

Recovering the Big Five from archival data:
The history of the Kelly/Connolly Longitudinal Study

Affiliation

Author Note

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Abstract

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Preface

The purpose of this paper is threefold. 1) We wanted to demonstrate that many of content/Preface.txt

the popular personality measures from the beginning of the last century do map into the Big Five (Goldberg, 1990), 2) We wanted to provide a detailed profile of the Kelly/Connolly Marriage Study on Personality and Aging (KCLS; Kelly & Conley, 1987) cohort, and 3) We wanted to tell the story of how the KCLS came to be. We hope that this fusion of history and measurement will encourage other researchers to look back thru the history of our field and rediscover other studies that still have more to tell us about our field. A handful of other researchers have done just that including Deary and colleague's (2011) work with the Scottish Mental Surveys of 1932 and 1947 (The Scottish Council for Research in Education, 1933, 1949), as well as Friedman and Martin's (2011) work with the Genetic Studies of Genius (Terman, 1925), rebranded as the Terman Study of the Gifted. However, there are still many more longitudinal studies sitting in universities, libraries, and basements, waiting to be recovered.

Abridged Historical Context with Study Timeline

Abridged Historic Context

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Measurement/Validity Study

Measures

Time 1

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Allport-Vernon Scale of Values (Vernon & Allport, 1931) measured how participants prioritized theoretic, economic, aesthetic, social, political, and religious values. Participants had to select between two “controversial statements” for 45 items. The test-retest reliability after 1 year was above .7 for all the individual scales except

the social scale, which was .57 (Whitely, 1938). However, after 25 years, the test-retest reliability was considerably lower (Kelly, 1955): all the individual scales except the social scale (.33) and the religious scale (.60), were approximately .50.

Bernreuter Personality Inventory (Bernreuter, 1933, 1935) was a pencil and paper test designed to measure participants' personality on the following traits: neurotic tendency, self-sufficiency, introversion, and dominance. Participants responded to each of the 125 statements by selecting "Yes", "No", or "?" However, these factors were notoriously unreliable (See Lorge, Bernholz, & Sells, 1935 for a summary of the debate); the scoring was redone, reducing the traits to Self-Confidence (F1C) and Sociability (F2S) (Flanagan, 1935). The reported reliabilities of the F1C and F2S are both above .80. The test-retest reliability after 20 years was .61 for F1C and .45 for F2S. CITATION?

Bell Adjustment Inventory (Bell, 1938) was another pencil paper personality test designed to measure student-adjustment in the following domains: School Adjustment, Health Adjustment, Vocational and Occupational Adjustment, Motor and Mechanical Adjustment, Social Adjustment, Home Adjustment, Emotional Adjustment, and Religious Adjustment. It was designed for young adults in high school and early college (Bell, 1939). The test-retest reliability after 13 years was .48 (Cantoni, 1955).

Kelly's Personality Rating Scale (Kelly, 1940), which had participants rate themselves on 36 personality characteristics relative to their peer group on a graphical scale. ADD INFORMAITON HERE.

Otis Self-Administering Test of Mental Ability: Form A (Otis, 1922), which measured participants' general intelligence. The test-retest reliability after one year was .71 (Traxler, 1934).

Strong's Vocational Interest Inventory, Men's blank (Strong, 1943), which assessed whether a participant's interest in a specific vocation matched those who were successful in specific vocations. The vocations were divided into 11 groups. Group 1 consisted of artists, psychologists, architects, physicians, and dentists. Group 2 consisted of

mathematicians, engineers, and chemists. Group 3 consisted of production managers. Group 4 consisted of aviators, farmers, carpenters, printers, mathematics-science teachers, policeman, and forest servicemen. Group 5 consisted of YMCA physical directors, personnel managers, YMCA secretaries, social science teachers, city school superintendents, and ministers. Group 6 consisted of musicians. Group 7 consisted of certified public accountants. Group 8 consisted of Accountants, office men, purchasing agents, and bankers. Group 9 consisted of sales managers, realtors, and life insurance agents. Group 10 consisted of advertising men, lawyers, and author-journalists. Group 11 consisted of presidents. The test-retest correlation after twenty years varied by group, but in general it hovered between .5 and .6 (Kelly, 1955). 7) Remmer's Generalized Attitude Scales on institutions and activities (Remmers, 1934), which had participants indicate their opinion on marriage, divorce, and church. It also had participants rate how much they enjoyed particular gender-specific tasks related to home maintenance, such as rearing children, housekeeping, entertaining, and gardening. These scales had the lowest test-retest correlations, ranging from .08 for marriage and .35 for gardening (Kelly, 1955). Kelly (1955) estimated that the long-term consistency of attitudes was 88) Personal Data Sheet (Kelly, 1978), which gathered demographic information such as race, age, creed, occupation, etc., 9) Physiological Data Sheet (Kelly, 1978), which gathered physiological information such as height, weight, and medical history, and 10) Views on Ideal Marriage (Kelly, 1978), which asked participants to rate how essential 35 items were to a happy marriage (1= Very essential, 5 = Decidedly not desirable). The long-term reliability of these have not been assessed.

The annual follow-up, collected until 1941, contained a questionnaire that was to be completed independently. No analysis has been done on these follow-ups (J. J. Connolly, personal communication, April 5th, 2013).

Major Appendices

- Historic Context, Full Timeline
- Annotated Bibliography of Publications Using the KCLS
- Marriage Manuscript

- Lowell Autobiography

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Appendix A
Marriage Manuscript

Marriage Study Manuscript

by E. Lowell Kelly

August, 1977

Memorandum.

Feb. 1961.

To: All Persons who served as Subjects in my Marriage Study.
From: E. Lawrence Kelly.

1. When I At the time, I last contacted you in 1955, I expressed the hope that I would be able to advise you shortly of the availability of a book reporting the outcome of our long time study of marriage. Alas, the my plans for such a book have not yet come to fruition. I am, therefore, at this time sending you enclosed a reprint of the one major paper which I have written thus far in the belief that you will find be interested in the findings there reported.

There are two reasons for

2. My failure to complete the book length report to my

more or less as expected.
A block of time to devote to the project

- (a) ~~Block of time to devote to the project~~
Each year, I seem to have been asked to undertake more and more other professional commitments. In 1957 for example, I ~~assumed~~ was second Chairman of our very large and well known Department of Psychology here at Michigan

(b) More relevant, has been the fact that, even with the help of a ~~large~~^{both} "Electronic Brain", the analysis of all of the data is going to be ~~quite~~ complex and time consuming. I could have dashed off a book on the basis of ~~a~~ ~~single~~ an粗略 and preliminary analyses of the data, but this I refused to do. Instead, with the help of a number of graduate students, I have been pursuing detailed analyses of the data over 6 years. At the moment for

It is the hope that the eventual book will represent a worthwhile contribution to our understanding of the ~~and~~ marriage. At the moment, for example, we are P.R.D. students interested in the relationship between

between the personality changes, discussed in the enclosed report and the reported happiness in marriage for each of you. Another is ~~now~~ trying to make some sense out of the question of how you paid off in marriage and its relation to outcome. The financing for this simple. ~~another~~ Still another one of my Ph.D. students is primarily concerned with identifying the varieties of role expectations with which couples enter marriage and comparing these with the actual role ~~expectations~~, ~~desires to accomplish~~ of husbands and wives.

3. I still of course plan to report all of this and itself other analyses in a ~~final~~ book but since even at this late date, I can't be sure when the book will be ready, I am sending you a reprint of ^{the results of} ~~only major~~ the first paper which has thus far reported the findings. I hope you will find it of interest - and especially, I want to call your attention to the last line of the footnote on page 662.

August¹² 1977

PREFACE - I

This book presents a long delayed account of a major research project, first conceptualized in 1929 and actually initiated in 1934. It reports the results of an extensive series of investigations concerned with variables important in the selection of marriage mates and related to the outcome of the resulting marriages. Although novel, even daring, at the time of its inception, this study is no longer either. This is because the intervening years have seen the initiation, execution and reporting of similar studies by several other investigators. Not only have their studies been reported in book form, but also rewritten in popular form for the layman. The result is that many of the findings reported in this volume will not be perceived as dramatic. Fortunately, however, for the most part they confirm and thus reinforce the conclusions reached by other investigators.

The author's interest in the bases of marital compatibility go back to 1929. As a graduate student in psychology at Stanford University he also served as a research assistant to Professor L. M. Terman and Catherine Cox Miles in their empirical studies of masculinity and femininity. Participation in this unusual research program resulted in his reading the pioneer studies in the "Psychology of Sex" by Havelock Ellis and in first learning about psychopathologies of

sexual behavior by reading authors such as Krafft-Ebing. It was, however, the publication of two important books in 1929, both of which were reviewed at Professor Terman's weekly seminar, that focused his attention on the problem of marital compatibility. These books were: "Factors in the Sex Life of 2200 Women", by Katherine B. Davis and "A Research in Marriage", by G. V. Hamilton. The former was a questionnaire study of an approximately equal number of married and unmarried women, the latter based on an extended controlled interview study of 100 married men and an equal number of married women, 45 of whom were married to each other. Both of these books, based on empirical research, provided for the first time some indication of the sexual experiences and practices of men and women both in and out of marriage. Like others I was fascinated by the findings as reported, but for reasons still not clear to me, I was not fully ready to accept the implications of both studies: that the primary basis for marital incompatibility was sexual incompatibility. In all fairness to Dr. Hamilton, however, I should quote a footnote which appears on page 3 of his book:

Note: although this book is a report of the results of an investigation which was specifically concerned with problems of sex in the marital relation, it is assumed in the text, as it was throughout my research, that the sexual aspects of marriage cannot be profitably

studied out of context with the more important nonsexual aspects of spousal life.

Perhaps it was this footnote, perhaps it was the fact that I was being trained as a psychologist rather than as a physician, but probably also because of other unidentified reasons, I found myself more and more unwilling to accept the general conclusion that marital compatibility was primarily a function of sexual incompatibility. Although entirely willing to accept the facts reported by both Davis and Hamilton, namely that marital incompatibility is almost always associated with problems of sexual maladjustment, I came increasingly to the view that sexual maladjustment was the symptom rather than the cause of marital incompatibility. In fact, during the next few months I formulated what first was a hunch, and then an hypothesis that marital compatibility was primarily a function of psychological or temperamental compatibility. I argued that given psychological compatibility, couples even though unsophisticated with respect to the matters of sex, would work out a satisfactory sexual adjustment, and, alternatively, couples lacking such psychological compatibility would, in spite of sophistication in matters of sex, find their marriage increasingly ^{un}satisfactory, and that the lack of such basic marital compatibility would display itself in problems in the sexual sphere. K

I became so strongly convinced of the essential correctness of this hypothesis that I seriously proposed it to Professor Terman as the basis of my doctoral dissertation. He agreed that my hunch was an entirely reasonable one, but quite properly pointed out that any adequate test of it would require resources simply not available to a graduate student, and in any event would necessitate a long term research project hardly appropriate for a doctoral dissertation. He urged me to find a more manageable problem for my thesis; at the same time he promised that he would do everything that he could to assist me in getting the necessary research support for a longitudinal study of marriage -- after I obtained my degree.

On receiving my doctorate in 1930, I accepted a teaching position at the University of Hawaii. This setting invited the study of inter-ethnic and even inter-racial marriages, but was hardly conducive to the type of investigation I had envisaged. During my two years at Hawaii, Drs. Terman and Miles completed the manuscript for the book reporting their studies of masculinity and femininity, and since I had assumed the responsibility for preparing one chapter of that book, Professor Terman and I engaged in a regular exchange of letters. Almost all of his letters included a plea that I should not become a "lotus eater" but make every effort to obtain a position on the mainland so that I might undertake

the research on marriage. Early in 1932 Professor Terman urged me to apply for a post-doctoral SSRC fellowship, promising that he would strongly support my application. A one year post-doctoral fellowship was awarded to me later that spring, and I decided to spend it in Germany in post-doctoral study at the University of Berlin and working in the marriage counseling clinics of Berlin. This plan was followed for the first half of the year, but early in 1933, Hitler came into power and one of his first acts was to order the closing of the marriage clinics. About the same time, I learned that because the depression had finally reached Hawaii, with a very sizable cut in the University budget, there was no longer an academic position at the University to which I could return. Thus I found myself without a job, in the midst of a major economic depression, when university positions were almost non-existent. Further, because of my being in Europe, I was unable to make any personal applications for a position. Under the circumstances I appealed to Professor Terman and my other mentors at Stanford for help in finding a new position. With their assistance, I obtained one of the few assistant professorships opening up in the United States in the fall of 1933, at Connecticut State College (now the University of Connecticut). ^{want it (?)} The remainder of my fellowship year was spent in Austria, familiarizing myself with psychological schools of thought there, and in continued planning for my longitudinal research on marriage.

Shortly after assuming my duties at Connecticut, in the fall of 1933, I received a long and somewhat astonishing letter from Professor Terman. In brief he told me that during the previous spring he had been so worried lest I not find an academic position in the midst of the depression, that he had applied to the Committee on Research and Problems of Sex, of the National Research Council, for a grant which would permit him to invite me to come to Stanford as a research associate and carry out my plans for a longitudinal study of psychological factors in marital compatibility! He indicated that although this request for a research grant had been approved, he had not informed me of it because he was certain that if I had known of its availability, I would not have accepted an academic position even if offered. He further stated his conviction that at my stage of professional development an academic position was far preferable to a time-limited research position, and for this reason he had not told me about the research grant.

In retrospect, I am certain that Professor Terman's judgement in the matter was sound. At the time, however, I was quite upset to think that the opportunity for beginning the marriage research had been delayed. Professor Terman assured me that he had no intent of carrying out the study which I had been planning, but noted that he was in the embarrassing position of having asked for and received

a grant to study psychological factors in marital happiness. At this point, he asked for my suggestions concerning ways in which we could either collaborate in the research or divide up the problem in a way that his project would not infringe on the one which I had envisaged. Because of the considerable geographic separation between us (long before the days of jets), a collaborative study did not seem practical. I, therefore, suggested that he utilize his already approved research grant to undertake a cross-sectional study, whereas I would continue to plan for a longitudinal study. We, therefore, agreed that the Terman study would collect data on essentially the same domain of variables as I had considered relevant, but that he would study these variables in a group of divorced as compared with a group of married couples and with varying degrees ^{levels} of marital happiness reported by his married group. In turn, Professor Terman promised to assist me in every way possible in the further planning of my research in securing the necessary financial support for carrying it out. In brief, then, it was agreed that our studies would be in many ways comparable, but that the unique feature of my own study would be that I would start by collecting a wide array of data on engaged couples and then follow them through several years of marriage, whereas he would obtain similar data on couples who had already been married for several years.

