A Summary and An Evaluation of Selected Experimental Placement Investigations in Elementary School Arithmetic

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A SUMMARY AND AN EVALUATION
OF SELECTED EXPERIMENTAL PLACEMENT INVESTIGATIONS
IN ELEMENTARY SCHOOL ARITHMETIC

A Thesis
Presented to
the Faculty of the Graduate School
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by
Joseph Ernest King
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CHAPTER I
THE PROBLEM AND DEFINITIONS OF TERMS USED

The educator of today confronted with the antipodal philosophies of Essentialism and Progressivism seeks to reconcile these viewpoints: the former with its roots sunk deep in the philosophies of Idealism and Realism, characterized by its set body of subject matter and specified methodology of formal drill; the latter stemming from Pragmatism and Naturalism, reputed to adhere to a curriculum constructed in-the-situation and to advocate adhesion to the child's inner urge. Emphasis, thus, for the Essentialist is subject-centered; for the Progressive, child-centered. How is the modern educator to reconcile these two antithetical viewpoints?

I. THE PROBLEM

The present investigation by summarizing and evaluating the extant experimental research on the placement of the arithmetic fundamentals has attempted to reconcile the Essentialist and the Progressive, at least in this one field. The viewpoint adopted was bi-polar, that is, the writer attacked the problem with the foreknowledge that he was dealing not only with a specified body of subject matter, but also with a group of children, each individual of which was at a definite readiness level. Thus according to the Essentialist viewpoint, the writer maintained that the various items in arithmetic, and in the other subject matter fields, possessed an intrinsic physical, mental, social, and emotional difficulty; in accord with the Progressive, it was held that each child followed a physical, a mental, a social, and an
emotional maturational sequence,\(^1\) during which development he was ready at certain times for a particular item of subject matter.

**Statement of the problem.** Placement of the arithmetical processes—a set body of subject matter with an intrinsic difficulty—at certain levels—each child having a fourfold maturational readiness—seemed to embody a practical application of the above theory. Thus, summarization and evaluation of the quantity and quality of the arithmetic research undertaken in reconciliation of this bi-polarity became an important and worthwhile task. Such was the undertaking of the present piece of research.

**Importance of the problem.** The present study was deemed important for two reasons: first, because of the nature of the problem itself; and secondly, because of the consequence of the principle of maturational readiness.

The amount of educational research accumulated during the four decades since 1900 was overwhelming. In general, the pragmatic viewpoint of educational science as a collection of unrelated facts and hypotheses had been adopted, and little attempt was made to synthesize new findings with past discoveries. Thus the student of education, considering the research on a single topic, such as placement in arithmetic, found numerous contradictory

\(^1\) The four categories of physical, mental, social, and emotional maturational have been employed for the sake of convenience. Upon analysis, these categories were subdivisible many times and only logically distinguishable. Under physical was understood the development of the various sensory organs and the motor skills; under mental, the inherited capacity plus the environmental background; socially, the child experienced certain contacts in a socio-economic surrounding and felt certain needs; emotionally, the child had certain attitudes and interests, the latter being greatly influenced by the methodology used in the presentation of the lesson.
bits of evidence. As noted by Brownell in the *Journal of Educational Research*:

In any event the curriculum worker has not solved his problem by locating relevant research. His obligation is not the one which is usually stressed, namely that of translating research into school practice. He has a more fundamental obligation, that of first determining what research, or which research, if any, to accept. Laudable as it is in motive, his eagerness to advance the cause of science in education by honoring its products is apt to be harmful in its consequences. He may be guilty of fostering error, of perpetuating mistakes. All research is not equally good. It is not even all good. Some of it is misleading; and some of it is bad. The printed word, especially when supported, or better, accompanied, by tables, statistical constants, and graphs still carries undeserved prestige.2

The present investigation, therefore, attacked this problem and attempted to summarize and evaluate the extant research on arithmetic placement in such a manner that it would be usable to the educator in any of his various capacities.

The major importance of the present investigation lay, however, in its attack upon one phase of the problem of maturational readiness. Experimental work in this field had largely been confined to the learning of motor skills in infants and to the initial stage of reading. Educators, however, were realizing that each item of subject matter had a particular readiness level. In his "Introduction" to the *Thirty-Eighth Yearbook of the National Society for the Study of Education*, for example, Washburne stated:

In fitting the curriculum to the level of a child's development, then, we should know the relation of any given unit of learning or of any experience to the child's physical development, his mental age, and his experiential background. We should reckon with his sense of need and

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should gauge the suitability of the material or of the mode of its presentation in terms of the child's interested response.3

Thus, training at the proper maturational level was tending to eliminate the excessive amounts of drill and *memoriter* learning which had characterized the Traditional school and which had been so vigorously attacked by the Progressives. By presentation of material at the proper maturational level of the child, failure and its accompanying feelings of inferiority were being reduced. The child being ready for a particular item of subject matter was attacking it successfully, thus experiencing satisfaction, encouragement, and growth in the proper direction. The classroom teacher who was unaware of the principles of placement and of maturational sequence was tending to antagonize the child and to create in him emotional disturbances toward subject matter and school in general. Knowledge of and action in accord with the psychological principles of development, however, were resulting in proper adjustment of the school to the child and of the child to the school.

II. DEFINITIONS OF TERMS USED

*Selected.* The investigations employed for analysis were those appearing in the yearly summaries by Buswell in the *Elementary School Journal* together with those found in the *Review of Educational Research* under the issues entitled "Curriculum" and "Psychology of the Elementary School Subjects" and dealing with the fundamental processes of addition, subtraction, multiplication, and division in arithmetic as regards whole numbers and

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fractions.

Experimental. Subject matter placement in the past was largely the result of current practice and the judgment of specialists and was based generally on deductive assumptions as to what should or should not be taught. Such placement took into consideration such principles as approaching the material from the psychological to the logical, from the near to the remote, from the past to the present, from the specific to the general, from the simple to the complex, and so forth. Rarely were these principles verified experimentally as to their appropriateness in relation to child development. The present investigation, therefore, attacked the problem of placement from one viewpoint only—location of material in accord with experimentally verifiable techniques and scientific thought.

The optimum technique for placement studies, however, was seen to be a rather individual one; this specificity of method was one reason why so few placement investigations were found in the field of arithmetic. Upon analysis, the procedure seemed to be composed of several steps: (1) a normative survey to determine the present placement of a particular topic; (2) the teaching of this topic both above and below the typical placement; (3) adequate measurement of the physical, mental, social, and emotional maturation of the learners prior to, during, and subsequent to the teaching period; and (4) the establishment of criteria of placement in relation to the subjects' hygienic development of their physical organs and the economical and efficient learning of the motor skills; in regard to their acquisition of the material in a meaningful fashion, thus presupposing such material to be intellectually
within their grasp; in proportion to the building up of their experiential backgrounds in a related and coordinated manner; in reference to their experiencing of contacts and situations for which they had a felt need; and finally, in respect to the realization and broadening of their interests and to the development of a wholesome attitude toward the school, the teachers, and the material which was being learned.

By definition, therefore, the only studies that should have been considered were those which measured up to the above criteria of an optimum placement investigation. However, even the research of the Committee of Seven, which was then considered the classic in the field of arithmetic placement, was slightly deficient in the fourth requirement. Thus, the writer deemed it advisable to summarize and evaluate certain studies, which, though they did not measure up to the optimum standards, were nevertheless significant contributions to the field of arithmetic placement. In this connection were noted the various investigations indirectly concerned with placement: error studies, analyses of the difficulty of learning various topics, methodology studies--all these contained potential placement data. However, in general, such investigations failed to provide sufficient statistics to permit the reader to form independent placement conclusions. Hence, only the more representative and pertinent of these researches were analyzed.

III. ORGANIZATION OF THE REMAINDER OF THE THESIS

After the reader is afforded an introduction to the field of arithmetic placement through a review of the present placement literature, he will be carried successively through the criteria which were employed in evaluating
the placement studies, through a summary of such investigations, through the actual evaluation of the more representative of these researches, and finally through the conclusions and recommendations which the writer considered appropriate in view of his findings.
CHAPTER II

REVIEW OF LITERATURE ON ARITHMETIC PLACEMENT

Now that the reader has been oriented as to the problem and its ramifications, it is appropriate to consider the extant literature in the field of arithmetic placement. Such literature was found primarily in three sources. The more extensive treatments on the curriculum generally devoted a few pages to the present status of placement in the various subject matter fields; various volumes of educational society yearbooks were given over either wholly or in part to arithmetic and considered in this presentation the placement of topics; thirdly, magazine articles have appeared which attempted to digest the research at particular levels of the maturational process. It was interesting to note that in almost all of these reviews, the foremost aim of the author was summarization with little concern for evaluation of the research considered.

I. PLACEMENT STUDIES IN CURRICULUM TEXTBOOKS

Discussion of placement studies received little space in curriculum textbooks. Caswell and Campbell deplored the present condition of placement studies, not only in arithmetic but in the various other subject matter fields as well:

The number of experimental studies that bear on grade placement is surprisingly small when considered in light of the emphasis that has been placed in the past quarter of a century on scientific procedures. There have been several limited studies dealing largely with skills and a very few that are more comprehensive in nature. When considered as a whole, however, investigations in any subject cover such a small part of the subject that they throw relatively little light on the problems of
grade placement in the large.\textsuperscript{1}

Their treatment of the actual situation in arithmetic was in accord with the above statement. Even though written in 1935, the book mentioned only the investigations of Taylor, Haggerty, and the Committee of Seven.

A more detailed analysis of the work of the Committee of Seven to the neglect of the various other placement studies was that made by Norton and Norton in their \textit{Foundations of Curriculum Development}.\textsuperscript{2}

\section*{II. PLACEMENT STUDIES IN EDUCATIONAL SOCIETY YEARBOOKS}

The \textit{Third Yearbook of the Department of Superintendence} investigated the curricular problems in arithmetic and their scientific solution.\textsuperscript{3} It was interesting to note that of the seven topics investigated by the Committee only one applied to placement—When should formal arithmetic begin—and, in this connection, only three studies were reviewed. Such a treatment only accentuated the recency of the concept of readiness, and thus the paucity of research in this field.

This same note was sounded in the \textit{Twenty-Ninth Yearbook of the National

\begin{thebibliography}{9}


\bibitem{3} Guy M. Wilson, "Arithmetic," \textit{Third Yearbook of the Department of Superintendence of the National Education Association}, 1926, pp. 35-110.
\end{thebibliography}
Society for the Study of Education.\(^4\) In Brownell's analysis of the techniques of research, there was not found one controlled-group study of grade placement.\(^5\) Similarly, in the chapter on the survey of previous research, only three pages were accorded to psychological studies, the majority of researches mentioned being inapplicable to the field of placement.\(^6\) The major contribution of the Twenty-Ninth Yearbook was its presentation of one of the first detailed accounts of the work of the Committee of Seven.\(^7\)

The most valuable contribution to the concept of maturational development was that furnished by Part I of the Thirty-Eighth Yearbook of the National Society for the Study of Education. Entitled "Child Development and the Curriculum," the yearbook was written under a threefold philosophy: it concerned itself first with the development of the child; then with data on the curriculum; and thirdly, with the reconciliation of the two previous viewpoints by means of an appraisal of present knowledge of the relation of


the curriculum to child development and of the methods of investigating the problem. A chapter by Brueckner on the "Development of Ability in Arithmetic,"\(^8\) and one by Washburne on the "Work of the Committee of Seven on Grade-Placement in Arithmetic"\(^9\) were the studies pertinent to this investigation. Brueckner's chapter contained an excellent summary and in some instances evaluation of the procedures in computational and in social arithmetic. Washburne assembled the various studies pro and con to the Committee of Seven investigation under one head, resulting in an excellent summary and evaluation of what that body was accomplishing.

### III. PLACEMENT STUDIES IN MAGAZINE ARTICLES

Various magazine articles appeared since 1930 which concerned themselves either with a summary of the entire field of arithmetic placement or with a digest of a particular part of that field.

A most comprehensive summary and evaluation of the child's number ideas at the period of readiness for arithmetic and at the initial stage of learning was that made by Foran in the *Catholic Educational Review*.\(^{10}\) The more important studies in the field were analyzed and a summary of the status

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10 T. G. Foran, "The Early Development of Number Ideas," *Catholic Educational Review*, XXX (December, 1932), 598-609; XXXI (January, 1933), 30-44.
of the child at these growth periods was given. Buckingham presented a
similar summary of the investigations regarding the time to begin the teach-
ing of arithmetic;\textsuperscript{11} Buckingham's study, however, was not as comprehensive as
that of Foran and tended to deal more with the research performed by himself
and MacLatchy.\textsuperscript{12} Woody, likewise, spoke of the various investigations found
at the readiness stage of learning arithmetic.\textsuperscript{13} Brownell summarized and
evaluated the researches concerned with arithmetic readiness at the various
stages of maturation.\textsuperscript{14} Finally, articles by Washburne, Raths, and Brownell
marked the development of the investigations carried on by the Committee of
Seven.\textsuperscript{15}

The literature on the topic of arithmetic placement reflected the
actual status of the research itself in its sparcity. The majority of
reviews concerned themselves with the teaching of arithmetic at the initial
stages of learning or were commentaries on the work of the Committee of Seven.

\textsuperscript{11} B. R. Buckingham, "When to Begin the Teaching of Arithmetic,"

\textsuperscript{12} Cf. post, pp. 44-52.

\textsuperscript{13} Clifford Woody, "A General Educator Looks at Arithmetic Readiness,"

\textsuperscript{14} William A. Brownell, "Readiness and the Arithmetic Curriculum,"
Elementary School Journal, XXXVIII (January, 1938), 344-54.

\textsuperscript{15} Cf. post, pp. 94-106.
CHAPTER III

CRITERIA EMPLOYED FOR EVALUATION OF ARITHMETIC PLACEMENT RESEARCH

The most difficult and the most frequently deleted task in the consideration of research is the statement of criteria. The present investigation attempted to analyze the research pertinent to arithmetic placement in terms of three major categories: (1) the problem, (2) the method, and (3) the results. Each of these classes was then subdivided into more practical and workable topics.

I. CRITERIA AS REGARDS THE PROBLEM

As stated by Anderson in his standards for the evaluation of curricular studies, the problem was to be significant, designed so that it produced meaningful results and enabled the relative weight of the various factors to be determined, and so set up that it was supported or refuted on the basis of the data collected.\(^1\) The editorial board of the Journal of Educational Research required that the problem be stated clearly and concisely and that its importance be explained. However, "it is not sufficient merely to formulate the problem briefly in the form of a question or declarative statement, but rather detailed definition and delimitation are necessary."\(^2\) Monroe and

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Engelhart elaborated upon the factors influencing the definitions of the problem:

To define a problem means to specify it in detail and with precision. Each question and subordinate question to be answered is to be specified. The limits of the investigation must be determined. If certain assumptions are made, they must be explicitly noted.

The definition of the problem affords a basis for the subsequent phases of educational research. It is the guide for the collecting of data. The data are to be analyzed, organized, and summarized so as to be most useful for answering the questions specified in the definition of the problem, and the conclusion is merely a statement of the answers resulting from the investigation.3

Thus, in the researches to be evaluated, the writer considered one of the criteria to be a clear, concise, and consistent statement of a problem significant in the field of arithmetic placement.

II. CRITERIA AS REGARDS THE METHOD

The method employed in carrying out the piece of research was to measure up to the standards of validity and reliability: validity, in that the method was appropriate to the problem under consideration and that the materials, processes, and procedures employed achieved what they purported to achieve; reliability, in that the method employed isolated the variable as completely as possible.

Validity of the method. The primary criterion under the validity of method was that of appropriateness: "Is the method of research employed in the investigation appropriate to the problem studied?"4 Thus, the investigator


4 Editorial Board of the Journal of Educational Research.
should have considered singly or as a whole the factors that influenced placement of subject matter—physical readiness, mental readiness, social readiness, and emotional readiness—measuring these with appropriate instruments. In the Thirty-Eighth Yearbook, Anderson called attention to the experiential factor of mental readiness: "Have the prerequisites of the skill in question been determined, including the effects upon it of incidental practice and of indirect stimulation? Are measures available of the information and skill level of the children prior to the experiment?"  

The devices for measurement employed in the experiment were to be valid, that is, the coefficient of correlation between the scores on the instrument used and the outside criterion should have been significant. Since the device under consideration was generally valid for a certain purpose under a certain set of conditions at a certain level, it was necessary that the materials employed were appropriate to the subjects, to the experimental method, and to the conditions under which the experiment was conducted, such as the time available and the qualifications of the persons who used the materials. However, as noted by Monroe and Engelhart:

Unless some unusual achievement is specified or implied, most tests designed to measure calculation skills are probably of rather high validity. They, of course, measure the current ability of pupils rather than the permanent residue of achievement. It is likely that the latter type of achievement should be considered, but few, if any, investigators have attempted to base their conclusions on it. Consequently, the present writers have not applied this more severe test in their evaluations. When


the achievement to be measured includes abilities other than calculation skills, the validity of the measures is an important matter, but it is very difficult to determine the degree of validity.

The selection of subjects to be used in the experiment should have been valid, that is the subjects were to be appropriate to the experimental methods, to the tests employed, and to the experimental factors. Thus, in investigations with control groups, the sampling of children in both the control and the experimental groups should have been similar, and the characteristics of the pairing or sampling procedure with reference to such factors as age, sex, socio-economic status, mental age, and school history were to be adequately presented. If the results of the investigation were to be of value to others, the report should have contained a concise, objective description of the subjects used.

The conditions, location, and time elements under which the experiment was conducted were to approximate those under which the results of the experiment were to apply. "This representative character may be planned with reference to parts of a city, parts of a country or state, parts of the United States, etc." And as stated by Anderson:

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8 Bixler, *op. cit.*, p. 16.


10 Editorial Board of the *Journal of Educational Research*.

Where the same experiments are conducted at different age levels, has the time factor or the amount of practice been controlled? When, for instance, children eight years old are compared with six-year-olds in the learning of reading, is a careful check made of the amount of time that both groups of children spend, either formally or incidently, at reading?\(^1\)

Again, an appropriateness of the environmental conditions to the experimental method and to experimental factors was demanded.

The processes employed, whether survey testing, the one-group, the parallel groups, or the rotation group techniques should have been valid.

The one-group experimental method is valid where the change produced by an experimental factor is not conditioned significantly by any preceding factor, and where the change effected by each experimental factor is measurable in equal units; the equivalent-groups method is valid where it is possible to equate groups; and the rotation method is valid where the change produced by an experimental factor is not conditioned significantly by any preceding factor.\(^3\)

For example, as regards motivation, the motivation of the children in the special group under consideration was to be controlled, or, in case a control group was used, the motivation should have been constant for both groups.\(^4\)

And again, in the curricular criteria as stated by Anderson:

Have valid measurements of the skill in question and of related skills been made at the beginning and at the end of the practice period? Have such measurements been made after a period of time, in order to determine the permanence of effects and the amount of review or practice necessary to reestablish former levels of skill?\(^5\)

\(^{12}\) Anderson, op. cit., p. 411.

\(^{13}\) Bixler, op. cit., p. 15.

\(^{14}\) Anderson, op. cit., p. 411.

\(^{15}\) Loc. cit.
It was not ordinarily sufficient to indicate the materials, processes, and procedures under investigation by name. The thing under investigation needed careful and objective definition.16

Finally, as regards validity of methodology, the tabular, graphical, and statistical analyses should have achieved the function they set out to achieve. Thus, the tabular and graphical means of analysis used in the investigation were to be the appropriate ones, and the statistical methods employed should have been applicable to the materials in hand.17

Reliability of the method. By reliability was meant that the "total net change in the trait or traits in question produced by irrelevant factors must be negligible, or the amount of such change must be measured and discounted by the application of a control experimental factor."18 The devices used for measuring, therefore, should have been free from such variable factors as sampling errors, subjectivity of scoring, inadequacy of directions, faulty administration, and so forth.

The selection of subjects representative as to number, age, sex, grade, intelligence, and so forth was to be controlled by a well-thought-out plan of sampling.

When the composition of the population is to be studied is known, the sample should have been selected as to include all the essential elements;

16 Editorial Board of the Journal of Educational Research.

17 Loc. cit.

18 Bixler, op. cit., p. 15.
when the composition of the population is unknown, the experimenter should select at random sample groups and continue selecting groups until comparison of the samples taken reveals a definite tendency for variations in one direction to occur as frequently as variations in another direction.19

Thus, the sample of children used was to be both adequate in size and typical in its selection. If such was not the case, the description of the sampling should have been presented in such a way that the results might be interpreted in the light of the sampling or compared with other studies. The description of the sample was to include at least chronological ages, mental ages, sex, grade location, and socio-economic status.20

The environmental conditions under which the experiment was carried out were to be kept constant, and the time of the experiment should have been analyzed as to its significance.

In deciding upon the time length of an experiment, the principle to be kept in mind is that one should aim to secure the maximum effect of the experimental factors with a minimum effect from irrelevant or variable factors.21

The processes employed were to be selected and sufficiently controlled in regard to such factors as might have materially affected the results of the experiment. The more important of these factors were the following: instructional techniques, skill of the teacher in using the instructional techniques, zeal of the teacher, personality traits of the teacher, instructional materials, time spent in learning activity.

19 Ibid., p. 17.
20 Anderson, op. cit., p. 411.
21 Bixler, op. cit., p. 19.
The significance of such factors varies with the character of the achievement, but usually none of them should be neglected. The skill and zeal of the teacher appear to be more significant than is commonly realized. Control of these factors may be attained by securing equivalence or by determining the effect of variation and by making appropriate allowance for this effect in interpreting the results.22

Finally, as regards the reliability of the method, the statistical computations and tabular representations were to be correct, that is, the reviewer should have obtained the same results as the experimenter in his calculations.

III. CRITERIA AS REGARDS THE RESULTS

Three characteristics of the researcher's statement of results were demanded: validity, reliability, and simplicity of formulation.

Validity of the results. The findings and conclusions should have been supported by the data presented.23 Thus, the conclusions were not to be contrary to the data due to strongly preconceived ideas of the results. As noted by Good, Barr, and Scates:

The absence of agreement between the conclusions and the facts is so complete in some studies that the collection of data in such instances appears to have been nothing more than a formality, influencing to a minimum the investigator's already preconceived conception of the phenomenon.24


23 Editorial Board of the Journal of Educational Research.

24 Good, Barr, and Scates, op. cit., p. 632.
Secondly, conclusions stated as universal truths and valid as regards the amount of data collected were not to be invalid due to inadequacy of sampling. For example, if the experimenter had scores of his pupils on intelligence and standardized achievement tests, he might have compared the means and standard deviations of these scores with the corresponding measures of the larger population. If this comparison indicated that his sample was typical of the larger population, generalizations might have been accepted with a reasonable degree of confidence. If the data did not satisfy this criterion of representativeness, the investigator should have refrained from generalizing, or limited his generalizations accordingly. Thirdly, all the conclusions which were potentially contained in the data should have been inferred. And lastly, the conclusions were to be adequate; as noted by Anderson, the results were to be presented in such a way that the trends with school grade, with chronological age, and with mental age, or any combination of these, could be determined.

Reliability of the results. Reliability of the results demanded their verifiability.

In any valid experiment the methods and materials developed must be tried in a number of school rooms, with different teachers, under ordinary working conditions. The proponent of a new method of technique is far too often like an evangelist in his fervor. He motivates children so highly that they achieve astounding results that cannot later be duplicated by

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others. The test of a method of instruction and of an educational material, as of a scientific procedure, is to be found not in its own uniqueness, but in the possibility of its reproduction with similar results by others.\textsuperscript{27}

The actual determination of statistical reliability in the placement was generally impossible due to the insufficiency of the data presented.

**Simplicity of formulation of the results.** Finally, the results should have been formulated simply and concisely. The conclusions were to furnish "answers specifically connected with questions asked in the statement of the problem."\textsuperscript{28}

These, then, were the criteria as regards the problem, the method, and the results. Each of the arithmetic placement investigations was analyzed according to these standards and was expected to measure up to every requirement.

\textsuperscript{27} Ibid., p. 404.

CHAPTER IV

SUMMARY OF ARITHMETIC PLACEMENT INVESTIGATIONS

Up to this point the reader has viewed the construction of the framework upon which the various placement studies in arithmetic will be summarized and evaluated. Since the foundation is now completed, a summary of the pertinent researches will be undertaken.

The onset of the twentieth century marked a new milestone in the teaching of arithmetic. The influence of the doctrine of social utility, the practical application of Thorndike's Laws of Learning, the prevalence of the diagnostic and remedial philosophy—all of these culminated during the decade between 1930 and 1940 in the development of two concepts: one, that individual differences were widespread; the other, flowing from this principle of individual variation, that each child underwent a physical, a mental, a social, and an emotional maturational development, during which growth he was ready at particular stages for particular items of subject matter. These two concepts gave rise to the question of placement—Where shall topics in arithmetic be taught? The answers to this query can be grouped under four heads: (1) investigations dealing with the number abilities of young children; (2) investigations advocating deferred arithmetic; (3) investigations placing the higher developmental skills; and (4) investigations employing the criterion of social readiness.

