~ been very tentative and everybody is crying for good and easy methods for estimating, in particular the ε's, and for control on the model. I am too! While you were here we went over what I knew by then and what I could suggest. At present I am again struggling with the problem, but I cannot say that I have yet made any decisive progress.

The pairwise analysis of items, as suggested by Leunbach (cf. X,3), is easy to apply and in case you have plenty of data it yields solid estimates of the ε-ratios. And since for each pair it is based upon a binomial distribution, say, confidence limits are of easy access for each $\varepsilon_i/\varepsilon_j$. The check on the model comes from comparing these estimates and should also lead to separate estimates of the ε_i's (apart from a chosen unit). But as regards the SE of such an estimate and an exact evaluation of the validity of the model the theory still leaves us in the dark.

From another point of view the pairwise analysis has a certain weakness in being rather wasteful, utilizing the data only partially. With all your material from 14000 individuals this may be quite acceptable, although I feel a bit suspicious about the solidity of the estimation of the extreme ε's, a point that may gain in importance if they are used as substitutes for the "true" ε's in a ξ-estimation.

The generalization of the pairwise analysis to m>2 we did discuss at some length last summer. It seemed hard to get through, but the possibility may be reconsidered.

As regards the utilization of

$$p\{(a_{oi})\,|\,(a_{\nu o})\} = \begin{bmatrix}(a_{\nu o})\\(a_{oi})\end{bmatrix} \cdot \frac{\prod \varepsilon_i^{a_{oi}}}{\gamma(\varepsilon_1,\ldots\varepsilon_k;(a_{\nu o}))} \quad X\ (5.22)^{x)}$$

a maximum-likelihood approach will of course eliminate the bracket-coefficients, but not their equivalent, the denominator. However, counting the number of $a_{\nu o}$'s equal to any r, say $c_1,\ldots,c_k$, we have

$$\gamma(\varepsilon_1,\ldots\varepsilon_k;(a_{\nu o})) = \prod_{\nu=1}^{k}(\gamma_r(\varepsilon_1,\ldots\varepsilon_k))^{c_r} \quad X,\ (6.6)$$

where

x)

November 16, 1966

Dr. George Rasch
Institute of Statistics
University of Copenhagen
Skt. Pedersstraede 19'
Copenhagen K., Denmark

Dear George:

Erling just spent two days with us here in Chicago and we did some good work together. Our topic was the case where a number of categories for an answer are more than two. After seeing the general maximum likelihood solution to this problem and realizing the difficulty in computing the more complicated symetric functions involved, we concentrated on the more typical case, where the researcher expects of the catagories to be ordered and is really only in doubt about whether the hypothesis of ordering fits the data and second what the weights opositions in order are. Solving this problem seems the simpler and we came to a possible solution requiring more less computing than the general case. That is where we are at now.

Our plan is to ge together again in a few months to try to create a computer program for doing the work and to test it out on some data. We hope to complete all this by next summer, and anticipate a third conference with Erling perhaps in July.

I am thinking now about your possible visit to this country next fall. I hope very much that will come to pass, and I would like to extend to you now a definite invitation to visit us here at the University of Chicago, whenever it is convenient for you .

The fall quarter begins about October 1st. Any time after that is o.k. with me. I will commit myself right now to provide you with room and board and whatever salary you think appropriate for you. I am not exactly sure how much money is available for this project, but at least $1,000, perhaps more. I am thinking that you might stay between two and four weeks, depending on your convenience and interest. My idea to use your presence here in two ways, first to give a condensed course for all those interested in your approach to mathematical psychology, and second to have your help on the particular project that I am working on right now.

As you react to my invitation, if you want to make a more substantial proposal involving more time and money, do not hesitate to do so. My proposal is based on what I imagine is the approximate amount of time you may want to spend in Chicago and the money I have immediatly in hand for the project. If you feel like

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67 2/0

13TH JANUARY 1967

Professor B.D.Wright
5835 Kimbark Ave.
Chicago,Illinois 60637.