By obtaining personality data before marriage, my results would not be subject to the inevitable criticism that the data had been

influenced by the relative degrees of success or failure in the marriages studied. Thus it was that Professor Terman and Paul Buttenwieser conducted an exploratory study in 1933 and 1934 involving a comparison of 341 married couples and 109 divorced couples and published the first account of that study in 1935. This exploratory study served as a base for planning their much more extensive, but still cross-sectional study of 792 couples, the results of which were published in the book, Psychological Factors in Marital Happiness (1939).

Unbeknown to the present author at the time, the sociologist, Ernest W. Burgess of the University of Chicago had as early as 1931 begun planning for a study of the feasibility of predicting success in marriage on the basis of information available before marriage. That project had been an outgrowth of an earlier prediction study, namely the attempt to predict, before a man was released from prison, whether or not he would succeed or fail on parole. The results of the Burgess study of marriage were published in 1939 in the book, Predicting Success or Failure in Marriage, by Burgess and Cottrell.

Fortunately Professors Terman and Burgess interacted considerably in the planning of their investigations, and I, in turn, was able to profit greatly from their experiences in developing questionnaires and other instruments used in data collection.

The detailed planning for the study reported in this volume began in 1933, culminating in a research proposal to the National Research Council in the spring of 1934. In part, because of the nature of the program of research proposed, and in part because of the youthfulness of the principal investigator, the Council did not fund the project in full, but allocated an initial grant of \$1500.00 for a preparatory work which included the devising of forms, selecting a test battery, and trying out different methods of securing subjects. On the basis of the work completed during 1934, the National Research Council in the spring of 1935 granted an additional sum of \$4,000 for beginning the actual field work. During 1935-36 approximately 150 couples were studied in the field, and preliminary analyses were made of the resulting data. A preliminary report on assortative mating for the first 100 couples was presented at a professional meeting in 1937. Because the cost of the field work had been considerably underestimated, the committee in the spring of 1936-37 made additional grants for the continuation of field studies.

Although our original research plan called for obtaining data on 500 engaged couples, the cost in both time and money of conducting the necessary field studies considerably exceeded the anticipated costs. The result was that we had to reduce our goal to 300 couples for whom complete data had been obtained by 1939. By this time, as

has been noted above, both Terman and Burgess and Cottrell had published book length reports on their cross-sectional studies.

Our own plans for data analysis, further data collection and eventual publication were relatively simple and straight-forward:

- (1) To analyze the already collected data and publish a volume reporting these analyses under some title such as "Assortative Mating".
- (2) To obtain reports of marriages or broken engagements from each of the cooperating couples on the anniversary of each marriage, and to secure reports on the progress of each marriage, independently from the husbands and wives. We planned to obtain these annual reports on marriage for a period of seven years, by which time we anticipated being able to evaluate, at least crudely, the relative success or failure of each marriage, and thus permit a new series of data analyses and the publication of a second volume reporting on: (a) the characteristics of individual men and women associated with success in the roles of husband and wife, and (b) the degree to which relative compatibility in marriage was a function of the particular combination of characteristics of the husbands and wives.

- (3) To reassess, after seven years of marriage, the personality characteristics of the husbands and wives which had been originally assessed at the time of engagement with the view of discovering the degree to which men and women became more similar during the course of

their marriage and the degree to which increasing similarities (or the lack thereof) was associated with marital compatibility.

The results of this third phase of the study were to be reported in a third volume entitled, "Personality Change in Marriage."

At the time of planning this original time schedule, it was recognized that seven years was not an adequate period to permit a final determination of the outcome of the marriages, but it was felt to be sufficiently long to permit at least a rough determination of the probable eventual success of each marriage and still permit reporting on the findings before the lapse of too many years. Similarly, it was recognized that some of the couples might not marry for several years after the original data collection, or indeed not at all. It was, therefore, anticipated that the three phases of the study could not be completed until at least ten years after the completion of the original data collection in 1939.

The fact that the full report of the study is only now being prepared indicates the hazards of planning such longitudinal research extending over many years. Shortly after completion of the original data collection on the engaged couples, I moved from the University of Connecticut to Purdue University with the expectation that the analysis of the data with respect to assortative mating would be shortly completed and the first volume published within a couple of years. Simultaneously, I continued the collection of the annual reports

on marriage, and even began planning for the eventual retesting of the subjects after seven years of marriage. Most of the original data on the engaged couples were analyzed and a paper summarizing the findings was presented at a professional meeting in 1940.

Already, however, much of my time and attention was being deflected by the preparations for World War II. Because I was a private pilot, I became involved in a series of researches on the selection and training of aircraft pilots. As a result of my own increasing commitment to research in the interests of national defense and also because of my feeling that the stress^{is} of war time might affect the marital adjustment of my subjects, I wrote to all of the couples advising them of my decision to don a uniform for the duration of the war, and also advising them of my intention to complete the planned follow-up studies after the war.

Do
we
have
this
letter?

In spite of these intentions, my plans were not carried out. On returning to civilian life in 1946, I accepted a new academic position at the University of Michigan and within a year found myself prevailed upon to undertake a major research project on the selection of graduate students for the rapidly developing professional specialty of clinical psychology. This, together with associated academic and professional activities, resulted in my being unable to give serious consideration to the marriage project until 1952. That year was spent in reordering all previously collected data and planning a

full scale follow-up to be carried out in 1953-54. Fortunately, a substantial grant from the Foundations Fund for Research in Psychiatry permitted carrying out a full scale follow-up. Fortunately, too, the cooperativeness of my subjects permitted us to obtain follow-up data on a very high proportion of the subjects originally studied between 1934 and 1939. Thus, instead of following my couples through seven years of marriage, I found myself in the unique position of having follow-up data on marriages of seventeen to eighteen years duration, after most of the couples had completed their families. These facts, together with the availability of new electronic computers, made possible and feasible entirely different types of statistical analyses than I had heretofore planned, and called for completely revamping plans for publication. For example, reanalysis of the original data with respect to assortative mating suggested that the patterns of assortative mating may be quite different for engaged couples who marry and remain married, as compared with those who marry and later are divorced. In some respects I was glad that I had not published the earlier planned volume on assortative mating based on all 300 couples as a single group. As we proceeded with the data analyses it became increasingly obvious that all of the original data would have to be recoded and reanalyzed. This, together with the vast amount of new data collected in the follow-up and the

availability of high speed computers necessitated rethinking the nature of the analyses to be done and the form of the eventual report on the overall research.

Because I had been elected President of the American Psychological Association for 1955, I hoped that I could present an overall summary of the findings on the marriage research as my Presidential address. This desire grew not only out of a wish to complete this already delayed report, but was enhanced by the knowledge that Professor Terman, now nearly eighty years of age, was definitely planning to attend the annual convention.

The data analysis did not proceed as rapidly as I had hoped. In spite of the availability of an excellent group of research assistants and the high speed computer, we simply could not complete the job in time for the Presidential address. Although computer time was freely available, the appropriate analytic programs had to be written, and in those days they had to be written in machine language -- always subject to lengthy periods of de-bugging. The upshot was that the only part of the data analysis ready to be reported in 1955 was that dealing with personality change in marriage. Thus, my Presidential address, entitled, "Consistency of the Adult Personality", originally conceived of as the third of three volumes in the series of reports resulting from the marriage study, was the first to appear in print. In fact, it is the only major

report of the study thus far published.

The next two years were spent in further data analysis, both of assortative mating and of the very extensive data resulting from the detailed reports on marriage. However, the time which I could spend on the project was greatly limited by heavy professional as well as academic commitments. In 1957 I was drafted to serve as chairman of one of the largest departments of psychology in the world, a heavy administrative load. The result was that I did not accomplish very much by way of further data analysis and practically nothing by way of writing up the findings. During this period three of my graduate students completed their doctoral dissertations using portions of the follow-up data, and by 1961 I felt that I was in a position to write a reasonably definitive report of the entire study. I therefore asked for and received a sabbatical leave for the academic year 1961-62 with the intent of completing this long overdue account. During the first two months I assembled the tables and completely outlined the present volume, with the full expectation of completing the writing during that year. Once again, my plans did not materialize.

This time the problem was that I was unable to resist the challenge of an invitation from Sargent Shriver to spend one year in developing the program for selecting the young men and women who would serve in the newly established Peace Corps. I, therefore, changed my

university leave from a sabbatical to a leave without pay, and spent most of 1961-62, not on this book, but as a member of the Peace Corps staff in Washington, D. C. The hours were long and the work was demanding, but was also very rewarding. However, because I was deeply concerned about the completion of the marriage book, I asked to be relieved of the departmental chairmanship and for a sabbatical leave for 1962-63. The University of Michigan granted both requests.

Unfortunately, what I had hoped would be a productive sabbatical turned out to be in fact a sick leave, although the nature of my illness was not diagnosed. I returned to teaching the following fall, but my poor health continued, and I was able to do little more than meet my classes. In 1965 I discovered that I had been suffering for three years from undulant fever, apparently acquired on a trip to South America in conjunction with my Peace Corps duties. On regaining my health in 1965, I again found myself heavily committed to a variety of university duties in addition to teaching, and thus it was not until I was granted a sabbatical for 1969-70 that I could return to the long overdue marriage book. Even then, further health problems and heavy professional commitments further postponed completion of the book.

— add

Thus it is that the research project which I originally proposed as a doctoral dissertation in 1929 is being completed almost 48 years

later, as I approach the last few years of my professional career.

I naturally regret the long delay in reporting this longitudinal project.

I apologize to my colleagues for not having been able to share my

data and findings with them earlier, but I am enormously pleased

at being able to complete it, all the more so since during

my long period of illness, it appeared to be a task that would never

be accomplished.

In presenting this book length report growing out of the long-term project, I wish to express my appreciation to the many institutions and individuals contributing to it. Only one who has carried out an extended longitudinal study can fully appreciate the many and varied obligations incurred. To the Committee for Research in Problems of Sex of the National Research Council I am indebted for grants which made possible the initiation of the project and collection of the original data between 1934 and 1939. A grant from the Faculty Research Fund of the University of Michigan in 1952 permitted planning the follow-up which was transformed into a reality by grants during the last two years from the Foundations' Fund for Research in Psychiatry. The three universities with which I have been associated have each contributed research facilities and an atmosphere conducive to research. During 1955 the International Business Machines Corporation facilitated the analysis of the data by making available time on one of its new electronic computers (the 650). A score of research assistants and

graduate students have contributed ideas as well as helping to carry out the actual work of the investigation. Finally, I want to thank the 600 subjects whose intelligent cooperation over twenty years made this study possible.

I am especially indebted to my first wife, Naomi Beck Kelly, who assisted me in the original data collection between 1934 and 1939, to Mrs. Karl (Joan) Lohmann, Jr. who served as my research associate from 1954 to 1956 and was responsible for recoding all of the original data as well as organizing and coding all of the follow-up data, and to my present wife, Lillian Kelly, whose assistance in secretarial duties, including computer programming, has been invaluable throughout the thirty+ nine years of our marriage.

E. Lowell Kelly
Ann Arbor, Michigan
January 15, 1978

CHAPTER IOVERVIEW OF THE RESEARCH PROGRAM

I. The program of research was designed to provide at least partial answers to five basic questions:

1... How do men and women pair off in marriage? More specifically, we were interested in identifying those variables which are operative in the choice of one's mate, those characterized by a tendency for likes to marry likes and those with respect to which opposites attract.

2... What characteristics (if any) of individuals are associated with subsequent marital compatibility? Expressed in another way, this was the equivalent of asking whether there are certain characteristics of individuals, men or women, which constitute what might be regarded as an aptitude for marriage and others associated with an unsuccessful marriage experience.

3... Are certain combinations of characteristics of husbands and wives associated with marital compatibility?

4... How do individual husbands and wives change during the course of marriage?

5... Are these changes related to the nature of the marital relationship established, and, if so, how? *do*

RESEARCH DESIGN

The overall research design was relatively simple, involving the following steps: (1) obtain the cooperation of a large sample of engaged couples. (2) Assess the man and woman member of each couple with respect to a wide array of variables, physical, psychological, and sociological. (3) Follow the outcome of each of the engagements to determine whether it led to (a) a broken engagement, (b) a marriage and subsequent divorce, or (c) a continuing marriage. (4) For those couples who married, assess the resulting marriage with respect to a wide variety of dimensions: its duration, number of children, and the relative satisfaction brought to the individual in each of several domains. (5) Reassess the marriage partners after a period of several years with respect to the variables originally assessed at the time of engagement. And (6) examine the relationships of the individual variables and combinations of variables assessed at the time of engagement (time one and at the time of the follow-up, time two).

Although certain details of the original research design had to be modified over the years, in essence it was executed as planned. Between

1935 and 1938, I enlisted the cooperation of 300 engaged couples. Each of the six hundred individuals involved was assessed with an elaborate battery of techniques yielding more than 200 variables. The battery of assessment techniques included a series of anthropometric measurements (including blood groupings), a battery of psychological tests, a thirty-six variable personality rating scale, and a personally administered questionnaire used to obtain essential personal background data. Each of the participating subjects agreed to advise me of the date of his or her marriage (or broken engagement). The original research plan called for an annual follow-up from each husband and wife on the anniversary of their marriage, but this did not prove to be practical. The actual follow-up study which provided definitive information concerning the outcome of all engagements, number of children born, and for most of the couples a detailed report on the outcomes of their marriage, together with a reassessment of couples on most of the original psychological tests was carried out in 1954-55. In spite of the fact that sixteen to eighteen years had lapsed between the time of the original testing and the initiation of the definitive follow-up program, we were successful in securing definitive information regarding the outcome of all 300 engagements, the number of children resulting from all of the marriages, and, for most of the couples, an extremely detailed account of the course of their marriage over the intervening years.

NATURE OF GROUP STUDIED

Ideally, any investigator using human subjects would prefer to work with a purely random sample of the population, thereby ensuring the complete generalizability of his results. Ideally, too, it is desirable to collect the data for an investigation in a manner in which the subjects do not even realize that they are being studied. However, unless the investigator is content to work only with very limited data, for example, that available in official records of applications for marriage licenses, neither of these ideals can be approached. In the present investigation, in which a very large amount of data was essential, it was necessary to utilize as subjects persons who were willing to participate in the research, and willing to spend many hours in providing the investigator with the types of information requested.

In brief we were confronted with the cold fact that an investigation of the kind envisaged must be carried out on a relatively selected sample of the population or not prosecuted at all. Since it was clearly recognized that it would be totally impossible to achieve anything approaching a random sample of the population of engaged couples in the country, our more modest goal was that of obtaining a relatively large sample of engaged couples which showed sufficient variability on each of the variables studied to permit a meaningful

analysis of the co-variation of these variables. Fortunately, as will be evident with the presentation of actual data, this goal was achieved. To the extent that the variation (technically, "variance") was less for our sample of subjects than for the population in general, it is likely that the degree of co-variance (correlation) was reduced. To the extent that this was the case, it is unlikely that any significant relationships found in our sample do not hold for the population as a whole; in other words, limitations of our sample have probably resulted in a failure to identify potentially important relationships among the variables rather than pointing to relationships which do not hold for married persons in general.