I. INVESTIGATIONS OF THE NUMBER ABILITIES OF YOUNG CHILDREN

Studies showed that the typical child arrived at the portals of
education with a definite apperceptive background in arithmetic. Socially, through his contacts and needs, and emotionally, through his interests and attitudes, he had acquired certain of the fundamentals of mathematics. The studies, however, of this period of readiness indicated the present status; no investigation was found which attempted to determine how much number the child was physically, mentally, socially, and emotionally capable of learning during these formative years.

According to Brownell, the manner in which the typical child took the various steps in the development of the ability to deal with concrete numbers seemed to be as follows: He first became proficient in the use of counting, "a method in which each of the objects is told off until the last number name stands for the total number of objects exposed;" counting developed in the child in three successive stages, acquisition of the number names, the number sequence, and finally the one-to-one correspondence. Next, the typical child acquired partial counting skill, "in which a part of the total number of exposed objects is taken as a group and the rest are counted." Then followed grouping, "in which a number of separate groups are recognized one after the other and the total number is apprehended by adding together the subtotals." And finally, he reached the stage of multiplication and conversion, "in which the objective representation of number is at once translated into abstract symbols and the number of objects is apprehended by means of these symbols without subsequent references to the objects themselves."¹

In developing successively more mature methods of dealing with concrete numbers, the typical pupil takes several rather clearly defined steps. First, he commonly develops counting to a degree of efficiency which satisfies his needs. He then begins to recognize certain combinations and arrangements of objects as groups that he can treat as aggregates. When he is required to apprehend any given number of objects which are exposed to him, he seizes upon some small familiar group within the total number of objects and counts the rest of the objects. Thus, if eight objects were exposed, he is likely to apprehend a group of four at once and count from that point. The next step is taken when, through much practice with concrete numbers or through the application of the abstract knowledge of numbers which he has acquired in school, he gives up counting entirely and, recognizing successive groups in the total number of objects exposed, adds together the subtotals. He takes the last step when at sight the objective representation suggests a translation into abstract symbols and the number is apprehended by means of these abstract symbols.2

Thus, he has a thorough understanding of concrete numbers; his concepts of the various numbers are adequate and sound; and the process of number combination possesses meaning for him. The additive combinations are to him simply the next logical step in his thinking about numbers, and he is likely to welcome the opportunity to habituate the combination as a means of expediting his thinking. He accordingly memorizes the combination and can recall it easily, correctly, and instantly whenever he has occasion to use it.3

The typical child was traced in his development through these various growth stages. At the age of six, he was capable of counting by rote to thirty,4 thus having mastered the first two skills of the counting process.

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2 Ibid., p. 110.
3 Ibid., p. 227.


He could count by tens to forty,\(^5\) and was able to count from twenty to thirty objects.\(^6\) He was capable of reproducing and naming the numbers from one to ten.\(^7\) He had some concept of unit fractions,\(^8\) and especially of one-half, one-third, and one-fourth when presented in concrete situations.\(^9\) McLaughlin noted as regards the counting process:

Rote counting has been shown to develop just slightly in advance of rational counting. It involves the memorization and accurate recall of a fixed order of numerical terms. Rational counting is a complex mental process dependent upon grasping the idea of one-to-one relation between these numeral terms and the items discriminated in an objective series.

Characteristic errors in the early stages of counting include failure to recall correctly the number terms, confusion in matching terms with the objects being enumerated, and inability to keep the place in either series.\(^10\)

From the data of Russell, it was ascertained that "seventy-five per cent of seven-year-old children are capable of noting differences in groups composed of ten concrete objects if the groups have an actual difference. If

\(^5\) Buckingham and MacLatchy, *op. cit.*, p. 508.
\(^6\) Buckingham and MacLatchy, *op. cit.*, p. 508.
\(^7\) McLaughlin, *op. cit.*, p. 349.
\(^10\) Buckingham and MacLatchy, *op. cit.*, p. 508.
the groups are equal, they are so noted by seventy-five per cent of the individuals of this age when they are composed of five or fewer concrete objects.\textsuperscript{11}

The most comprehensive study as regards the four stages prerequisite to the combinations was made by Brownell:

First, the pupils in the first three grades who served as subjects did not readily apprehend the visual concrete numbers exposed to them in the form of number pictures. Second, the pupils did not in general employ abstract methods, such as counting by 2's, 3's, and 4's, below the third and fourth grades although all had received training in these abbreviated forms of counting and in the additive combinations by the middle of the second grade.\textsuperscript{12}

The reason for such immaturity was explained by McLaughlin thus:

Recognition of aggregates, though facilitated by ability in rational counting involves more complex mental processes. Group recognition with the youngest subjects did not, except in a few cases, extend beyond two, but with the older and brighter subjects it extended to three, four, and, in some cases, to five. The more mature children by numerous methods and insights showed that they regarded the group as having a distinct entity of its own dependent upon, but not the same as, that of its several constituent items. This is a new relational factor as significant for later development as the principle of correspondence is in counting. Methods that tend to make this new relational factor articulate, employ matching of small groups in pairs or doubles; recognition of aggregates by general form, symmetry, or familiar analogies; extension of the pairing function into rational counting by twos or larger units; a breaking up of larger groups into smaller ones readily counted or 'seen.' These and other insights aid the singling out of a new factor that in turn becomes an essential factor in the growth of ability to combine aggregates.

Among three- to six-year-old children the process of combining has not advanced very far. Few of the youngest even sense the task. For the four-


\textsuperscript{12} Brownell, \textit{op. cit.}, p. 61.
year-old it is largely a process of counting. Competency is distinctly greater with two than with three numbers. This may be explained, in part, by the greater complexity in the mental process involved in holding in mind the partial sum until the third number is added to obtain the total sum. Ability in the combining function differs with different types of experience. Combining visible objects is much less difficult as a process than combining the same number of objects seen in timed exposure, or as imagined, or as abstract numbers. The obvious explanation offers that in the first situation the rational counting facilitates the process more readily.13

As regards the same question, Brownell asked:

Why did not the pupils in the lowest grades employ these abstract methods of apprehending the number pictures? Why must one wait until Grades IVA and VA to find pupils in general making use of items of arithmetic knowledge and skill which are taught in the first grade? Why did the early instruction fail to function until years after it had been given? Probably all the pupils in Grades IA and IIA tested in this investigation knew that 4 + 4 is 8, but few of them made use of this knowledge when they were required to apprehend :: :: . Evidently their knowledge was not used because they did not recognize in :: :: an opportunity to use 4 + 4. For pupils in the first three grades, concrete numbers as shown in the number pictures and abstract numbers as found in the additive combinations are apparently little related to each other. Their knowledge of abstract number relations has not developed out of their experience with concrete numbers; rather, knowledge of abstract number has been acquired as a separate body of facts. So isolated are these two bodies of knowledge that an opportunity for the application of abstract knowledge was not recognized.14

Examination of the last two lines of Table XIII shows that counting was generally superseded as a method of apprehending the domino number pictures as early as the third grade, where nearly all the pupils (four out of five) apprehended the numbers by using groups of five. This early use of short cuts with the domino arrangement is in striking contrast with the long-continued use of counting by the same pupils when dealing with the quadratic and triangular patterns.15

13 McLaughlin, op. cit., p. 352.
14 Brownell, op. cit., p. 37.
15 Ibid., p. 40.
In the addition processes, the typical child of six years was capable of reproducing correctly five out of ten of the forty-five easy addition combinations when they were presented in verbal problems, and of answering all of these combinations correctly when they were presented by means of concrete objects.\textsuperscript{16}

Valid development of ability in the addition facts was thus described by Brownell:

The additive combinations must be taught to children in such a way that they will appear as a natural extension of the children's experience with concrete numbers. If the first-grade teacher confines her instruction to concrete number and attempts, on the basis of concrete and semi-concrete materials, to develop the pupils' notions of numbers and their understanding of the principles of number combination, the second-grade teacher must begin at this point. She will first assure herself of the nature of the pupils' number concepts by means of concrete materials. If she finds that they still employ immature methods of thinking of concrete numbers, she will bring them in their thinking to the stage which has been called 'multiplication and conversion.' She will do this in order that the pupils may be able to make the transition from concrete number without confusion and wasted energy. Children who use multiplication and conversion in apprehending concrete numbers are thereby demonstrating the fact that the objective representation of number is little more than the starting-point for apprehension; the really important phases of apprehension are abstract for them. The number of objects exposed is determined by processes which have little relation to the objects themselves. Pupils who have reached this stage in dealing with concrete numbers in abstract terms should be ready to think of purely abstract numbers.

This preliminary work in concrete numbers will introduce in a natural way some of the simpler additive combinations. After the children have been permitted to verify the number facts--by counting if necessary, but preferably by grouping, multiplication, and the more mature methods--they may be urged to memorize the verbal statements. Furthermore, the relation

\textsuperscript{16} Buckingham and MacLatchy, \textit{op. cit.}, p. 509.

Woody, \textit{op. cit.}, p. 199.
between the various combinations as such will be emphasized. Children will be encouraged to trace the relation between the facts they are learning and to use facts which are learned early as means of learning later facts. No number combination will be presented, nor allowed to remain in the child's mind, which is separate and isolated from similar facts. Gradually the children will be led to the stage of meaningful habituation, where each fact, while related to the others in meaning, will be capable of prompt, correct, unhesitating recall. Drill will appear at this point in teaching for the first time, and it will be used for its legitimate purpose, namely, to increase efficiency in the most mature way of dealing with numbers.17

Similarly, in relation to this concept that teaching in Grades I and II would be effective to the extent that it took the child where he was and continued to help him find satisfying answers to his questions and problems, and as it equipped him with added skills and knowledges to meet new problems, Baxter said:

There seems to be quite a disparity between the pupils' mental maturity and the capacity required for mastery of the processes of arithmetic as taught in the elementary school. The school has not been able apparently to supply a sufficiently increasing body of experience for pupils to acquire a meaningful background of understanding for the processes to be learned. While the degree of understanding will always vary with individual pupils, there is enough evidence to warrant consideration of the need for affording pupils a much richer experiential background before introducing them to number symbols.18

Until pupils use arithmetic terms and have had experience with quantitative relationships, they are not ready mentally for the manipulative steps of the arithmetic process. Curricular practice has assumed that pupils can learn arithmetic through memorization of facts. Quantitative thinking will never be developed through memorization. It is therefore imperative that an introduction to a topic in arithmetic be based upon a pupil's actual first-hand experience with the arithmetic concept.19


19 Ibid., p. 229.
In brief, then, in the words of Buckingham:

No more important dictum is to be found in pedagogy than this: Meet the child where he is. Don't meet him where you think he is. Know where he is. Don't meet him where he ought to be or where his mother says he is. It is important that there should be no mistake about the interests and abilities of the pupil whom we are taking in hand.20

Such facts as to the arithmetical development of the typical six-year-old child led to paradoxical conclusions:

What are the implications of these studies? One interpretation is that since children, on their own, have learned so much about number, they should be allowed to continue on their way for another year or two at least. A second interpretation is that the possession of so large a stock of usable number ideas and skills is proof positive of readiness for direct teaching. The two interpretations point in diametrically opposite directions. The second interpretation requires the immediate introduction of 'systematic' instruction; the first, the postponement of such instruction.21

II. INVESTIGATIONS ADVOCATING DEFERRED ARITHMETIC

Both of the above alternatives as stated by Brownell have been subjected to objective verification. An investigation advocating the beginning of formal arithmetic in Grade I was that of MacGregor;22 this study, however, was performed in the Scottish schools where the organization so significantly differed from the American school system that the results did not seem applicable. An early investigation by the Committee of Seven likewise advocated


the teaching of formal arithmetic in Grade I; this study, however, has shown the desirability of postponing formal instruction. A number of researches were made as to the effects of postponing formal instruction until the child was ready for it. Taylor and Irwin advocated postponement for one year, Wilson for two years, and Benezet for six years. Brownell lent his support to the deferred arithmetic philosophy and stated:

In the light of these facts, there is reason to ask whether current methods of teaching number do not require a too early, or at least too abrupt, transition from concrete number to abstract number, and whether it might not be well to give pupils further acquaintance with concrete representations of numbers and of number combinations before introducing them to symbolic statements of number relations.


24 Mabel Vogel Morphett and Carleton Washburne, "Postponing Formal Instruction: A Seven-Year Case Study." Paper read at the Symposium on the Effect of Administrative Practices on the Character of the Education Program, St. Louis, Missouri, February 27, 1940.


29 Brownell, The Development of Children's Number Ideas in the Primary Grades, op. cit., p. 47.
These facts seem to justify the type of number-teaching which gives unhurried experience with concrete objects for the greater part of the first year, followed by a gradual induction into the additive and subtractive combinations on the basis of semi-concrete materials, such as number pictures. Certainly the slow instruction in the first grade in the Chicago school, designed to build up rich, meaningful concepts, had lasting favorable effects. The delay in beginning more formal instruction did not hamper progress. The pupils in the third grade were up to the norm for the grade, and the pupils in the fourth grade were considerably above the norm. The significance of the pupils' scores on standardized tests is all the greater in view of the fact that the total amount of time given to arithmetic instruction in the first grade in the Chicago school is actually less than that allotted in the Champaign and Urbana schools. The conclusion seems to be justified that it is wise to proceed slowly at the early levels of arithmetic instruction. The data show that it took the Champaign and Urbana pupils from two to three years to overcome their handicap of slight acquaintance with concrete number and of too early induction into the mysteries of abstract number. Failure to develop adequate concepts of concrete numbers had to be atoned for by two years of experience under relatively unfavorable conditions.30

Note, however, that Brownell's emphasis as well as that of the researches of Taylor, Irwin, Wilson, and Benezet was not upon the deletion of arithmetic per se from the primary-grade course of study, but rather the abandonment of arithmetic as it had been taught in the past and the consideration of the social and emotional maturity level of the child in the presentation of the facts of formal arithmetic. The investigation of Brownell was commendable in that it analyzed the prerequisite skills necessary for learning addition. Whether, however, the child was physically immature, mentally incapable and inexperienced, socially without need of, and emotionally disinterested and negativistic toward such prerequisites at the preschool, the kindergarten, and the primary-grade levels was yet to be determined. Undoubtedly, the postponement of the teaching of arithmetic solved the immediate

30 Ibid., p. 60.
problem of readiness, but with the overcrowded intermediate-grade course of study, it was only creating a new difficulty. No scientific investigation had yet shown that children were definitely unready for learning the four prerequisites of the fundamental processes and even of these processes themselves at the lower levels. The major need in this initial period of arithmetic learning seemed to be the development of a comprehensive investigation as regards the child's readiness for counting, for partial counting, for grouping, for multiplication and conversion, and finally for the formal skills of addition and subtraction. A knowledge of the necessary sensory-motor development and of the mental background and maturity of the child when he was taught under a methodology geared to his needs and interests was sorely lacking. Before the advantages of postponing arithmetic to higher levels were propagandized, before it was finally concluded that children were definitely not ready for arithmetic in the preschool and primary-grade years, it seemed necessary to validate scientifically the hypotheses upon which such principles were founded.

III. INVESTIGATIONS AT THE HIGHER DEVELOPMENTAL LEVELS

The higher developmental levels in arithmetic ability were dominated by the comprehensive investigation which had been carried on by the Committee of Seven since 1926 and which was still in progress. The Committee's recommendations were couched in two forms, the minimal mental-age level for learning by 75 to 80 per cent of the individuals and the recommended mental age level for teaching. The following table lists these two placement
figures for the various processes:31

<table>
<thead>
<tr>
<th>Topic</th>
<th>Minimal Mental Age Level In Years</th>
<th>Recommended Mental Age Level In Years</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Addition Facts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whole Numbers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sums 10 and under</td>
<td>6-7</td>
<td>7-8</td>
</tr>
<tr>
<td>Sums over 10</td>
<td>7-8</td>
<td>8-9</td>
</tr>
<tr>
<td>Column addition</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Three-place numbers in three-digit columns</td>
<td>9-10</td>
<td>9-10</td>
</tr>
<tr>
<td>Three-place numbers in four-digit columns</td>
<td>10-11</td>
<td>10-11</td>
</tr>
<tr>
<td>Fractions</td>
<td>10-11</td>
<td>10-11</td>
</tr>
<tr>
<td><strong>Subtraction Facts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whole Numbers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Easier fifty</td>
<td>6-7</td>
<td>8-9</td>
</tr>
<tr>
<td>Harder fifty</td>
<td>8-9</td>
<td>9-10</td>
</tr>
<tr>
<td>Fractions</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Common Fractions</td>
<td>10-11</td>
<td>10-11</td>
</tr>
<tr>
<td>Uncommon denominators</td>
<td>14-15</td>
<td>14-15</td>
</tr>
<tr>
<td><strong>Multiplication Facts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whole Numbers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Products of 20 or less</td>
<td>8-9</td>
<td>9-10</td>
</tr>
<tr>
<td>Products of more than 20</td>
<td>11-12</td>
<td>11-12</td>
</tr>
<tr>
<td>Fractions</td>
<td>12-13</td>
<td>12-13</td>
</tr>
<tr>
<td><strong>Division Facts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whole Numbers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Division by a one-place divisor</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dividends of 20 or less</td>
<td>9-10</td>
<td>9-10</td>
</tr>
<tr>
<td>Dividends of more than 20</td>
<td>11-12</td>
<td>11-12</td>
</tr>
<tr>
<td>Division by a two-place or larger divisor</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two-place divisor and one-place quotient</td>
<td>11-12</td>
<td>11-12</td>
</tr>
<tr>
<td>Two-place divisor and two-place quotient involving naughts, remainder, and trial-divisor difficulties</td>
<td>12-13</td>
<td>12-13</td>
</tr>
<tr>
<td>Two-place divisor and three-place quotient</td>
<td>13-14</td>
<td>13-14</td>
</tr>
<tr>
<td>Fractions</td>
<td>12-13</td>
<td>12-13</td>
</tr>
</tbody>
</table>

The investigations of the Committee were corroborated by the study of Fowlkes 32 in the learning of multiplication by third-grade children, disputed by the investigation of Norem and Knight 33 who placed the learning of the one-hundred multiplication combinations at the mental age of ten years and seven months and also by the study of Grossnickle 34 who set as the requisite mental age for learning division with a two-figure divisor that of nine years and seven months. These two latter investigations, however, employed methods which were significantly different from that used by the Committee of Seven. The value of the discrepancy in the placement of these studies was not the discrediting of either investigation, but rather the note of caution to the educator who thus became aware of the fact that the difficulty of a given item of subject matter was affected by more than the variable of mental age.

IV. INVESTIGATIONS EMPLOYING THE CRITERION OF SOCIAL READINESS

The influence of the factor of method was illustrated in the social


and emotional maturational placements carried on by Smith, Reid, Wahlstrom, Hanna, Harap, who set out to show the value of taking into consideration the child's interests and needs in the placement of curricular material. The particular method of handling the problem by each investigator was open to criticism, however, on several accounts. In general, all of the experimenters failed to isolate the social and emotional variables. The only value deducible from the adjustment of methodology to the social and emotional readiness of the learner was either acquisition of the process at a lower level than under a less adapted methodology or the more efficient and economical learning of a particular topic at the present placement. Smith found percentages of the total of the four most frequently used operations in the out-of-school life of first grade children to be as follows: addition, 35 per cent; counting, 23 per cent; subtraction, 12 per cent; and fractions, 8 per cent. These four operations made up 78 per cent of the total number


36 Florence Reid, "Incidental Number Situations in the First Grade," *Journal of Educational Research*, XXX (September, 1936), 36-43.


of operations which were used on certain occasions in the out-of-school life of five hundred first-grade pupils. Reid employed a specific environment in teaching first-grade children and determined by means of a check-list the incidental number situations in this environment. A similar investigation to that of Smith was Wahlstrom's school-pupil survey of the amount of computational arithmetic which occurred in the social experiences of third-grade children. Hanna showed that in an activity program certain skills in the four fundamental processes were learned much earlier when a felt need arose than their present placement. Harap and Barrett indicated the effect of the use of an activity program in the learning of the fundamentals at the third grade level. And finally, Harap and Mapes used a similar activity program in the teaching of the multiplication and division of fractions in Grade VA resulting in the placement of the topic at the lower end of the Committee of Seven's desirable range of teaching this topic.

It may be seen from the above summary of placement research that the field was almost barren. Except for the work of the Committee of Seven, the scientific structure of the arithmetic course of study was without foundation. However, the Committee of Seven emphasized but one of the variables of maturation. The other investigations noted should give impetus to a further study of the physical, the social, and the emotional factors in the placement of particular topics.
CHAPTER V

EVALUATION OF ARITHMETIC PLACEMENT INVESTIGATIONS

The reader is now prepared for the application of the placement criteria as set down in Chapter III to the various investigations as summarized in Chapter IV. Again, the curricular researches on arithmetic placement are divided into four categories: (1) investigations dealing with the number abilities of young children; (2) investigations advocating deferred arithmetic; (3) investigations placing the higher developmental skills; and (4) investigations employing the criterion of social readiness.

In the case of each evaluation, a copy of the critique was sent to the original experimenter. The reader is referred to the Appendix for an alphabetically arranged reproduction of these replies.¹

I. INVESTIGATIONS OF THE NUMBER ABILITIES OF YOUNG CHILDREN

The general procedure for the investigations determining the quantity and quality of the number knowledge possessed by pre-school and primary grade children was that of the normative survey. No investigation was found which attempted to control the environmental influences and to present situations which were rich in number. Such a study, however, was not only feasible but necessary if there was to be ascertained how much number knowledge the child was physically, mentally, socially, and emotionally capable of mastering at various developmental levels.

¹ Cf. post, pp. 153-73.
The study of William A. Brownell.² The most analytic study of the development of children's number ideas was that carried out by Brownell.

The problem. The investigation concerned itself with the following problems: (1) the ascertainment of the comparative difficulty of apprehension of numbers from three to twelve when they were presented to children in visual concrete form as "number pictures"; (2) the methods characteristic of selected children in apprehending visual concrete number; (3) the isolation of the factors involved in the ability to apprehend visual concrete number and the measurement of the influence of these factors in the development of that ability; (4) an analysis of the mental processes employed by individual pupils in the recognition and use of concrete number, in learning the simple addition combinations, and in learning to add three digits in a column.³ Of these four phases Brownell stated that the last "bulked the largest in the monograph."⁴

The method. The basic data were collected by means of survey testing, the tests that were developed being given to a total of 1,858 children in the first seven grades in eight schools in Champaign, Urbana, Danville, and Chicago, Illinois. The subjects were described as to their chronological ages, mental ages, intelligence quotients, school grades, and accuracy scores


³ Ibid., p. 1.

⁴ Letter from William A. Brownell, July 5, 1940.
on the number pictures test. Six sets of exposure cards employing the "number picture" with black circles pasted on white cardboard in the form of quadrats, diamonds, domino, triangular, odd, and linear patterns were employed as the materials. The length of the testing period was controlled so as to insure precautions against fatigue. The procedure and order of exposure were carefully controlled. The apparatus employed was of the tachistoscope variety especially designed "to assure precision in control without introducing undesirable sources of distractions." The method employed by Brownell thus measured up to all the standards of validity and reliability. Supplementary to this main technique were the case study and the statistical methods.

As regards the evaluation of his method Brownell stated:

This whole section plays up too prominently the work with the number pictures. The 'basic data' for this part of the monograph were obtained, as you state, with the tests you describe, but the basic data for the later work were collected by means of combinations and addition tests, plus interviews.

The results. The major contribution of Brownell's study was not in the field of placement but in the category of experiential knowledge necessary in the young child for the development of number ideas.

Brownell's results were to be interpreted in the light of the method by which the children were taught. Thus he stated:

5 Brownell, op. cit., p. 12.

6 Letter from William A. Brownell, July 5, 1940.
Arithmetic instruction is similar in the six schools which furnished the subjects, and it is probably typical of the instruction in the better schools in the country. In the first grade of these schools the pupils are given some experience with number in concrete forms—with sticks, buttons, and the like. They next become acquainted with the domino form of number picture used in this study, the purpose being to improve accuracy and speed of counting. After a few weeks of such work, the pupils are introduced to counting by 5's, 2's, and 4's and then to some of the simpler additive combinations. By the end of the first grade their arithmetic work deals entirely with abstract numbers. By the middle of the second school grade they have been taught all the simple additive combinations and some of the subtractive and multiplicative combinations.7

Then, he noted:

The pupils tested in this investigation had had considerable experience with the domino arrangement before taking up the abbreviated form of counting by 5's. Their experience with this pattern carried over well into use in the present study and made it possible for the large majority of the pupils in the third grade to use abstract methods of apprehension with the domino number pictures. On the other hand, their training with this pattern had been narrow and specific and did not lead them to generalize the possibility of using abstract skills in concrete situations involving other patterns and other numbers.8

And again,

The explanation of the differences recorded in Tables XXIII and XXIV is that the Chicago pupils had been subjected to a type of instruction in number in the early grades different from that to which the pupils in the other schools had been subjected. The course of study for the first grades in the Champaign and Urbana schools has been briefly sketched (p. 34). The work in the Chicago schools is characterized by a much less rapid pace.9

The criticism, then, of Brownell's results was that they indicated placement of certain number abilities of children in accord with a particular method.