Dear Ben,

I am sorry having delayed my answering you quite a bit, my excuse being that once more my health has cheated me. The idea in the operation was that of removing the focus of my recurring renal infections. In other respects the operation has done me a lot of good - that I am still getting easily tired is, I take it, temporarily only - but the infection has showed up twice during the fall, the second time even in a particularly painful form so Paul turned me into the hospital once more. After another convalescence period in Læsø I now feel better again, but I think you will understand that as long as my health is as unstable as all that I do not feel like leaving my base for an extended period. Now my doctors have started a medical treatment, possibly to be continued indefinitely, and for the outcome I am considering the winter term as experimental. Under these circumstances I have abolished my Australia plan, but I hope to be fit for a separate trip to the states som time in 1968.Plans for that we may talk over when you come over here.

The item analysis for more than two categories does seem to present considerable technical difficulties in spite of Erlings optimism when he was leaving. If I interpret your remarks on it correctly you are going to try an a priori assumption of one-dimensionality of the parameters. In some cases it may be a way out and in principle it could be generalized to assuming r=2,etc.

I am very anxious to know how it works, not least whether and how you get cheated when actually r=2, while working on the assumption that r=1. Simulations may be illustrative.

Did you ever catch a hold on the Birnbaum estimation that was "floating all over"? The symmetric generalization of Birnbaums logistic I thought of is the following:

January 25, 1967

Dr. George Rasch
Institute of Statistics
University of Copenhagen
Skt. Pederstraede 19
Copenhagen K., Denmark

Dear George:

What a wonderful warm experience to see your letter on my desk today.

When I didn't hear from you I feared that perhaps your health was still molesting you. I am very sorry to hear that that has been the case, but I have to confess that it is a relief to me that you are at least feeling as well as you seem to be now, and could get off a letter.

Since you are definitely going to be in Denmark this summer, I shall plan to come and visit you, bringing as much of our progress as I can along.

I am not just sure at the moment what organization my material will take, but I think I shall try to bring you examples of each thing we have done, and where a problem comes up try to bring together enough of a work out with it to give you something to think about. But I hasten to assure you right now that my main object in coming will be not to burden you with the details of the problems I encounter in applying your ideas, any more than you want to deal with them, but primarily to see you and take up whatever ideas you want to talk about.

As I look back over the last three or four years, while I realize that I have been a slow student, I must tell you that I have a growing, very real sense of progress in understanding what you have tried to teach me, and in developing a well founded point of view on measurement.

What have we been doing? Our main effort has still been on a very simple case of one parameter per person, one per item, one category of response. We have convinced ourselves that the very elementary item-wise iteration is the fastest method when there are more than 15 items. However, in order to get an asymptotic estimate of the matrix of covariances among item estimates, for the very last iteration we do the multivariate Newton method that Erling leads in. We have found that the standard errors of item estimates are very well approximated; in addition, by simulating replications on persons of fixed ability parameter, we have shown that our estimate of the standard error of ability estimates is also very close. At the moment we are troubled by a very slight bias in the ability estimate itself. This is where we take the person's score and from it, on the basis of the item estimates, make an estimate of his ability. We think the bias is due to the way we simulated cases, however, we want to go into that again in about a month and see if we can clarify the situation. Overall, however, things look very good. By enlarge the estimates are unbiased and the standard errors are well approximated.

In applying the method to some empirical data we were able to identify very clearly some items which had no place in the test. In this connection we are trying to work up some illustrative data showing that Rasch item estimates are more stable than the classical item estimates, when the standardizing sample is shifted.

We have also done some work on how you decide whether an item is conformable or not. At present we have three criteria we consider. One is the kind of chi square based on how well the observed data on that item and the easiness estimate for that item fit together. The second approach is to look at the slope of the graph of that item against the marginal over all items. The third method, which we are just getting into is to look at the stability of estimates for that item over score groups. The idea being that the more stable the estimates are the more the item fits in the set. To this end, any comments or suggestions you may have about discriminating bad items from good ones would be welcome.

We abandoned the effort to get the second item parameter for the time being but now, with the good idea in your letter, we may take it up again and see if we can get somewheres. All our efforts to deal with it the way Birnbaum proposed were fruitless.

Our second preoccupation continues to be how to deal with the case of more than two categories. We too decided that it was unrealistic to work on the hypothesis of one dimension underlying the various categories. Erling's full treatment of the problem is unmanageable even on a giant computer when there are more than five or six items. It is fantastic how the number of calculations and storage space necessary mount, as the number of items goes up to say, twenty, and the number of categories goes up to say, six. A little elementary arithmetic convinced us that we would never be able to solve any problems of that size with the algorithm Erling proposed.