SECURING THE COOPERATION OF ENGAGED COUPLES

Because it was desired to make the admittedly selected sample of subjects in this study as representative as possible of the larger population, several efforts were made to call the research to the favorable attention of all engaged couples living within a geographical radius sufficiently close to the investigator's home base to permit arranging for a data collecting session at a location convenient to both the couple and the investigator. To this end, it was arranged to have a number of newspaper releases throughout the

state of Connecticut. In addition, the author described the study to many different groups of young people throughout the state. In all these efforts to provide favorable publicity for the project, the following points were emphasized: (1) The research was being conducted solely to provide answers to important but as yet unanswered questions concerning engagement and marriage. (2) Participants in the study were being invited to participate primarily as a contribution to knowledge. (3) The invitation to participate was being extended to all engaged couples, whether or not a formal announcement of their engagement had been made, but was limited to those individuals sufficiently interested in the problem to be willing to devote several hours to the project at the outset and to provide progress reports on their marriages over a period of several years. (4) The investigator would at no time offer the cooperating couples any advice regarding the apparent suitability of the couples for marriage or in resolving problems which developed during the course of the marriage. This seemingly non-humanitarian stance was deemed essential to the success of the research, and essential to avoid the criticism that the investigator had in part determined the outcome of the engagements and resulting marriages.

Finally, (5) although couples would receive no financial remuneration for participation in the project, each individual would receive, in return for his participation, a personality profile reporting his position on a series of personality variables as assessed by the tests which he had completed. [say, insert something, like this]

Although the first couples studied were those with whom the investigator happened to be personally acquainted, an effort was made to identify as many engaged couples as possible throughout the state of Connecticut. Letters of invitation to participate in the project were sent not only to these couples, but to any other couple whose plan to marry came to the investigator's attention. The most common source of information regarding such un-announced engagements was that of couples who had already participated in the research.

Only about one in twenty couples to whom invitations to participate in the research actually became subjects in the research. Numerically, then, the sample represents a very selected group of engaged couples. Unfortunately data are not available on which to base firm conclusions as to why such a relatively small proportion of those invited to participate in the study elected not to do so. Since many of the invitations elicited no response at all, we made no systematic effort to tally the reasons for declining the invitations. However, on the basis of correspondence and conversations with many of the couples who did not participate, it is possible to identify a number of actual reasons and to infer a number of additional probable reasons for nonparticipation.

The most frequently stated reason for non-participation, and one which was obviously valid for many of the couples, was that, in

spite of a sincere interest in participation, they simply could not find the time in their busy schedule of activities before their marriage to contribute the time which participation involved. Participation required the man and woman individually to spend several hours in the filling out of self-administrating forms and arranging to meet with the investigator at a mutually convenient location for about three additional hours of data collection before the marriage. Since in many cases, couples received the invitation to participate only shortly before the marriage, the problems of scheduling both on the part of the couples and the investigator were not inconsiderable.

Other couples stated frankly that they regarded marriage in general and their own personal relationship to be so personal that they did not wish to subject it to the scrutiny of any scientific investigation, no matter how objective. Still others were skeptical of the possibility of any useful information resulting from the research and therefore felt that it was not worth contributing any of their time to it. Most of the couples to whom invitations were sent, however, simply did not respond and hence we have no real basis for judging their lack of response.

It was anticipated that an investigation of the kind proposed in the letter of invitation would be more favorable received by persons of superior intelligence and educational background. That this

was the case is indicated by the fact that the actual sample of 300 couples turned out to be superior to the general population in both intelligence and education. The IQ equivalent of the mean score on the Otis Self-Administering Test of Mental Ability was 115 for the males and 112 for the females at the time of original testing. Seventy-five percent of the men had at least one year of college, and nearly twenty percent had some sort of graduate or professional training. Only one percent of the men had not attended high school. The females were somewhat less selected on the basis of education, but even so, approximately two-thirds of them had attended college for varying lengths of time. In a word, our invitation to participate in the research tended to select those who were intelligent enough to understand the purpose of the investigation, those who were sufficiently well educated to appreciate its potential significance, and those who were sufficiently cooperative to be willing to sacrifice several hours of their own time to the prosecution of an investigation which would probably have little or no value to them personally.

It is likely that many of the couples who did not respond to the invitation did not do so because they felt that a project of the nature proposed was foolish or even ridiculous, that marriage and marital relationships are too sacred for scientific study and still others who were probably so insecure and uncertain in their interpersonal

relationships that they feared possible harm might result from their participation in the project. Naturally, too, our sample did not include impetuous couples who decided to marry on the spur of the moment, couples who concealed their intent to marry from even their closest associates until the time of marriage, nor did it include couples who married in order to avoid having an illegitimate child.

In general our sample of subjects turned out to be very similar to those studied by Terman, Burgess, and Cottrell, and later by Burgess and Wallin. Whatever factors seem to have resulted in the selection into or out of this sample seem to have been operative in all three studies.

Since it is not possible to secure data of the kind and extent utilized in this study on an un-selected sample, it is perhaps reassuring to remind ourselves that the findings of studies on marriage are probably of interest primarily to the kinds of people who participate in the research leading to the findings.

There was one sub-group of the couples who declined to participate in the project, about whose marital adjustment the author is extremely curious. These were couples in which either the man or woman responded, indicating a sincere interest in the project and a desire to participate in it, but reported that he or she had been completely unable to

persuade her partner to participate. Regretfully, we are unable to report whether such a division of opinion regarding the project was predictive of broken engagements or the extent to which it was predictive of later problems of marital adjustment.

TYPES OF DATA COLLECTED

TIME 1.

The nature of the questions asked in undertaking the present study required that data be collected with respect to an extremely wide array of variables. Since neither previous research nor theory pointed to the primacy of any particular variable or set of variables, it was decided that all subjects should be assessed on as many variables and as many different kinds of variables as feasible with the instruments available and still keep the total time required from participants within reason. The actual battery of assessment devices finally selected provided data with respect to over 200 variables for each member of the couple. These will be listed and described briefly by category:

A. Physical Characteristics

Because evidence pointed to the probability of assortative mating with respect to some physical traits, the decision was made to include a fairly lengthy list of anthropometric measures in the physical assessment. These included the following: height, sitting height, leg length, arm length, hand length, neck length, forearm circumference, hand circumference, calf circumference, stomach girth, hip circumference, chest--maximum expiration, chest--inspiration, shoulder breadth, iliac width, chest depth, weight. These basic measurements in turn made it possible to compute a number of other indices, for example, chest expansion, and also indices of body build which will be described in more detail at the time the data are presented.

The physical examination also involved pricking the finger to obtain a blood sample for immediate blood typing in the four major groups, administration of the Ishahara test of color blindness, noting eye color, hair color, and whether the hair was naturally straight, curly, or wavy. Subjects were also asked several questions concerning the presence or absence of specific disorders experienced either by the subject or present in his family. These included nervous disorders, heart trouble, breathing disorders, lung disorders, tuberculosis, twitches or tics, insanity, and epilepsy. A sample of the actual form used in

recording these physical characteristics will be found in the Appendix,
A-1.

B. The Battery of Psychological Tests

From the time he began to plan this investigation, the author was determined to assess as many characteristics of the subjects as possible with standardized techniques. This decision was made not only by virtue of the fact that such instruments provided objective indices of an individual's position on a trait continuum (as contrasted with subjective amounts of the trait, inferred by an interviewer¹), but also because they would permit meaningful comparisons of the traits of the subjects in this study as compared with other normative groups in society.

Although the number and variety of psychological assessment devices was not nearly as great in 1934 as at present, there were already enough such instruments available to make difficult selection decisions

1. In retrospect, it is of interest that this decision was made many years before the author became involved in a series of studies designed to ascertain the relative reliability and validity of interview judgments. Fortunately for the present investigation, none of the subsequent work concerning the validity of assessments by personal interview, either by me or others, suggests that this was an unwise decision.

necessary regarding the inclusion or exclusion of a test in the battery. An effort was made to sample most of the areas of personality for which objective tests were available at the time and to utilize instruments which had been constructed and standardized in accordance with the principles of sound psychometric practice. The actual battery included in 1934 included the following standardized instruments:

the Otis Self-Administering Test of Mental Ability
the Allport-Vernon Scale of Values
the Bernreuter Personality Inventory
the Bell Adjustment Inventory
Strong's Vocational Interest Blank
two of Remmers' Generalized Attitude Scales.

A brief description of each of these instruments and comments regarding the basis for its selection follows.

The Otis Self-Administering Test of Mental Ability.

The desirability of including a measure of intelligence seemed obvious. This particular test was selected because of (a) its high reliability, (b) the fact that it required only thirty minutes for administration, and (c) the ease of administration, resulting from its omnibus construction, thus necessitating timing only the beginning and the end of the test.

The Strong Vocational Interest Test

This test was selected, in part, because it had been used by Terman in his study of marital happiness, but also because it seemed to sample and to provide information regarding several areas of personalities not tapped by other tests in the battery. In addition to the availability of scores for many different occupations, scores were also available for masculinity, femininity, and interest maturity -- traits which might well be associated with marital compatibility. The men's form of the test (actually Form B, Student) was used for both males and females in order to permit direct comparison of the scores for both members of the couple.

The Bernreuter Personality Inventory

This instrument was included because of its widespread use at the time and also because of its apparent efficiency, yielding as it did scores on a number of different traits from a single inventory.

The Bell Adjustment Inventory

This inventory was added to the battery in spite of some duplication of items with the Bernreuter Inventory, primarily because it yielded scores in the potentially meaningful areas of home and health adjustment.

The Allport-Vernon Scale of Values

This instrument was also in relatively wide use at the time and was thought promising, since it measures the relative strength of six ~~625~~

basic interests or motives in personality: economic, aesthetic, social, political, and religious values.

The Remmers Generalized Attitude Scales

The importance of attitudes as determinants of human behavior was so obvious that it seemed necessary to include several measures of attitudes, particularly attitudes with respect to institutions and activities crucial to the marriage relationship. Since appropriate instruments were not available for measuring all the attitudes deemed to be important, and also because the Remmers Generalized Attitude Scales seemed to provide measures of several attitudes with considerable economy of subject's time, two of the generalized attitude scales were used: (a) Attitudes toward any institution. This forty-five item general scale was printed in a form to permit assessing each subject's position on the pro--con continuum toward three institutions: marriage, divorce, and the church. (b) Attitude toward any activity. Again the generalized attitude scale format was used to provide measures of each subject's attitude toward five activities: rearing of children, housekeeping, entertaining, care of the lawn, and gardening.

C. The Personality Rating Scale

Because it appeared likely that the battery of objective instruments described above did not tap all potentially important aspects of personality which might be operative in assortative mating or marital compatibility, the author developed a thirty-six trait graphic rating scale for use in this investigation (a copy of this scale is included in Appendix A-2). The personality variables or "traits" included in this scale were chosen by reference to other available scales, from additional traits suggested by colleagues and, finally, by elimination of traits for which very low coefficients of reliability were obtained in a try-out form.

In view of the very real difficulty in choosing appropriate trait names, it was decided to avoid all mention of traits as such, and simply ask the rater to answer the questions about the individual being rated. Thus, instead of asking the rater to rate a subject on "intelligence," that is, asking how much of this trait a person has, the question was phrased, "how intelligent is he?" The rater was asked to place a mark somewhere on a five inch line, characterized by only three points on the scale: an appropriate adjective or phrase at each extreme and a heading of "most people" in the middle of the scale. It was felt that the term "most people" would be more meaningful to untrained raters than the more technical term "average," and also that it might result in less skewed distributions of the resulting

ratings than would be obtained by using "average" at the middle of the line. The use of the question form to elicit ratings has the disadvantage of requiring separate male and female forms of the scale (that is, using "he" or "she"), but since several thousand forms for each sex were needed, this was not a serious disadvantage.

This rating scale was used to collect three types of ratings:

(a) Self ratings: throughout this volume such ratings are designated M_s or F_s for male and female subjects respectively.

(b) Ratings of each subject by his research partner. These ratings are designated M_m or F_m , the subscript standing for "mate."

(c) Ratings by others. At the time of the original data collection each subject was asked to address envelopes to five persons who knew him well enough to rate him accurately, and ratings of each subject were requested from these acquaintances. These average ratings by others are designated M_o and F_o throughout this volume. This rating scale thus provided assessments of what subsequently has come to be called the "self-concept." It also provided a personality profile of each subject as seen by his research partner, and also by his acquaintances. Furthermore, the differences between these three sets of ratings provided promising indices of discrepancies in the perception of our subjects as seen by themselves, by their partners, and by others.

whether or not the research partners were agreed on this question.

Items 78 to 80 asked subjects to indicate the three characteristics most admired and least admired in people, and perceived differences between themselves and their research partner. Questions 81 through 94 were designed to ascertain the subjects' previous dating history together with several critical facts concerning the present courtship.

Question 95 sought to ascertain the perceived attitude of the parents of each subject toward the anticipated marriage. And, finally, questions 96 to 98 dealt with the facts regarding previous marriages (if any).

? were there any

Page six of the personal data sheet is entitled, "Your Views About the Ideal Marriage." On this page each subject was asked to express his opinion as to how important it is for the ideal marriage that each of the 33 possible conditions be met. This form was used exactly as it had been originally prepared by Burgess () and also used by Terman.

Finally, the last page of the personal data sheet asked several additional questions, designed to elicit free response answers of potential value. Subjects were asked why they chose that particular person for a life partner, why they preferred him or her to all others they had known, why they felt their marriage would be successful. They were also asked whether there was anything which made them feel that the outlook for a successful marriage was not as favorable as they might hope. Finally, the last two questions asked the subject to briefly characterize his or her father and mother.

This completes the description of all of the instruments used in the original (193 -3) data collection ("Time 1").

METHODS AND PROCEDURES USED IN DATA COLLECTION (TIME 1)

On learning of the engagement (formal or informal) of any engaged couple, the investigator sent a personally signed letter of invitation, along with a brief description of the project to each member of the engaged couple. The only exception to this general procedure occurred in those cases in which the information regarding the engagement was received so late that it would have been impossible for the couple to participate in the research no matter how interested or willing. Unfortunately, this was a fairly frequent occurrence and resulted in the loss of many potential subject couples.

The letter of invitation included a return post card on which the subjects indicated their interest or lack of interest in participating in the research. On receiving the information that a couple was willing to participate, each member of the couple was sent an envelope containing all of the self-administering inventories described above, together with a set of instructions emphasizing the importance of each subject's responding to each question of each instrument personally and without consultation with his or her research partner. It was explained that this was a condition necessary both for the objectivity of the research and also in order to assure the uniqueness and individuality of the resulting personality profile promised to each subject in return for participation. It is impossible to judge the extent to which this admonition was followed. However, since

members of a couple had nothing to gain by collaboration, and on the basis of other evidence to be reported later (correlations of test scores between research partners at Times 1 and 2), it may ^{also} be concluded that collaboration in filling out the forms was minimal.