7 Brownell, op. cit., p. 34.
8 Ibid., p. 41.
9 Ibid., p. 59.
Yet, as implied by the author himself, this placement might have been altered if the technique was changed. Brownell, thus, made no attempt to secure optimum placement, but rather determined the fallacies of learning under present conditions.

In summary, the work of Brownell was perhaps the most analytic and carefully controlled investigation with which the present study dealt. The author's purpose was not one of placement, but rather a genetic study of the number ideas. Were this technique, however, taken over in a study whose sole purpose was placement, and were the number of subjects increased, the result would undoubtedly have been momentous in the initial period of learning arithmetic. For example, the author stated:

The question was raised whether the plateau in Grade IIIA in the development of mature methods of dealing with concrete numbers is inevitable and unavoidable or whether, on the other hand, the plateau could be eliminated or at least reduced in extent by the proper kind of instruction. The point was made that the plateau in the second grade in this school is shorter and less marked than that in the second grade in the schools reported on in chapters II and III. An explanation for the superior progress of the pupils in the Chicago school was found in the type of instruction provided in the first grade. The conclusion followed that more of this kind instruction, begun earlier in the first grade and continued for some time in the second grade, might reasonably be expected to maintain progress in the development of ability in concrete number at a fairly even rate through the four grades.\textsuperscript{10}

Since, however, Brownell considered the learning of addition and subtraction dependent upon the knowledge and mastery of the prerequisite skills of counting, partial counting, grouping, and multiplication and conversion,\textsuperscript{11}

\textsuperscript{10} \textit{Ibid.}, p. 108.

\textsuperscript{11} \textit{Ibid.}, p. 229.
the importance of further study on this question was undeniable. The
author's analyses of the sequence of development in number ability was most
comprehensive; however, no attempt was made to denote the physical, mental,
social, and emotional maturity of a well-sampled group of individuals in
these various stages. Admittedly, this was not the author's purpose; the
present study by no means goes on record as criticizing him for this defi-
ciency, but rather seeks to point out the value of such a further analysis.

Except for the few instances noted, Brownell was generally in accord
with the writer's analysis of his research:

With regard to your evaluation of my monograph, let me first say that
I am gratified with the many favorable comments. So far as the facts of
the study are concerned (apart from the evaluation which is your own
business), your analysis is sound at most points. The chief weakness is
to overemphasize the work with concrete number pictures. As a matter of
fact, this part of the monograph is much smaller than that which deals
with children's thought processes in dealing with abstract numbers, and
the comparative space allotment accords precisely with the relative
importance of the two parts.\textsuperscript{12}

The study of B. R. Buckingham and Josephine MacLatchy.\textsuperscript{13} The research
by Buckingham and MacLatchy on the number abilities of children when they
entered grade one was an important contribution to the field of curricular
research in arithmetic.

The problem. The problem as stated by the authors was this: "What

\textsuperscript{12} Letter from William A. Brownell, July 5, 1940.

\textsuperscript{13} B. R. Buckingham and Josephine MacLatchy, "The Number Abilities of
Children When They Enter Grade One," Twenty-Ninth Yearbook of the National
number does the child six years to six years-and-a-half know when he enters the first grade?"\textsuperscript{14} The meaning, however, which the authors attached to "number" was not ascertained until a description of the primary investigation was begun ten pages later; here, the reader was told that the interview test—the means employed by the investigators to ascertain an answer to their problem—sampled counting ability, number concepts, and number combinations. Thus, only by inference was the reader able to arrive at the authors' definition of terms. The problem, however, though lacking in clearness and conciseness of statement, was an extremely significant one in the field of arithmetic placement at the primary levels.

The method. The method employed to determine the number ideas possessed by preschool children was that of an interview test, "the purpose of which was to measure certain aspects of the number knowledge of young children."\textsuperscript{15} Though a copy of the test was supplied, no attempt was made to indicate its validity by statistical means. By logical analysis, the test was seen to deal with number only in its computational function, neglecting the sociological, informational, and psychological aspects; however, even in the computational function, such items as vocabulary development, comparison in counting, fractions, and measurement were omitted. The authors explained such deletions by stating that "the job which the teachers had to do in

\textsuperscript{14} Ibid., p. 475.

\textsuperscript{15} Ibid., p. 484.
giving the test was already exacting; more would have been an imposition.\textsuperscript{16}

And again, "The present test, in the interest of brevity, disregards differentiation and finding and requires reproduction and naming—in other words, the easiest and hardest of the functions."\textsuperscript{17} Emphasis on brevity and ease of administration seemed to occupy a higher place in the authors' hierarchy of test characteristics than the items of validity and reliability.

The description of the subjects was made only in terms of their chronological ages, the fact that they had received no instruction in number, and whether or not they had attended kindergarten.

The data were furnished by the authors in a simple and complete statistical manner, and fully enough presented to permit the reader to reach independent conclusions.

The materials employed in the test seemed to lack standardization. Thus, the authors stated in Tests II, III, IV, and VI, "the beads used in kindergarten, pennies, small blocks or cubes, beans, or buttons are suggested."\textsuperscript{18} Terman in his Second Revision of the Stanford-Binet Test of Intelligence noted that standard procedures must be followed, that is, "the procedure for giving these tests has been carefully standardized for each test situation and should be followed without deviation."\textsuperscript{19} Thus, in Test IV of the Stanford-Binet occurring in the sixth year, "Number Concepts," the

\begin{itemize}
  \item \textsuperscript{16} Loc. cit.
  \item \textsuperscript{17} Ibid., p. 488.
  \item \textsuperscript{18} Ibid., p. 522.
  \item \textsuperscript{19} Lewis M. Terman and Maud A. Merrill, Measuring Intelligence (Boston: Houghton Mifflin Co., 1937), p. 52.
\end{itemize}
materials specified were twelve one-inch cubes. Buckingham and MacLatchy, however, though the same materials were to be employed in four out of their six tests, made no attempt to standardize their material. Again, the directions for administering and scoring were at times ambiguous; "If the pupil becomes confused in his counting, the number after which this occurs is his score."20 A further explanation or a practical example seemed necessary to clarify the authors' idea of "confused." In Test V, "Fundamental Combinations in Problems," the authors stated, "After the game idea has been established, read each problem slowly."21 If scores were to be comparable on this test, it seemed that the authors needed to be more explicit in their directions as to exactly how and when this game idea was "established." Finally, in Test VI, "Fundamental Combinations with Objects," the authors made the statement: "Let the pupil work this out for himself if he can."22 Was the test administrator to infer that if the subject could not, the answer was to be furnished? Again, in this same test, no mention was made as to whether the ten combinations were all to be presented even in the case of failure, or whether, after a certain number of failures, the test was to be discontinued. Leaving decisions such as these up to the subjective judgment of each individual experimenter was the type of procedure that tended to make any test unreliable.

20 Buckingham and MacLatchy, loc. cit.

21 Ibid., p. 523.

22 Loc. cit.
The sampling of the subjects was doubly emphasized:

It is to be understood, therefore, that the pupils who participated in this experiment were between six and six-and-a-half years of age; had first entered grade one in September, 1928; had received no instruction in number; and were selected at random from all the children in the various classes represented to whom this description applied.23

Though the authors stated that their subjects had received no instruction in number, they preferred no proof of the fact, and investigations such as that of Smith tended to indicate that their assumption was an invalid one.24 Informal instruction by parents, siblings, and in situations involving arithmetic were a common experience of nearly all preschool children. The authors stated that their selection was random, but yet they set down as the only control of this sampling the fact of selection by the teacher from an alphabetically arranged list of the first six pupils whose birthday occurred between the specified dates. Since, however, the composition of the population as a whole was unknown, the proper procedure would have been to continue selecting random sample groups until comparison of the samples taken revealed equal variations in all directions. The authors answered this criticism in a later article by stating:

Further evidence of this randomness came from the records which showed no differences traceable to the section within a city, to the kind of community, or to the part of the state in which the children lived. For these reasons the percentage summaries of the answers of this large group may be considered typical for the mass of six-year-old children

23 Ibid., pp. 485-86.

who enter school each September.\textsuperscript{25}

The results. In the summary of the primary investigations,\textsuperscript{26} there was found that in rote counting by ones about ninety per cent of the children succeeded at least as far as ten. However, the actual data were collected in class intervals having as upper limits the unit digits of one and six; thus, the data stated that eighty-seven per cent of the children on one trial and eighty-nine per cent on the other trial were able to count at least as far as eleven. The authors justified their conclusion by asking: "How many children can count to ten? Certainly ninety per cent, since the jump from ten [for which no direct data is available] to eleven [where the above statistics are available] is relatively hard to make."\textsuperscript{27} No attempt, however, was made to document their "relatively hard to make." The question, admittedly, was purely academic; however, if the research was posited as scientific, it should have adhered to the scientific spirit and based its conclusions on facts, not subjective estimates. Again, the conclusions were stated in terms of what the writers found that "six-year-old children possess";\textsuperscript{28} the data, however, were collected in terms of pupils between "six and six-and-a-half years of age."\textsuperscript{29}

\textsuperscript{25} Josephine H. MacLatchy, "Number Abilities of First-Grade Children," \textit{Childhood Education}, XI (May, 1935), 344.

\textsuperscript{26} Buckingham and MacLatchy, \textit{op. cit.}, p. 508.

\textsuperscript{27} Ibid., p. 491.

\textsuperscript{28} Ibid., p. 508.

\textsuperscript{29} Ibid., p. 485.
The authors stated as regards Test III: "In order to abbreviate the testing, it has been assumed that ability to respond correctly in respect to a given number implies in general the ability so to respond in respect to a smaller number. The teacher began each of the three trials with the number five."30 Yet, conversely, the authors contended: "Not infrequently a pupil whose command of the situation was unstable succeeded on five at the first trial (and may even have succeeded on one or two higher numbers), only to fail on five at the second trial."31

The authors were in general cautious in the presentation of their data so as not to leave the reader with any false interpretations. The one exception to this rule was found in the interpretation of Test V, "Fundamental Combinations in Problems": "Very nearly half the children got five combinations right."32 "Between these two extremes, no record and a perfect one, the percentages ranged from twelve per cent who gave the correct sum of three combinations to six per cent who gave the correct sums of nine of them."33 Again, "the median superior child knows the sum of 6.8 addition combinations of ten presented in problems; the median child of average ability knows the sums of 4.8 of ten addition combinations presented in problems; the median

30 Ibid., p. 495.
31 Ibid., p. 496.
32 Ibid., p. 509.
child of less than average ability knows the sums of 1.5 of the ten addition combinations in problems."34 However, as noted by MacLatchy, "the largest percentages of the children are familiar with combinations in which one is added to a larger number which is given first. The combinations least familiar are made by adding numbers of similar size together."35 Whether, therefore, the authors were correct in attributing the results of such a test to a knowledge of the combinations rather than to the ability to count seemed a debatable one.

The authors presented a summary of their investigation, but no formal attempt was made to generalize from the data collected. In a later article, MacLatchy hinted at the relationship between the counting process and the combinations; "Combinations in which one is added to larger numbers are most familiar, larger numbers added to one come next, two added to larger numbers rank together in familiarity."36 The results attained by the experimenters, however, seemed to demand a more thoughtful and systematic statement of the various educational implications contained therein.

A popular statement of the data by MacLatchy described the median six-year-old who had attended kindergarten and the median six-year-old who had not attended kindergarten. Supposedly, therefore, the only variable was that of the systematic organization of experience as found in the kindergarten contrasted with the informal experience of everyday life. The first

34 Josephine MacLatchy, "A Phase of First-Grade Readiness," Educational Research Bulletin (Ohio State University), X (October, 1931), 380.
36 Loc. cit.
characteristic of the individual, however, was that of his mental age: "The median six-year-old who had attended kindergarten had a mental age of six years six months; the median six-year-old who had not attended kindergarten of five years ten months." Again, by Buckingham:

In the investigation as to the number knowledge of first-grade children which Doctor MacLatchy and I conducted, children who had received the benefit of kindergarten training were superior to those who had not received such training in every test which we employed. This can only mean that kindergarten experience calls for the use of number.

The reader could, therefore, draw one of two conclusions, either that the mental ages together with the number ability of these two individuals were directly affected by the kindergarten training resulting in an eight months difference mentally between those trained in the kindergarten and those not so trained, or that the authors had violated two fundamental principles, one of the equivalent-groups method in that they had not equated the groups mentally before the experiment began and the other of failure to isolate the variable of kindergarten training.

MacLatchy corresponded with the writer as regards the evaluation asking the nature of the piece of research. Though informed of the purpose of the investigation she failed to defend any of the criticisms leveled against her study.


39 Letter from Josephine MacLatchy, July 10, 1940.
The study of Katherine McLaughlin. 40 Another investigation of the number ability of preschool children was that made by McLaughlin.

The problem. McLaughlin set out to accomplish two ends: (1) the analysis of the development in preschool children of three phases of quantitative experience—counting, recognition of number aggregates, and combination of aggregates was considered. Thus, the variables of physical, experiential, social, and emotional development should have been held constant; and the author should have realized, as the title of her article failed to admit, that she was neglecting such phases of number ability as concepts, fractions, subtractions, etc. The problem, however, as stated was clear, concise, and consistently adhered to throughout the article and was significant in the field of arithmetic, placement.

The method. Tests were employed to determine the relationship between intelligence and number ability. No mention was made of the intelligence test used, and only a brief sketch was given of the number tests. The same tests were employed at all age levels; however, since a copy of the test was not furnished with the data, it was impossible to determine whether the author was at all times testing for number ability or at others for vocabulary difficulty.

The subjects were one hundred and twenty-five children enrolled in nursery schools or kindergartens ranging in chronological age from thirty-six to seventy-two months, and so selected as to form three age-groups of

comparable I.Q. The subjects were thus described in terms of their chronological ages and their intelligence quotients.

The testing conditions were comparable to those described by Terman in his Second Revision of the Stanford-Binet. 41

The sampling of the subjects was definitely limited. Selection was made on the basis of chronological age and intelligence quotient, the subjects being taken from those enrolled in nursery schools and kindergartens. Such children, however, were a selected group in that they had been exposed to such a systematic and controlled environment.

The results. As regards her data, McLaughlin stated:

The use of age-groups of approximately the same I.Q. made it possible to compare attainment at successive age-levels. Comparison under these conditions assured that three-year-olds would attain approximately the same development a year later as that of the four-year-olds of comparable intelligence. 42

Such an assumption would have been most useful were intelligence the only variable to be considered in this vertical analogy; however, the influence of physical development, of experiential background, of contacts and motivational influences, of interests and attitudes should have been determined before the author made such a broad statement.

The data for Series II, "Recognition of Group and Aggregate Number," and for Series III, "Combining Aggregates of Two or Three Numbers," was incomplete. Though the author stated that scores for each of the separate exercises indicated that progress from year to year was steady but greater

41 Terman and Merrill, op. cit. pp. 52-68.
42 McLaughlin, op. cit., p. 348.
in some elements than others, no attempt was made to express the quantitative results of the experiment. The author's observations in both cases centered upon methodology.

McLaughlin noted that her data "offer objectively derived inventories of abilities to deal with number ideas at early age levels; such inventories helping to supply a scientific basis for teaching and learning in grades one and two." The majority of the data collected, however, were of a qualitative nature, the author making no attempt to indicate the validity of the observations. Whether the data collected for individuals who were of a particular I.Q. and who had received the advantages of pre-school and kindergarten training were generalizable to all the "early age levels" was not objectively demonstrated.

Finally, the author stated as a practical value of her research that it defined one type of test which should be used in determining mental maturity for first-grade entrants. Such a conclusion was based on two premises which McLaughlin's data did not verify: (1) since there was a positive relationship between number ability of preschool children and their intellectual ability, that there, therefore, existed a casual relationship and that number ability could be employed as a measure of intelligence, and (2), if the first hypothesis was granted, that number ability was a valid instrument of mental measurement, that is, whether all individuals of a particular potential intelligence quotient would have developed to the same degree in number skill in varying environment.

McLaughlin made no reply to this evaluation.

43 Ibid., p. 353.
The study of Ada R. Polkinghorne. The determination of the knowledge of fractions possessed by children in the primary school was the aim of the investigation carried on by Polkinghorne.

The problem. Polkinghorne set out to determine what concepts of fractions primary children possessed, when they acquired these concepts, and how they acquired them. Such a study was allocated in the field of maturational development to experiential growth and felt need.

The method. The children's knowledge of fractions was obtained through the formulation and administration of a comprehensive series of tests. The validation of the tests was not furnished. The author did not explicitly state whether her test purported to test recognition ability or recall ability in fractions. From an example furnished, the two would seem to have been employed indiscriminately: "The child was given a square and a pair of scissors and he was asked to give the examiner a half of the square; in the next part of the test he was shown a square that had been cut into halves and he was asked to tell what he would call each piece." Polkinghorne answered this criticism by stating:

The children were given an opportunity to make two types of response to every item of the first two tests. The first response was verbal. The second was objective, that is, the child actually performed with his hands.

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46 Letter from Ada R. Polkinghorne, June 28, 1940.
Upon analysis of the original report, it was found that the "Tests on Unit fractions" were about evenly divided between the ability to recall a fractional concept and to recognize a fractional concept.\textsuperscript{47}

Again in the sample items, the purpose was to find out if the subject possessed the concept of one-half of four: "The examiner places 4 pennies in a row and says, 'Here are some pennies. I am going to pick up some of them. (She takes away one penny.) Have I picked up one-half, one-fourth, or one-third of these pennies."\textsuperscript{48} Such an error was undoubtedly due to a misprint; however, a similar error in the actual test situation would have been sufficient to destroy the validity and reliability of the particular test.

The reliability of the method was questioned in that "the judgment as to whether a response was satisfactory or unsatisfactory was made in this way: If a child's response was correct and he demonstrated by his actions, or explained with words, why he knew he was right, his response was counted as satisfactory."\textsuperscript{49} Precise standards of satisfactory and unsatisfactory responses seemed to be lacking, and thus, the unreliability of subjective scoring might have entered into the results. Polkinghorne answered this criticism thus:

I do not agree with this at all. If you show a child four pencils and ask him to give you a half of them, either he gives them to you or he doesn't. In this study, some children picked up two pencils and handed


\textsuperscript{48} Ada R. Polkinghorne, "Young Children and Fractions," \textit{op. cit.}, p. 354

\textsuperscript{49} \textit{Ibid.}, p. 355.
them to me immediately. Some said, "I don't want to break these nice new pencils." You knew more clearly and definitely that that child had not acquired the concept of one-half of a group, than you do in many group tests where children are asked to mark one-half of four balloons or boxes or whatever the item might be. In the latter test the child has no opportunity to explain with words. The acquisition of the understanding is judged by one type of performance only. Yet you would probably accept that sort of response as valid and reliable.

This study of the concepts of fractions acquired by young children was an interesting one to me. It revealed that young children know much more about fractions than we often think they do. It was a very pleasant experience to the children and the tests were given under conditions that were unusually favorable. The children were given every opportunity to do their best and to show what they knew. Because I gave every test myself, I know that the administration was identical for each child, and I know that the standards were precise and unvarying.50

A statement such as this might have well been condensed and included in the magazine digest of the work. No mention was therein made that the author herself gave and scored all of the tests and that she adhered to the selected criteria of satisfactory and unsatisfactory responses as were found in her unpublished study.51

The subjects were described according to grade, mental age, and chronological age group. The two hundred and sixty children employed in the experiment were obtained from the Elementary School of the University of Chicago; the group thus tended to be selected.

The results. The conclusions as to the grade placement of fractions which were presented by the author at the end of her study were all based on the hypothesis:

50 Letter from Ada R. Polkinghorne, June 28, 1940.

It seems reasonable, too, that if primary children learn so much about fractions without systematic instruction of any sort, there can be no question about their ability to learn more about fractions under direct teaching. 52

The author then proceeded to set down certain skills in fractions for the first, second, and third grades. Such an assumption, however, was fallacious in two respects: Even though the child had experienced a need for fractions in certain specific situations, this became no guarantee that he was physically mature, mentally capable, and emotionally enough interested to be subjected to formal instruction in fractions. And secondly, the data furnished by the author indicated that through natural stimulation children had learned something about fractions; such data did not indicate what to do in school.

Polkinghorne was much more conservative in her actual thesis as regards what should be taught than she was in her magazine article. In her concluding chapter of the thesis she stated:

The present investigation concerning what children learn from their experience, about fractions, should be followed by a study, the purpose of which is to determine what children in the primary school can learn about fractions as a result of teaching. 53

The study of Ed M. Russell. Russell published in 1936 the results of an experiment to determine the arithmetical concepts of children in the preschool years, the kindergarten and the first and second grades. The work had

52 Polkinghorne, "Young Children and Fractions," op. cit., p. 358.

as its underlying philosophy the principle that the promotion of child
mastery of arithmetic depended upon the determination first of all of "what
number ideas children have and in what situations children employ mathemati-
cal concepts." 54

The problem. The problem as stated by Russell was the determination
of how large a quantity children could deal with and the manner in which they
actually worked with quantities. Specifically, the experiment was concerned
with the responses of children from four to eight years of age to quantitative
situations of more and less, their understanding and use of words denoting
more, less, many, equal, and same, the perceptual limits beyond which they
could not make distinctions between quantities, and the manner in which they
went about the task of making distinctions between quantities.

The method. The author reported that a preliminary study was made to
determine the best method of presenting quantities to the child in order that
he might judge which was larger or largest; 55 these preliminary studies were
not furnished. The method finally selected employed the presentation of two
quantities in the form of two groups of blocks. In Group A, Test I, the
experimenter presented two irregular groups of blocks asking the subject,
"Which pile has the most blocks?" If the subject had difficulty with this
question, the following were given: "Are the piles of blocks the same?"

Educational Research, XXIX (May, 1936), 647.
55 Ibid., p. 648.
"Does one pile have more blocks than the other?" "How many blocks does this pile have?" Tests II and III of Group A employed the same method but varied the materials: in Test II, each pile had both small and large blocks in it; in Test III, painted blocks were employed. One subject took the first half of Test II and the second half of Test III. In Group B, the author stated that the same materials as in A were employed with the following exceptions:

It was found necessary to change the proportions of big to little blocks in the combinations of Test II. In too many of these combinations, it was found that the greater number of big blocks appeared in the larger group. Since the results indicated that the big blocks were influencing judgments when the child was to look for the larger group, the combinations were changed in such a fashion that a guess on this basis alone would be correct only fifty percent of the time.56

Such an admission did not engender the reader's support as to the validity of the instrument. The questions in Experiment B were phrased thus: "Are the piles of blocks the same?" "Are the piles of blocks equal in number?"

The method employed, therefore, was an individual technique whereby the mathematical concepts of the subjects which had been learned from environmental influences were tested.

The validity of the devices employed was based upon the assumption that the test given and the procedures employed in the testing measured what they purported to measure. Since the tests were constructed to measure quantitatively the perceptual limits beyond which certain subjects could not make distinctions between quantities, and qualitatively the understanding and use of words denoting more, less, many, equal, and same, the terminology employed should have carried out this purpose. To test the subject's

56 Ibid., p. 650.
conceptual knowledge of more and less, Experiment A was devised. The question was stated as follows: "Which pile has the most blocks?" How, with such a statement, was the author to test for the subject's knowledge of more and less? Indeed, he himself noted that "questions involving the words most and least were understood in many cases where the words more and less were not understood."57 Note also that when the piles were the same, the child needed to possess an understanding of the concept of equality or he was unable to answer. This difficulty was obvious in the data presented for the identical combinations in Tables V and VII.58 In the alternate question in Experiment A, the subject was asked: "Are the piles of blocks the same?" "Does one pile have more blocks than the other?" "How many blocks does this pile have?" Though it was difficult to introspect as to the thought processes of the individual confronted with these questions, the divergence of terminology seemed to indicate that the experimenter was not testing for the same concept in each case. In fact, the author later stated: "The nature of the directions, whether the experimenter has the child looking for differences or sameness, seem to play an important part as far as the child's arithmetic performance is concerned."59 Yet, the first alternative question on Test A, which was testing for the concept of more and less, was so phrased that the child was "looking for sameness." Test B interchanged the terms equal and same: "Are the piles of blocks the same?" "Are the piles of blocks equal in number?". An error in the author's phraseology was here noted in that an ellipsis had occurred in the first question. In Tests II and III,

57 Ibid., p. 649.  
58 Ibid., p. 653; 654.  
59 Ibid., p. 652.
the number of blocks might have been the same, but their shape and color may have varied. It seemed necessary, therefore, for the sake of validity to specify, "Are the piles of blocks the same in number?"