As an alternative, we are sampling terms from the symmetric functions involved, and estimating the symmetric functions in this way. In principle, some of those symmetric functions have billions of terms in them, asking us to sample several thousands terms at random approximate the symmetric function in this way and make another round in the iteration of the item parameters. Of course we do not know yet whether this will work, but we like the idea and would benefit very much from your opinion of it.

About when to come to Denmark. If I am willing to come to Laeso to see you, do you care when I come? Or to put it another way, if you would tell me which times are most convenient for you, I would try to fit in with them.

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9th February, 1967

Professor B.D. Wright,
5835 Kimbark Ave.,
Chicago, Illinois 60637,
U.S.A.

Dear Ben,

May I suggest your coming to Læsø for 2 or 3 weeks beginning with say August 13. Alternatively any time during September, but then in Copenhagen. As regards the Læsø-plan it is essential for laying hands on a suitable livingplace that I quite soon get informed if and, if so, in what number you may come.

I am looking forward to having a look at what you have been doing; I dare say I am much concerned about the technical troubles. The computation of the χ_γ's and their generalizations to $m>2$ is really a nasty bit. Off-hand I have no notion as to how your sampling may work. Recently Bentzon (at the Serum Institute) suggested a mathematical investigation of them under various assumptions about the log ε_i's. He actually suggested a normal distribution of them or a Γ-distribution of the ε_i's themselves. I feel somewhat inclined to utilize the fact that the χ_γ's for logarithmically equidistant ε_i's, i.e.

$$\varepsilon_i = \varepsilon_0 \cdot \alpha^i , \quad i = 0, \ldots k-1, \tag{1}$$

can be expressed in a rather elementary way, but I have not yet studied the corresponding situation for $m>2$.

Another way out may be to work with items in small groups, to be supplemented with a direct method for chaining tests which would seem rather simple for $m = 2$. Leunbach is in the process of Algolizing it and hope to try it out in near future. For $m > 2$ I also think it works, but the details still have to be worked out. Anyhow it seems quite obvious that direct estimation of the ε_i's becomes untractable with increasing number of items so we have to look for something else.

A couple of years ago we considered the possibility of utilizing the analogue to Chapt. X,3 for $m > 2$. At that time we abandoned the idea as impractical. Recently I have reconsidered

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9th February, 1967

it and I think there is a point we missed by then. Of course the number of cases where category g in item i meets category $h \neq g$ in item j may not be large, even with many observations at disposal, but for a fixed pair of items there is a way of pooling all of the pairs (g,h) leading to a fairly simple estimation of the distance between the item parameters. The method is equivalent to using for $k = 2$ the conditional distribution of the a_{+i} vectors given row-marginals, having broken the data up into groups according to those, i.e. according to the $a_{\nu i} + a_{\nu j}$ - vectors, i.e. according to combinations of categories g and h. Each such distribution is binomial and the distributions are independent, the latter fact giving the possibility of pooling. The result is as follows:

Write

$$(2) \qquad \mathbf{e}_g = (0, \ldots \overset{g}{1}, \ldots 0),$$

$$(3) \qquad a_{\nu i} = \text{observed } \mathbf{e}_g,$$

$$(4) \qquad p\{a_{\nu i}\} = \frac{1}{\gamma_{\nu i}} \cdot e^{(\varphi_\nu + \psi_i)a^*_{\nu i}}$$

$$(5) \qquad n_{gh} = \text{no. of persons}$$

$$\text{with } (6) \qquad a_{\nu i} + a_{\nu j} = \mathbf{e}_g + \mathbf{e}_h,$$

$$(7) \qquad m_{gh} = \text{no. of those of them with } a_{\nu i} = \mathbf{e}_g.$$

Then ~~therefore~~

$$(8) \qquad \sum_{1 \leq g < h \leq m} (\mathbf{e}_g - \mathbf{e}_h) m_{gh}$$

is for given n_{gh}'s a sufficient estimator for $\psi_i - \psi_j$. Equating its mean value

$$(9) \qquad \sum_{1 \leq g < h \leq m} \frac{n_{gh} \cdot e^{(\psi_i - \psi_j)(\mathbf{e}_g - \mathbf{e}_h)^*}}{1 + e^{(\psi_i - \psi_j)(\mathbf{e}_g - \mathbf{e}_h)^*}} \cdot (\mathbf{e}_g - \mathbf{e}_h)$$

to the estimator gives the maximum likelihood estimate of the

March 14, 1967

Dr. Georg Rasch
Institute of Statistics
University of Copenhagen
Skt. Pederstraede 19'
Copenhagen K., Denmark