The forms used in the initial mailing were:

Strong Vocational Interest Blank
Bernreuter Personality Inventory
Bell Adjustment Inventory
Allport-Vernon Scale of Values
Remmer's Generalized Attitude Scale Toward Any Institution
Remmers Generalized Attitude Scale Toward Any Activity

All of the above forms were to be filled out without any specified time limits and the instructions for each were sufficiently specific so that little difficulty was experienced in responding to them by the sample of subjects used. The total time spent in filling out these self administering forms varied from three to four hours, depending on the subjects' rate of reading and responding.

Also enclosed in the envelope with the self administering forms was a post card asking each subject to indicate two or more alternative dates when the couple might meet with the investigator for an additional data collecting session of about three hours. The subjects were also asked to indicate whether they would prefer to come to the writer's laboratory, whether he should come to the home of one of the members of the couple, or if it would be preferable to arrange for this session

to be carried out at some other location, for example, a church or YMCA in a convenient location. For many couples a considerable interchange of correspondence and many long distance telephone conversations were needed in order to arrange this face-to-face data collecting session.

On meeting the couple, the first few minutes were spent in establishing rapport with the subjects, and answering such questions as they might have concerning the project. The balance of the data collecting session was conducted in accordance with the following schedule:

- (1) The man and woman were seated on opposite sides of a table separated by a cardboard barrier which made it impossible for either of them to see the other's blank or the responses which he was making on it. Each member of the couple was given a copy of the 36 trait personality rating scale, and asked to rate his or her partner on each of the 36 traits as accurately and frankly as possible. Subjects were assured that neither would ever see the other's ratings. After this rating of mates had been completed, the subjects were then asked to go back over the same blank, and, using a different check mark, rate themselves as frankly and accurately as possible on each of the 36 scales. This rating task seemed to be one generally enjoyed by most couples and hence well fitted for the initial portion of the "interview."

(2) After rating each other and themselves, the couples were then jokingly told that they probably were so much in love with each other (and perhaps with themselves!) that they had probably given us biased ratings. They were further told that since we would very much like to know how they really should be rated on each of these traits (and because they too would like to know how they appear to others). Then each subject was asked to address five envelopes to persons who knew him or her well enough, and who they felt would be willing to rate them on the same scale they had just used in rating themselves and each other. Because experience showed that only 70-80% of acquaintances returned these scales, the subjects at the same time provided the names and addresses of three alternative acquaintances to be used as judges in case one or more of the first five did not reply. Subjects were advised at this time that the average of the ratings of their five acquaintances would be used in constructing a personality profile to be returned to each of them. By this time a very satisfactory degree of rapport had been established with the subjects.

(3) Next followed the simultaneous administration of the Otis S-A Test of Mental Ability to both members of the couple. During the thirty minutes spent in taking this test, the investigator was able to check for omissions the various forms which had been filled out previous to the appointment, and to prepare the rating scales for mailing.

(4) Following the administration of the Otis test the subjects who were still seated on opposite sides of the table (or in different parts of the room) were then asked to fill out the Personal Data Sheet. The better educated persons among our subjects were able to complete this form with little supervision, but in any event throughout the period of completing this form the investigator moved back and forth from the man to the woman in order to answer any questions or to suggest fuller responses to items. Because of the difference in the rate of writing, it was unusual for both members of the couple to be at the same place in the questionnaire or to complete it at the same time.

(5) The first member of the couple to complete the questionnaire was then asked to change into a sterilized bathing suit for the anthropometric measurements. After these measurements had been completed for both members of the couple, a small blood sample was taken from the finger or ear lobe of each, and with equipment carried by the

investigator, an immediate blood typing was carried out according to the standard (Moss) technique. Unfortunately, because of problems in maintaining a supply of suitable serum, there was considerable missing data with respect to this particular variable.

This data collecting session occupied from two to four hours, depending on the writing speed of the participants and the amount of non-essential conversation which occurred. The last few minutes of each session were occupied with a review of the nature of the personality profile which each member of the couple would receive.

Each couple was also asked to be sure to remember to notify the investigator of the date of their marriage and plans for the annual follow-up on the anniversary of each marriage explained. The session closed with the investigator reiterating the critical importance of the subjects' continuing their participation in the research if we were to obtain answers to the questions which we had asked.

From the above, it will be seen that an effort was made to utilize the advantages of, and minimize the limitations of, each of the two major techniques for data collection in a large scale study of human subjects. The chief advantage of the interview is its flexibility, but in a study such as this, involving the collection of data on so many variables for so many subjects, wherever possible we chose the questionnaire. However, because of the limitations of the questionnaire procedures certain of the data, as noted above,

were obtained in what might be called a highly controlled interview, or, alternatively, a personally supervised questionnaire situation.

THE INVESTIGATORS

Although the data collection could have proceeded much more rapidly by utilizing many different "interviewers", it did not seem wise to do so because of the possible deleterious effects on rapport and possible interviewer differences in techniques of making anthropometric measurements, blood typing, and so on. Ideally only one interviewer should have collected data on all of the couples, but because of the problems in scheduling the personal data collecting sessions, it became increasingly necessary to schedule two data collecting sessions simultaneously, often in different locations. Therefore, the author's wife (N.B.K.) who had participated in developing the research design and methodology and who had been present at many of the early data collecting sessions was drafted to serve as his alter ego for such sessions. A duplicate set of anthropometric devices, blood typing equipment, etc., was procured. And after careful training, N. B. K. was permitted to conduct the data collecting sessions on her own. No other research assistants were used in data collection.

Responsibility for data collection was distributed as follows:

E.L.K. only.....	101
N.B.K. only.....	123
Both	75

The above totals to only 299, and the reader may well ask who collected data on the missing couple. The answer is simple: One particular couple, extremely intelligent and well educated, literally begged to be included in the research project. However, we were completely unable to schedule a personal data collecting session with them. With some reluctance, the author agreed to permit participation by this couple even though it meant that there is missing data for many variables on them.

Couple 300

CLINICAL PREDICTIONS OF OUTCOME OF THE ENGAGEMENTS

Although all of the couples were presumably "in love" and all anticipated marriage at the time of the data collection, the interviewers could not but notice marked differences in the behavior of the members of the couples -- their attitudes toward each other, toward the research, and toward life in general. Therefore, a decision was made to have each interviewer, immediately after the

data collection session, record his or her judgment regarding the probable outcome of the engagement. A simple one to five numerical rating scale was used. The distribution of ratings shown in Table 1-1 resulted.

Insert Table 1-1 about here

As can be seen, the ratings of both interviewers tended to be slightly skewed toward the favorable end of the continuum, those of ELK more than of NBK. The failure of the interviewer to record ratings for 32 couples was probably most often a function of fatigue due to the lateness of the hour when the data collection session was completed.

For the 69 couples seen and rated by both interviewers, their independently recorded predictions correlated at $r = +.70$. Although the basis for these clinical impressions was not clearly explored, it appears that the two interviewers did tend to agree fairly well in their prediction of ^{outcomes} outcomes. Unfortunately, however, the validity of these ratings turned out to be very low (as will be reported in Chapter).

Table 1-1
Engagement Outcome Ratings

Couple Rating"	Interviewer - ELK	Interviewer - NBK	Total (One or Both)
5 - favorable prediction	6	8	14
4 -	70	57	127
3 - Average Typical	42	95	137
2 -	20	29	49
1 - unfavorable prediction	3	8	11
TOTALS	141	196	337
MEAN	3.40	3.15	3.26
Rated by both Interviewers			69
Rated by one interviewer only			268
Not rated by either			32
Grand total of couples			300

INFORMATION NOT COLLECTED AT TIME 1

In spite of the number and variety of instruments used for data collection at Time 1, the careful reader cannot but have noticed that no effort was made to obtain detailed information regarding the sexual history of the subjects and their attitudes toward sexual practices. The almost complete absence of detailed sexual information such as that earlier reported by Davis (), Hamilton (), and Dickinson and Beam (), resulted not from oversight, but reflected a studied decision based on the following two considerations:

(1) It was judged extremely unwise, perhaps even fatal, to the success of the project to request such highly personal information under the conditions of the data collection. It will be remembered that Davis collected her data using unsigned questionnaires, thus providing her respondents with complete anonymity. Although Hamilton and Dickinson had seen their subjects in interviews, both were physicians and their data had been collected in a patient-doctor relationship. Since neither of these conditions held for this project it was feared that any effort to collect detailed facts regarding sexual behavior would markedly reduce the willingness of young couples to participate and thus result both in a smaller and less representative sample of engaged couples.

(2) Such data as were deemed relevant to this investigation could be obtained later, near the end of the follow-up program. By waiting until several years of marriage had elapsed, it was felt that the subjects would be far more willing to provide highly personal information about both their pre-marital and marital sex history.

Accordingly, work was begun in 1936 to develop a fairly detailed "Sex History" form to be sent to all couples after the last (seventh) Annual Follow-up form had been received. It was fully anticipated that some, perhaps many, subjects would refuse to complete it. If so, this would result in missing data for these persons and perhaps make it impossible to provide a meaningful analysis of sexual factors in the marital compatibility of the total group. Even so, it would have permitted an interesting comparison of the personality characteristics of those willing and unwilling to provide such information.

In 1936 and 1937, two "Sex History" forms were prepared. The first form covered childhood experiences, source and nature of sex instruction received, and pre-marital sexual experiences both with the marital partner and others. The second form sought to ascertain the details of each subject's sexual experiences with his research partner and extramarital sex activities (if any). Each of the two forms were prepared in a "male"

and "female" edition. In decisions regarding the content, the wording of the questions and their response alternatives, in these forms, the author relied heavily on the published reports by Davis (), Hamilton (), and Dickinson and Beam (). In addition, Dr. R. H. Dickinson personally reviewed early drafts of the forms and offered many helpful suggestions for their improvement.

None of the sex history forms on which much effort had been spent were ever utilized in the project; in fact, they were not even printed! As noted in the Preface, and explained in the next section of this chapter, our plans to obtain seven annual follow-ups had to be abandoned in 1942. However, the work on these forms was not without value; they were extremely useful in preparing two of the forms used in the definitive follow-up at Time 2 (see below).

THE ANNUAL FOLLOW-UP FORMS

It will be recalled that the original plans called for obtaining, from each husband and wife, seven annual "Reports on the Marriage," one on each anniversary of the marriage for seven years. Fortunately, it was not necessary to develop an entirely new instrument for assessing the year-to-year degree of compatibility achieved by our

married couples. R. O. Lang, in 1932, had first tackled the problem in The Rating of Happiness in Marriage, an unpublished M. A. thesis under the direction of Burgess at the University of Chicago. Burgess and Cottrell subsequently added other questions to Lang's questionnaire and devised a set of scoring weights which permitted computation of an "index of marital adjustment." Terman revised the Burgess form slightly and developed scoring weights using somewhat more rigorous statistical procedures; the resulting scores provided what Terman () called his "index of marital happiness." Whether the resulting index was labelled marital "adjustment" or "happiness" was not critical; the items included in the forms all dealt with behaviors which appeared to be related to marital adjustment and all were correlated with self-ratings of marital happiness. Although far from a perfect instrument, the Burgess-Cottrell-Terman items appeared to be reasonably satisfactory for obtaining criterion data in this study. Furthermore, it seemed desirable to utilize in our longitudinal study the same criterion measures as had been utilized in the earlier cross-sectional investigations.

With the permission of and encouragement by both Burgess and Terman, the author utilized their experiences and forms in developing the "Annual Follow-up Form" (Appendix). It contained the same items and followed a very similar format to Part IV, "Your Present Marriage," of the Information Schedules as used by Terman. While most of the

differences between Terman's forms and the one used in this study were a slight change in the wording of questions, a few of them are substantial and worth noting:

- A) When asking for the frequency of agreement or disagreement, (our item 2, Terman's item 3) the Kelly form omitted "matters of recreation" and added "sexual relations," "dress" and "division of household duties."
- B) The annual follow-up form includes a few more specific "complaints" regarding spouse behavior.
- C) The item "If I could make my wife (husband) over, I would change ..." (Items 80 and 81) was added.
- D) The annual follow-up form asked for three bits of information which might change from year to year (Items 12, 13, and 14).
- E) Page 4 of the annual follow-up forms asked the respondent to write at length about any aspect of his marriage which he had not been able to describe adequately in checking responses to specific questions.

Fortunately, because of the method of weighting responses used by Terman, these modifications of his form did not necessitate any change in scoring. For example, the weights for "agreement" were based on the average amount of agreement on all of the items, the weight for "complaints" based on the "total complaint score", and so on. Therefore, the Annual Follow-up Form was routinely scored using Terman's scoring weights, and the resulting scores for our group are directly comparable to those for the subjects in Terman's study.

Printing of the follow-up forms was not completed until April, 1937. Beginning at that time, each husband and wife received one of these annual follow-up forms. They were mailed in separate envelopes with instructions that they should be filled out independently and returned in the separate self-addressed stamped envelope provided. Since 37 of the 83 couples assessed in 1935 had married before the end of the year, their first follow-up form arrived after their first anniversary, but they were asked to complete the forms as of the end of the first year of marriage, and promised that the subsequent follow-up forms would be mailed on time.

Thereafter, these annual follow-up forms were mailed to each couple each year a few days before the anniversary of their marriage. This routine procedure was followed until September, 1942, at which time ~~all couples were advised that the author was entering the military service and would contact them again after the war for a definitive follow-up~~

CHAPTER IITIME 2: RE-ASSESSMENT AND THE DEFINITIVE FOLLOW-UP PROGRAM

In 1952, we began planning the details of the final stages of the data collection for this project. Because there had been no communication between the investigator and any of the subjects since 1942, our first concern was in re-contacting both members of all original couples and describing our definitive plans for this final stage of data collection.

Fortunately, we had not only the last mailing addresses of all couples (1939-40), but had recorded at the time of the original data collection the addresses of their parents. In addition for each member of the couple, we had the names and addresses of seven or eight friends who had been asked to rate them on the personality rating scale. Thus, in spite of the interval of many years, we felt that it would be possible to locate practically all of the original subjects. Fortunately this proved to be the case; for all the original 300 couples we were able as a minimum: (1) to ascertain the outcome of the engagement, and (2) for those couples who had married, to learn whether the marriage had been broken by death or divorce, and to ascertain the number of children born to each marriage.