The subjects employed were described as to their intelligence, their grade in school, their sex, and their chronological age. The reason for employing two different groups of subjects, one for Experiment A and the other for Experiment B, was not furnished by the author. The average I.Q. of the group in Experiment A was 106.1; of the group in Experiment B, 109.7.

Thirteen of the individuals in Experiment A were in kindergarten, twelve in first grade, and four in second grade, a total of twenty-nine; in Experiment B, ten of the subjects were in kindergarten, ten in first grade, and five in second grade, a total of twenty-five. In Experiment A, there were eighteen boys and eleven girls; in Experiment B, thirteen boys and twelve girls. The average chronological age for the subjects in Experiment A was 6-9; in Experiment B, 7-1.

No mention was made by the author in the article as to the conditions, location, and time elements under which he conducted his experiments. Since that time, Russell stated:

The experiment was carried through by the writer during the University Summer Session.

The writer contacted two elementary school principals of the City of Lawrence who had a record of the pupils concerned in the study. The names of average pupils were requested; you have noted that the groups actually were above average. The writer contacted each child in his own home, carrying along the necessary materials. The time for each test was recorded; unfortunately I do not have access to these data in Barbourville. Thirty minutes per test would not miss the actual average time per test very far.50

Letter from Ned M. Russell, June 27, 1940.
The author in his tabular analyses made no attempt to indicate the responses of individual subjects, but rather gave the group average. In Table I, the combinations of large and small blocks and of colors were given for Test II and Test III; however, no specification was presented as to whether Test II and Test III were from Experiment A or Experiment B, or whether these same combinations were employed for both tests. Russell preferred this information, and "materials for experiments A and B are the same except for a few changes."61

The author took care to insure the reliability of his measuring instruments. The larger group of objects was placed to the right and to the left of the subject in random order. The smaller group was placed first in some instances and the larger one first in other instances. However, as previously noted, the author specified that "one subject took the first half of Test II and the second half of Test III, while the next subject took the second half of Test II and the first half of Test III." Upon analysis of Table I, where the number of small and large blocks were noted in each combination, it was found that in the first twelve presentations the same number of large blocks appeared in both the small and the large combinations; in the last eleven examples, however, six large blocks appeared in the small combinations and only two in the large, and in the last eight combinations the large block was found consistently in the smaller combination. There seemed, thus, to be a sampling error in the actual construction of the test; and the individual scores did not appear comparable on this particular

61 Loc. cit.
Again, in the use of the colored blocks, the following frequencies were observable in the combinations; in the smaller group, white, twenty-seven; blue, twenty-six; green, twenty-six; yellow, twenty-one; and red, twenty-one; in the larger group, white, twenty-nine; blue, twenty-nine; green, twenty-nine; yellow, twenty-nine; and red, thirty-three. The author made no attempt to explain why an equal number of each colored block was not used.

The subjects were not representative. Intellectually, they were in the high-normal group; and according to school grade, the sampling was heavily weighted from the kindergarten and first grade.

No mention was made of the time length of the experiment. Such a factor, especially in the testing of young children, bore greatly on the reliability of the testing situation.

The method of scoring employed by the experimenter was not mentioned.

Russell supplied this data:

The experimenter carried mimeographed copies of all combinations employed in each experiment. If the child failed to answer correctly the first question put to him, a failure for that particular combination was recorded.  

However, in cases such as the following:

Fourteen A missed the combination 4-6 on Test II being influenced by the greater number of large blocks in the "4" group. But the significant statement was added, "This (pointing to the "6" group) had the most little ones though." Fifteen A stated in connection with 4-6, "This one is bigger with big blocks. That one is bigger with little blocks."  

It seemed necessary that if the author was not going to furnish the entire scoring procedure so that the test might have been administered by any reader.

62 Loc. cit.

least, for the reader’s knowledge of the results, he might have indicated his position on such controversial issues as these.

The results. The results were stated by the author thus:

The child, four and a half to five years of age, readily understands the terms most, both, and biggest. Words denoting same and equal are not comprehended. The child can compare groups of blocks up to ten with remarkable accuracy, although he has a visual notion only of three or perhaps of four.

The seven-year-old child uses such terms as many, most, more, and some. The words same and equal are not fully comprehended. Counting by ones is a difficult method for differentiating groups and is not accurate above five. The child will form subgroups first which have unequal value mathematically. At a later stage in the differentiation process, counting by ones is employed.64

The data upon which the first conclusion as to the subjects four-and-a-half to five years of age was based were experiments by Decroly and Degand,65 and "a partial check of these points by the experimenter who gave Test I, Experiment A, to nine preschool children with an average age of four years and ten months."66 If the author wished to incorporate such data into his results, it seemed feasible to include a description of these nine preschool children in the proper place.

As previously noted, the terminology employed in the tests seemed to test in Experiment A for the subject’s knowledge of which pile contained the most blocks; therefore, was it to be assumed that this pile contained more

64 Ibid., pp. 662-63.
65 Ibid., p. 656.
66 Loc. cit.
than the other pile, and this number of blocks was *many*, and, since this pile contained the *most* blocks, the other pile contained the *least* blocks which was *less* than the number contained in the former pile. Upon such an anthropomorphic inference did Russell build his conclusion. In addition, it was to be noted that the writer in testing the subject's knowledge of *most* and *equal* did so in one specific relationship pattern. No attempt was made to allow the child to recall this idea or to recognize it in other situations in which he might have had greater familiarity with the concept. Thus, the seven-year-old child, from the data furnished by Russell, did not use or recall such terms as *many*, *most*, and *more*, but was capable of recognizing them when presented in a particular context.

Though the author cited two particular cases in which difficulty was experienced in the understanding of the concepts of *equal* and *same*, the data showed that *equal* was understood much more thoroughly than *most* in the identical combinations, though not as thoroughly in the non-identical combinations. Yet the author contended that "mathematical terms denoting *same* and *equal* are not likely to be comprehended; at least, the teacher cannot proceed on the assumption that the child will be able to use these terms."67 If such a conclusion was contained in the data, the writer did not furnish the reader with the facts upon which he might have based his independent judgment.

The author's conclusions as regards counting were as follows:

The responses from the younger children show that counting was not essential for the child to comprehend complex mathematical situations of more and less.\textsuperscript{68}

Counting by ones is a difficult method for differentiating groups and is not accurate above five.\textsuperscript{69}

Such a conclusion was definitely contrary to that found by Buckingham and MacLatchy.\textsuperscript{70} Russell's data on counting, however, exhibited two defectual characteristics: (1) they were not quantitatively obtained; rather, they were the results of qualitative observation by the experimenters; and (2) Russell employed counting as a means rather than as the end of the test itself. Russell stated as regards these objections:

Your objection on the problem of "counting" is interesting. I wonder if we could draw this conclusion from Buckingham and MacLatchy: After a test of counting objects, Buckingham and MacLatchy assume that children understand the names they give to the objects? I believe this assumption from Buckingham and MacLatchy represents their thinking. But, on the other hand, I found that when counting was used as a means [your phrase] these children did not comprehend number names (their significance) adequately up to ten.\textsuperscript{71}

The author again concluded: "It is not likely (as many have maintained that the first grade or second grade pupil will be mature enough to master completely and understand isolated addition and subtraction facts. Formal work such as drill over these arithmetic facts should be discouraged."\textsuperscript{72}

\textsuperscript{68} \textit{Ibid.}, p. 663.

\textsuperscript{69} \textit{Ibid.}, p. 656.

\textsuperscript{70} \textit{Cf. ante.}, pp. 44-52.

\textsuperscript{71} Letter from Ned M. Russell, June 27, 1940.

\textsuperscript{72} Russell, "Arithmetical Concepts of Children," \textit{op. cit.}, p. 663.
Such a conclusion seemed to be a broad inference from the available data. That the author had ascertained from a limited number of cases the experien-
tial background of the individual in arithmetic at the time he arrived at
school age was admitted. However, his study had made no attempt to determine
what experiential background was necessary for formal work in addition and
subtraction. Thus, he was assuming his minor premise, when he drew the con-
clusion that children with this known experiential background were unready
for a skill demanding a certain prerequisite amount of experience.

Finally, it would have been interesting to re-perform Russell's experi-
ment using a more precise method, a greater number and better sampling of
subjects, and a more precise statement of the results.

The study of Clifford Woody. The last investigation to be consi-
dered as regards the number abilities of young children was that of Woody.

The problem. The purpose of the study of arithmetical backgrounds of
young children by Woody was "to present some results obtained from an inves-
tigation designed to ascertain facts concerning the amount of arithmetical
knowledge and skill possessed by children in the primary grades at the time
at which formal instruction in arithmetic is introduced." By the "time at
which formal instruction in arithmetic is introduced," the author meant the

73 Clifford Woody, "Arithmetical Backgrounds of Young Children,"
Journal of Educational Research, XXIV (October, 1931), 188-201.

74 Ibid., p. 188.
time at which a definite period in the teaching schedule was set aside for presenting facts and concepts according to a definitely schematized plan, being differentiated from incidental instruction given in response to the number needs often encountered in primary classrooms. A more rigid definition of the problem seemed necessary in the light of the efficacy of incidental instruction in arithmetic; thus, Woody might have ascertained his data from children in the primary grades not only at the time "at which formal instruction in arithmetic is introduced," but also before which time no systematically-planned incidental instruction in arithmetic had been given by the school.

Concerning this, Woody wrote:

Considerable difficulty was experienced in defining properly the term "formal instruction." It was pointed out, I think, that one of the difficulties was that superintendents reported formal instruction was made as informal as possible and that informal instruction was made as systematic as possible. Naturally the writer was conscious of this in setting up the investigation. Nevertheless, he has visited numerous schools and finds that considerable variation prevails in the practice of teaching arithmetic. In some schools a definite schedule is set aside for teaching arithmetic, even as low as Grade One. In other schools, no definite schedule for teaching arithmetic is set up until in Grade Two or even as late as in Grade Four. In some of the schools the statement was made that they teach some arithmetic but it is incidental to the child's purpose and to the needs of other subjects; however, they state that at a given time in the life of the child they introduce a definite systematic program of arithmetic. It was out of this maze of conflicting statements that some definition of formal instruction had to be set up. After many conferences with teachers and supervisors, the definition was formulated. The inadequacy of the definition was perfectly apparent at the time of setting up the investigation, and if I remember correctly, was frankly admitted in the description of the article.75

75 Letter from Clifford Woody, August 7, 1940.
The method. An inventory test consisting of two hundred and four
different arithmetical situations to which the subjects made response was
employed for determining the arithmetical backgrounds of children. The test
was constructed on the basis of (1) a variety of situations existing in the
social environment of the child; and (2) simplicity for use with children in
kindergarten and Grade I and difficulty for use with children in Grades II
and III. The test was composed of three parts: Part I of rote counting
without objects, counting by enumeration, recognition of number as a group,
and reading of numbers; Part II, of size of numbers, telling time, fractions,
United States money, linear measure, liquid measure, and solving verbal pro-
blems; and Part III, of exercises in addition and exercises in subtraction.
The sampling of the test neglected the ability to select a certain number of
objects from a given number, development of concepts, and comparison in count-
ing. Again, the tests were designed merely to measure the child's readiness
as regards his experiential background in arithmetic.

In regard to this, Woody wrote:

The interview blank, itself, was formulated in a conference with
selected teachers and principals of the Ann Arbor schools. There was no
definite criteria for selecting the particular topics that were included,
other than a perusal of published materials dealing with the teaching of
primary arithmetic and with courses of study indicating the types of skills
which should result from formal instruction in arithmetic. As one who
has taught courses in the psychology of arithmetic for a number of years,
I included in the inventory blank some questions in which I had a personal
interest. There was no effort on the part of those responsible for the
investigation to take a complete inventory of the child's knowledge of
arithmetic and its relations. No doubt this would have been desirable,
however, this inventory contained over two hundred items and required a
considerable amount of time for its administration. A complete inventory,
such as you have indicated in some of your sections, would have required
a much longer test than seemed feasible. I personally would have been
anxious to have obtained information on a much greater number of questions
under each division of the test. I have no apology to make for the omission of a number of topics. It was not an oversight as you might suggest, but was a limitation subject to the amount of time available for administering the test. As it was, the public schools objected to the time-expenditure involved in giving the test. 76

Two thousand, eight hundred and ninety-five subjects were selected from thirty-nine different school systems widely scattered throughout the United States. Each primary teacher interviewed six pupils selected at random from those in her room. The subjects were described according to school grade and sex; no attempt was made to indicate their mental ages or socio-economic status.

Woody himself stated, "It should be emphasized that the test was merely an interview test and in no sense standardized." 77 However, from a logical analysis of the test, it appeared valid and the directions most explicit. The test had no time limit, and "the child was given only one opportunity to respond to each item, but he was allowed all of the time he desired in making his response; if it was evident, however, that he was unable to make a ready response he was confronted with the next exercise." 78 In cases similar to this, however, Terman did not hold to such a stringent viewpoint:

If the subject does not understand the question, or asks what is meant, it is permissable to explain only by repeating the pertinent part of the formula, unless an alternative form of the question is given in the manual.

76 Letter from Clifford Woody, August 7, 1940.

77 Woody, op. cit., p. 190.

78 Ibid., p. 190.
to take care of such an emergency. The examiner may even repeat the
to question more than once if the child remains silent, but except in the
case of young children repetition is not often called for and in general
is to be avoided.79

Woody partially agreed with this criticism:

I note you criticize the interview technique in that it seemed to you
entirely too stringent. I am inclined to think that for some of the
exercises your criticism is justified; for other parts of the exercises,
allowing only one opportunity to respond seemed necessary. Since the
main purpose of the investigation was to find the amount of arithmetic
which the child actually knew, we were interested in seeing to what extent
he could automatically count from 1 to 100; in knowing the extent to which
he could tell time as indicated on faces of clocks; in knowing the extent
to which he could recognize groups of objects, etc. The material was not
presented to the child in formal problem situations in which he had to
figure out the answers. The situations presented, as a general rule, were
simple and he either knew the answers or did not. Each interviewer made
a definite note of the child's responses. I am frank to say to you that
probably the most important part of the investigation, the results of
which have not been published, is in the nature of the responses which the
child made. Each interviewer obtained an extensive history of most inter­
esting facts concerning the nature of the processes involved by the child
in arriving at his answer. For instance, when we exposed domino patterns
and asked him to point to the domino which had just five spots on it,
de­finite information was recorded as to whether he recognized the number
of spots at once or whether he had to count each one. It was most inter­
esting to note that when we called for patterns of five, the child might
count the number of spots on the domino which had the five-spot pattern
and then recognize the same pattern the next time that it appeared without
counting each spot. It might be interesting to state that on the little
additions test, many children arrived at the correct answer by putting
down marks and then counting the marks. Others arrived at the answers by
a process of double counting. All sorts of interesting processes were
used in arriving at the answers. The point which you are criticising
simply gives you finally the number of responses correctly made. It hints
at the fact that some methods of arriving at responses are interesting and
suggests that because the child got the right answer that is no sign he
didn't need instruction in arithmetic.80

79 Terman and Merrill, op. cit., p. 54.

80 Letter from Clifford Woody, August 7, 1940.
The results. Woody emphasized in his results that "examination of the median scores indicated that the children in the Kindergarten responded correctly to approximately one-fourth of the exercises; the children in Grade 1A to nearly one-half of them; and the children in Grade 2A, to about three-fourths of them. These facts are all the more significant when it is realized that the children under consideration had received no formal instruction in the school."81 Upon consideration of the developmental levels, however, it was realized that formal instruction was a very insignificant variable in the maturational sequence. Though the subjects from kindergarten to grade three were alike in that they had received no formal instruction, they differed radically in their physical development, in their experiential background, in their felt-needs, and in their interests and attitudes.

One of the conclusions of the author was that "the exercises which involved counting the twenty circles and pointing to them in order proved to be much easier than those involving rote counting to 100 by 1's."82 The author, however, disregarded two principles: first, that he was not employing a common base for comparison, and that he should have stated the percentage of correct responses either to rote counting to twenty by ones or to counting one hundred circles and pointing the order while counting; and secondly, when such a comparison was made and in view of the fact that counting circles and pointing the order while counting was made up of three

82 Ibid., p. 196.
skills—number names, number sequence, and a one-to-one correspondence—the first two of which were equivalent to rote counting, it seemed possible for the child to lift himself by his own bootstraps to a level for which he did not possess the prerequisite skills.

In response to this criticism, Woody replied:

On page three of your report I note you criticize the conclusion "the exercises which involve counting the twenty circles and pointing to them in order proved to be much easier than those involving rote counting to 100 by 1's." These two exercises were set up as two independent exercises. There was no effort on my part to make an intensive study of the processes involved in counting. I am aware of the difficulties between rote and rational counting. I am also aware of the differentiations which you make in connection with this process and I agree with your statements concerning them; however, I feel that you ought not be too hard on my statement as I did not start out to make an intensive investigation of the relationship between rational and rote counting. I merely put in these two exercises and found to my surprise that a large portion of the children could count and point to 20 and count by rote to 100, and that these two tests are often set up in courses of study as the objectives to be attained in primary arithmetic. All that I was trying to do was to find out how the child reacted to these situations. To criticize me as disregarding two fundamental principles is really setting up a 'man of straw' and then attempting to knock him down.83

The conclusions of Woody were stated in terms of what "the children" knew and were capable of accomplishing. However, the range of testing included individuals from the kindergarten to Grade 3, individuals who seemingly varied enormously in physical, mental, social, and emotional development. To state the conclusions of a scientific investigation, the data of which were gathered from such divergent sources, under one all-inclusive term was verging on the unscientific.

83 Letter from Clifford Woody, August 7, 1940.
Again Woody replied:

No one is more aware of the fact that I grouped together children from the kindergarten through Grade Three, although in the tabulations these were isolated. I did not take into consideration any other factor than the fact that they were in the public schools and were going to be introduced to the formal study of arithmetic during the next year. That was the only point I was interested in. I note your charge of my being unscientific. I think you are just a little bit unfair, as the types of controls which you have suggested were not necessary in the type of investigation with which I was dealing.84

Need the reader a more positive proof of the author's "unscientific-ness" than his own admission of the lack of variable-control.

Finally, one of Woody's conclusions, "In general, the facts in this table suggest that children in the primary grades have some knowledge and understanding of such fractions as halves, thirds, and fourths, and that such concepts are within the comprehension of such children,"85 did not seem to follow from his data. That a knowledge of halves, thirds, and fourths in connection with their concrete presentation with the apple was possessed in varying degrees by children from kindergarten to the third grade was furnished by the data. However, there was a definite gap between knowledge of fractions in concrete situations and knowledge of fractions as "concepts."

And finally in reply to this criticism, Woody wrote:

In your final paragraph I note you object to my statement that children have some knowledge and understanding of such fractions as halves, thirds, and fourths. Possibly I should have stopped there. However, as

84 Letter from Clifford Woody, August 7, 1940.

85 Woody, op. cit., p. 199.
one familiar with practices in the teaching of arithmetic, I feel impelled to point out the fact that there is no particular reason why some experience with fractions should not be given to children in the primary grades. It is my conviction that simple number experiences involving fractions is as much a part of the life of the primary child as are other processes. While it is admitted that the tests situations set up were thoroughly inadequate to warrant a sweeping conclusion, I am willing to venture that a much more extensive testing program would justify the contention made. Of course, I am willing to admit that primary children are not capable of handling all of the complicated processes involved in fractions and in a complex knowledge of fractions as a concept, yet they do have the ability to understand the meaning of halves, thirds, and fourths, the number of halves that make a whole, that two-fourths equal one-half, etc. This is certainly a part of the fractions concept.86

It would appear that the writer and Woody differ as to their definitions of the terms, "knowledge," "understanding," and "concept."

II. INVESTIGATIONS ADVOCATING DEFERRED ARITHMETIC

The second group of investigations were those advocating postponement of the type of arithmetic instruction which was offered in the schools. Such researches, again, were deficient in that it had not been clearly ascertained that the child was incapable of learning at these early levels. Their major contribution, however, lay in the fact that they took into consideration the needs and interests of the learner in mastering certain arithmetical topics.

The study of the Committee of Seven.87 An investigation by the Committee of Seven begun in 1926 and concerning itself with the optimum time for beginning arithmetic marked the first effort of this body in the field of placement.

86 Letter from Clifford Woody, August 7, 1940.
87 Carleton Washburne, "When Should We Teach Arithmetic?—A Committee of Seven Investigation," Elementary School Journal, XXVIII (May, 1928), 659-65.
The problem. The problem as stated by Washburne was this: "Will pupils whose arithmetic instruction does not begin until the third grade catch up with pupils who begin formal instruction in arithmetic in the first grade?"88 And again, "Is there any definite and important gain in children's arithmetic knowledge as a result of beginning formal arithmetic instruction as low as the first grade or the second, or may arithmetic be profitably postponed until third grade?"89 Such a problem touched the heart of the deferred arithmetic movement. However, the numerous variables influencing its actual solution demanded a most exacting type of scientific research.

The method. Approximately five thousand sixth-grade pupils in fifteen Middle Western cities in schools of various sizes were employed. This group was divided into three equal parts which began formal arithmetic in Grades I, II, and III respectively. All of the pupils were tested in March, 1927 for their intellectual ability and their apperceptive background in the various arithmetic processes. The groups were then paired as to chronological age and intelligence quotient. Of the various investigations on deferred arithmetic, the method employed by the Committee of Seven was perhaps the most appropriate and the most scientifically controlled. In such a problem there was a demand for the parallel-group technique. While Taylor, Irwin, Wilson, and Benezet all indicated that they employed such a method, in none of these

88 Ibid., p. 660.

89 Loc. cit.
researches was the care exercised by the Committee of Seven in the equation of its groups employed. The one criticism of the Committee's techniques was that all the measures were applied after the groups had reached the sixth grade.

The battery of arithmetic tests were of the computational type. The pairing of the subjects was made as to chronological age and intelligence quotient. Various other factors such as initial background in arithmetic, socio-economic status, social and emotional maturation were neglected. The groups were well equated as to mental and chronological age. Whether, however, the other variables had any influence on performance was not indicated by the Committee. The conditions, location, and time elements under which the experiment was conducted were noted for their typicality, but not for their similarity. "It was found that the amount of time varied amazingly from school to school."90 Such variable factors as instructional technique, zeal of the teachers, personality traits of the teacher, and instructional materials were not controlled.

The results. The Committee concluded that "the sixth-grade pupils who began their formal arithmetic in the first grade have a distinct advantage in terms of arithmetic ability over the pupils who began arithmetic in the second grade and that the latter pupils in turn have the same advantage over the pupils who began arithmetic in the third grade."91 They accounted for

90 Ibid., p. 664.
91 Ibid., p. 665.
such a condition in that "the uniform superiority of the pupils who began arithmetic in the first grade over the pupils who began it in the second and third grade is due to the fact that they had an earlier start." However, such a statement was a rather broad generalization. Since other variables were present besides those of the "early start," it seemed necessary to include such factors in a statement of conclusions.

It was interesting to note that Washburne himself was aware of the deficiencies of this 1926 investigation, and thus set out in 1932 to re-perform the experiment. A more rigid matching of the groups was carried out, and the equation was made according to mental age, chronological age, and home environment. Twenty-five children were used in the experimental group, and the technique stressed the individual rather than the group approach in analysis. The experimental group played informally with numbers during their first year-and-a-half of school; the control group devoted one-third of their time beginning in the first grade to formal work in reading, writing, and arithmetic. After the first semester of the second year, all of the children, both experimental and control, were given approximately the same program which "consisted of many activities but of individualized systematic work in the academic subjects, not necessarily growing out of the activities, but, whenever opportunity presented itself, related to them."  

92 Loc. cit.

93 Mabel Vogel Morphett and Carleton Washburne, "Postponing Formal Instruction: A Seven-Year Case Study." Paper read at the Symposium on the Effect of Administrative Practices on the Character of the Education Program, St. Louis, Missouri, February 27, 1940.
In measuring the results at the middle of the second year, the experimental group "was definitely inferior in academic work to their controls." In addition facts, they graded 2.6 against 3.0, and in subtraction facts, 2.9 against 3.0. At the end of the second grade, the median score for the experimental group for the average of the battery of tests was grade 2.6 against the controls 3.4. At the end of the third grade, the experimental group surpassed the control with a grade of 4.4 against the controls 4.2. By the end of fourth grade, they had a superiority of half a grade, and thereafter kept and slightly increased this lead." Vogel and Washburne were to be commended for their conclusions drawn from the above data. They did not contend that their investigation had completely isolated the variable of deferred instruction, nor did they state that from their results it was to be concluded that arithmetic must be postponed to the middle of the second grade. Rather they stated:

The consistency of the data leads one to a strong suspicion that postponing systematic academic instruction until at least a year and a half after children have entered school, and substituting for it a large variety of educational experiences, whets children's appetite for learning and results in increased progress throughout the child's elementary-school life.