Dear Georg:

The news is better than good. It is marvelous. We are having surprisingly good success with the M>2 model. The pair-wise algorithm that you reminded us about is marvelously quick and surprisingly efficient. It will certainly serve as a most excellent starting point for any iterations to meet the maximum likelihood criterion. Maybe in some cases the pair-wise approach will be as good as the maximum likelihood approach unless one is willing to spend quite a bit of computer time improving the estimates.

In order to distinguish the work we are doing from what is ordinarily done, I am calling the easiness estimates "Rasch easinesses" and the ability estimates "Rasch abilities." I hope this use of your name is agreeable to you.

It is not only that you initiated this idea more than fifteen years ago, but there is a very important difference between the position that you take and that of Allan Birnbaum. Birnbaum sees the log model as just another possibility. He is quite willing to abandon it for other approaches. In contrast you say quite boldly and soundly that this particular model is <u>the</u> way to analyze this kind of data if the data will at all permit. In fact, sometimes you say that when a test cannot be analyzed by this model, this may raise questions as to its value as a test. I think your position, that this approach has characteristics which give it first priority is a very important one and I predict that the future will bear you out.

Summing up our current work:

We have coded and tested two different programs for estimating the parameters in the M>2 case. We also have a program which simulates data according to a 2 or 3 factor model, adds random noise and provides us with as many cases as we want of natural looking data but of a <u>known</u> structure.

We also have a principal component routine for factoring rectangular matrices so that when a matrix of estimated item category parameters is obtained, we can factor it into item and category component and evaluate its rank.

The first fastest and surprisingly accurate algorithm is the pair-wise approach. In this algorithm we cross tabulate the category responses for each pair of items, take the log of the ratio of symmetric cells, and average these logs over categories and items. The resulting average when normalized so that all marginal means are zero, forms a very good estimate of the generating parameters.

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26th October 1967

Professor Benjamin Wright
5835 Kimbark Ave.
Chicago, Illinois 60637
U.S.A.

Dear Ben,

Thank you for letters.

As regards the talk on October 28th I am afraid I can be of no assistance, because I have been away for a while - to a criminologists seminar and afterwards to Laesoe - and just returned. But the substance of the talk we did go over in August, I think.You are trying to popularize and that you have got to do your own way. Only the remark about "robustness" (p.21) is not quite tasty to me and also seems somewhat in conflict with your requirement of equal discrimination on p.20-21.

I am glad to hear that organizing next fall is progressing well. Your financial scheme would seem acceptable, also for bringing Nille with me.By the way, how is the tax situation?

As regards facts to brag with: To the list of my publications in English given in your ETS talk I can only add that "An informal report on a theory of objectivity in comparisons" has just appeared, though only mimeographed, in the proceedings from the NUFFIC international summer session in science on "Psychological measurement theory" held in The Hague, July 14-28th 1966; Ed. by L.J.Th.van der Kamp and C.A.J. Vlek, Leyden 1967.

If there is any point in mentioning the works of my cooperators you may recall the Erlings book of 1966 has an English summary and the same holds for Eggert Petersens thesis which is just about to come out. Furthermore there is Matthiessens paper on "Infant Mortality in Denmark 1931-60", Copenhagen 1965. Finally Stene has handed in a paper in German: Einführung in Raschs Theorie der psychologischen Messung, to appear in the proceedings of a psychological seminar held in Düsseldorff last March.

The institutions enjoying my consultations regularly are:

3/13/73

Notes on Georg Rasch

My teacher, colleague and friend, Georg Rasch was born in Denmark in 1901. During the 1920's he studied theoretic and applied mathematics, publishing a number of papers and finally earning a doctor of philosophy in mathematics at the University of Copenhagen in 1930 with a 200-page thesis on "Matrix algebra and its application to differential and difference equation". An advance treatment of matrix calculus anticipating by many years what developments in this country.