Naturally we hoped to obtain the continued cooperation of as many of the subjects as possible in this definitive follow-up stage of the study. It was too much to hope, however, that all of the living members of the original group of 600 would cooperate fully at this stage. However, considerable care was taken in the formulation of the letters and other materials sent to the original subjects in an effort to re-establish our originally good rapport, and to persuade each subject of the contribution that could be made by assisting in the final stage of the data collection. As in all previous contacts with our subjects, individual letters and accompanying materials were sent in separate envelopes to each of the original subjects, together with self-addressed and stamped return envelopes. This was done for two reasons: to remind the subjects that, as in the original data collection, it was extremely important that each subject, although a member of a couple, was participating as an individual without collaboration or collusion in filling out the forms, and because at this point in time we could not be at all sure which couples might have been divorced or might be experiencing stress in their marriage.

The first mailing, sent to the most probable working address for each subject, included the following materials:

(1) A chatty one page and personally signed letter explaining the reasons for the interruption of the annual follow-up programs and reasons for the delay in recontacting them after World War II. There was

also a reminder of the many changes that had occurred in the world and in the personal lives of all of us in the intervening years.

The letter closed with a plea to each subject that he read the enclosed Confidential Progress Report (described below), and complete and return two simple enclosed forms (Forms 19 and 20, described below).

(2) Confidential progress report to participants; this was a four page printed brochure. Part one reminded participants of the original goals of the study, part two reviewed the general plan of the study, including an agreement by each subject to complete the annual report form for the first seven years of marriage. The next section summarized the present status of the study and reported on the status of the couples at the time we last heard from them, sometime between 1935 and 1952: This status report indicated the number of couples who reported broken engagements and marriages, the number of children born, and the geographical distribution of the couples. Section five reported the essential findings of our earlier analyses of the data with respect to assortative mating.

The last section of this form, titled "Next Steps" described in detail the schedule for collecting further data from the participants. This was broken into three stages: (1) The subjects were asked to fill out and return immediately Form 19, a three by five card indicating the preferred address to which future correspondence should be sent, and Form 20, Basic Follow-up Information. On this

one page form we asked each subject to confirm or correct our records as to the outcome of their engagement to their research partner, and to report on their present marital status (for example, engagement broken, date of marriage, divorced, death of research partner, remarriage, etc.) This form also called for information concerning each of the children born to the subject: sex, the year of birth, age of death ~~of~~ deceased, whether by first or second marriage, and whether or not the child was adopted.

The subjects were told that their further participation would involve completing a series of forms which would be sent to them in two sets: Set A would consist primarily of the personality assessment inventories which they had first filled out at the time of their engagement, and Set B would include additional forms: one on which they could update their own personal life experiences since the time of original testing, and another which would call for a fairly detailed report of the marriage to their research partner. They were advised that each of these sets of forms would require from two to three hours of time, were again reminded of the importance of responding to them as individuals (that is, without discussion with anyone else), and again reassured of the complete confidentiality with which their responses would be treated.

Finally, the subjects were told that as soon as they had completed both Sets A and B of the follow-up forms, they would each be sent a check for \$5.00 as a token payment for the time which we had asked them to spend in this phase of the project.

This four page brochure concluded with a statement of plans for further data analysis and publication of the findings.

Thanks to the availability of multiple addresses and informants for each subject, ^{we} were successful in locating the present whereabouts of the 587 surviving subjects; thirteen of them had died during the intervening years. More importantly, we were able to ascertain the outcome of each of the 300 original engagements: 22 of the couples had broken their engagement, and 278 of the engagements had culminated in the marriage of the research-partners. Of these 278 resulting marriages, 12 were dissolved by the death of one of the subjects, and 39 of the marriages had been terminated by a divorce.

(Actually, there were 40 divorces among the married group, but one couple married in 1937 with three children was divorced in January, 1954 after seventeen years of marriage. However, these subjects ~~were~~ remarried the same year!)

Form 20, providing basic follow-up information (see Appendix concerning the outcome of the engagement and composition of the family of all couples who married, was obtained for 287 of the original 300 couples. Regretfully, neither member of 13 of the

couples was willing to provide this minimal Time 2 follow-up information. This included six couples who married and are still married, one broken engagement, and six couples of the divorced group. Fortunately, it was possible to obtain information about these couples from parents of the subjects or from acquaintances who had been asked to rate members of the couple at Time 1.

DATA COLLECTED AT TIME 2

As noted above, we decided to ask the subjects to provide Time 2 data in two stages. This decision was made for two reasons: because of the very extensive information needed, it appeared likely that some subjects, upon receiving all the many forms at the same time might refuse to participate because of the time demanded. Furthermore, because of the highly personal nature of some of the information requested in certain of the forms, it seemed preferable to secure at least the first block of information from those who might balk at filling out the second set of forms.

Set A was made up primarily of forms designed to reassess subjects on the same variables for which data was collected at Time 1.

Ideally, we should have liked to obtain retest scores on all of the original measures. But the necessity of limiting the total time which

could be requested of subjects at Time 2 dictated some reduction in the retest battery. The first test eliminated was the Otis S-A Test of Mental Ability. This is a timed test, and it seemed doubtful that subjects could accurately administer it to themselves under strict time limits. Furthermore, evidence available from other investigators (Owens, 1953) indicated that scores on such instruments were remarkably stable over long time intervals. The second of the original instruments not included in the test battery at time two was the Bell Adjustment Inventory; it was eliminated for two reasons: (1) the fact that it provided essentially the same information as the Bernreuter Inventory and (2) because items on the Bell inventory were worded primarily for high school students (about a quarter of the items dealt with adjustment to the parental home). Finally, although we should have very much liked to have gotten personality ratings by present friends of the subjects, we did not do so, primarily because of the considerable expense of securing such ratings. Also, this type of assessment had proved to be one of the more difficult aspects of the original data collection.

The result was that the envelope containing the Set A forms contained six booklets, as follows:

- (1) The Bernreuter Personality Inventory (a four page, large yellow booklet)

(2) The Remmers Generalized Attitude Scales (this was a four page blue booklet), reproducing the same items that had been used to measure attitudes toward homemaking activities, and toward marriage, divorce and church.

(3) The Allport-Vernon Study of Values. (Although a new form of this booklet had been published in the interim, we used the original form in order to permit direct comparability of the resulting scores).

(4) The Strong Vocational Interest Blank (a small yellow booklet).

(5) The Personality Rating Scale. This was a reproduction of the same scale used at Time 1. Separate male and female forms were prepared, and each subject was asked again to rate his research partner with a check mark ("✓"), and then go back through the scale and rate himself with an "x". The instructions directed those not presently married to their research partner to rate the individual as he last knew him and to indicate the year on which the ratings were based.

These five forms, all essentially identical to those used at time one, provided for the reassessment of the subjects on 103 variables. The sixth and last form included in Set A was a completely new instrument entitled, "Word Rating Form". This booklet had been prepared on the basis of research on the Semantic Differential (Osgood, Suci, and Tannenbaum, 1957). This form was a small, 24 page

mint-colored booklet asking the subject to rate 22 words on each of ten seven-point continua (for example, weak-strong, active-passive, pleasant-unpleasant, etc.). The 22 words for which ratings were obtained were:

winter	home
table	marriage
love	god
wife	anger
me	air
fun	power
mosquito	sex
mother	divorce
window	husband
child	my life
shame	father

As will be noted, about half of these object words are relatively neutral, and the other half are relatively closely associated with marriage and the family. According to Osgood, ratings of this kind enable the investigator to locate the connotative meaning of each word for each subject in three dimensional space. It was hoped that use of this device would provide us with a potentially useful "index of semantic harmony", perhaps associated with the quality of communication between spouses and, hence, their overall marital adjustment.

THE FORMS IN SET B

After filling out and returning the Set A forms, each subject was then sent a bulky envelope ^{containing} consisting of four forms designed to obtain all of the further information necessary for the project. These forms included:

Personal History II

This was an eight page lithoprinted form. It was designed primarily to ascertain the changes that had occurred in the life of each individual subject since the time of the original data collection. Information was requested with respect to level and kind of terminal education, military experience if any, present employment and earnings, work

experiences and preferences for different types of work, church attendance and denomination, the use of alcoholic beverages, physical and emotional health, both at present and over the last twenty years. Subjects were also asked again to rate their childhood happiness and the marital happiness of their parents and to indicate the extent to which they felt that their life goals had been achieved.

The next section again asked for a report on the source and adequacy of early sex education, and judgments of the completeness of information regarding sex at ages 10, 16, at the beginning and end of their engagement, and at present. They were also asked to report on the extent of their physical relationships with others up until the end of their engagement: with their research partner, with others for whom they had affectionate attachment, and with casual dates.

Page six of this form, entitled, "Your Views About the Ideal Marriage", asked subjects to indicate their present opinion as to the desirability or undesirability of 33 conditions regarded by some as important for the ideal marriage. These were identical to 33 opinion statements to which they had responded in the Personal Data Sheet at Time 1.

The last two pages of the Personal Data Sheet were also designed to elicit the subjects' opinion with respect to a number of other issues which were judged to be of potential importance in marital adjustment.

Most of these questions were related to marriage and the family: For example, "the family is a sacred institution, divinely ordained" and "some equality in marriage is a good thing, but by and large the husband ought to have the main say-so in family matters." A few of the items, however, were of a much more general nature: e.g., people tend to be divided into two distinct classes, "the weak and the strong", and "if people would talk less and work more, everybody would be better off."

At the very end of this form subjects were asked their opinion as to the relative status of men and women in modern society: e.g., "do men or women lead the harder life, get the greater satisfactions in life, or are tied down the most?" The final question in this Personal Data-II Form read: "Suppose that (by some miraculous reincarnation!) you could live a second life, would you choose to be a man or woman?"

It will be noted that this form contained no questions relative to the subjects' previous or present marital status. Every question was equally applicable to all subjects, whether they had never married, married and divorced, or were presently married to their research partner or to someone else.

In contrast, the next two forms (like the Word Rating Form of Set A) were specifically designed to assess the degree to which subjects were perceptive of and therefore perhaps better able

to understand their research partners.

Personality Rating Scale -- Prediction of Partner's Self Ratings

This form consisted of a reprint of the 36 personality rating scales previously described, but with a new set of instructions on the first page. In brief the instructions were to fill the form out "in the way you think your research partner rated himself." We of course emphasized the importance of doing this without discussion with the partner, and acknowledged the difficulty of the task, but asked each subject to give us his best guess of his partner's ratings.

Krout Personal Preference Scale

This scale was developed by Maurice and Joanna Krout Tabin () and published by them. The instrument was based on an extension of the Freudian theory of successive stages in psycho-sexual development and represented an effort to assess the level of psycho-sexual development with a paper and pencil inventory. Although this instrument had not been subjected to a rigorous statistical validation, we decided to use it in this research for two reasons:

- (1) It promised to assess, albeit somewhat crudely, certain facets of personality which had not been assessed with the psychometric instruments used at the time of the original data collection.

(2) Much more importantly, the nature of the items used in the Krout scale made it a promising tool for use in a further attempt to assess our subjects' understanding (empathy?) of their research partner.

The Personality Preference Scale consists of 100 items, ten in each of ten sections. The subject is merely asked to encircle "L, I, or D" to indicate: "I like it", "I feel indifferent about it", or "I dislike it". The nature of the items can best be indicated by choosing one at random from each of the ten sections:

sleeping curled up
eating soft boiled eggs
steaks well done
giving things away
proofreading
posing for a picture
strong athletic girls
reading sports page
collecting antiques
raising money for charity

The Krout Personal Preference Scale is scored by assigning a priori weights on +1 or 0 or -1 to the three alternative responses to each item. The resulting ten scores are presumably related to successive

stages of psychosexual development as follows: prenatal, early oral or oral passive, late oral or oral aggressive, early anal, late anal (((Lowell----I can't figure this out. We must look it up)))) ?

In our adaptation of the Krout for this investigation, the subject was asked to respond to the items for himself on pages 1 and 2 of the form. Pages 3 and 4 repeated the same items, but here the subject was asked to respond in the way he thought his research partner would have responded to each of the items.

This brings us to the last, but most elaborate, of the forms included in Set B.

Report on Marriage to Research Partner.

This was a 20 page 8½ x 11 lithoprinted form which attempted to provide all potentially relevant information with respect to the nature of the marriage, and especially the degree of compatibility achieved in the marriages between our research partners. It was organized in two parts: Part A (nine sections) and Part B (six sections).

Part A, Section 1 concerned the Marriage Ceremony. It included but two questions: "Where were you married," and "Who conducted the ceremony?"

Part A, Section 2 -- Relationships with Other People. The specific questions were designed to ascertain the nature of the subject's relationships with his mother, his father, his mother-in-law, his father-in-law, and his closest friend.

Part A, Section 3 -- Running the Household. This concerned the division of duties between the spouses, their children, hired help and others, with respect to housework and physical maintenance of the house and yard.

Part A, Section 4 -- Recreation and Social Activities. These items sought information regarding the nature and extent of the family's recreation and social activities and the degree to which these were characteristically individual or shared with the spouse.

Part A, Section 5 -- Earning and Spending the Family Income -- This section included questions on the family's housing, its kind, its value; the family automobiles; the degree to which the family made use of installment purchasing; the financial status of the family; and an estimate of the amount of money available to the family over and above the bare necessities during the last few years. It also

sought to ascertain the priorities of each family with respect to the expenditure of the family income, a comparison of the family's standard of living with that of neighbors, parents, in-laws, best friends, etc., and, finally, a rough estimate of the net worth of the family at the time of the report.

Part A, Section 6 -- Religion. This concerned the family's religious activities and practices, their extent and importance, and whether shared by spouse and children.

Part A, Section 7 -- Sexual Relationships. There each subject was again asked to rate the adequacy of his and his spouse's preparation for sexual relations at the time of marriage, the anticipations of both self and spouse with regard to sexual relations. Information was also requested on the actual frequency of intercourse during the first three years, the middle period, and the last three years of marriage, as compared with the subject's preferred frequency during the last three years and the subject's perception of his spouse's preferred frequency for the same period. The male form asked for a judgment of the frequency of impotence, the female about adequacy of orgasms. Finally, the subject was asked to rate the considerateness of his spouse with respect to his own feelings about sexual relations.

Part A, Section 8 -- Children and Their Upbringing. Each subject was asked whether they planned to have any more children, and to answer again two questions asked at the time of the original data collection: the number of children wanted, and whether or not he and his spouse were in agreement on this. Subjects were also asked, "If you could have had your choice, how large a family would you have had during your marriage?" and "whether or not your own wishes concerning the size and composition of your family changed during your marriage." The last part of this section included questions to be answered only by individuals with children: the weight which was given to the ideas and feelings of children in making family plans, frequency of getting together with the children both at home and away from home, and whether or not the spouse participates in these activities. The final question of this section asked for a free response reply to the question "what would you say are the major problems that have developed in bringing up your children?"

The wife's report on marriage also included an additional single sheet calling for a detailed record of all pregnancies occurring during their marriage, their outcomes, and the nature and effectiveness of the contraceptive practices.