And again, "the experiment should be repeated in a number of places so as to get a much larger number of children and a greater variety of conditions."
Such repetition seemed necessary to find the answers to questions as to why the control group, which was arithmetically superior to the experimental group at the beginning of the third grade, did not remain so; as to what the effect of predetermined activities rich in arithmetical experiences would have been; and so forth. The Committee of Seven was forging ahead in the vanguard of the "meaning arithmetic" movement; only with the assistance and cooperation of other investigators in the field would the vital problems of readiness at all developmental levels be solved.

Washburne noted his agreement with this analysis.98

The study of Joseph S. Taylor.99 One of the first investigations advocating the postponement of formal arithmetic was that of Taylor.

The problem. The aim of the experiment was to compare the results and note the advantages of beginning arithmetic in 2A, the first semester in second year, rather than in 1B, the second semester in first year.

The method. The method employed by Taylor was the parallel-group technique. No regular arithmetic work except counting was given to the children in two IA classes, the extra time being devoted to English work; in another two IA classes, the regular program in arithmetic was followed. It was necessary to take into consideration the fact that Taylor performed his

98 Letter from Carleton Washburne, July 12, 1940.

experiment before the advent of the scientific movement in education. Thus, many of the criteria of experimental technique which were considered commonplace at the time of this investigation were unknown at the time his research was carried out.

The tests employed were the arithmetic tables of the 1A and 1B grades, and the written work of 1A, 1B, and 2A arithmetic. Emphasis in the tests was obviously on the computational functions. The description of the subjects in the two groups was entirely lacking except for the heading of one table which divided the 2A class of September, 1914, into the brighter class of the grade and the poorer class of the grade. No apparent attempt was made to equate the groups. The teaching method was not stated. However, such data seemed essential if the reader was to judge the validity of the controls employed.

The results. The author concluded that the classes which omitted number work during the first year of school not only held their own against that group that had number, but actually outstripped their competitors. Such a conclusion followed from the data; however, such a conclusion might not have been due solely to the fact that arithmetic was postponed. Such factors as superior capacity in the experimental group, greater experiential background in the arithmetic skills, incidental number situations in and out of the school situation, and various other unmeasured variables might have produced this result. From the data obtained, the author further inferred:

Hence it is hard to escape the conclusion that the time now given to arithmetic in the first year is worse than wasted; for when that time was devoted to English in P.S. 16, the children were able to read about three
times as much matter as classes of the same grade where arithmetic was studied.100

It was difficult to evaluate this conclusion for a variety of reasons, the lack of adequate measurements and controls in the experiment, the fact that the author failed to teach the subjects who were beginning arithmetic in the first grade more arithmetic, that is, when the two groups, the taught and the untaught, entered the 2B grade the taught group was unquestionably superior to the untaught group in arithmetic, and thus, if they were carried along at the same rate as the untaught group, these differences would still have been wide at the end of the 2B grade.

In summary, it appeared that Taylor's argument was not against teaching arithmetic in the first year-and-a-half of schooling, but that it was leveled against the type of arithmetic that was preferred. In this connection, the study contained a conclusion which had passed unrecognized by the author, that is, that children were socially and emotionally capable of learning arithmetic under the traditional method when they had reached the second semester of second grade.

No reply was obtained from Taylor in respect to the evaluation of his research.

The study of Elisabeth Irwin.101 Another study advocating deferring formal arithmetic, this time until the IIA Grade, was that of Irwin.

100 Ibid., p. 93.

The problem. The purpose of Irwin's investigation was a parallel-group study to determine the effect on arithmetic ability of the postponement of formal teaching and the curtailing of the amount of time spent on it during the elementary school course. The significance of such a problem in the field of maturation was weighty: if the individual who had received no experiences in arithmetic for the first year-and-a-half was capable of achieving as much as the individual who received formal instruction throughout this period—the two individuals being equated as to their various characteristics and all other variables being controlled except that of instruction—if such an investigation were to have been carried out, a major contribution would have been made to the concept of readiness.

The method. The method employed was that of the control versus the experimental group. The control group was taught under the traditional type of curriculum; and the experimental group work under a project method. Both groups were selected from a class of public school children in New York City and were described and equated as to their school grade, intelligence quotients, and educational quotients. The equating was done at the conclusion rather than at the beginning of the experiment. The group as a whole was intellectually superior, the intelligence quotients ranging from 100 to 135. The major issue in the investigation seemed to be that of the control of the variable. No attempt was made to measure the experiential background of the two groups in arithmetic before the experiment was begun; no attempt was made to determine how much arithmetic the experimental group had learned incidentally before it began formal instruction in the IIA Grade; the motivational
influences acting upon the individual's interests and needs did not seem to have been equated for both groups. In short, the method employed was not one whose purpose was to show the effect of postponing formal arithmetic for a year and a half; but rather whose purpose was to contrast an informal type of arithmetic instruction adapted to the social maturational level of the individual with the formal type of instruction employed in the traditional curriculum.

The results. The single conclusion of the author rested on this principle:

Children with I.Q.'s from 100 to 135 seem to learn as much as in academic subjects when they begin one and a half years later and when they spend less than one-third as much time daily on these subjects as do the children who spend full time for six years.102

Such a statement, however, denoted a lack of proper teaching techniques or materials or methods to bring about this failure of the control group to surpass the experimental group. The author's data seemed to be more in accord with a conclusion such as this: Children with I.Q.'s from 100 to 135 seemed to learn as much in academic subjects when they were taught under an informal activity program as when they were taught under the traditional curriculum.

The value of Irwin's experiment for the placement of arithmetic subject matter was almost nil, due to her lack of adequate measurements. Since the variable of social readiness was isolated, the author should have tested

102 Ibid., p. 107.
at each grade level to determine whether the optimum social maturational age had been reached.

In response to this evaluation, Irwin wrote:

I have read your summary of the Woodchuck article which I wrote in April, 1928, and quite agree with your conclusion that the results concerning the placement of arithmetic have no value. I therefore think it would be better to omit the whole thing as it wasn't an experiment conducted in any way to concern the teaching of arithmetic. I believe the whole thing is too general to have any value for your purposes.\(^\text{103}\)

The study of Guy H. Wilson.\(^\text{104}\) The results of an experiment to determine the social readiness of the first and second grade child for the arithmetic fundamentals, thus necessitating the postponement of formal instruction until the third grade were given by Wilson in his "New Standards in Arithmetic."

The problem. The purpose of the experiment was to test out the two questions raised by the Department of Superintendence in their Third and Fourth Yearbooks: (1) Should formal drill be replaced by informal informational type of arithmetic in grades one and two? and (2) If the program for grade three is simplified as suggested in the Fourth Yearbook will it be possible to approach letter perfect results in addition and subtraction by the close of the third year?\(^\text{105}\)

\(^{103}\) Letter from Elisabeth Irwin, July 4, 1940.


\(^{105}\) Ibid., p. 351.
The method. The author attempted to determine the result of an incidental program of number on the first and second grade child. The method employed was that of "building up number concepts, extending number experiences, and building a basis for understanding and thinking in number situations" without any effort on memorization of facts, without pressure, or without annoying checkups. Through the organization of some kind of store, by means of games, and other types of activities, "the children in this experiment were encouraged to actually gather information and become intelligent as to the use of numbers." During the third year, the time was divided between a definite systematic attack on drill in addition and subtraction, and the continuation of the informational work in the ratio of four periods to one.

The subjects were described as to grade-level and socio-economic status, that is, if the statement--"City A was used in this study because the children from that city were of the same general foreign type as in City B" was regarded as a valid description. Chronological age, mental age, experiential background in arithmetic were omitted. In Table I, an attempt was made to use the control group method by comparing City L which used the informal arithmetic with City B which used the formal type. Even though a prerequisite of the control-group method was the equation of the groups, the author concluded: "It appears from these data that the simple first decade

106 Loc. cit.
107 Ibid., p. 352.
108 Ibid., p. 353.
facts were better mastered, and there is no doubt that they were better understood.\textsuperscript{109} The approximation of the experimental conditions to the actual teaching situation was almost one-to-one; crowded room conditions, use of regular teachers, and so forth, tended to make the results applicable to normal conditions. The author made no attempt to measure the number ability of the subjects in addition and subtraction before the experiment or before the initiation of the formal work in the third year.

The results. The author seemed to have potentially contained in his data the characteristics of the experiential background necessary for success in arithmetic, in that he had shown that children were capable of success in arithmetic at the end of second grade when certain experiences were furnished them. The statement of such criteria might have been a greater contribution than the one Wilson preferred.

The results of the hypothetical traditional group employed by Wilson were described in terms of the 1916 Courtis Standards in addition—that of sixty per cent accuracy for grade three. The author then stated that the results in Lawrence gave a class average of 97.8 per cent. However, the author remarked: "It should be noted that the test used was not the Courtis Series B. It was instead the Wilson Survey Test, Form II. This test is not as difficult as the Courtis Series B."\textsuperscript{110} Thus, the author employed tests

\textsuperscript{109} Ibid., p. 354.

\textsuperscript{110} Ibid., p. 355.
with two sets of norms for comparable purposes. As regards this criticism, Wilson stated:

The significant comparison here is not with former standards of Courtis, but with City B and City R in which drill in arithmetic was prescribed in grades 1 and 2.\footnote{Letter from Guy M. Wilson, June 27, 1940.}

This statement of the author was not borne out by the original article; no comparison between City B, City R, and City L was found at the close of Grade III.\footnote{Wilson, \textit{op. cit.}, pp. 355-58.}

Wilson's contribution, therefore, seemed to be that of stressing the need for social readiness in the fundamental arithmetic skills. The child was not socially matured in the first and second grade to profit from formal instruction; not until the third grade was reached should a systematic methodology be employed. Wilson's study, again, was not comprehensive enough; though it showed that the child was experientially mature at the beginning of third grade, it did not demonstrate that he was not also ready at the beginning of the second grade.

\textit{The study of L. P. Benezet.}\footnote{L. P. Benezet, "The Story of an Experiment," \textit{Journal of the National Education Association}, XXIV (November, 1935; December, 1935), 241-44; 501-03; XXV (January, 1936), 7-8.} Benezet described his experiment advocating the postponement of formal arithmetic instruction until the sixth grade in more of a narrative than of an expository manner.
The problem. The problem selected by Benezet was the comparison of a traditional method of beginning arithmetic in the first grade and a purposeful activity approach, formal arithmetic not being taught until the sixth grade.

In the first place, it seems to me that we waste much time in the elementary schools, wrestling with stuff that ought to be omitted or postponed until the children are in need of studying it. If I had my way, I would omit arithmetic from the first six grades. I would allow the child to practice making change with imitation money, if you wish, but outside of making change, where does the eleven-year-old child ever have to use arithmetic?

I feel that it is all nonsense to take eight years to get children thru the ordinary arithmetic assignment of the elementary schools. What possible needs has a ten-year-old child for a knowledge of long division? The whole subject of arithmetic could be postponed until the seventh year of school, and it could be mastered in two years' study by any normal child.114

The author's fundamental assumptions, unsubstantiated by objective proof, were not in accord with findings such as those of Smith.115

The method. Though not definitely stated as such, the method of Benezet fell into the category of the control versus the experimental group:

I picked out five rooms—three third grades, one combining the third and fourth grades, and one fifth grade. I asked the teachers if they would be willing to try the experiment. They were young teachers with perhaps an average of four years' experience. I picked them carefully, but more carefully than I picked the teachers, I picked the schools.116

No mention was made of testing for the arithmetical background of the two

114 Ibid., p. 241.
116 Benezet, loc. cit.
groups. The control group was taught in the traditional manner—the author made no attempt to set down this course of study; the experimental group was given practice in estimating heights, areas, and so forth, no formal arithmetic being introduced until the seventh grade.

One test employed by the author to determine the effect of his arithmetic-less curriculum was that of orally questioning the children as to what they had been reading. Due to the fact that more time had been spent on reading in the experimental room, these children "fairly fought for a chance to tell me what they had been reading," while the children in the traditional room were "hesitant, embarrassed, and diffident." The tests used for final comparative purposes were not designated; the only indication that the reader possessed of their validity and reliability was that "about this time Professor Guy Wilson of Boston University asked permission to test our program." The majority of testing devices employed by the author were thought problems, no attempt apparently being made to determine their validity, reliability, or standardization; in addition, such problems seemed to be weighted in favor of the experimental group since it was this group that was being taught to "read, reason, and recite."

In the initial experiment described by Benezet, "three of the four schoolhouses involved were located in districts where not one parent in ten spoke English as his mother tongue." Later, the author spoke of the educated portion of the parents. No description of the subjects as to sex, age, mental age, school history, and only the brief description of socio-economic status noted above was available. Whether any attempt was made to equate the groups, the reader was unable to determine. In brief, a careful and objective defini-
tion of the materials, processes, and procedures under investigation was either only partially supplied or was lacking in entirety. The author presented no statistics as to the effect of his two methods of teaching, the majority of results being couched in the stenographic reports of the reactions of children in solving a particular problem stated by Benezet.

The results. From the first experimental group as tested by Wilson, it was found that in the earlier tests the traditionally trained people excelled—the tests involved not reasoning but simply the manipulation of the four fundamental processes. "By the middle of April, however, all the classes were practically on a par and when the last test was given in June, it was one of the experimental groups that led the city. In other words, these children, by avoiding the early drill on combinations, tables, and that sort of thing, had been able, in one year, to attain the level of accomplishment which traditionally taught children had reached after three and one-half years of arithmetic drill."117 Tests showing the results of experiments following this preliminary period were not furnished.

The author implied that children in the primary grades were unready for arithmetic, and thus systematic instruction should have been postponed. Benezet, however, must realize that he had isolated only the single variable of felt need in the social maturational pattern of the child. Thus, his conclusion was valid if it was meant to state the child's social unreadiness for the method of presentation which characterized the traditional curriculum.

117 Ibid., p. 244.
Whether the children who were required to master long division before leaving the fourth grade and fractions before leaving the fifth would have been capable of such tasks were the methodology changed, was a question the author made no attempt to answer. This seemed to be the important problem in curricular research, not the evasion of the issue, as was exemplified by Benezet in his postponement of instruction to a grade where the curriculum was already too overcrowded to provide sufficiently for an initial arithmetic program.

III. INVESTIGATIONS AT THE HIGHER DEVELOPMENTAL LEVELS

Placement of topics at the higher developmental levels was dominated by the investigation of the Committee of Seven, and, due to the thoroughness and scope of this research, all other investigations were evaluated as to their agreement or disagreement with that of the Committee.

The study of the Committee of Seven. A review of the entire field of maturational placement whether in arithmetic or in the various other subject matter fields revealed no more comprehensive study than that carried on by the Committee of Seven under the chairmanship of Washburne. This study had been initiated in 1926 and was still in progress at the time of this evaluation.

The problem. The problem as stated by Washburne in the Twenty-Ninth Yearbook of the National Society for the Study of Education comprised two phases of arithmetic readiness, mental readiness and experiential readiness: (a) At what stage of a child’s mental growth, as measured by intelligence tests, can he most effectively learn certain phases of arithmetic? (b) What
degree of mastery of the more elementary facts and skills is necessary for the effective learning of each of the above topics?\textsuperscript{116} In the Committee's report in the Twenty-Ninth Yearbook, these two aspects of readiness were given almost equal consideration. However, in the latest report, in the Thirty-Eighth Yearbook of the National Society for the Study of Education, the Committee more or less disregarded the phase of experiential readiness, merely according it one sentence throughout the entire report: "It should be borne in mind that all recommendations as to mental age placement presuppose reasonable mastery of foundations, i.e. possession of the knowledge and skill pertaining to prerequisite topics."\textsuperscript{119} The Committee was to be both commended and criticized for such a position; commended in that their problem was made more specific and thus their variable more isolated, and criticized in that experiential readiness was a concept about which more expert and thorough description was needed--and there appeared no more competent body than the Committee of Seven to investigate the arithmetical background necessary to begin a particular process.

As regards the problem, therefore, no investigation was found in the field of arithmetic placement which attempted such a thorough and significant piece of research. The Committee set forth to isolate the mental maturational factor in placement of arithmetic subject matter and carried out this


The method. The technique of the Committee of Seven was as follows:

(1) The approximate grade placement of a unit of arithmetic was determined either by a survey of practice or by preliminary experiments in a few schools; this approximate placement was termed the "central grade."

(2) The cooperation of schools willing to teach the topic at the central grade, or one grade lower, or one or two grades higher was secured. The spread of mental ages within each group of the cooperating three or four grades resulted in a distribution of mental ages in final scores covering usually at least five or six years.

(3) The children in the cooperating schools were given intelligence tests to determine their mental ages, pretests to determine their existing knowledge of the topic to be taught, foundations tests to discover whether or not they had the prerequisite knowledge and skill and in some cases the prerequisite experience and concepts for learning the new topic.

(4) There was a brief teaching time allowed after the administration of the first form of the foundations test for the teacher to attempt to bring the children to a reasonable mastery of such foundations as seemed to be lacking.

(5) A second form of the foundations test (equivalent form) was given, and the retention test results were later compared with the results of this second form of the foundations test and those of the intelligence test.

(6) The time and method of teaching were controlled in that the number of minutes per day and number of weeks of teaching, together with a general teaching outline indicating the methods to be used were stated. Teaching tests were employed during the teaching period in order to help the teacher determine the progress of the children.

(7) A final test was given at the end of the teaching period to determine the immediate learning of the children.

(8) A retention test was given six weeks after the final test with no intervening review. Children making scores on the foundations test which had been shown to be predictive of failure were omitted from the final tabulation. Children making such scores as to indicate previous teaching of the topic were eliminated. Thus, the Committee's recommendations were based upon retention of children who had not had considerable previous knowledge of the topic, but who had achieved a fairly adequate mastery of prerequisite topics, and, in some cases, of prerequisite concepts.
Measures of readiness employed by the Committee of Seven consisted of the foundations test and the intelligence test. The Committee itself was aware of the fact that more predictive measures of readiness might be found in the future:

Measures of children's concepts and experiences, their needs and interests, and measures of that phase of mental growth most closely correlated with success in arithmetic would presumably be more effective in determining children's readiness for a given topic than measures of mere knowledge of prerequisite skill and general level of mental growth as measured by an intelligence test.\(^{120}\)

Thus, the Committee isolated the maturational variable of mental readiness and constructed techniques to measure the child's capacity and his experience in the field of elementary arithmetic.

The experiment embraced 255 cities and towns and sixteen states, involving 1190 teachers and 30,744 children. The adequacy of sampling was observable from the graphs contained in the presentation of the data, which furnished a spread of mental ages within each group of the cooperating three or four grades resulting in a distribution of mental ages in the final scores covering usually at least five or six years. Description and sampling of the subjects was effected through their mental ages and individual results on the various arithmetic tests.

As to the validity of the tests used in measuring, the Committee employed two types of instruments: the intelligence test and the arithmetic tests. The Committee presented no statistics to show that the arithmetic tests—the pre-tests, the foundations tests, the final tests, and the retention tests—measured what they purported to measure. Only in Washburn's reply to Raths was there found any attempt to indicate validity in the

\(^{120}\) \textit{Ibid.}, p. 320.
instruments employed:

First, the foundations tests gave indication of validity by predict­ing, with as much accuracy as could be expected, the learning of the process for which they were to be foundations; in other words, they measured what we wanted them to measure. The process tests, it will be remembered, were in three equivalent forms; a pre-test given before the teaching process began, a final test given at the close of the teaching process, and a retention test given six weeks later. The sharp rise in scores between the pre-test and the final test during the teaching period and the slight fall between the final test and the retention test are clear indications that the tests measured what we wanted them to measure, namely, the result of the teaching.121

Washburne made the assumption that his tests were valid; his arguments tended to demonstrate the consistency rather than the validity of his instruments. However, as noted by Monroe and Engelhart:

Unless some unusual achievement is specified or implied, most tests designed to measure calculation skills are probably of rather high validity. They, of course, measure the current ability of pupils rather than the permanent residue of achievement. It is likely that the latter type of achievement should be considered, but few, if any, investigators have attempted to base their conclusions on it.122

Since the Committee of Seven’s tests measured in general "calculation skill," there appeared no major dispute as to the validity of these instruments.

The purpose of the foundations test was to determine the status of the child as regards the prerequisite knowledge and skill for mastery of a particular topic. Since in these prerequisites were included the child’s basic


concepts and experiential background, the foundations tests were expected to include measures of such background. However, in many of the tests, mechanical facility in the formal skills was the only ability sampled. For example, the foundations test for long division tested only simple multiplication, subtraction, and short division.

Again, the tests should have sampled equally all of the various types of skills involved in a particular ability. The sample retention test in subtraction as given in the Twenty-Ninth Yearbook failed in this respect. Below are indicated the skills which the test should have sampled and the skills which were actually sampled:

**SKILLS WHICH SHOULD HAVE BEEN SAMPLED IN A SUBTRACTION RETENTION TEST**

Subtraction Combinations With Bridging
I. The thirty-six difficult combinations
   A. Subtrahend under five
   B. Subtrahend over five
II. Higher decade subtraction facts
   A. One-place number subtracted from a two-place number
   B. Two-place number subtracted from a two-place number
      1. Remainder is a one-place number
      2. Remainder is a two-place number
   C. Two-place number subtracted from a three-place number
      1. Borrowing in one step only
         a. Unit column
            1' Three-digit remainder
            2' Two-digit remainder
            3' One-digit remainder
         b. ten column
            1' Three-digit remainder
            2' Two-digit remainder
            3' One-digit remainder
      2. Borrowing in both steps
         a. Three-digit remainder
         b. Two-digit remainder

D. Three-place number subtracted from three-place number

1. Borrowing in one-step only
   a. Unit column
      1' Three-digit remainder
      2' Two-digit remainder
      3' One-digit remainder
   b. Ten column
      1' Three-digit remainder
      2' Two-digit remainder

2. Borrowing in two steps
   a. Three-digit remainder
   b. Two-digit remainder

<table>
<thead>
<tr>
<th>SKILLS SAMPLED</th>
<th>IN THE COMMITTEE OF SEVEN'S</th>
<th>SUBTRACTION RETENTION TEST</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) 271</td>
<td>C, 2, a</td>
<td>(5) 601</td>
</tr>
<tr>
<td>-68</td>
<td></td>
<td>-303</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) 127</td>
<td>C, 2, b</td>
<td>(6) 602</td>
</tr>
<tr>
<td>-69</td>
<td></td>
<td>-297</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(3) 540</td>
<td>C, 1, a, 3'</td>
<td>(7) 49</td>
</tr>
<tr>
<td>-16</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(4) 766</td>
<td>Three-digit from three-digit</td>
<td>(8) 506</td>
</tr>
<tr>
<td>-125</td>
<td>no borrowing</td>
<td>-199</td>
</tr>
<tr>
<td></td>
<td></td>
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</tr>
</tbody>
</table>

The test was open to criticism, therefore, in that it did not sufficiently sample the skills and was excessively weighted with hard items.

The one-group technique presupposed constancy in the factors of pupils, teachers, and school setting; variability in the experimental procedure plus such changes as were taking place in the group or teacher with the passage of time and with maturation.124 However, the different types

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of home backgrounds, experiences, earlier arithmetic instruction in a variety of textbooks and under a variety of teaching methods and school systems together with individual variance of method by each teacher depending upon his experience and background—such non-constant factors as these would have tended to affect the results. The Committee made no attempt to take these factors into consideration, but rather stated that they tended to cancel each other due to the large number of children and teachers. 125

Similarly, the possibility of practice effect on these "practically equivalent" 126 forms of the tests, either in the type of test or in the method of attack, may have tended to affect the results as set down by the Committee.

In scoring the tests, the Committee of Seven gave credit only for the correct answer. In the more advanced processes, however, the actual error might have been made in one of the prerequisite skills. Brownell, in criticizing this point, contended that credit given for the understanding of the crucial element in the process would be more fair and would thus tend to lower the Committee's high mental age standards. 127 From the pragmatic viewpoint, however, process knowledge was of little avail, if the process did not "work," that is, if the child was incapable of obtaining the correct answer.

The two types of tables were presented in the Committee's data: (1)


126 Ibid., p. 300.

average scores made by the subjects on the retention tests were plotted against their mental ages; and (2) the percentage of subjects making a score of eighty per cent on the retention test was plotted against their mental ages. Thus, mental ages corresponding to the eightieth percentile were read off from the table directly. The tables were clear and self-explanatory.