Georg Rasch got into statistics quite by accident. A medical friend asked him whether some curves he had observed of the resorption rate of cerebal spinal fluid in cats could possibly be hypobolic. Georg found them actually to be exponential and published a paper (about it) with his medical friend in a 1931 medical journal. This began a long series of inquiries, short course, consultations finally leading to a professorship in statistics at the University of Copenhagen and the directorship of the University Institute of Statistics.

Beginning in 1934 with the Danish State serum Institute, Georg gradually became, over the years, the chief statistical consultant to the Danish Military Psychology group (1952); the Danish Institute for Educational REsearch (1955); the Danish Institute for Research in Mental Hygiene, jobs he has held until his retirement last year.

~~Georg studied with Ronald Fisher in England, on a Rockefeller Fellowship in 1935 and 1936~~. Because of the spontaneous interest in statistics which his consultations in Denmark began in 1931, Georg went to England in 1935 and 1936 on a Rockefeller Fellowship to study with Ronald Fisher.

The discovery which brings him tonight, he made in February 1952 and so we are present at just past its 21st birthday. This discovery were recounted in the first chapters of his 1960 book. It is of the utility of the multiplicative Poisson distribution to characterize objectively the reading abilities of students and the difficulties of texts.

Georg attended the first international statistics institute in Washington in 1947. He was a visiting professor in the statistics department here at the University of Chicago in 1960 and a visiting professor in the education department in 1968.

Bangsbogårdsvej 3
DK 9940, Byrum
Denmark

Oct. 13th., 1978.

Dear Ben,

As you presumably already know and anyhow will realize from the enclosed copy of a letter to Sheila Berg I have indeed cooled down to acquiescing to a reprinting of Prob. Mod. - such as it is! (possibly after some misprints and the like have been removed).

Which does not mean that today I am satisfied with the book. On the contrary, already years ago I played with the idea of rewriting it completely in view the develloppments both as regards access to electronic facilities and as regards the theoretical foundation, since 1960.

However, lecturing in Australia, on what ~~by then~~ I knew by then, and at the same time receiving from my young friend Kåre Borchsenius very advanced contributions to the theory of specific objectivity, convinced me that my primary obligation for the next few years must be to concentrating upon the specific objectivity, clarifying its foundations and demonstrating how it lies at the bottom of general statements with well defined fields of validity. - In which connection I may have to return to the weakest part of the book, viz. chs. VI and VII.

But even if a revision of these chapters - and, in fac some moresections - are indeed desirable, I don't think that ch. VI will do much harm, because nobody will use its technique any more - to somebody it may even turn out to be a challenge to establishing as detailed and effective model controls on a theoretically firm basis.

Ch. VII is a different and deeper problem.

Its real limitation lies in starting from pretending that it is known what "measuring" is, not only of accelerations - which are the reactions to be observed - but also of mass and force which in fact should be derived from the structure of the reactions. That such derived measures might have to be confronted with directly defined measures of mass and force is a further complication that presumably would require supplementary (thought-) experiments.

All of which I did not realize in 1960 - neither did Maxwell in 1876, nor did Newton some 200 years earlier.

So far, so bad, as regards what I attempted to do in ch VII. But today the situation has changed to the worse: even direct measurements leave a lot to be desired as regards its conceptual clarity. That this is so, is a rather recent discovery of where the usual axioms of measurement come from.

That current theory of measurement looks like the mathematics of real numbers may be very convenient, but if that were a necessity then it should be derived from something more serious than a pure analogy. And in particular the attempts at generalizing to multidimensional measurement so far have seemed to end up with purely mathematical ingenuity, without any necessity.

What to do, then?

Well, the theory already developed for comparing object with specific objectivity within a framework where the objects react to contacting a set of agents can be applied directly to a situation where the set of agents is the same as the set of objects, thus providing for objects being "compared" by means of contacts with other objects, that, I think, is what lies at the bottom of "direct measurements".

Realizing this is, however, so recent a discovery to me, that you will not even find it in the reprint that I am anxious to send to you*), but it can in fact be derived from it.

My reason for at all mentioning this matter here is to make clear that even a revision of Ch VII would not be feasible within the framework on which the whole book has been built up.

And this is my final argument for leaving the book as it is!

Writing the next book I have started - finishing it will take some time.