Part A, Section 9 -- Past Events. The first question in this section asked, "Have you and your wife lived apart at any time during your marriage", and if the answer was "yes", to indicate the dates, the length and the reason for the separation. The remainder of this section listed 38 events or circumstances which may or may not have occurred in the marriage. Subjects were asked to check the occurrence of each particular event or circumstance in their marriage for each of three periods: first three years, middle period, last three years. After noting the occurrence or non-occurrence of these events and circumstances, the subject was asked to go back and encircle those check marks which indicated events or circumstances which they felt served to lessen the happiness of their marriage.

In assembling this list of items, an attempt was made to include all experiences which had been suggested by others as having an impact on marital adjustment. Examples include: relatives living in the home, excessive use of alcohol (by self or spouse), a wife working for pay, military service, serious difficulty in making ends meet financially, unplanned pregnancy, and extramarital affairs.

Male & Female Forms²

Part B -- You, Your Wife, and Your Marriage

and (Your, your husband, and your marriage)

Part B, Section 1 --- The parts You and Your Wife Played.

(husband)

This section was designed to evaluate perceptions of each subject with respect to the importance of the different roles played by husbands and wives in a marriage, and to assess the degree to which each subject believed that he and his spouse performed well in these several roles. In the husband's form, this section began with the following paragraph:

In some ways, life is like a play. You each take a turn at playing a number of different parts. At various times you are a bread-winner, handyman, host, participant in community affairs, friend and companion to your wife, lover and sexual partner to your wife, and father. You have probably found that you are naturally better case for some of these parts than you are for others. Some men play the parts of father and handyman best, others may be best fitted for breadwinner, host and participant in community affairs, and still others may be best as friends to their wives.

L--> cut out?

The format of this section asked each subject to indicate how well he felt he played each of the roles, how well he thought his spouse had played his or her roles, and how important it was that each of them should play the several roles well. The designated roles were as follows: husband, breadwinner, handyman, host, participant in community affairs, friend and companion to your wife, lover and sexual partner to wife, father. For the wife, the roles were:

housekeeper, cook, hostess, participant in community affairs, friend and companion to husband, lover and sexual partner to husband, mother.

The members of childless couples were of course instructed to omit responses with respect to father and mother.

Part B, Section 2 -- Your Views on What Is Important . This section sought to determine each subject's attitudes and opinions concerning the importance of a wide variety of activities, behaviors, and conditions of potential importance in marital adjustment, about matters concerning which there are wide differences of opinion. Examples indicating the wide variety of items included are the following:

You attend church and take part in religious activities

Your life be free from sudden changes and unexpected events

Your home be clean and in order at all times

You have sexual intercourse every time you desire it.

For couples with children, there were eight additional items, such as:

Your children grow up to hold religious beliefs similar to yours

Your children receive a college education

Your children marry persons of at least equal social standing

Part B, Section 3 -- Your Wife (in male form), Your Husband (in female form). It will be recalled that the Personal Data Sheet II asked each subject to respond to a series of questions concerning his present and past physical and emotional health. They were also asked about the use of alcoholic beverages and whether or not it was a problem for either them or their spouse. Note that the subject was not asked to respond as he had thought the spouse had responded, but rather to indicate what in his best judgement was the correct answer to this group of questions.

In Section 3, the subjects were also asked to judge the degree of similarity of their spouse in both physical and personality characteristics to their own father, mother, sister, brother, or a former friend of the opposite sex. They were also asked to indicate the degree to which they felt that their spouse had helped or hindered them in obtaining their own goals in life and finally to list the three traits or characteristics which they most admired in their spouse and the three traits or characteristics which they wished their spouse did not have.

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Part B, Section 4 -- How You Get Along -- In this section we tried to evaluate each subject's perception of the frankness with which problems were discussed in the marriage, the extent to which each subject felt he had had to change his own early ideas, habits, and expectations with respect to ten areas in order to arrive at the way the family now does things. The areas designated were: relationship with relatives, choice of friends, recreation or social activities, earning family income, spending family income, running the household, religion, sexual relations, size of family, bringing up children. Subjects were asked to respond for themselves and for their spouses. They were also asked to indicate their perception of the degree of similarity or difference between members of the couple at the present time, and to indicate the relative degree of husband and wife influence with respect to these ten areas.

Finally, this section also included questions asking subjects to judge the degree to which each felt he understood the ideas and feelings of his spouse and how well his spouse understood his ideas and feelings. In this connection, the subject was asked to indicate how serious disagreements between him and his research partner were usually resolved, and how satisfied he was with the resolutions to such disagreements.

Part B, Section 5 -- Looking Back Over Your Marriage. This section, covering two pages of the "Report on Marriage", was designed to permit each subject to give a number of fairly global ratings to questions bearing on the overall success of the marriage and the degree of marital adjustment achieved in it. Although it was anticipated, on the basis of earlier published studies, that there would be a high intercorrelation among the responses to the items in this section, it was felt that each subject should be given an opportunity to evaluate his own marriage in a variety of ways.

The first question was, "In terms of the things which you expect from marriage, how satisfied would you say you are with your marriage?" Answers were indicated on a six point rating scale which went from "completely satisfied" to "completely dissatisfied."

A second question was: "If you had your life to live over do you think you would:

- definitely marry the same person
- most likely marry the same person
- perhaps marry the same person
- not marry the same person
- not marry at all

The third was, "Have you ever regretted your marriage?" with responses on a five point scale from "constantly" to "never."

The fourth question was, "Have you ever considered divorce or separation from your wife?" There were four alternatives: "seriously, somewhat seriously, not seriously, have never considered it."

Question five asked whether the subject had ever consulted with a professionally trained person regarding problems in his marriage.

If the answer was "yes", the subject was also asked to report the number of consultation visits and the period over which the consultation extended.

Question six was: "How would you say your present feelings of love for your wife (husband) compare with your feelings at the time of your marriage." The response alternatives included "much deeper than they were" through "about the same" to "much less deep than they were."

Questions seven and eight asked the subject to look back over his life and to provide free response answers with respect to (a) the sacrifices entailed in his marriage and (b) to indicate the things which marriage had brought that could not have been obtained without the marriage.

Question nine asked the subject to respond on a five point continuum to the question: "On the whole, do you feel that being married has helped or hindered you in obtaining the goals which are really

important in your life?"

Finally, Question ten called for an overall global rating of the happiness of marriage on a seven point scale ranging from extraordinarily happy to extremely unhappy.

The last part of Section 5 reminded the subject that many couples experience ups and downs with respect to the level of adjustment at different periods of marriage and asked them to rate (on the same seven point scale) the happiness of their own marriage during each successive two year period of its duration. Although it was recognized that such retrospective ratings for the different time periods were subject to considerable distortion, the resulting profile promised to be useful in identifying those marriages which had shown marked variation in level of adjustment at different periods of the marriage.

Part B, Section 6 -- Anything Else? The last page of the Report on Marriage contained two questions to which subjects were encouraged to provide free responses.

Question one: "Because marriages are as varied as the personalities of the people who make them, we may not have given you a chance to tell us about everything which is important to understand your particular marriage. If we have missed anything, won't you add it here?"

Question two: "This last question is one which we are certain will be asked by many of those who read the report of this research: 'Did participation in this research have any influence, either positive or negative, on the lives and marriages of the participants?' While we can never be sure of the answer to this question, it would be most interesting to report the opinion of our participants. What is yours?"

This completes the description of all data collection forms used in the investigation at time two. Naturally, both Set A and B forms were accompanied by detailed instructions and special sheets of instructions were prepared for subjects whose responses to Form 20 indicated that their engagement had been broken or that the resulting marriage had been broken by death or divorce.

LEVEL OF PARTICIPATION BY SUBJECTS IN THE TIME 2 FOLLOW-UP

Since, as has already been noted, neither member of thirteen of the original 300 engaged couples had provided even the minimal follow-up information requested in Form 20, we could not anticipate full participation by our subjects in the far more time demanding phases of time two data collection. Although Set A forms were sent to all surviving subjects (even if they had not returned Forms 19 and 20), there was no anticipation that we should achieve a level of return approaching 100%. However, we did hope to obtain Time 2 data for a sufficiently large proportion of the still married group to permit meaningful analyses of the resulting data, and to allow justifiable conclusions based on these analyses.

Our degree of success in securing the continued cooperation of the original 300 engaged couples is summarized in Table 2-1.

Insert Table 2-1 about here

As will be seen, complete Time 2 data were returned by both the husband and wife for 166 of the 227 still married couples (i.e., all forms of both Sets A and B). Some follow up data was obtained for

Table 2-1

AMOUNT OF TIME TWO DATA OBTAINED FOR THE ORIGINAL 300 ENGAGED
COUPLES GROUPED ACCORDING TO THE OUTCOME OF MARRIAGE

Category	Combinations Of Dates	Still Married	Broken Engagement	Death Of One Spouse	Divorced	Total Group
1	Complete, i.e., A & B, Male & Female	166 (73.2%)	8 (36.3%)	*	14 (35.9%)	188 x 2 (62.7%)
2	A & B, M only F			*	1	1 x 1
3	A & B for M neither F	7	6	3	4	20 x 1
4	A only M neither A & B F	2				2
5	^{only for M} B, M neither F	1			1	1 x 1
6	A only M A and B, F	1		*	1	2 x 1
7	A only M, A only F	1		*		1
8	^{A or B} , Neither M A & B, F	19	5	6	9	39 x 1
9	^{A or B} , Neither M A only F	1		1		2
10	None, i.e., Neither A & B for M or F	29 (12.3%)	3 (13.6%)	2	9 (23.1%)	44 (18.0%)
TOTALS		227	22	12	39	300

*These categories not possible for the couples whose marriage was broken by death.

376
27
39
42

an additional 32 of the still married couples, but neither Sets A or B of the forms were returned by 29 couples in this group. As was anticipated, the level of cooperation for the subjects whose engagements were broken or whose marriage ended in divorce was considerably less. Even so, some follow-up data (M or F) was obtained for 19 of the ~~22~~ broken engagements, and 30 of the 39 divorced couples. It was encouraging to note that 9 of the 12 surviving members of the marriages broken by death provided complete Time 2 follow-up data

While we should perhaps be grateful that such a relatively large proportion of our original groups of subjects were willing to devote so much time in cooperating in the final phases of data collection for this project, one cannot but wonder about the reasons for the failure to do so on the part of 88 of the original 600 persons who initially promised to continue their cooperation throughout the follow-up period. In a few cases such individuals wrote a note on the reverse side of Form 19. A certain number of the reasons given for not continuing participation seemed reasonable; e.g., "I am quite ill and simply do not have the energy to fill out all of the forms", or "I have tried to forget all of the unpleasant aspects of my relationships with my research partner and do not want to be made to think about them again." In most instances, however, the reason given was even more difficult to interpret: "am not interested",

"do not wish to participate further," or "I have concluded that studies such as yours do more harm than good."

[If time permits I hope to undertake an analysis of the personality characteristics as assessed at Time 1 of the non-cooperating as compared with the cooperating groups of subjects. However, this analysis has not been made, and if we get time to do it, we will add another section to this chapter]

CHAPTER IIIASSORTATIVE MATING

The broad question of "How do men and women pair off in marriage?" is both intriguing and complex. It is ^{of} interest not only to scholars in many different disciplines, but to all persons who observe the process of pairing off among their friends and acquaintances. Such observations by laymen have resulted in two diametrically opposed conclusions: "opposites attract" and "likes attract". Which is true?

The complexity of this apparently simple question is highlighted by the fact that both principles may be operative! This apparently paradoxical statement may be readily understood by reminding ourselves that most marriages involve the pairing off of persons of opposite sex, but of the same race. This simple illustration emphasizes the fact that these apparently contradictory principles are the result of failure to phrase the question properly. Instead of asking simply, "How do men and women pair off in marriage?" it is obvious that the question must be asked with respect to each possible characteristic by which human beings differ. The problem is further complicated by the fact that for any particular characteristic, neither of the above "principles" exhausts the list of logical possibilities. Empirical evidence may reveal that certain traits are totally inoperative in mate

complete homogamy for the variable of race. Equally important is the fact that the degree of either homogamy or heterogamy may assume intermediate values, indicating a strong, moderate, or weak tendency for a characteristic to be operative in assortative mating.

Because of the large number of different characteristics which may be operative in assortative mating, it would be desirable to have a universally applicable index or measure to indicate the degree to which assortative mating occurs. This would permit a direct comparison of the degree or strength of assortative mating, characteristic by characteristic. Unfortunately, however, human characteristics (or variables) fall into two very different categories with respect to the manner in which they are assessed. Many characteristics are assessed and described by creating a set of mutually exclusive classes or categories into which persons may be sorted; the individual is then described as belonging to one of this set of categories. For the biological characteristic of sex, two categories suffice--male and female. In the case of eye color, several alternative categories are needed; this is also the case with respect to such characteristics as race, ethnic stock, religious preference, and many other sociological variables. Such variables, assessed by assigning a person to one of various alternate categories, are called "categorical", "nominal", "qualitative" or even "countables." Obviously, anything approximating rigorous assessment of a given

characteristic requires that the set of categories used be mutually exclusive, clearly defined, and exhaustive. Since it is often impossible to create a set of categories which meet all of these desiderata, it is often necessary to utilize an additional category designated "other", "mixed", or "miscellaneous".

The second broad class of human variables is alternatively labelled "continuous", "metric", or "measurable". Such a variable is conceptualized as a continuous dimension, any point on which may correspond to the location of an individual on that dimension. Then, utilizing an appropriate measuring device, an individual is assessed by determining that point on the dimension which is most descriptive of him. This measurement process involves the assumption of the underlying continuum, the acceptance of an agreed upon instrument or technique of measurement, and the assignment of numbers to the individual to indicate his appropriate location on the continuum. Thus, it is commonly accepted practice to conceptualize the height of humans as a continuum, to utilize a ruler or other generally accepted instrument for measuring length, and to report an individual's height in inches or centimeters. Other often used continuous variables for assessing human beings are weight, age, intelligence, and shoe size!

Quantitative assessments differ from qualitative assessments of human beings in several important ways. The former permit assessing each individual as properly falling on any of a very large number of

points on a continuum, whereas qualitative assessment merely permits labeling an individual as being a member of a particular group of similar individuals. Second, the conceptualized continuum of a quantitative variable permits the ordering of persons on the continuum; that is, A is taller than B, C weighs less than D, etc. In the case of qualitative variables it is difficult to argue (except on the basis of prejudice!) that men are more or less than women, that one religious preference is more or less or better or worse than another, or that democrats are more or less than republicans! In short, quantitative data may be ordered, while qualitative data may only be differentiated. These very real differences in the nature of the variables and their mode of assessment permit the utilization of certain types of statistical analyses with quantitative data, which are not possible with qualitative data, and vice versa. For example, one can readily compute the average height of any group of individuals, but he must describe the religious preferences of the group by noting the proportions of the total group falling into each religious category.