The selection of the eighty per cent score as the acceptable standard for mastery of a particular topic was a purely arbitrary criterion. The Committee, in its latest report, did not make as exorbitant a claim as was made by Washburne in the Twenty-Ninth Yearbook when he maintained, "There is a definite level below which the attempt to teach any given process is usually futile."

Ten years later, however, he conceded: "Obviously, if a teacher or a school system is satisfied to have a smaller percentage of children reach mastery or is willing to use a lower standard of mastery, lower levels of mental age may be chosen as the points at which to teach the various topics."

The Committee, as noted previously, considered the individual differences in subjects and in teachers to be a non-variable factor. Whether or not the large number of teachers and of subjects tended to cancel these differences was a matter of debate. Nevertheless, the Committee deemed such changes insignificant, since no attempt was made to measure their effect. As

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regards the reliability of the test, Washburne stated:

The reliability of the tests was not statistically checked, but it is shown by the high relationship between the retention-test scores and final-test scores, the retention and final tests being equivalent forms of the same test, as already indicated. 131

The results. As regards the validity of the Committee's results, the generalizations were made on the all-or-none principle. Processes were placed by wholes, and subprocesses were included in this general placement. However, there is a significant difference in the difficulty of subtracting 31-5 and 208-199; yet, the Committee's placement located both of these processes at the same level. The tests, therefore might have been improved in two respects: (1) They might have been made more analytic, and thus all the various sub-processes be taken into consideration; and (2) they might have sampled each sub-process more efficiently and thus have guaranteed more valid generalization.

Since the results of an experiment are valid only under the conditions of the experiment until otherwise demonstrated, the conclusions reached by the Committee of Seven were valid only in terms of their particular teaching method and teaching time. Even though Washburne stated that "the methods outlined are those readily usable by most good schools and substantially the ones in use in many good schools," 132 the objection that "changes in the order

131 Washburne and Voas, op. cit., p. 406.

of teaching the sub-skills, in the quality of previous instruction, in the
length of the daily period, in the effectiveness of motivation, in the number
of days allotted the topic, in the thoroughness of diagnosis and remedial
instruction--changes in any of these details may be enough to invalidate the
Committee's findings, "133 was valid. This criticism, however, was made not
against the controlled experiment technique employed by the Committee, but
rather against the Committee's tendency to generalize their results for all
methods of instruction. Theoretically, such a conclusion might have been
valid; its general acceptance, however, awaited experimental verification.

The Committee failed to derive all conclusions that were potentially
contained in its data in that no recommendations were made as to the methods
and means of enriching the curriculum. Such enrichment would have resulted
in each child possessing the proper experiential background necessary for the
learning of a particular topic when he had reached the maturational level for
that topic. The Committee seemed to have fallen into the former conviction
that the maturational level was a phenomenon-to-be-avoided, not an event-to-
be-prepared-for.

As to the verifiability of its results, the Committee recommended that
its experiments be repeated by others under identical conditions and under
conditions in which some one or two elements were varied, in order to verify
and supplement the present findings. An excellent verification of the
results of the Committee of Seven was the investigation carried on in the

133 Brownell, "A Critique of the Committee of Seven's Investigations
on the Grade Placement of Arithmetic Topics," op. cit., p. 497.
Chicago Public School System. The revision of the Chicago Course of Study in arithmetic was made in terms of the results of the Committee. Results from three survey tests so far given at semester intervals to random samples of three thousand, ten thousand, and two thousand pupils in each of grades three to eight showed that increases in efficiency in terms of per cent based on points gained each semester were as follows:

<table>
<thead>
<tr>
<th>Grade</th>
<th>Gain First Semester</th>
<th>Gain Second Semester</th>
</tr>
</thead>
<tbody>
<tr>
<td>3B</td>
<td>3</td>
<td>27</td>
</tr>
<tr>
<td>3A</td>
<td>19</td>
<td>0</td>
</tr>
<tr>
<td>4B</td>
<td>10</td>
<td>6</td>
</tr>
<tr>
<td>4A</td>
<td>31</td>
<td>16</td>
</tr>
<tr>
<td>5B</td>
<td>6</td>
<td>17</td>
</tr>
<tr>
<td>5A</td>
<td>6</td>
<td>10</td>
</tr>
<tr>
<td>6B</td>
<td>13</td>
<td>15</td>
</tr>
<tr>
<td>6A</td>
<td>7</td>
<td>15</td>
</tr>
<tr>
<td>7B</td>
<td>19</td>
<td>1</td>
</tr>
<tr>
<td>7A</td>
<td>15</td>
<td>41</td>
</tr>
<tr>
<td>8B</td>
<td>20</td>
<td>22</td>
</tr>
<tr>
<td>8A</td>
<td>16</td>
<td>43</td>
</tr>
</tbody>
</table>

Average: 14.9%

The above gains in so short a period coupled with the fact that teachers and pupils are better satisfied because getting better results than formerly are here offered as the share of the bit of evidence contributed by better grade placement of arithmetic topics in the elementary school.\(^{135}\)

In the earlier report of the Committee, recommendations were made in specific numerical values and ages to be attained before the pupil was considered ready for a particular process.\(^{136}\) In the later reports, however, recommendations were made in terms of periods or general developmental stages. Indeed, since the human organism and the numerous variables characterizing its

\(^{134}\) J. T. Johnson, "An Experiment in Grade Placement of Curriculum Units in Mathematics." Paper read before the American Educational Research Association at the St. Louis Convention, February 27, 1940.

\(^{135}\) Loc. cit.


growth and development were being dealt with, and since the research was intended for consumption by the average school teacher, the Committee's shift from specific and dogmatic recommendations to general and variable placements was both more reasonable and more in accord with present psychological practices.

The curriculum reorganization as advocated by the Committee of Seven was undoubtedly an improvement over the traditional curriculum with its non-scientific basis and frequent violation of common sense principles, often resulting in a large number of failures. The Committee of Seven experiments appeared to be the forerunner of a new epoch in curriculum construction but needed to be followed by numerous other investigations. As Washburne himself stated:

The findings of the Committee should be used merely as points of departure, and whenever new and refined or more extensive experiments indicate that a particular topic should be adjusted up or down in the curriculum, the findings of these experiments should be promptly substituted for the findings of the Committee.138

With respect to the evaluation of the Committee of Seven investigation, Washburne wrote:

Thank you for letting me see the enclosed research report. I have read it with interest. I am much too busy right now to be able to comment on details. As a whole I think it's a very good report. There naturally are some points at which I would take issue with you if time permitted.139


139 Letter from Carleton Washburne, July 12, 1940.
The study of John Guy Fowlkes. An excellent example of an indirect placement study was that carried out by Fowlkes. The purpose of the study concerned itself with determining the relative difficulty of the one hundred basic multiplication facts; but only by inference could the reader conclude that, since the subjects were capable of learning the one hundred basic multiplication facts at this maturational level, the facts should be here placed.

The problem. The actual problem as stated by Fowlkes was a controlled study of the learning of multiplication by third-grade children. In the development of this topic, the author concerned himself primarily with the question of the difficulty of the multiplication facts, comparing his study with that of Clapp. Though the author undoubtedly selected a most significant problem, the actual statement was rather broad. Implied and actually noted in the study under the heading of the learning of multiplication by third-grade children were such important considerations as individual differences in the subjects, variations in teaching procedures and materials, difficulty of the multiplication facts, and so forth. The problem, therefore, seemed to demand a more specific definition.

The method. The method employed in the experiment was composed of three steps: (1) the subjects were taught the one hundred combinations over a period of twenty days, thirty-five minutes of each day being devoted to the

work, by means of text material alone, the teacher doing as little teaching as possible; (2) remedial work was carried on by means of printed directions and devices rather than oral instructions; and (3) the relative difficulty of the one hundred combinations was determined when they were developed by the pupils individually and when the frequency of drill on each combination was the same.

The subjects were thirty-one children in the first half-year of the third grade. They were described as regards their intelligence quotients, which ranged from 94 to 137, with a median of 104.5; their experiential background in the actual skill to be taught, showing a range of from zero to sixty-six, with a median of twenty-three in the number right. No indication of chronological age, mental age, socio-economic status, school history, or experiential background in addition and subtraction were included in describing the subjects.

The experiment seemed to employ the one-group method. However, the author made a comparison between the experimental group and the achievement of the other third grades and the fourth grade of the entire school system of Madison in the one hundred multiplication facts. In this comparison no attempt was made at equating the two groups, a technique which would have seemed to be a simple one in view of the fact that only thirty-one children were used in the experimental group and were described only as regards their I.Q. and skill in multiplication combinations; such a pairing would have given more substantiation to the above statement, since from such a statement the reader was unable to determine the relation of the experimental group and the various factors which might differentiate it from the regular third and
fourth grade group.

The results. The results in which this thesis was interested were those concerning the mastery of the one hundred basic multiplication facts. The median number of right answers to the one hundred basic facts on the last day of the experiment was ninety-one, the range being thirty-nine, and $Q_3$ and $Q_1$, ninety-eight and seventy-nine respectively. The mental age of these Grade IIIB children should have ranged approximately between eight-and-a-half and nine-and-a-half years. In comparison with the Committee of Seven as to method and results, the study by Fowlkes was in close accord. The teaching time of Fowlkes was twenty-days; that of the Committee of Seven was thirty-eight. The retention tests of Fowlkes were given immediately after teaching; those of the Committee of Seven were given after a six-week period. The data of Fowlkes contained the number of right answers made by the pupils at the lower quartile; the recommendations of the Committee of Seven were made in terms of the lower quartile. The Committee of Seven placed the multiplication facts with products of twenty and less at the mental age of 8-9, those with products of more than twenty at the mental ages, 9-10; Fowlkes indicated that the subjects located at $Q_1$ gave seventy-nine per cent of the right answers to the one hundred basic multiplication facts on the last day of teaching.

As regards the evaluation of his study, Fowlkes wrote:

It seems to me that you have reviewed the study in the learning of multiplication facts reported by me rather well. In connection with your data about the pupils you may have guessed that a good deal of information was available including the factor that you mention in the paragraph which I have marked, but it would not seem particularly important to include
them in the report.\textsuperscript{141}

The study of Grant M. Norem and F. B. Knight.\textsuperscript{142} Another investigation indirectly concerned with placement at the higher developmental levels was that of Norem and Knight.

The problem. The study by Norem and Knight was concerned primarily with ascertaining the relative difficulty in the learning of the one hundred multiplication combinations by twenty-five third-grade children. Only indirectly was the question of placement inferred from the data presented.

The method. The method employed consisted of an individual drill and test program, "an earnest attempt to do nothing which could not be easily done in any typical third-grade classroom."\textsuperscript{143} Two pretests were given to determine the subjects' knowledge of the multiplication combinations before formal instruction; the combinations not responded to correctly in both of these tests were the object of a learning and drill program employing the additive method until mastered. The children were allowed to practice only when working in school and then only when an observer could easily count their practice and immediately correct their errors. The three factors of practice, error, and speed were measured. The subject was tested on a mastered combination once a week for a period of six weeks and then once a month

\textsuperscript{141} Letter from John Guy Fowlkes, June 28, 1940.


\textsuperscript{143} Ibid., p. 553.
to a limit of three months. The devices used for measurement were the polygraph for response time, observation for the amount of practice, and counting for the number of errors.

The subjects employed by Norem and Knight were described as "typical third-grade children." However, in the table furnishing the individual differences in the children used in the experiment, the average chronological age of the twenty-five subjects was eight years and ten months; the average mental age, ten years and seven months; the average I.Q., 121.3; and the average arithmetical experiential background in the multiplication combinations before formal teaching, a knowledge of thirty-two of the one hundred combinations. Such a description seemed to be in discord with the authors' hypothesis of the typicality of their subjects. The description of the subjects, however, was excellent. Norem replied to this statement: "As to the typicality of our subjects, they were definitely above average in ability, but they were third grade children doing their work in their regular school room."144

Though the authors contended in regard to their use of an observer to count the practice and correct the errors that "this aspect of the experiment after all approximates a good learning situation quite possible in a classroom,"145 the average teacher would find difficulty in employing such a technique. Again, the presence of this observer as a prerequisite to the

144 Letter from Grant M. Norem, June 28, 1940.

145 Norem and Knight, op. cit., p. 553.
child's practice of the multiplication combinations was certainly not a typical learning situation. As regards this point, Norem wrote:

We were forced to deviate from the regular practices of the school to an extent that would permit observation of the amount of practice, I doubt that the methods we used had learning values that would rate as superior to regular classroom practices. Our procedure is described fully in my unpublished thesis. 146

The data "were carefully surveyed, summarized, and finally organized into a form which the experimenters judged would set the results out in a serviceable fashion." 147 Thus, the tabular and graphical means of analysis used in the investigation were appropriate, and the statistical methods employed were applicable to the materials at hand. One criticism of the data, however, might be made in that the teaching period was not specified; the reader was furnished with the average number of learning responses, but was given no indication as to the time of a specific learning response. Such data were necessary for comparative purposes. Norem supplied this information: "The working time for each pupil was about three minutes each day (each school day) between January 4 and May 18. On days that the children were tested the working time was somewhat longer. "148

The individual record charts were compiled from data assembled by the observers. The authors failed to state, however, the qualifications and the training of these observers. Such a statement was the reader's only indication of the reliability of the devices used in measuring, the absence of subjective scoring, faulty administration, and so forth. In reply to this

146 Letter from Grant M. Norem, June 28, 1940.
147 Norem and Knight, op. cit., p. 554.
148 Letter from Grant M. Norem, June 28, 1940.
criticism, Norem wrote:

As explained in my unpublished article, the assistants (or observers) who worked with me were trained by me to follow a definite plan. The procedure followed was simple and did not vary from individual to individual. The assistants were all University students. 149

The selection of subjects definitely did not represent a random selection of the population. The intelligence quotient range of ninety-eight to one hundred and forty-eight indicated an intellectually superior group.

The results. No mention was made in the results as stated by the authors of the problem of placement. Only by inference, therefore, could the reader conclude that under the methodology as specified by the authors, and with individuals of the intelligence level and arithmetical background of the subjects employed, the one hundred multiplication facts were learnable in the third grade. Thus, the authors did not furnish the standards upon which mastery of the combinations should have been based. Rather, they stated: "The charts exhibited all practices, all errors, and all undue hesitations which were interpreted as evidence of incomplete learning. These form the basis for the data in this report." 150 To measure the difficulty of the combinations, such criteria were sufficient; to determine the placement, as to whether the subject could be taught this combination most economically and efficiently at this developmental stage, however, a limit needed to be set as to the amount of practice, the number of errors and undue hesitations beyond which the skill might have been better postponed. The only indication of such

149 Letter from Grant M. Norem, June 28, 1940

150 Norem and Knight, op. cit., p. 553.
data was given by the authors, when they stated, "From a study of Column C we gain an idea of the amount of practice needed by the average child for learning the combinations when the learning situation approximates typical conditions." Thus, it seemed that all of the subjects mastered the combinations, and thus placement at this level was valid. As regards this point, Norem wrote:

As explained in my unpublished article, 'A combination was considered unlearned until the child responded to it with a correct rapid response when the combination was first presented to him in a given work period. In the case of the drills from two to four correct rapid reactions were required, obtained at intervals between which other combinations were drilled.' Mastery is probably never absolute; it is a matter of degree.

The conclusions drawn from the data of Norem and Knight seemed to invalidate the statement of Washburne:

Multiplication facts with products over twenty are not adequately learned at a mental age of ten years, nine months; only fifty-six per cent of the children of this mental age make scores of seventy-six per cent or more, even when they have an adequate foundation of addition facts.

However, in the data of Norem and Knight, an individual with a mental age of nine years made 221 total errors in responses and 2,128 total learning responses, while an individual with a mental age of thirteen years and six months made 267 total errors in responses and 2,316 total learning responses; and the data as a whole supported the contention that individuals possessing an average I.Q. of 121.3 and an average mental age of ten years and seven months were capable of learning the one hundred multiplication combinations. The

151 Ibid., p. 558.
152 Letter from Grant M. Norem, June 28, 1940.
incongruency between the results of Norem and Knight and those of the Committee of Seven seemed to lie not in the conclusions per se, but in the method of arriving at the conclusion; whereas the Committee of Seven employed a group method, a specified teaching time, testing for retention after six weeks, and a lower quartile achievement standard, Norem and Knight employed an individual method, a learning and drill program until the skill was mastered, testing for retention once a week for a period of six weeks and then once a month to a limit of three months, the average number of responses in this "maintenance work" being 694 during the six week period, and no specified standards of mastery.

The study of Foster E. Grossnickle. A study brought forward by Brownell as contrary to the Committee of Seven's placement of long division was that by Grossnickle.

The problem. The main purpose of Grossnickle's study was to determine by means of the parallel group technique whether the increase-by-one method or the apparent method of estimating the quotient was the better; the findings in this connection did not concern the present research. However, the secondary purpose of the investigation was from the placement point of view, and set out to determine whether the individuals who had mastered the pre-requisite skills for long division and who had reached a certain mental age were capable of succeeding in long division. Grossnickle confirmed this


statement of the problem:

Grade placement was a by-product of this study. I was not interested in grade placement in this investigation. I wanted to see which of two methods is better. I did show quite conclusively that division can be taught in Grade 4. 156

The method. Due to the fact that placement was an aim subsidiary to determining the value of the two methods of estimation of the quotient, the investigation attacked the placement of long division in only a single grade. The method employed was as follows: the author supervised the teaching of division with a one-figure divisor in the first half of Grade IV, using the same type of technique which he planned to employ in the actual experiment during the last half of Grade IV. For such a procedure, the author was to be commended in that he was thus accustoming both the teachers and the subjects to the materials and thus reducing any influence which this variable might exert in the actual experiment. The teaching of the two-figure divisor was begun in February and a total of seventy-six teaching days were devoted to the work. Practice material was delivered to the teachers at the beginning of each week, collected at the end of the week, scored by pupils under the experimenter's direction, and returned to the pupils with new practice material of the following week. A statement as to the teaching method employed was vague: "The practice material, with suitable instructions, was delivered to each teacher at the beginning of the school week." 157

For the remaining twenty-three days of teaching and practice the group using the apparent method was given practice in making the quotient one, two, or three less than estimated. The other group was shown how to

156 Letter from Foster E. Grossnickle, July 3, 1940.
correct the quotient figure by making the estimated figure one or two less in some cases and one or two more in other cases. 158

The correction by the pupils of the scored errors and accomplishment of new work required three or four periods of thirty-eight minutes each; the remaining period or periods was used by the teacher for number work non-related to division and its concomitant processes. A practice sheet having no errors was returned to the experimenter's file; a practice sheet having errors was corrected by the pupil before the paper was collected. Four tests were administered from the beginning to the end of the experiment, on the first day, on the thirty-fifth, on the fifty-third, and on the seventy-sixth day. The tests were not equivalent forms but increased in difficulty. The time for the tests was determined when the majority of pupils turned in perfect papers on certain type of estimation. The method of scoring employed in these tests, however, was irregular: a "Correct Score" was designated as the number of correct figures in the quotient and in the remainder, and an "Estimation Score" represented the number of correct estimations in an example. The tests employed seem to have been constructed with a great amount of forethought. The four types employed were described, but a sample only of the last was given; upon analysis, the reader could ascertain the validity of the author's claim that the test "contains all the difficulties of estimation which the writer considers possible when the divisor is a two-place number, except in the case of the division demons (13-18)." 159

The subjects were described as to the type of educational program they

158 Ibid., p. 672.

159 Loc. cit.
received—the criterion used being the ratio of expenditure to the average daily attendance—their intelligence quotients, and their prerequisite skills in the one hundred basic facts in subtraction, in the ninety basic facts in multiplication, in the ninety even facts in division, and with the use of a one-figure divisor in long division. No statement, however, was made as to their individual mental ages, a description which seemed necessary for valid mental placement. Again, the ability of the subjects on the prerequisite skills appeared exceptionally high; for example, the mean number of errors, in long division with a one-figure divisor was $3.16 \pm .202$, which number of errors, the author himself stated, "is lower than the number for any of the groups of students in Grades V-XV, inclusive, who had taken the same test in a previous investigation."^{160}

The method was deemed a very precise one with its use of exactly-timed teaching periods and predetermined practice sheets. Thus, the results were necessarily specific and in terms of the method employed. Whether such results were generalizable to the conditions under which the experiment was expected to apply cannot be determined from the data supplied.

Finally, as regards methodology, the author tested for the prerequisite skills of long division, but failed to test for the presence of the ability to estimate the quotient in long division when the divisor is a two-figure number; thus, how much the group knew concerning the topic before it was taught was not ascertained. Grossnickle stated as regards this criticism that none of the pupils had ever been taught two-figure division previously.\^{161}

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^{160} Ibid., p. 675.

^{161} Letter from Foster E. Grossnickle, July 3, 1940.
Incidental learning, however, might have taken place and thus should have been tested for.

The results. The conclusions which concern the present research were those made by the author as regards placement of long division:

There is no discernible reason why division with a two-figure divisor cannot be learned in Grade IV when the pupil has attained a mental-age level of about nine years and seven months.\(^{162}\)

This conclusion was supported by the data presented, provided there was read into the conclusion the specificity of the method and of the scoring under which the data were gathered. The conclusion, however, was not in accord with the investigation of the Committee of Seven which has already been analyzed and has been deemed a valid and reliable experiment. Grossnickle attempted to resolve this disagreement thus:

The sequence of difficulties in the investigations by the Committee of Seven may not have received the close scrutiny that was followed in this study. Another factor may have been the vigilant watch for errors in this study. Every pupil was required to solve all examples correctly before he could proceed to the next practice exercise.\(^ {163}\)

Similarly, Washburne stated:

The subjects' ability in short division was unusually great; the teaching time was fifty per cent greater; the tests were given immediately after teaching, and therefore do not represent retention six weeks later, as do those of the Committee of Seven; and success is measured by mean achievement, whereas the Committee of Seven is lower quartile achievement; i.e., the Committee's recommendations are based on the mental age at which three-fourths of the children can achieve mastery. Therefore, the Grossnickle results as published cannot be validly compared with those of the Committee of Seven and can neither verify or controvert the Committee's findings.\(^ {164}\)

\(^ {162}\) Grossnickle, op. cit., p. 677.

\(^ {163}\) Ibid., p. 676.

\(^ {164}\) Washburne, "The Work of the Committee of Seven on Grade-Placement in Arithmetic," Thirty-Eighth Yearbook, op. cit., p. 318.
In summary, it must be kept in mind that the purpose of Grossnickle's study was not grade placement. Thus, any criticism which may be advanced as to his failure to apply his investigation at the various mental age levels, to make his tests more analytic, and so forth, was not validly made. The fact that his data indicated the optimal level for teaching long division using the type of subjects, method, and scoring procedures which he employed was two years below that of the Committee of Seven was merely an indication that placement was not as absolute as some writers would lead the reader to believe. Individual differences in maturational level, in the method used, and in the scoring procedures adopted were merely added variables whose influence must be recognized in the interpretation of placement results.

IV. INVESTIGATIONS EMPLOYING THE CRITERION OF SOCIAL READINESS

Finally, certain researches were found which placed topics according to the needs and interests of the learner. Such investigations were guided by the philosophy that learning went on most effectively when the child enjoyed what he was doing, or when he was so strongly motivated by an alluring purpose as to be unaware of the monotony involved in the enterprise. When a child accepted an enterprise as his own, he worked diligently and understandingly to bring it to a successful conclusion.

The study of Nila B. Smith. The problem undertaken by Smith was an investigation of the use of arithmetic in the out-of-school lives of first-grade children.

The problem. The exact occasion in which arithmetic was actually used in the out-of-school lives of five hundred first-grade (IA) pupils in Detroit as well as the arithmetical operations employed on such occasions was the problem as stated by Smith. Such a problem was a significant one in the field of arithmetic placement, since the location of a particular topic of subject matter was so conditioned by the needs and interests of the individuals studying it.

The method. The method employed by the author in determining the social usage by children of certain arithmetical skills was that of the personal interview. Such a method was excellent as the starting point of placement according to social maturity; however, a further development would have been that of objectively testing the hypotheses resulting from the interview facts. Smith made no attempt to follow up her criteria of social readiness by isolating the various other variables of physical and mental development.

The interview was administered by the regular room teacher and no set form seemed to have been followed in obtaining a statement either of the child's successive experiences or of his uses of arithmetic in these experiences. The writer stated, however, that after four or five interviews the children became accustomed to the idea and in most cases voluntarily gave fairly continuous statements of their activities.

The author explained the limitations of the method by stating that since the results represented such a large number of interviews, the probability was great that the results approximated very closely the true facts concerning the general type of arithmetical experiences which first grade children encountered in their daily out-of-school lives. Such a hypothesis would
have been true as the regards the reliability of the measuring device, but not as regards the validity.