*) Last bulletin: The Danish Philosophical Yearbook, t. 14, 1977 is hoped to be out before Xmas. - Reprints

To this end a meeting with "Specific Objectivity" as its main subject would be a strong impetus, also if the one you have suggested is limited to a psychometric circle.

A further discussion of "the conditions on which I could be tempted to join it" seems - if I interprete your hints rightly - to presume my receiving a letter from somebody in Washington, which as yet does not appear to have materialized on any of my adresses.

To all these very technical matters I may add that both Nille and I enjoy being 'pretty well preserved'. As a matter of fact we have

1) ~~passed~~ celebrated our golden wedding in february

and recently

2) passed ~~of~~ our 77'th birthdays.

And are still going strong - though with such minor limitations as are our respective ordeals.

If the meeting ~~suggested~~ ~~in~~ becomes a reality I think that Nille will be happy to join us - she has from her youth kept a soft spot for South England - What about Claire, could she also be persuaded? And if so, what about ending up with a visit to Lasø?

With our joined love to both of you from

Nille & Georg.

P.S. To the matter of what you may need for the "Introduction" I shall return shortly. G.

Bangsbogårdsvej 3 Oct. 3., 1978.
DK 9940 Byrum
Denmark

Dear Sheila Berg.

Thank you very much for your clarifying interpretation of the Licensing Agreement, which has removed my former concern.

Accordingly I enclose the signed Agreement, to which, however, I have added a request for my approval of the Introduction by Ben Wright, the content of which still has to be considered.

It is of course unavoidable that the book, having been used for 18 years, should now be outdated in several respects, what with electronics, but also as regards theatrical progresses. None the less the Danish Institute for Educational Research still gets a number of requests for it every year. – Well, one of my young friends may be right when telling me: "In that book you have written many things that you have said elsewhere."

Anyhow, whichever shortcomings it – as of today – suffers from, straightening them out must await some future opportunity.

I did have quite some difficulty in recovering the three subjoined fresh copies of the Work in question, but by promising the owners compensations by copies of the new editions I succeeded in persuading them.

Which brings me to an additional question: Do I get a moderate number of free copies, or may I buy some at favourable price?

Finally a technical matter; Over the years I have located a number of misprints and minor mistakes, to which Ben and others may add some. Would it be feasible to have them removed?

Looking forward, then, to the reproduction of my apparently still "good old work", I am

Sincerely yours

G. Rasch.

cc. Ben Wright.

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Dear Ben

Referring to your last letter I must disappoint you. What you indicate about my dawning realization of how a proper analysis of testdata should be carried out is a phantasy of your own which has no connection whatsoever with realities!

I did do something in 1947 that shook the psychologists and educationalists in Scandinavia - so much that one of the leading Danish newspapers (Berlingske Tidende) brought an extensive interview with me (a full page). My main point was obvious, even though I had no better data than the raw scores per person (some 1200 conscripts) in a very mixed sort of intelligence test (an "omnibus test") selected from a large Swedish study of intellectual achievements in various directions of children at different ages.

Benefiting from that the psychologist E. Rubin & E. Tranekjær Rasmussen, then heading the just established (in 1944) course in Educational Psychology, undertook the construction of a new Danish Intelligence Test, which should be ready when the negotiations about establishing a section of psychology within our Defense had been finished. As a teacher in statistics at the said course I acted as a statistical consultant for the group, constructing and trying out the new intelligence test.

But mind you: By then I had no previous experience whatsoever in test psychology and knew about "standardization" only what I could read in existing textbooks. An a narrow time limit.

So I had to start from sample rawscores from a test that on purpose was composed of items covering as large a variety of intellectual performances as at all feasible - a real omnibus test!

Under these circumstances I could do no better than to throw such light on the raw score distribution as the data offered. And they did offer some external data for each person, viz.:

a) Living place while growing up: Capital, towns, rural districts.

b) Father's occupation (ranging from manual workers to bankers and professors).

c) Own school education (4 groups) from 4 forms or more.

What I found was to begin with what is found everywhere:

1) Growing up in the capital leads to higher raw scores than growing up in other towns, which again was better than growing up in rural areas;

2) Better social conditions when growing up gave better results than more poorly conditions;

3) Better educations lead to higher intelligence scores than poor education.

Wellknown and with obvious comments! But when the criteria were intersected then the picture changes completely. In short:

<u>For any given education the distribution of the raw scores is completely independent of both the social and the geographical criteria!</u>

A result I by then was not able to trace in the literature then available to me.