Any variable that has been assessed on a quantitative continuum may be transformed into a qualitative or categorical variable, either for purposes of discussion, or, in order to permit using a statistical technique applicable only to qualitative data. For example, an investigator knowing the individual heights of all members of a group

may, for practical reasons, elect to categorize individuals as "tall" or "short", depending upon whether the individual falls above or below the average or median height of the group. Alternatively, he might create three categories--tall, average, and short. To do so, however, results in losing some of the precision embodied in the original measurement of height; consequently, it is not ordinarily done in precise work. Note well that, by contrast, it is not possible to transform qualitative assessments into quantitative ones.

The above brief digression into the theory of measurement and the use of statistics was necessary to emphasize the methodological problems involved in attempting to derive a universal index for expressing the degree to which homogamy or heterogamy is operative in assortative mating with respect to any given characteristic. In the case of qualitative variables, the simplest approach might seem to be to say that such and such a percent of males marry females classified in the same category. A bit of thought, however, indicates that this is not a very satisfactory solution to the problem even for qualitative variables. This is so because the quoted percentage would be a function not only of the number of categories used but also of the relative proportions of individuals of each sex falling in each of the categories.

In the case of quantitative variables, the most commonly used index or measure of the amount and direction of assortative mating is the Pearson Product-Moment Coefficient of Correlation, generally simply called "correlation", or "r". This involves, conceptually, at least, plotting each male-female pair as a single entry on a bivariate distribution, with the value for the male being scaled on either the X or Y axis, and the corresponding value for his partner on the other axis. In the case of complete homogamy or heterogamy, all such entries would fall in a straight line; i.e., the tallest man would be paired with the tallest woman, the next tallest man with the next tallest woman, and so on until the shortest man would be paired with the shortest man would be paired with the shortest woman. In this example of complete homogamy, the resulting coefficient of correlation would be +1.00.

In the case of complete heterogamy, all such entries would also fall on a straight line, but the slope of the line would be such as to result in a coefficient of correlation of -1.00. For traits showing complete random mating, the entries for husband and wife pairs would be randomly distributed in a roughly circular or random pattern on the bi-variate diagram, and the resulting correlation or "r" would be 0.00. To repeat, complete homogamy is shown by an r of +1.00, complete heterogamy by an r of -1.00, and a complete lack of relationship by a correlation of zero.

Unfortunately the Pearsonian "r" cannot be used with qualitative data. However, it is most desirable to have an index roughly comparable to the "r" to allow us to express the extent of degree of assortative mating for qualitative or categorical variables. Fortunately, the "Coefficient of Contingency", designated "C", has been found to be relatively satisfactory for this purpose. Its value ranges from 0.00, indicative of completely random mating to a possible value of 1.00, indicative of complete heterogamy or homogamy. Computationally, the sign of "C" is always positive, but inspection of the bivariate table permits the investigator to note whether a non-chance distribution is more characterized by "like with like" (homogamy) or "like with unlike" (heterogamy). The investigator can then add the appropriate positive or negative sign for homogamy and heterogamy, respectively.

Unfortunately, the Coefficient of Contingency is not directly comparable to "r" in magnitude, and has two further limitations:

(1) the maximum value of the coefficient is a function of the number of categories used, and (2) the value of the coefficient may be unduly inflated by the use of categories for which the absolute number of pairings is very small. In spite of these limitations, however, "C" is sufficiently comparable to "r" to permit using it in estimating the relative degree of assortative mating with qualitative variables.

The reader wishing more detail on these measures may consult McNemar (1969).

BACKGROUND

As a subject of scientific inquiry, assortative mating has been seen as a promising field by both biological and social scientists. The interests of biologists, specifically geneticists, grow out of the fact that, for any characteristic even partially subject to genetic control, homogamy of marriage partners tends to increase the variability of any given characteristic or trait in their offspring. And to the degree that differential fertility is associated with particular combinations of the characteristic involved in homogamous mating, there is a resulting change in the distribution of the gene pool which has direct implications for anyone interested in human eugenics.

Only ten years after Darwin published The Origin of Species (1859), Galton published his Hereditary Genius (1869), a biographical study clearly documenting the fact that eminent men tend to have eminent offspring, whether the characteristics associated with eminence are transmitted biologically or socially. (It's interesting to note that Galton was Darwin's cousin, which in itself makes the point nicely.) Later Galton became fascinated with the extent to which both physical and psychological traits are inherited and he developed a number of instruments and techniques for making both anthropometric and psychometric measures. By 1900, there was already a considerable amount of evidence to justify the conclusion that, for physical

characteristics, at least, assortative mating did occur in man, and that homogamy rather than heterogamy was operative.

Social scientists, especially anthropologists and sociologists, on the other hand tended to be concerned with the degree to which social and cultural variables operate in assortative mating. Sociological research has tended to stress such variables as race, ethnic origin, residential propinquity, socioeconomic status and level of education. Here again, assortative mating has been demonstrated to occur for practically all of the major variables studied and again the rule has been homogamy rather than heterogamy. For certain of the variables the degree of homogamy has been found to be much higher than is true for any physical characteristic.

Based on an extensive study carried out in New Haven, Connecticut in 1949, Hollingshead reports that "the racial mores place the strongest, most explicit, and most precise limit on an individual as to whom he may or may not marry". Although interracial marriages were legal in Connecticut at the time the study was conducted, not one of the 1,077 couples studied involved an interracial marriage. Homogamy is also dominant with respect to religion. Hollingshead found that 91% of these marriages involved partners from the same religious group. When grouped by religious affiliation, 97.1% of Jews married Jews, 93.8% of Catholics married Catholics, while 74.4% of Protestants married Protestants. The resulting Coefficient of Contingency was

How much
propinquity?
ACM?

0.77, a value typical of that found by other investigators. Hollingshead also found a marked tendency for marriage partners to have similar ethnic backgrounds, hardly surprising in view of the known relationship between religion and ethnic origin, but still distinctly operative within religious groups. He also found a marked tendency for marriage partners to have both of them come from the same class of residential background (six classes, graded from best to worst type of residential area.) The Coefficient of Contingency for this index of class status was also .77. Finally, and not surprisingly, husbands and wives tended to pair off homogamously with respect to age ($C = .80$).

Many early studies relying on ratings rather than psychometric measures, reported low, but positive correlations between spouses, pointing toward the probable operation of the rule of homogamy with respect to psychological variables. In 1929, Jones reported an average husband-wife correlation of +.55 for intelligence, measured with standardized tests. In a classic paper, Burgess and Wallin, in 1943, reported contingency coefficients for 51 different traits for 1,000 engaged couples. For all but six of the 51 social characteristics studied, the extent of observed over expected resemblance between members of the couples was highly significant. The signs of their coefficients of contingency was positive for all fifty-one traits.

Given the fact that assortative mating does occur, and the further fact that homogamy rather than heterogamy seems to be the rule, there has been much speculation and some theorizing as to the possible bases for mate selection. In western societies, where arranged marriages are extremely rare, each individual is presumably free to select his marriage partner from among a very large number of unmarried persons of the opposite sex. However, the potential field of eligibles is greatly reduced by purely geographical considerations. Practically everyone chooses his marriage partner from among a very much smaller circle of opposite sex acquaintances, and an even smaller group of acquaintances with whom he has developed a friendship. And, since for most individuals, this circle of friends is composed of others living in the same neighborhood, going to the same school, and attending the same church, etc., it is not surprising that there is so much homogamy with respect to these characteristics. One investigator (Clark, 1952) found that more than half the adults married in Columbus, Ohio during the period of his study, had lived within sixteen blocks of one another at the time of their first date. Kephart (1961) found that the chances are about 50-50 that the "one and only" lived within walking distance. Because propinquity (living in the same neighborhood) is correlated with many other social and cultural factors -- income, educational aspirations, occupational and vocational choice, even voting behavior -- it is clear

that the resulting homogamy of such socio-cultural variables results in maintaining and perpetuating the values and beliefs of members of each community. The degree to which social homogamy is critical to the continuity of family and other social institutions is dramatically illustrated by the following quotation from Eckland ():

If present day mate selection operated under truly random conditions, considering their proportion in the population, Negroes would be more likely to marry whites than other Negroes, Catholics more often than not would marry Protestants, and a college graduate would be more apt to marry a high school drop out than marry another college graduate. In a like manner, about as often as not, dull would marry bright, old would marry young, democrats would marry republicans, teatotalers would marry drinkers. What would be the end result of this kind of heterogamy? A new melting pot or chaos?

Although geographical propinquity and the socio-cultural variables associated with subcommunities are clearly operative in the choice of a marriage partner, all of them together still do not explain why an individual male chooses a particular female (or vice versa). Even if one's choice may be limited to opposite sex friends of the same religion, social class, education level, and so on, the field of choice is still sufficiently large to necessitate further selection. Except for the probably rare event of "falling in love at first sight", most marriage partners are friends before they are lovers. Obviously, therefore, the variables and processes that are operative in the development of a friendship are usually involved in the choice of one's

mate. Available evidence strongly suggests that most friendship pairs are relatively homogamous with respect to at least one and usually more commonly shared interests, activities, attitudes, values, etc. The sometimes slow, often imperceptible, but at other times sudden and overwhelming shift of a relationship from that of friends to lovers is but little understood. Since science has not provided an adequate explanation, this fascinating topic remains largely in the domain of the novelist and playwright. We know that the process has biological roots and hormonal overtones, but no one can predict with much certainty which friendships will lead to love, and which will not. We can predict, however, that when a pair of opposite sex friends "fall in love", it is highly probable that they will seriously consider marriage.

Several individualistic, as contrasted with socio-cultural theories, have been proposed to explain mate selection. Some early scholars, like many romantic young people, believed that there is a particular man for each particular woman (and vice versa) -- individuals perfectly created for each other. A modern variant of this was put forth by Carl Jung, who suggested that falling in love involves being caught by one's "anima." According to this theory, every man inherits (the mechanism not being specified) an anima which is an "archetypal form" of a particular female image which he carries with him in his genes. Presumably, then, when the

right woman comes along, one who corresponds to the archetype, the man is instantly "seized" by his hereditary predispositions. Since no empirical evidence has been found to support Jung's theory, it remains an interesting speculation.

Another psychological, more precisely, psychoanalytic, theory argues that men and women choose their mate on the basis of their opposite sex parent image. Thus, a man is presumed to seek a woman like his mother and a woman a man like her father. Although it is easy to find individual couples for which such selection has apparently been operative, there is no systematic evidence to support the parent image theory.

As has already been noted, there is widespread popular belief in the opposing principles of "like attracts like", and "opposites attract". Although neither of these widely held popular positions have been developed into a systematic theory, it is of interest to note that in 1958 Winch proposed a formal "principle of complementary needs". In this theory, Winch argues that each individual tends to seek a mate who will provide him with maximum need gratification and hence result in a pattern of complementary needs in marriage partners. According to this principle, dominant women would choose submissive men rather than dominant or aggressive ones. Several investigations have been made to substantiate the principle of complementary needs, but thus far the evidence has been inconclusive at best.

Finally, mention should be made of what has been called the "exchange theory of mate selection." According to this Marxian view, marriage is an exchange involving both the assets and the liabilities which each partner brings to the relationship. To the extent that the assets and liabilities of marriage partners tend to result in a partnership of potential value to both of them, the exchange theory might be thought of as a special case of heterogamy. Thus, for example, a physically attractive woman may marry a man from a higher social and educational level. Or, a woman who ranks low in physical attractiveness might marry well if she comes from a sufficiently wealthy family. Such examples, however, are probably more the exception than the rule. To the extent that they do occur, they probably tend to result in marriages that are less homogamous than they otherwise would be.

Before leaving this discussion of the theories of assortative mating, it is well to remind ourselves that whatever variables and processes are involved in mate selection, they are for the most part not clearly recognized by the immediate participants. Just as most individuals are unaware of the origins of their preferences, attitudes, beliefs, and values, it is probable that most individuals are not aware of the basis on which they choose their mates. This relatively mysterious process is further complicated by the fact that many of the variables on which mate selection is empirically based are

so highly inter-correlated that it becomes extremely difficult to differentiate the direct bases of homogamy as contrasted with the indirect bases of choice. This problem was already recognized by Pearson in 1900 when he wrote:

....for two very distinct characters, stature and eye-color, we have found quite sensible measures of homogamy. We can not doubt in the face of this that like actually tends to mate with like in the case of man. Whether this arises directly from choice in stature or eye-color, or from choice of correlated organs it is not possible to determine. If from the latter source, then, it is very probable that the measure of homogamy would be somewhat larger, if we could hit upon the directly selected organ.

SUBJECT CHARACTERISTICS

Before examining our findings with respect to assortative mating, it seems desirable to characterize the total group of subjects. It comprised 300 couples, the members of which definitely anticipated marriage, even though many couples were not formally engaged. Almost all lived in Connecticut or a bordering state. These couples were a select group, having accepted an invitation to participate in a long-time research on marriage which involved contributing several hours of their time before marriage, and the promise to report annually on their marriage. It is, therefore, not surprising that the group turned out to be superior to the general population in education and general ability. The IQ equivalent of the mean score on the Otis S-A Test of Mental Ability was 115 for the men, and 112 for the women. Three-fourths of the men and two-thirds of the women had some college education, and one-fifth of the men had at least some graduate or professional training. At the time of the first data collection, the age of the subjects ranged from 18-51; however, nine-tenths of them were between 21 and 30. On the average, the men were about 19 months older than their research partners. Only 82% of the men and 89% of the women reported being members of any church; of these 81% were Protestant, 11% Catholic, and 8% Jewish.

RESULTS--ASSORTATIVE MATING

With this background, let us now examine the process of assortative mating operative in our 300 couples. For the most part, the findings for this group tend to confirm the generally accepted rule that if assortative mating (i.e., non-random) occurs, homogamy is the rule. Our findings, however, differ from those of previous studies in two ways: (1) data are presented with respect to a larger number and for a greater variety of variables, and (2) for most variables, separate analyses are reported for three categories of couples. These are made up by grouping according to the eventual outcome of the engagement: the 22 couples who broke their engagement, the 39 couples who married and later divorced, and the 222 couples who married and remained married for 18-20 years. For certain variables the degree to which assortative mating occurs appears to differ depending on the outcome of the engagement. To the extent to which this is true, we can perhaps discern potential bases for later compatibility or incompatibility already at the time the couples selected each other as potential marriage partners. Separate analyses were also made for the 12 couples who married, but whose marriage was dissolved by the death of one of the research partners. However, because of the small N, the findings have such low reliability (i.e., they could have arisen by chance), that these data are not reported. For certain of the

qualitative variables, these 12 couples were included in the "still married" group.