The subjects were described according to school grade and as to the type of home from which they came. This latter description was made only in terms of "high class, average, and poor American homes, and Italian, Polish, and Jewish homes." 166

The results. The results stated the relative frequency of the various arithmetical operations. However, these operations were grouped under the main categories of addition, counting, subtraction, and so on. The various analyses of the skills contained under each of these main divisions indicated the incompleteness of the author's results. If it was to be concluded from the statement of the results that all difficulties of addition ranging from simple column addition to long column addition with numbers of irregular left margin were to be found in the social life of the child, the results were properly stated; if not, a further analysis of the phases of addition should have been included.

The first conclusion of the author was concerned with the arithmetical processes that should have received emphasis in the first grade in order to enable the children to meet the arithmetical needs in their everyday lives. There seemed a tendency for this conclusion to read implications into the data which had been collected. The data showed that from the viewpoint of social maturity the child was ready for some type of addition, subtraction, work in fractions, and so forth; however, the data told nothing of the

166 Ibid., p. 623.
physical development, the intellectual capacity, or the experiential background of the child in connection with his readiness for these topics. The conclusion, therefore, might well have been restated to read that the arithmetical needs of children in their everyday lives should receive emphasis and thus become a means in learning the arithmetical processes.

No reply was received from Smith as to the validity of the evaluation of her research.

The study of Ebba Wahlstrom. The second investigation to be reviewed of those employing the criterion of social readiness was that of Wahlstrom who set out to determine the computational arithmetic in the social experiences of third-grade children.

The problem. A school-pupil survey of the amount of computational arithmetic which occurred in the social experiences of third-grade children was the purpose of the experiment carried out by Wahlstrom. The data of the author contained a survey of what arithmetical topics were used by third-grade children, not what might be used. Though a child might not have been socially ready for a particular item of subject matter under the present environmental conditions, that was not to say that he might not have been potentially ready providing the extrinsic factors were present to condition this social maturation.

The method. The method employed by Wilson in his social utility investigations was taken over by Wahlstrom. This was the school-pupil-survey method, in which the children were asked to report to the teacher problems confronting them in actual situations of everyday life. "The pupils were requested to report orally to the teachers their own actual experience which involved arithmetical computations. These were stated in the form of problems and were recorded verbatim."168 The validity and reliability of such a process was not considered by Wahlstrom to be of any significance.

The subjects were located in eight different states and in three different types of schools. This statement and the fact that they were third-grade pupils was the only description furnished.

The results. The author indicated by her results that the arithmetic of third-grade children demanded even further simplification of current practices found in the present third-grade curriculum. However, she implied in her findings a very sane viewpoint in that even though such a situation was typical of her time, it was not necessarily desirable. Whether the more difficult number processes were not used by these children as a result of lack of need or whether they were not intellectually capable of employing them was not determinable from this study. The author determined what computational arithmetic was used by third-grade children, but she failed to determine why such arithmetic was used; in other words, though the study purported to isolate the variable of social maturation, it did not sufficiently do so.

Wahlstrom likewise failed to reply to the evaluation sent to her.

168 Ibid., p. 124.
The study of Florence Reid. An investigation of the incidental number situations occurring in the life of a first-grade child comprised the study of Reid.

The problem. The problem set by Reid was to determine the needs of first-grade children in social arithmetic situations for the various phases of number—"The purpose of the study was to determine the number of social situations of either concrete or abstract character that would arise over a definite period of time and to make an analysis of the quantitative vocabulary used." 

The method. The method employed was that of a check-list constructed by tabulating the social situations. A representative sampling of the daily school environment was obtained by formulating a schedule of observations including a cross section of all subjects taught in first grade. How these tabulations were made, how validity and reliability were insured was completely omitted from the author's discussion. Likewise, no mention was made of the subjects used in the experiment.

The results. The author's conclusions were valid as regards her data, but could not be stated as universally true. The author employed a specific method; she used situations which seemed to be inherently more replete with certain number skills than others. And yet, she drew such conclusions as

169 Florence Reid, "Incidental Number Situations in the First Grade," Journal of Educational Research, XXX (September, 1936), 36-43.

170 Ibid., p. 37.
"The fact that addition situations occurred many more times than subtraction may tend to prove that addition is a natural part of the child's growth"; 171 and again, "the use of fractions has no great use for children at the first-grade level."

The author continually spoke of the child's environment and posited her conclusions in the terms of such an environment. However, environment as employed by the author was not a generalizable situation, but rather a specific factor. In view of her use of such particularized situations as keeping a calendar, the daily attendance chart, and so forth, it seemed difficult to build her conclusions on the background of "social situations." As far as discernible, Reid set out to determine the present status of the child's social readiness for number. She did so for certain specific situations which were imposed upon the child. This method was a step in advance, however, of the survey technique employed by Smith and Wahlstrom. The author had not only attempted to find out what the subjects knew under regular environmental conditions, but also what they knew in terms of a specific environment. However, in using such a method, the results should have been stated in terms of this specific environment; for example, as shown in the study of Harap and Mapes, 172 a predetermined activity program which potentially contained all of the various desired skills produced a greatly different result from a situation in which no care had been given to insuring the presence of the desired

171 Ibid., p. 42.

skills. Similarly, in Reid's study, if situations loaded with incidental learning possibilities in the various arithmetic skills had been employed, a very different conclusion would probably have resulted. Thus, the author's conclusion that frequency of use was undoubtedly an important phase of curriculum construction was a valid one, but not in terms of the present study. Reid employed only one set of conditions in determining the social readiness of the children for arithmetic; her real solution lay in determining the optimum environment.

In reply to this evaluation, Reid has written:

I think you no doubt have made an adequate evaluation of my research. I simply wanted to show that considerable arithmetic was a part of the child's school 'environment' and that these concepts were not unlike those faced in life outside the school. In addition, that frequency of use of certain concepts has definite inference for the teacher in planning activities. 173

The study of Paul R. Hanna. 174 The importance of the activity approach advocated by Hanna rested not so much in the actual placement of arithmetic as an organized body of subject matter, but rather in the exposition that the children were capable of some of the skills involved in the four fundamental processes which had ordinarily been placed at higher grade levels than those advocated by Hanna.

The problem. The purpose of Hanna's investigation was that of surveying the opportunities for the use of arithmetic in the "activities" curriculum

173 Letter from Florence E. Reid, July 31, 1940.

However, in contrast to the activity program of Harap and Mapes,\textsuperscript{175} "no large activity was selected solely because it offered unusual opportunities for arithmetic."\textsuperscript{176} The author contended that in the development of most activities, arithmetic was necessary many times. Thus, arithmetic became a means to the solution of a meaningful and purposeful problem rather than an end in itself.

The method. The method employed by Hanna was essentially an observation technique in that each teacher "was asked to record on prepared blanks every situation faced by individuals or by her entire class in which there was a need for quantitative thinking and manipulation."\textsuperscript{177} Such a method was not considered by the present study an appropriate one for placement of arithmetic topics. The fact that a particular problem or a certain computation was observed in connection with a definite activity did not afford the information as to the mastery of such a topic. The subjects employed were described as regards their school grade only.

The results. The results with which the present study was concerned were those which placed a particular topic below that of other experimental studies in the field of placement. Thus, it was necessary to make an assumption as to the normality of the subjects which Hanna employed and to set the

\textsuperscript{175} Harap and Mapes, \textit{op. cit.}, pp. 515-26.

\textsuperscript{176} Hanna, \textit{op. cit.}, p. 88.

\textsuperscript{177} Ibid., p. 90.
The investigation was valuable in that it suggested certain hypotheses which necessarily needed objective verification before any definite conclusions could be reached regarding placement.

In reply to the above evaluation, Hanna wrote:

I have looked through your summarization of my research recorded in the Tenth Yearbook of the National Council of Teachers of Mathematics. I find no particular comments or criticisms to make. You have noted, but I believe you might stress even more, that this study of mine was not one in which we were attempting to establish the maturation placement of arithmetic. Rather, we were attempting to discover the range of opportunities in which children might learn the meaning of numbers. You will note that the research did not indicate that children could or should master all of the possible combinations in the four fundamental processes presented in these opportunities.

The study of Henry Harap and Ursula Barrett. Another investigation as regards the social and emotional placement of arithmetic subject matter is that carried on by Harap and Barrett.

The problem. The problem as set up by Harap and Barrett was to determine the influence of an activity program in third-grade arithmetic on the learning of the fundamentals. Such a study was an extremely significant one in the field of placement as viewed from social maturation. This fact was realized by the authors themselves when they stated:

Learning goes on most effectively when the child enjoys what he is doing, or when he is strongly motivated by an alluring purpose as to be unaware of the monotony involved in the enterprise. When a child accepts an enterprise as his own, he works diligently and understandingly to bring

178 Letter from Paul R. Hanna, July 3, 1940.

it to a successful conclusion. In the daily life of the child, it is natural for him to express himself through all of the senses and to derive satisfaction from movement, manipulation, construction, play, inquiry, and companionship. He is particularly annoyed by neglect of these outlets of self-expression when it results from felt restraint.

Perhaps the greatest stimulus to learning is the child's degree of at-home in a situation. The individual seems disposed to learn better when he begins with a setting that is familiar or, at least, understandable. Under these circumstances an experience has meaning and the child is mentally comfortable. At this point, his acceptance of the purpose is probably determined, and his attitude toward the activity is strongly felt. Soon he becomes aware of the goal and of every successive step toward its accomplishment. As the pupil progresses toward his goal, all the incidental facts and processes fit together neatly into a coherent experience. The definition of the problem, however, was not as concise as it might be. The authors left unexplained their concept of the fundamentals and the skills and abilities which they involved. Harap called to the writer's attention the fact that the statement of "thirty-four basic steps" defined the fundamentals. The nature of these steps—the processes, skills, and problems, contained under each—did not, however, appear to enjoy common definition; and thus Harap needed to be more explicit as to his "thirty-four basic steps."

The method. Since the problem was one of social readiness, adjustment of the school program by means of activities to the needs of the pupils was the method appropriate to the investigation. An inventory was made of the pupils' knowledge of the thirty-four basic steps by an initial and final test. The validity of these devices was not discussed. An average public school situation involving a normal class of forty-three children taught by an average teacher was employed. The students were described as to their intelli-

180 Ibid., pp. 188-89.

181 Letter from Henry Harap, June 26, 1940.
gence quotients, which ranged from 67 to 123, the average being 100.6.

The authors did not indicate the reliability of their sampling. Harap replied to this statement: "We made no sampling. It had nothing to do with our purpose. We took Miss Barrett's class exactly as we found it."\textsuperscript{182} The reader will find upon a re-examination of the criteria for a placement investigation that reliability of sampling was definitely considered to "have something to do with the purpose."\textsuperscript{183}

Certain variable errors seemed prevalent in the various techniques employed. The experiment was limited to the arithmetic periods; thus, the contrast between the traditional and progressive approach appeared emphasized. Again, the teacher and pupils were undergoing a new experience, and such factors as excessive zeal and motivation might have brought about the results obtained; the teachers and pupils, therefore, should have been trained in the activity approach, and, only when the factors extraneous to the learning situation had been overcome, should the experiment have begun.

The results. The results were stated in terms of the pupils' per cent of mastery of the steps set up as the goal of the work of the grade. No further conclusions were drawn.

All in all, the authors had an excellent piece of research, but their presentation was rather meager. They neglected to present all of the statistics which the reader needed to interpret the investigation validly. As

\textsuperscript{182} Loc. cit.

\textsuperscript{183} Cf. ante, pp. 18-20.
regards this criticism, Harap wrote:

The word, 'neglect,' is not the right word to use. We deliberately chose not to include the statistics because we were writing for a journal for the classroom teacher. We were interested in making it easy for the teachers to follow.\textsuperscript{184}

Again, Harap fell into the error of failing to determine optimum placement. From his results, it was seen possible to place his fundamentals in the third grade. Whether, however, using a similar method, they could be moved even further down into the grades was not attempted. Harap strenuously objected to any placement terminology being applied to his research:

Your reference to the determination of optimum placement, again, has nothing to do with the purpose of the study. Ours was not a study in grade placement. We started with the skills already assigned to the grade as found in the Cleveland course of study and as outlined in the printed materials distributed among the schools.\textsuperscript{185}

However, with regard to social and emotional maturation—in reference to the development of the needs and interests of the learners—Harap's work certainly appeared to fall into the category of a placement investigation. He showed that children in third-grade were socially and emotionally ready for his "arithmetic fundamentals"; it would have been worthwhile to know whether children of less mature development were also ready when this particular method was employed.

The study of Henry Harap and Charlotte E. Mapes.\textsuperscript{186} Utilization of the social and emotional maturational level of the child was the basis upon

\textsuperscript{184} Letter from Henry Harap, June 26, 1940.

\textsuperscript{185} Loc. cit.

which Harap and Mapes constructed their arithmetic activity program to determine the placement of fractions and denominate numbers. Harap insisted that "the determination of the placement of fractions and denominate numbers was not our problem; these process were taught in every fifth grade in the city of Cleveland and we accepted them."\footnote{Letter from Henry Harap, June 26, 1940.} The divergence of Harap's opinion and the writer's on this point seemed to lie in the definition of placement. In the writer's opinion, if Harap's study showed that his class experienced a felt need and an intrinsic interest in fractions and denominate numbers due to his method of instruction, the study was thereby to be labeled an investigation of social and emotional placement.

The problem. To determine the effect of an activity approach on the "multiplication and division of fractions and denominate numbers which are commonly learned in the second term of Grade V,"\footnote{Harap and Mapes, \textit{op. cit.}, p. 515.} was the problem as set forth by the authors. The factors of physical and mental maturation were held constant, and an attempt was made to determine the effect of a motivating social and emotional environment on the learning results of the subjects.

The method. The level of skill of the subjects was determined before the activity program was begun. Harap held that the determination of the skill level of subjects "is inconsistent with" the possibility of his investigation being a placement study.\footnote{Letter from Henry Harap, June 26, 1940.} However, the writer fully explained in
foregoing chapters the necessity of the determination of the learner's exper-
iential background before the investigation was undertaken. 190

The activities chosen did not grow out of the subjects' individual
felt needs, but rather "were selected deliberately because they were rich in
the application of the fundamental processes in the multiplication and divi-
sion of fractions and in denominate numbers." 191 Nevertheless, the authors
stated, "The activities were genuinely real on the child's level of matur-
ity." 192 Thus, the units were based on socially real situations or activities,
situations being selected, however, which were rich in the use of fractions.
The authors failed to explain the social reality of "all the work of the
pupil being kept in a notebook, which was frequently checked by the teacher." 193
Harap attempted to make this explanation by asking: "What is unreal about
keeping one's computations in a notebook? Can computations be recorded any-
where except on paper in some form?" 194 Harap apparently either missed the
point of the criticism or was relatively ignorant of the education-is-life
dictum of the Progressives. It might be asked if he has kept all of his life-
work in a notebook, "which was frequently checked by the teacher," in which
"no error was left uncorrected," and which "proved to be a valuable source of
information and reference."

190 Cf. ante, pp. 16; 19.
191 Harap and Mapes, loc. cit.
192 Ibid., p. 515.
193 Ibid., p. 518.
194 Letter from Henry Harap, June 26, 1940.
The teacher directing this activity unit possessed special ability in teaching arithmetic. The subjects were selected from a Grade VA class in which multiplication and division of fractions and denominate numbers was commonly learned. The subjects were described according to school grade and intelligence quotient. The environmental conditions under which the experiment was conducted could hardly be characterized by their typicality or resemblance to those in which the results were to be used: a curriculum laboratory with specially skilled teachers, the subjects possessing a median I.Q. of 113, and the furniture being movable. Harap strenuously objected to this criticism of his conditions:

The curriculum laboratory had nothing to do with the school or with the experiment. It was located at the University, ten miles away. The school was one of many typical public schools in Cleveland. The teachers had nothing to do with the University. The University had no laboratory School. The teacher was a regular fifth grade teacher in an average Cleveland public school. Such a statement, however, was not in accord with the original description of the school situation:

The experimental class was an ordinary class in a typical metropolitan school in Cleveland. It should, however, be pointed out that the school is designated as the arithmetic curriculum center, in which new units of work are developed for eventual distribution throughout the city or for inclusion in the next city-wide course of study. The teachers in the school are selected because of their special ability in teaching arithmetic. According to records of the general ability of the pupils based on the National Intelligence Tests, Scale A, Form I, the intelligence quotient of the pupils ranged from 92 to 135, the average being 113. Unlike other experimental, laboratory, or private schools, the class was a typical crowded group of thirty-seven pupils. The classroom had practically no permanent learning equipment or supplies other than movable desks. The materials and equipment used in the several units were improved and

195 Loc. cit.
assembled when they became necessary, the resources of the school and the homes of the children and the neighborhood stores being drawn on. 196

The functional arithmetic course included seven units, each lasting from eight to twelve periods. "With the exception of one, all the units involved some purchasing transactions by the pupils. All except two of the units involved the selection and preparation of food. The quilt-making unit involved some designing and sewing. Four of the units involved selling transactions."197 No attention was given during the unit to the order of the occurrence of the steps in fractions; no external practice or drill was introduced; no more than the usual time allotted to arithmetic was devoted to computation. "The experimental class was an ordinary class in a typical metropolitan school in Cleveland."198 The reader might have judged for himself the ordinariness and typicality of the class from the following statements: the school employed was the arithmetic curriculum center of Cleveland; the teachers were selected due to their special ability in teaching arithmetic; the intelligence quotients of the subjects ranged from 92 to 135 with an average of 113; the room was equipped with movable furniture.

The results. The results indicated that the pupils in an arithmetic activity program based on real situations in school and social life achieved an average mastery equivalent to eighty-four per cent of the processes. How this mastery compared with that of an equivalent group taught in the traditional manner, the reader had no means of knowing. Harap sought to justify

196 Harap and Mapes, op. cit., p. 516.
197 Ibid., p. 517.
198 Ibid., p. 516.
his failure to use the control-group method in the learning of fractions and denominate numbers by pointing out that in a later experiment with decimals, he used a control group and found that his "experimental group mastered an average of 26.2 processes or 97 per cent of the total, as compared with a mastery of 18.2 processes or 67 per cent of the total for the control group." How such a result applied to the present investigation was difficult to deduce. The purpose of the experiment was to determine the effect of the activity approach; it was impossible to judge the effect validly if a standard was not provided against which the effectiveness of this technique could be measured. The Committee of Seven placed the learning of the multiplication and division of fractions at the mental ages of twelve years and thirteen years; the individuals employed in the experiment of Harap and Napes being in the VA grades should have had a median chronological age of 11.5, a given I.Q. of 113, and thus a median mental age of 12.9. The use of an activity program, therefore, evidently did not lower the placement age for the multiplication and division of fractions. Harap, as regards this point, called attention to the fact that "on page 521 you will find that the lower half of the class with an average I.Q. of 104.4 did as well as the upper. This is considerably lower than the placement age that you give as determined by the Washburne study." If the mental age of these individuals possessing I.Q.'s

199 Letter from Henry Harap, June 26, 1940.


201 Letter from Henry Harap, June 26, 1940.
of 104.4 was to be determined by taking the median chronological age, their mental ages were those of eleven-year-olds. However, the median chronological age might have not been their true chronological age. Since Farap had the data at hand as to the correct mental ages of these individuals, the burden of proof seemed to lie with him.

The results, however, suffered from two defects as regards placement: (1) a control group should have been employed for the purpose of comparison; and (2) the optimal level at which multiplication and division of fractions and denominate numbers could be taught by means of the activity approach should have been determined by teaching at various grade levels under the same conditions and under variations of these conditions. Farap disapproved of this second conclusion in that placement was not the authors' aim: "Since (2) was not the author's aim, it could hardly be called a defect."202 Again, it was a question of definition. It may be noted, however, that it was not stated that the authors' results were defectual per se, but merely in the light of the present investigation to the extent that potential placement conclusions were not actualized.

The authors stressed the fact that "the average intelligence quotient for the experimental group was a very minor factor in accounting for the outcome of the experiment."203 Such a conclusion was a logical begging of the question, for the authors have assured this conclusion in their premises.

The mental age of the subjects was placed high enough so that all individuals

202 Loc. cit.

were mentally capable of mastering the processes taught. The only validity
the authors could have found in such a statement would be the result of
performing the experiment at various ability levels. Since the factor of
mental age was held a constant, it would hardly have been expected to exhibit
an influence on the results. The fact as stated by Harap that "the authors
did nothing of the kind; they took the class exactly as they found it" was no more than a frank admission that their conclusion as regards intel-
ligence begged the question. The Committee of Seven set as the mental age
requisite for mastery of fractions and denominate numbers within the year
range of twelve to thirteen. Harap employed his activity program with a
group of individuals, the median of which was 12.9 years of age mentally.
Since mental ability to succeed was thereby assured, the reader would not
anticipate any influence from the variable of intelligence. Again, Harap's
real contribution would have been the determination of the lowest mental age
at which his specific activity method was capable of producing optimum results.

The authors in their third conclusion that "if what the pupils already
knew at the beginning was eliminated, it is found that the pupils learned
79.5 per cent of the basic steps in the half-grade," were treading on very
unsafe psychological ground. On the presupposition that the tests actually
tested everything that the individual had ever learned in regard to the multi-
plication and division of fractions and denominate numbers, how could the
authors have been certain that their teaching was not merely a means for

204 Letter from Henry Harap, June 26, 1940.

205 Harap and Napes, "The Learning of Fundamentals in an Activity
Program, op. cit., p. 525.
recall rather than an actual learning situation.

An argument against the logical arrangement of subject matter was advanced in the fourth conclusion—"The fact that the arithmetical steps appeared in random order did not hinder the learning process."

The authors however, denied their own conclusion in the example stating, "The recipe called for $2\frac{1}{2}$ dozen, but the class wanted to make 7 dozen. To determine how many times to increase the recipe, the pupils found it necessary to divide 7 by $2\frac{1}{2}$. This process was a new step: dividing an integer by a mixed number. The teacher called attention to the divisor and asked what must be done with such numbers before dividing. The mixed number was changed into an improper fraction, and the usual procedure in division of fractions was then applied."

A random order of allowing the various skills to occur was followed, true; but the necessary prerequisites for a given skill were either known beforehand or were taught before the subjects were capable of performing that skill. Harap answered this criticism thus: "All that is claimed is that the steps were not learned in logical order (in order of difficulty). No claim is made that each step could be learned without instruction. Such a claim would be absurd." The writer did not imply an "order of difficulty" when he spoke of logical order, but rather "a connection, as of facts or events, in a rational or pre-determined way." Here was where Harap's "random order of learning" became incompatible.

207 Ibid., p. 518.

208 Letter from Henry Harap, June 26, 1940.
Finally, when the authors stated that the number of times which a step was repeated had nothing to do with the degree to which it was mastered and implied the logical conclusion from such a statement that drill was therefore unimportant,\textsuperscript{209} they had not isolated their variable of repetition in the data. To draw forth such a conclusion, the data should have contained processes of equal initial difficulty, processes similarly motivated, and processes drilled in different amounts.

The investigation of Harap and Mapes has made a fitting conclusion to this chapter on the evaluation of the various arithmetic placement studies.

\textsuperscript{209} Harap and Mapes, "The Learning of Fundamentals in an Activity Program," \textit{op. cit.}, p. 525.
CHAPTER VI

CONCLUSIONS AND RECOMMENDATIONS

The most obvious conclusion from a survey of the placement literature in the field of arithmetic was the scarcity of such type of study. If the fourfold method consisting of (1) survey of present practice, (2) instruction at various levels, (3) measurement of the subjects physically, mentally, socially, and emotionally, and (4) establishment of criteria of placement in accord with the principle of meaningful and valid growth of the entire organism—if such a method was demanded for a valid placement study, it was the rare investigation that fulfilled all four demands.

I. CONCLUSIONS

From the studies reviewed and evaluated certain principles were generalizable:

1. The various subject matter items seemed to have an intrinsic physical, mental, social, and emotional difficulty.

2. Conversely, each child possessed a physical, mental, social, and emotional maturational sequence during which development he was ready at certain times for a particular item of subject matter.

3. Education, thus, was bi-polar—the adjusting of subject matter of a certain difficulty to the child of a particular maturational level.

4. A task of scientific education was, therefore, to determine the difficulty of each item of subject matter in the curriculum and to gain a more accurate knowledge of the child's maturational development.
5. Since the difficulty of the subject matter could be only indirectly inferred from its effects on individuals of a particular physical, mental, social, and emotional age, the construction of valid, reliable, and sufficiently analytic instruments for such measurement was necessary.

6. Progress in such measurement was more discernible in the fields of physical and mental maturation than it was in the evaluation of social and emotional development. A need, therefore, existed for more valid and reliable instruments in all fields of growth.

7. When individuals were categorized as to their various developmental ages, some type of provision for individual variation seemed necessary in order that learning might be achieved most efficiently and economically.

8. The teacher, thus, not only was to be an expert in subject matter and methodology, but also was to possess an ever-increasing background of child nature.