How well known is it today?

So <u>that</u> was what happened to me in 1947.

After that I pursued my teaching in statistics to educationalists and to groups of mathematicians and actuarians - in the spirit of Laplace, Kapteyn and R.A. Fisher till the end of the fifties, when I began to master my own ideas.

But parallel to that I worked as a consultant to the group of military psychologists (since 1952) and the Danish Institute for Educational Research (since 1955). Apart, of course, from free lance work in almost every field of experimental and observational research.

But if you wish to know how I came across the famous Multiplicative Model for Dichotomic Responses (MMDR), I can tell it quite precisely, and it had in fact nothing to do with item analysis, but was a pure mathematical byproduct to the first discovered multiplicative model, viz. the one linked to the Poisson distribution!

You know about the problem I was faced with on my return from India ab. February 1st in 1952: having to follow up the development in reading ability in a number of individuals, tried out through some years with different reading tests (based on records of both misreadings and reading speed).

A purely selfish hope for mathematical simplicity conducted my choice of statistical tool to the Poisson distribution, whose parameter then should characterize both the reader and the text. Mathematically it was obvious that a very nice theory would come out of it, if that parameter happened to be a product of one parameter for the person and one for the test.

My goodness - it did work in practice!!

But having tried it out over some months I became again mathematically infatuated:

How the deuce could such a peculiar sort of Poisson distribution come into existence?

Poisson (ab. 1838) himself stumbled upon his version of it as a limiting case of the binomial distribution with large n and a parameter $\rightarrow 0$, inversely proportional to n. Much later, I think, the proof was extended to cover the sum of many small independent random variables, provided the mean value of the sum has a finite limit as the number of terms $\rightarrow \infty$, who first did that I really don't know.

However, if a machinery like that should lead to the MPM for the number of reading mistakes, then we must go back to the single words of a text, each of which is rather easy, but all of them - or at least a majority of them - totalling to something not at all negligible and even characteristic of the text as a whole.

Well, this <u>can</u> be specified in an infinity of ways, but it would seem an obvious presumption that each word has its own probability of being misread by a given person. And so I was driven right down to specifying probabilities for dichotomies with parameters referring to two distinct sources: the person and the word - a point of view that had never occurred to me before. And furthermore, when the words were lumped together to make a "text", then - according to my empirical studies - the probability of so and so many words being misread ought to be a parameter of two factors, one pertaining to the person, the other to as much of the text as was read.

This, I shall think, could hardly be achieved unless the parameters $\lambda_{\nu i}$ (ν= person, i = word) of the person's probability of misreading word No. i of the text, written on the always permissible form

$$\frac{\lambda_{\nu i}}{1+\lambda_{\nu i}}\ , \quad \lambda_{\nu i} \geqq 0$$

was assumed to be the product of a person factor and a word factor, i.e.

$$p\{+|\nu,i\}=\frac{\xi_\nu \varepsilon_i}{1+\xi_\nu \varepsilon_i}\ ;\ p\{-|\nu,i\}=\frac{1}{1+\xi_\nu \varepsilon_i}$$

There you are: my discovery was a somewhat intuitive achievement, but wholly within my own mathematical playground - with no relation to any actual item analysis problem!

Of course, afterwards I immediately realized that I might have stumbled upon a tool for formalizing, thus handling such problems, and as soon as my substantial report on the reading retarded children had been delivered - 7 months after my return - I made a temporary analysis of accessible data on Raven's matrix test, which I found on the whole beautifully represented by the new model. And then I returned to the 1947-data and realized that they did not at all agree with the model! But on sorting the - by intention - extremely inhomogenous test according to subject matter things looked much better within each "subtest" - though not as good as for the Raven test.

This work - carried out as a consultant to the newly established group of military psychologists - I could present to the head, Poul Borking, of that group, who immediately saw the significance of my discovery, called in a fellow psychologist, Børge Prien - whom I already then knew very well - and assigned the following task to him: constructing four subtests covering quite different subject matters, i.e. requiring different fields of intellectual activities, each subtest fulfilling the demands of the new model as well as at all possible, having it ready for use - i.e. a table for transforming raw scores to model-measurements for each subtest. All of it ready for being printed exactly 6 months later! Because it had to be used for intelligence testing of the several thousands of conscripts - with a view to selection and distribution of the selected to different units within the defense - starting November 1st. Børge got a staff of young psychologists and students, with me as statistical adviser through the wealth of analyses required. - I dare say we worked fast and fine in 1953! And this was before the era of electronic computers - paper and pencil all of it - and of course a small electric computer!

Well, for computing and evaluation I could by then not offer anything better than a primitive paper and pencil procedure - the outcomes of which you can read about in Prob. Mod., the material of which largely stems from 1952 and 1953. - Not until 1958/59 when I had to write the book, was a mathematical theory for the item analysis worked out.

Well, friend, this is the real story about how the Multiplicative Model for Dichotomic Responses came into existence - so please, drop your nonsense about 1947. And consider how erratically a human mind may work!

The information offered here you may use at your own discretion (but no misrepresentation!) at any opportunity you think fit.

And now to the next letter to you, hopefully reaching you in another couple of days.

Yours as ever,

Georg

Dear Ben

We got this letter handwritten from Georg who is not able to leave his island of Læsø, and thought it appropriate to take the time needed to type it - thus delaying it ab. 1½ month. We hope the increase in readibility compensates.

Yours

Gustav Leunbach

Review of the cooperation of professor B D Wright, University of Chicago and professor G Rasch, University of Copenhagen

In the spring term 1960 GR, as a visiting professor at the Department of Education of the University of Chicago gave a series of lectures based upon a monography he by then was just giving the finishing touch. In this book new methods in theory and practice of psychological measuring were developed, and later on they were carried over to Social Sciences at large until ~~in the end~~ they ended up as a very general methodology covering both Social and Natural Sciences - a field which he, now having retired as a professor, is able to concentrate upon, thanks to an ample grant from the National Council of Social Sciences.

It goes without saying that such a development requires contacts in in many different directions, and in this regard the cooperation with Dr. B.W., professor in psychology and education, has given much inspiration to and been of great practical use for GR.

This work, which was initiated while he attended the teaching of GR in 1960 largely goes in the following three directions.

The methods of BR were developed at a time when the era of electronic computing had barely been heralded in this country, the access to such facilities thus being very limited. In contributing to the development of the techniques of electronic computational for the models in question and in furthering their utilization BW carried out a very great and most useful work, which of course can only be duly recognized by those who realize how forbidding the computational obstacles looked at that time. In continuation of this work he cooperated with a number (11) of universities and other (8) big institutions in developing programmes designed for their special problems and adapting them to their particular electronic equipments.

Nextly he has himself been active in applying the said methods in diverse fields within the area of psychological and educational testing ~~and~~ also by inspiring others to do so, with the result that by now we are looking forward to a comprehensive evaluation of the applicability of the methods in this whole area.

However, this ~~whole~~ development was only possible due to his extensive teaching and giving speeches on the matter. In his own faculty he has given regular courses on the said methods since the spring term 1967. Talks to large ~~assemble~~ gatherings he has given some ten times since 1965, some of them in special sessions on GR's ~~sess~~ methods at ~~annual~~ periodical meetings of psychological and educational societies. Special mention deserves his organization of two 5-days training courses in 1969 and 1970, sponsored by the Governmental Department of Education, on which occasion some hundred psychologists and educationalists from all over the States and Canada were introduced to the elements of the theory and practice of GR's methods. ~~And at present he and GR are planning two visits of longer duration (5-6 months) to Denmark in order to produce ~~publication~~ substantial publication~~

On the whole, since his first visit to Denmark in 1964 BW has practised an almost unbelievable activity in this field, and results have certainly not been lacking. Thus already 6 dissertations have been produced – 2 of them under his direct guidance – and furthermore several leading institutions in educational psychology and testing are now, under his advice, turning to constructing their test batteries in accordance with consequences of GR's theories. In between he himself has published, in part together with with students of his, three significant papers on the topic. And at present he and GR are planning during one or two visits of longer duration (5-6 months) in 1973 and/or 1974 to produce a substantial paper on the comprehensive study indicated above. This plan may be seen as a climax of their cooperation during four shorts stayings over here (1964, 65, 67 and 70) and during a second period of GR as visiting professor at UC (1968/69).

June 18, 1972

GR.

Copy of a signed letter is to [illegible]