II. RECOMMENDATIONS

Such generalizations were deducible from the data which had been assembled in the field of arithmetic placement. Such conclusions, too, gave rise to certain recommendations pertinent to the field not only of arithmetic but also of the subject matter categories and of learning in general.

The absolute placement of a particular skill in arithmetic could not be determined except in the light of certain variables. The physical development of the learner played a part in placement at the period of readiness and during the initial stage of learning. The mental factor of ability as measured by the intelligence quotient, and of past experience as determined by
arithmetical achievement could be specified in numerical values. These two factors were combinable in the arithmetic achievement ratio and thus became the fundamental variable in placement. The social factors, socio-economic status and need, and the emotional factors, interest and attitude, were directly related to the method employed in the teaching of the skill. Thus, if the traditional method of formal drill was supplemented by the project method of meaningful activity, the placement of the topic might be materially altered. Therefore, when the placement of an item of subject matter was determined it was to be stated in terms of accomplishment quotient and method. However, for each accomplishment quotient, there appeared to exist a particular optimum method which would result in the lowest age location.

This, then was the problem for future investigators: each particular skill in arithmetic was to be analyzed into its lowest subordinate skills. Each of these skills was to be taught to groups along the normal curve of accomplishment. Even here, however, another variable might arise in that individuals of a given mental age might possess different chronological ages which would tend to influence the placement. With these individual groups, for example, one having an arithmetic accomplishment quotient between 90 and 110, various methods were to be employed. One of these methods would result in a lower placement age than would the others. Thus, the particular sub-skill would be placed for a particular arithmetic accomplishment quotient group when a particular method was used. Any deviation from either the sub-skill, the accomplishment quotient level, or the method would result in inadequate learning.

Through this entire process, however, the emphasis was to be upon the
individual learner. The aim of all placement research, as well as of the educative process itself, was the assurance of valid and meaningful growth. The end product was to be an individual physically competent in the various sensory-motor skills, an individual mentally advanced to the fullest extent of his capabilities, an individual socially mature as to his needs and experiences, and finally an individual emotionally well-balanced in his attitudes and in his interests.
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APPENDIX

A. FACSIMILE OF LETTER SENT TO WRITERS OF PLACEMENT INVESTIGATIONS

6434 Eggleston Avenue
Chicago, Illinois
June 13, 1940

Mr. Carleton Washburne
Superintendent of Schools
Winnetka, Illinois

Dear Mr. Washburne:

At the present time I am preparing a research report in the field of elementary school arithmetic. In this investigation I have attempted to summarize and evaluate the most prominent researches dealing either directly or indirectly with maturational placement. Thus it is that I have considered your report and have evaluated it in the light of the criteria which I am employing. This evaluation has been made only in terms of the published information, that is, the research that would be readily available to the curriculum worker.

I have felt it desirable for you to examine and to comment upon the evaluation which I have made. I need not assure you that I will greatly appreciate any trouble you may take in correcting or amplifying the analysis which you will find enclosed in this letter.

Sincerely,

Joseph E. King
Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

Dear Mr. King:

I am very sorry not to have been able to answer your letter of June 13 before now. This letter of yours, addressed to me at Durham, N. C., followed me here to Ann Arbor where I am teaching this summer. There was some delay in the arrival of the letter, and since then I have been pretty busy with my course work.

First of all, let me congratulate you upon having undertaken what appears to have been a very carefully thought out program of evaluation. Of course I do not have your criteria. I wonder if among them you are including attention to the time when studies come out. What I have in mind is this: we still are getting research reports which reveal no awareness on the part of investigators of the trends in thinking about arithmetic. Such studies, which would have to be described as "good" studies in the year in which they appeared, must now be called "poor," in the light of newer developments. Conversely, some early studies, relatively few of them, were surprisingly forward looking, and should be given credit on that score.

With regard to your evaluation of my monograph, let me first say that I am gratified with the many favorable comments. So far as the facts of the study are concerned (apart from the evaluation which is your own business), your analysis is sound at most points. The chief weakness is to overemphasize the work with concrete number pictures. As a matter of fact, this part of the monograph is much smaller than that which deals with children's thought processes in dealing with abstract numbers, and the comparative space allotment accords precisely with the relative importance of the two parts.

I have made certain entries in the margin of your paper, the numerals 1 to 6, which are keyed to comments which follow:

1. Alter to read: " ... of apprehending the numbers from 3 to 12."

2. You have correctly noted the four problems, but it should be stated at this point, I think, that the fourth problem bulks largest in the monograph.

3. This whole section plays up too prominently the work with the
number pictures. The "basic data" for this part of the monograph were obtained, as you state, with the tests you describe, but the basic data for the later work were collected by means of combinations and addition tests, plus interviews.

4. "Patterns" rather than "shapes."

5. Perhaps what I have said in 3 above covers this.

6. All these quotations apply to the work with the number pictures. I think your later statement is good, namely, that I was concerned with the poor results of then current methods of instruction.

I am sorry that my letter is so poor typographically. I have no secretary this summer and am whacking out my correspondence on an unfamiliar machine.

Good luck to you in your venture. By the way, for what purpose are you preparing your analysis?

Very truly yours,

William A. Brownell
Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

Dear Mr. King:

Your letter of June 13 chased me down to Florida and then back to Wisconsin and inasmuch as summer school opened last Monday it has been impossible for me to write you before now.

It seems to me that you have reviewed the study in the learning of multiplication facts reported by me rather well. In connection with your data about the pupils you may have guessed that a good deal of information was available including the factor that you mention in the paragraph which I have marked, but it would not seem particularly important to include them in the report.

If I can be of any further assistance, do not hesitate to call upon me.

Very truly yours,

John Guy Fowlkes

JGF:dw
Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois  

Dear Mr. King,

Please excuse my delay in answering your letter of June 13 addressed to me at the Jersey City State Teachers College. I have been away for some time and am now just getting caught up with my work.

I made notes at certain places on your manuscript to indicate a certain point of view. I suggest that you look in the Elementary School Journal for my article which appeared this past January. I think that this article puts the stamp of approval on Brownell's criticism of the work of Washburne. I think that I have shown beyond a reasonable doubt that the contention of Washburne is absolutely false. Personally, I believe that the Committee of Seven's report is so worthless that it should never have been published.

Last summer at the University of Minn., a student wrote his doctorate on the report of the committee as it applies to division of decimals. He showed that the conclusions of the Committee were absolutely false just as I did with division of whole numbers. The whole basis for a sound platform for grade placement consists in the method of presentation. If meaning is basic or if drill is basic, the conclusions for grade placement are certain to differ.

Very sincerely yours,

F. E. Grossnickle
Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

Dear Mr. King:

Your letter addressed to me at the Lincoln School, Columbia University was finally forwarded to me here at my Stanford address.

I have looked through your summarization of my research recorded in the Tenth Yearbook of the National Council of Teachers of Mathematics. I find no particular comments or criticisms to make. You have noted, but I believe you might stress even more, that this study of mine was not one in which we were attempting to establish the maturation placement of arithmetic. Rather, we were attempting to discover the range of opportunities in which children might learn the meaning of numbers. You will note that the research did not indicate that children could or should master all of the possible combinations in the four fundamental processes presented in these opportunities.

Under my guidance here at Stanford three additional researches have been made in this same field. They are reviewed in a preliminary manner by the three authors in the latest publication of the California Elementary School Principals' Association— their Yearbook entitled *Element Interests*. This yearbook can be obtained by writing to Mr. Ray Dean at the David Lubin School, Sacramento, California.

Cordially yours,

Paul R. Hanna
Professor of Education
George Peabody College for Teachers  
Nashville, Tennessee  
June 26, 1940

Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois  

Dear Mr. King:

I am enclosing my reactions to your critical summary of the studies in arithmetic. I should say that these studies are only remotely related to the problem of maturation. The result of trying to fit the study into your criteria is a distortion of the original presentation.

In the margin I have indicated numerals which I shall use in giving you my reactions:

Reactions to Evaluation of the Research of Henry Harap and Ursula Barrett

Again your attempt to fit this report into your pattern of a grade placement study gives an entirely unfair and distorted picture of the research project. Your criteria do not apply since the original study was not concerned with grade placement. The specific reactions follow:

1. The concept of fundamentals here is the thirty-four basic steps or processes in integers as stated on the second page of your abstract.

2. We made no sampling. It had nothing to do with our purpose. We took Miss Barrett's class exactly as we found it.

3. The word, neglect, is not the right word to use. We deliberately chose not to include the statistics because we were writing for a journal for the classroom teacher. We were interested in making it easy for these teachers to follow.

4. Your reference to the determination of optimum placement, again, has nothing to do with the purpose of the study. Ours was not a study in grade placement. We started with the skills already assigned to the grade as found in the Cleveland course of study and as outlined in the printed materials distributed among the schools.

Reactions to Evaluation of the Research of Henry Harap and Charlotte E. Mapes

1. The determination of the placement of fractions and denominate numbers was not our problem. These processes were taught in every fifth grade in the city of Cleveland and we accepted them.

2. This statement, as you see, is inconsistent with your state-
3. What is unreal about keeping one's computations in a notebook? Can computations be recorded anywhere except on paper in some form?

4. The curriculum laboratory had nothing to do with the school or with the experiment. It was located at the University, ten miles away. The school was one of many typical public schools in Cleveland. The teachers had nothing to do with the University. The University had no Laboratory School. The teacher was a regular fifth grade teacher in an average Cleveland public school.

5. If you will reread page 521, you will find the evidence that the average intelligence of the group did not affect the results.

6. A continuation of the experiment which is reported in the May, 1936, number of the Journal of Educational Research includes a comparison with a control group (page 689), which confirms our earlier results. A reprint of this study is going out to you under separate cover.

7. On page 521 you will find that the lower half of the class with an average I.Q. of 104.4 did as well as the upper class. This is considerably lower than the placement age that you gave as determined by the Washburne study.

8. Since (2) was not the author's aim, it could hardly be called a defect.

9. The authors did nothing of the kind. They took the class exactly as they found it.

10. All that is claimed is that the steps were not learned in logical order (in order of difficulty). No claim is made that each step could be learned without instruction. Such a claim would be absurd.

Very sincerely yours,

Henry Sarap, Associate Director
Division of Surveys and Field Studies

EH:LD
Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois

July 4, 1940

Dear Mr. King:

I have read your summary of the Woodchuck article which I wrote in April, 1928, and quite agree with your conclusion that the results concerning the placement of arithmetic have no value. I therefore think it would be better to omit the whole think as it wasn't an experiment conducted in any way to concern the teaching of arithmetic. I believe the whole thing is too general to have any value for your purposes.

Thank you for sending it to me.

Very truly yours,

Elisabeth Irwin

EI:AJ
Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

My dear Mr. King:

I have just run through your paper, "Evaluation of the Research by B. R. Buckingham and Josephine McLatchy," which you recently sent to me. Will you please tell me what use you intend to make of this material? Is it to be published? If so, where?

My general criticism of your presentation is that I doubt if you realize the difficulties of testing six-year old children. For instance, on page 40 you say, "The materials employed in the test would seem to lack standardization." As far as I am concerned I am glad they did, particularly in the light of what I have learned about the arithmetic knowledge of children in the last fourteen years.

I shall be glad to give you my opinion of your criticisms if you will tell me the use to which you wish to put your evaluations.

Yours very sincerely,

Josephine MacLatchy
Assistant Editor
State Teachers College
Minot, North Dakota
June 28, 1940

Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

Dear Mr. King:

I enjoyed reading your evaluation of the study carried out by F. B. Knight and myself very much. I think you have really done a fine job of it in view of the fact that you had only the published article to guide you. A problem we are confronted with today in the publication of research is the limited amount of space we are allowed when we publish it; the space allowed too often fails to permit the adequate presentation of research.

I might make a few comments and you can consider them as you like. (1) As to the typicality of our subjects, they were definitely above average in ability, but they were third grade children doing their work in their regular school room. (2) We were forced to deviate from the regular practices of the school to an extent that would permit observation of the amount of practice. I doubt that the methods we used had learning values that would rate as superior to regular classroom practices. Our procedure is described fully in my unpublished thesis. (3) The working time for each pupil was about three minutes each day (each school day) between January 4 and May 18. On days that the children were tested the working time was somewhat longer. (4) As explained in my unpublished article, the assistants (or observers) who worked with me were trained by me to follow a definite plan. The procedure followed was simple and did not vary from individual to individual. The assistants were all University students. (5) As explained in my unpublished article, "A combination was considered unlearned until the child responded to it with a correct rapid response when the combination was first presented to him in a given work period. In the case of the drills from two to four correct rapid reactions were required, obtained at intervals between which other combinations were drilled." Mastery is probably never absolute; it is a matter of degree.

The numbers in the above paragraph correspond to the numbers written along the margin of your paper.

Sincerely yours,

Grant M. Norem
June 28, 1940

Mr. Joseph E. King
6434 Eggleston Avenue
Chicago, Illinois

My dear Mr. King:

Your letter has been sent to me here in Houghton where I am away from my thesis and from the article to which you have referred. I am interested in your attempt to evaluate my study with some set of standards which you are using as your criteria. You have said, in your statement, that the study was not conducted with a representative group of children, that the tests are neither valid nor reliable, and that the conclusions are fallacious. Is there anything left of the study, according to your standards? I cannot see that there is and if there isn't, why go to the trouble to include it in your study?

Actually, the study proved very interesting to me. I wrote it and worked it out with Henry C. Morrison and Harry C. Gillet and Nelson Henry. They were as interested in the study as I was. Mr. Morrison read the thesis thoroughly, soon after I had completed it and he said it had turned up as much definite evidence as he had found in any study he had read. Perhaps you did not know Mr. Morrison but he was as critical a reader as one could expect to find. The study has had favorable criticisms from many people who have read it carefully. I cannot feel, in the light of my own experience with it, and in the light of judgments that have been passed upon it at various times, that it is quite as bad as you suggest. I believe it has proved its worth and I do not believe your evaluation of it at this time will have much weight, especially since it is based upon the brief article rather than upon the study itself.

The study was conducted with lively, interesting children. The materials were live and interesting. The examiner was genuinely interested and absolutely impartial. The tests were prepared to give objective evidence and I fully believe they did. I believe they tested the thing they were supposed to test. I believe the results proved that they did. I think a reading of the study would show you they did. I believe the tests are reliable and I believe I proved they are reliable and also valid. I am unable, too, to see that the conclusions are fallacious.

I have been attempting in my teaching to find out what first grade children can learn about fractions as a result of teaching. I have been turning up some interesting evidence in the teaching too.
I judge, from the last paragraph of your evaluation that you are one of the people who would hold off the teaching of arithmetic until late. Actually the children love it. I do not advocate formal teaching of fractions but my children really enjoy informal work with them.

Yours very truly,

Ada R. Polkinghorne
Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois  

Dear Mr. King:

I wish to apologize for my apparent neglect of your letter of June 13. I am teaching in summer school and it has been a long season.

I think you no doubt have made an adequate evaluation of my research. I simply wanted to show that considerable arithmetic was a part of the child's school "environment" and that these concepts were not unlike those faced in life outside the school. In addition, that frequency of use of certain concepts has definite inference for the teacher in planning activities.

Will your report be published or will it be available? I shall appreciate an opportunity to read the complete report.

Sincerely,

Florence E. Reid
Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois

June 27, 1940

Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois

Dear Mr. King:

I am indeed interested in your critique of the May, 1936 article in the *Journal of Educational Research*.

You are aware, no doubt, that the stimulus for this study came from Dr. R. F. Wheeler who represents the Gestalt view in psychology. The data for the article were obtained in the summer of 1933.

Up to this time (1933) several outstanding studies in the literature indicated that a considerable proportion of school children in grades one and two were not mastering arithmetic. Most of the remedial devices offered only some change in the drill procedure.

Dr. R. F. Wheeler and Dr. E. T. Perkins suggested, instead, that a study be made of the earliest arithmetic concepts of children, and of the natural development of these concepts. Our results led to conclusions on the psychological development of number which differed from the then prevalent theories. (The Thorndikeian view may be considered typical of this time.) The final conclusions based on experiments 1933-1935 appear in my Ph.D. thesis (1936) at the University of Kansas. As a part of the study, I may add, I taught an arithmetic class daily for one year in the second grade of the Lawrence, Kansas Public Schools. Your results which were emphasized were:

1. The child's ordinal and cardinal number concepts differentiate together.  
2. Rote counting is not an adequate indication of the development of number concepts.  
3. Average first grade as well as average second grade pupils make little if any use of abstract numbers.  
4. Drill work in addition and subtraction combinations should be postponed until at least third grade for the benefit of the majority of pupils.  
The thesis itself contains experimental support for the above mentioned conclusions. No abstract of the thesis has been published in an educational journal. I wished, however, to mention these data in reply to your criticisms which strike at fundamental issues of the problem.

Very truly yours,

Red M. Russel
Mr. Joseph E. King  
6434 Eggleston Avenue  
Chicago, Illinois  

Dear Mr. King:

In general O.K.  
See notes on your copy  

G. M. Wilson  

Boston University  
Boston, Massachusetts  
June 27, 1940
Winnetka Public Schools
Winnetka, Illinois
July 12, 1940

Mr. Joseph E. King
6424 Eggleston Avenue
Chicago, Illinois

Dear Mr. King:

Thank you for letting me see the enclosed research report. I have read it with interest. I am much too busy right now to be able to comment on details. As a whole I think it's a very good report. There naturally are some points at which I would take issue with you if time permitted.

You seem to have failed to see my reply to Brownell's criticism, since you quote Brownell frequently and never quote my answer to him. I am accordingly sending you a reprint of this answer.

With best wishes, I am

Sincerely yours,

Carleton Washburne
Mr. Joseph E. King  
6424 Eggleston Avenue  
Chicago, Illinois  

My dear Mr. King:

I must apologize for not answering sooner your letter of June 13. With the rush of registration and summer school it has been virtually impossible to give correspondence the proper attention. It may now be too late for my reactions to the article dealing with the study of arithmetical backgrounds of young children. As one responsible for the investigation, I presume that I am as fully aware of the strength of the investigation and its shortcomings as any individual.

The investigation was not undertaken with the idea of throwing light upon maturity or the gradation of subject-matter. It was undertaken merely to throw some light upon the amount of information which children have at the time formal instruction in the subject is introduced. Considerable difficulty was experienced in defining properly the term "formal instruction." It was pointed out, I think, that one of the difficulties was that superintendents reported formal instruction was made as informal as possible and that informal instruction was made as systematic as possible. Naturally the writer was conscious of this in setting up the investigation. Nevertheless, he has visited numerous schools and finds that considerable variation prevails in the practice of teaching arithmetic. In some schools a definite schedule is set aside for teaching arithmetic, even as low as Grade One. In other schools, no definite schedule for teaching arithmetic is set up until in Grade Two or even as late as in Grade Four. In some of the schools the statement was made that they teach some arithmetic but that it is incidental to the child's purpose and to the needs of other subjects; however, they state that at a given time in the life of the child they introduce a definite systematic program of arithmetic. It was out of this maze of conflicting statements that some definition of formal instruction had to be set up. After many conferences with teachers and supervisors, the definition was formulated. The inadequacy of the definition was perfectly apparent at the time of setting up the investigation, and if I remember correctly, was frankly admitted in the description of the article. While the investigation was carried on rather extensively in various cities throughout the United States, a very intensive system of interviewing was carried on in the Ann Arbor Public Schools. It was in this city that the investigation was instigated. Here almost all of the interviewing was done by three substitute teachers who were especially trained for administering tests. The interview blank, itself, was formulated in a conference with selected teachers and principals of the Ann Arbor schools. There was no definite criteria for selecting the particular topics that were included,
other than a perusal of published materials dealing with the teaching of primary arithmetic and with courses of study indicating the types of skills which should result from formal instruction in arithmetic. As one who has taught courses in the psychology of arithmetic for a number of years, I included in the inventory blank some questions in which I had a personal interest. There was no effort on the part of those responsible for the investigation to take a complete inventory of the child's knowledge of arithmetic and its relations. No doubt this would have been desirable, however, this inventory test contained over two hundred items and required a considerable amount of time for its administration. A complete inventory, such as you have indicated in some of your sections, would have required a much longer test than seemed feasible. I personally would have been anxious to have obtained information on a much greater number of questions under each division of the test. I have no apology to make for the omission of a number of topics. It was not an oversight as you might suggest, but was a limitation subject to the amount of time available for administering the test. As it was the public schools objected to the time-expenditure involved in giving the test.

I note you criticize the interview technique in that it seemed to you entirely too stringent. I am inclined to think that for some of the exercises your criticism is justified; for other parts of the exercises, allowing only one opportunity to respond seemed necessary. Since the main purpose of the investigation was to find the amount of arithmetic which the child actually knew, we were interested in seeing to what extent he could automatically count from 1 to 100; in knowing the extent to which he could recognize groups of objects; knowing the extent to which he could tell time as indicated on faces of clocks, etc. The material was not presented to the child in formal problem situations in which he had to figure out the answers. The situations presented, as a general rule, were simple and he either knew the answers or did not. Each interviewer made definite notes of the child's responses. I am frank to say to you that probably the most important part of the investigation, the results of which have not been published, is in the nature of the responses which the child made. Each interviewer obtained an extensive history of most interesting facts concerning the nature of the processes involved by the child in arriving at his answer. For instance, when we exposed domino patterns and asked him to point to the domino which had just five spots on it, definite information was recorded as to whether he recognized the number of spots at once or whether he had to count each one. It was most interesting to note that when we called for patterns of five, the child might count the number of spots on the domino which had the five-spot pattern and then recognize the same pattern the next time that it appeared without counting each spot. It might be interesting to state that on the little additions test, many children arrived at the correct answer by putting down marks and then counting the marks. Others arrived at the answers by a process of double counting. All sorts of interesting processes were used in arriving at the answers. The point which you are criticizing simply gives you finally the number of responses correctly made. It hints at the fact that some methods of arriving at responses are interesting and suggests that because the child got the right answer that is no sign he didn't need instruction in arithmetic.
On page three of your report I note you criticize the conclusion "the exercises which involve counting the twenty circles and pointing to them in order proved to be much easier than those involving rote counting to 100 by 1's." These two exercises were set up as two independent exercises. There was no effort on my part to make an intensive study of the processes involved in counting. I am aware of the difficulties between rote and rational counting. I am also aware of the differentiations which you make in connection with this process and I agree with your statements concerning them; however, I feel that you ought not be too hard on my statement as I did not start out to make an intensive investigation of the relationship between rational and rote counting. I merely put in these two exercises and found to my surprise that a large portion of the children could count and point to 20 and count by rote to 100, and that these two tests are often set up in courses of study as the objectives to be attained in primary arithmetic. All that I was trying to do was to find out how the child reacted to these situations. To criticize me as disregarding two fundamental principles is really setting up a "man of straw" and then attempting to knock him down. No one is more aware of the fact that I grouped together children from the kindergarten through Grade Three, although in the tabulations these were isolated. I did not take into consideration any other factor than the fact that they were in the public schools and were going to be introduced to the formal study of arithmetic during the next year. That was the only point I was interested in. I note your charge of my being inscientific. I think you are just a little bit unfair, as the types of controls which you have suggested were not necessary in the type of investigation with which I was dealing.

In your final paragraph I note you object to my statement that children have some knowledge and understanding of such fractions as halves, thirds, and fourths. Possibly I should have stopped there. However, as one familiar with practices in the teaching of arithmetic, I feel compelled to point out the fact that there is no particular reason why some experience with fractions should not be given to children in the primary grades. It is my conviction that simple number experiences involving fractions is as much a part of the life of the primary child as are other processes. While it is admitted that the test situations set up were thoroughly inadequate to warrant a sweeping conclusion, I am willing to venture that a much more extensive testing program would justify the contention made. Of course, I am willing to admit that primary children are not capable of handling all of the complicated processes involved in fractions and in a complex knowledge of fractions as a concept, yet they do have the ability to understand the meaning of halves, thirds, and fourths, the number of halves that make a whole, that two-fourths equal one-half, etc. This is certainly a part of the fractions concept.

I hope you will pardon this long letter. It should indicate to you that the investigation as set up was not based on superfluous thinking and that its interpretation was not as unscientific as your article seems to suggest. In general, I can say that this was one of the most worthwhile
investigations which I have undertaken because of the light which it throws upon the knowledge of arithmetic possessed by young children before formal instruction in the subject. I am sorry that I have not had time to write up for publication the investigation.

Yours very truly,

Clifford Woody
Professor of Education
The thesis, "A Summary and an Evaluation of Selected Experimental Placement Investigations in Elementary School Arithmetic", written by Joseph Ernest King, has been accepted by the Graduate School with reference to form, and by the readers whose names appear below, with reference to content. It is, therefore, accepted in partial fulfillment of the requirements for the degree of Master of Arts.

Dr. Scanlan
October 3, 1940

Mr. Laughlin
October 5, 1940