Of the 300 original couples, 22 or 7.3% did not marry their research partners. Of the 278 resulting marriages, 39 (14.0%) were later broken by divorce and 12 (4.3%) were broken by death. Some 18 years later (Time 2), 222 (81.7%) of the 278 couples who married were still married (SM).

While it is pleasant to be able to report that 4 out of 5 of the actual marriages were "successful" (in the sense of surviving over 18 years), the relatively small N's of couples in the broken engagement (BE) and divorced (DIV) groups necessarily results in the statistics and results for these groups being less reliable than is the case for the larger still married group. Any statistic, for example, an average or a correlation, if properly computed, is a correct description of the sample on which it was based. However, whenever one wishes to infer that the statistic for a sample is typical of the other samples of the same kind, it is necessary to remember that every statistic based on a sample is subject to a "sampling error." For most statistics, this error is inversely proportional to the number of cases in the sample on which it was computed. This sampling error is ordinarily designated as the standard error (S.E.) of the statistic. The standard error serves to remind the investigator, and the reader, of the fact that

the sample statistic reported is only one of many values which might have been obtained for similar samples of the same size. The magnitude of the S.E. of any statistic indicates the probable range over which the statistic might be expected to vary for many different samples. In general, 2 out of 3 times, the statistics for a comparable sample will fall within \pm 1 S. E. of the obtained values; 95 times out of 100, the statistic may be expected to fall within \pm 1.96 S.E. of the obtained value; and 99 times in 100 it may be expected to fall within \pm 2.58 S.E.

Formulae are available (see McNemar, 1969) which permit estimating the S.E. for any statistic: e.g., a mean, a mean difference, or coefficient of correlation (r), or the difference between r 's based on two samples. Before an investigator can conclude that any difference of means (or of r 's) for two samples is "significant," he must ask the question of precisely how likely is the difference to be one that could not have occurred by pure chance. This is most frequently done by computing a critical value or critical ratio (CR) as follows:

$$t = CR = \frac{\text{Difference}}{\text{SE}_{\text{diff}}}$$

(The symbol "t" is used with very small samples, "CR" with larger ones).

If this ratio is ± 1.96 or more, he may safely conclude that a difference as large as he found between his two samples could happen by chance only 5 times in 100. If the CR is ± 2.58 , he may conclude that it would have happened by chance only 1 time in 100 sample comparisons.

It is common practice to say that the obtained difference is "ns" (not statistically significant) if the CR is less than ± 1.96 .

Another common way of stating this is to say that if the CR is larger than ± 1.96 the probability level is $<.05$ (significant at the .05 level), and that if the CR is larger than ± 2.58 , the probability level is $<.01$ (significant at the .01 level). The usage of these conventions will become more obvious as we discuss the findings, below.

Before presenting the data concerning assortative mating, it may be desirable to review the sampling error of Pearson's coefficient of correlation. As will be recalled, the complete absence of any relationship between two variables yields an r of 0.00. However, it rarely happens that the computed correlation for any sample of randomly paired measures will turn out to be exactly 0.00. If one computes the value for many samples of randomly paired measures, the average correlation will be found to be 0.00, but the computed values of the r , by chance alone, will be about equally often + and -. The range of such purely chance r 's varies inversely as the square root of the number of cases in the sample:

$$SE_r (.00) = \frac{1}{\sqrt{N}}$$

Thus, for samples of 25 values paired by chance, one might expect approximately two thirds of the computed r's to vary from +.20 to -.20 (that is, $1/\sqrt{25} = .20$); about 95% of them to vary +.40 to -.40, and about once in a hundred to be as large as +.50 or -.50 by chance alone.

While the computed r based on a sample constitutes one's best guess regarding the value for the population, one must always ask if it is large enough to justify the conclusion that the r for the population is other than zero. As with differences between means, it is common practice to include one's confidence in the correlation's being non-zero by indicating whether or not it is significant at the .05 or .01 level of probability.

The fact that every r has a standard error means that one does not dare to conclude that the difference between the r 's for two samples is real without determining the probability that the difference is statistically significant. And when the N of one or both of the samples is small, the difference of the sample r 's may be rather large entirely by chance.

In the tables that follow, the significant (i.e., non-chance) r 's are indicated by the symbol " * " if they are significant at the .05 level, and by " ** " if they are significant at the .01 level.

The coefficient of contingency, "C", used to indicate the degree of association between qualitative variables is also subject to sampling variations. However, since "C" is derived from χ^2 , which is in itself a test of the significance of variation from independence, the significance of "C" is estimated from the χ^2 and the number of degrees of freedom for each table. The degrees of freedom may be calculated by multiplying one less than the number of rows in the table by one less than the number of columns (see McNemar, 1969). The significance of the computed "C's" are also indicated by a single or double asterisk.

With this statistical background, we are now prepared to examine the data regarding assortative mating for our couples.

ASSORTATIVE MATING--RESULTSRace

All subjects were Caucasian, not by design, but because no individuals of any other race accepted the invitation to participate in the research. For our sample of 300 couples, therefore, there was complete homogamy with respect to race.

Geographical Propinquity

In general, the two members of our couples tended to have lived in the same neighborhood at the time that they met: approximately one-third reported living on the same street, within a few blocks, or less than a mile apart; another third lived one to five miles apart at the time; and less than 7% lived more than 100 miles apart. There were no significant differences on this variable for those couples who later broke their engagement, those who married and later divorced, or those who married and remained married.

Ethnic Stock

All of our subjects were American born, but because many were second and third generation Americans, they were asked to report the ethnic background of their parents. Of those subjects providing this information, only about 10% responded "American." The largest single category was "English" (25%), followed by "Mixed" (21%), Jewish (16%), German (8.5%),

Irish (4.5%), Scotch (2.5%), French (2.5%), Scandinavian (2.5%), Russian-Polish (2.5%), and Other (4.5%). With this variability it was not surprising that considerable homogamy with respect to ethnic stock was found. The values of C are shown in Table 3-1.

Insert Table 3-1 about here

The values of χ^2 from which these C's were derived were significant at the .01 level for the Still Married group (SM). Because of the small N's in the Broken Engagement (BE) and Divorced (DIV) groups, χ^2 was not significant, but it is of interest that the resulting C's are of the same general magnitude.* Thus we find that there was a marked tendency for persons to select a mate with the same ethnic background. This tendency was most pronounced for those subjects who responded "Jewish." Of the 46 men who reported Jewish fathers, 43 (93.5%) were engaged to women who reported their fathers to be Jewish; one was engaged to a woman who reported her father's stock as Russian, one to a father with "mixed" ethnic stock, and one to a girl with an Irish father!

*for procedure for computing and interpreting χ^2
 $\text{Engaged } \chi^2 = 3.74 \text{ Df } 2 \text{ S. diff. } 0.05$

Table 3-1

Contingency Coefficients by Ethnic Stock of Subjects

	Group		
	Still Married (SM)	Broken Engag. (BE)	Div- orced (DIV)
Ethnic Stock of Father	.73**	.80	.77
Ethnic Stock of Mother	.73**	.81	.76

These findings suggest that the America of the 1930's was less of a "melting pot" than has been supposed -- at least it had not been boiling long enough to result in panmixia of ethnic stock for our subjects in the late 1930's.

Church Membership

Subjects were asked to indicate their church membership at Time 1; 84 of the 600 subjects (14%) chose not to reply to this question. The number (and percents) falling in the several categories are shown in Table 3-2.

Insert Table 3-2 about here

Homogamy with respect to declared church membership was about as great as that for ethnic stock; the values of C were: Still Married (SM), .73**, Broken Engagement (BE), .83, Divorced (DIV), .80.

For readers who find it difficult to interpret the coefficient of contingency, the following bi-variate distribution is presented for the 267 couples where both members responded to the question regarding church affiliation (Table 3-3).

Table 3-2
Church Membership Reported At Time 1

	<u>Number</u>	<u>Percent</u>
Congregational	132	25.6
Episcopal	82	15.9
Methodist	47	9.1
Baptist	42	8.1
Presbyterian	14	2.7
Other Protestant	51	9.9
Quaker, Unitarian		
Universalist	7	1.4
Catholic		
(Roman & Greek)	57	11.0
Jewish	47	9.1
None	36	7.1
Totals....	516	100.0

Insert Table 3-3 about here

Unfortunately, our total sample is too small to permit an analysis of homogamy with respect to church membership within each of the ethnic groups. However, as has been noted earlier, Hollingshead found homogamy for ethnic origin operative within religious groups ():

As will be noted, there is a strong tendency for both men and women to select mates of the same religious affiliation. For this sample, the ratios of the actual to the chance frequencies of the four relevant diagonal cells are as shown in Table 3-4.

Insert Table 3-4 about here

The sum of the actual frequencies for these four cells is 209 as compared with 131.9 for the sum of their chance frequencies; expressed in another fashion, for 78.3% of the couples, the man and woman were of the same faith, as compared with the percentages of 49.4 if chance alone had been operating.

Table 3-3

ASSORTATIVE MATING WITH RESPECT TO CHURCH AFFILIATIONThe Pairing of 267* Engaged Couples by Stated Religious AffiliationDenominations Grouped by the Three Major Faiths and None

Church Aff.	Women				Men	
	None	Jewish	Catholic	Protestant	Totals	Row Percent
Men	Protestant	8 (21.9)	0 (12.9)	11 (19.2)	158 (122.0)	177 66.3%
	Catholic	2 (3.1)	0 (2.0)	13 (2.7)	5 (17.2)	25 9.4%
	Jewish	4 (2.2)	14 (1.4)	0 (2.0)	0 (12.4)	18 6.7%
	None	19 (5.8)	7 (3.7)	0 (5.1)	21 (32.4)	47 17.6%
Women	Total(s) (column)	33	21	29	184	267 couples
	Percent	12.4%	7.9%	10.9%	68.9%	100.0%

*Of the 300 subject couples, 20 men and 16 women chose not to answer this question; 3 of these men and women were engaged to each other (chance = 1.1)

Note: The numbers in parentheses indicate the cell frequencies expected if pairing had been completely random with respect to religious affiliation

Table 3-4
Ratio^d
Actual over Chance Frequencies by Religion of M and I
of Diagonal Cells

Prot-Prot	1.30
Catholic-Catholic	6.66
Jewish-Jewish	10.00
None-None	3.28

Other Family Background Variables

Nearly 40% of the subjects had fathers or mothers who had immigrated to the United States. The C values for ~~this~~ ^{birthplace of parents.} are shown in Table 3-5.

Insert Table 3-5 about here

It will be noted that these C values are somewhat lower than for Ethnic stock of parents (Table 3-1), but they are ^{also} still very substantial.

Socioeconomic Status of Parental Home

A socioeconomic index was derived from each subject's answers to a series of questions about his or her parental home. The Pearsonian correlations for this variable were as follows: Still Married (SM), $r = +.52^{**}$; Broken Engagement (BE), $r = +.52$; Divorced (DIV), $r = +.60^{**}$; and All $r = +.48^{**}$. Several other variables related to the parents and parental home reflect homogamy, but the size of the correlations tended to be somewhat lower, as shown in Table 3-6.

Insert Table 3-6 about here

Table 3-5

Birthplaces of Parents

	Still Married (SM)	Broken Engagement (BE)	Divorced (DIV)
Birthplace of Father	.68**	.78	.68
Birthplace of Mother	.65**	.73	.67

Table 3-6
Other Parental Variables

Variable	SM	BE	DIV
Estimated Parental Income	+.38**	+.35	+.02
Education of Father	+.21**	+.29	+.46**
Occ.Status of Father	+.21**	+.24	+.34*
Mother's Employment	+.36*	+.53	+.62
Education of Mother	+.24**	+.39	.33*
Occ.Status of Mother	+.36**		
Estimates of Cultural and Intellectual Status of Parent's Home	+.06	-.03	.19
Age of Father	+.29**	-.31	.44
Age of Mother	+.37**	-.10	.01
No. of Siblings	+.02	-.18	+.35*

Personal Characteristics of the Subjects

Age -- The r's for age were as follows: SM, +.58**; BE, +.28; DIV, +.35*, A11, +.44. These values are somewhat lower than reported by other investigators. For example, Spuhler's () summary shows 11 of 14 reported values of +.75 or greater. And Pearson and Heron using a sample of 5,317,520 English marriages found a husband-wife age correlation of +.92. The considerably lower correlation for age in our sample is no doubt primarily due to the fact that we had a selected sample of persons (those willing to participate), with the result that the variability of our subjects on age was relatively small. In any event, we had a group which did not weight age of prospective mates as heavily as do most persons when they select a marriage partner.

Physical Characteristics --Quantative Variables

For the total group of subjects, significant positive MF correlations were found for all of eight variables. The actual values are shown in Table 3-7.

Insert Table 3-7 about here

Table 3-7
Correlation of M and F Subjects for 8 Physical Characteristics

Variables	SM	BE	DIV	All Couples	Med. Value Found in Other Studies
Height	+.29**	-.26	+.64**	+.32**	+.18
Weight	+.15*	-.07	+.53**	+.23**	+.21
Chest Expansion	+.46**	.23	+.53*	+.42**	
Pignet Index	+.26**	-.09	+.63**	+.29**	
Chest-Shoulder Index	+.51**	.53**	+.65**	+.52**	
Sex Const. # 1	+.09	+.28	+.13	+.14*	
Sex Const. # 2	+.23**	+.33	+.04	+.22**	

|
 not available

Looking first at the values for all couples, we note that our research partners were slightly more similar in height than in weight, but even more similar with respect to chest expansion, a characteristic not directly observable, but perhaps correlated with an observable characteristic; that is, degree of participation in physical activities. The highest correlation, +.52, is for the Chest-Shoulder Index (ratio of shoulder breadth to chest circumference, multiplied by 100). While this index also is not directly observable, it does reflect an observable aspect of body build, and for these couples it was much more operative in mate selection than any of the other three indices of body build. The Pignet index is presumably an estimate of Body Capacity (Height-Chest Circumference + Weight)

Both of the measures of Sexual Constitution are presumably indications of masculinity-femininity of body build; #1 is Breast Circumference--Hip Circumference; #2 is two times leg length divided by height. Like Height, Weight, and Chest Expansion, the means of these indices showed very significant sex differences, but neither of them were as important in mate selection as Height or Chest Expansion for our subjects.

Of even greater interest than the positive correlation of all variables for all couples is a comparison of the relative magnitude of the r's for the three subgroups of couples. Note that, with respect to Height,

the r for the Divorced group is +.64, while for the Broken Engagement group it is -.26, and for the Still Married group it is .29. The three differences among these values are all significant at the .05 level. Note also that the relative magnitude of the three correlations ranks them in the same order for the first five of the eight variables and that the Divorced group were more similar than the Still Married group on seven of the eight variables. It would appear that, for some reason, those couples who married but were later divorced, were, consciously or unconsciously, giving more weight to their similarity in these physical traits than those whose marriages survived. In doing so, the later divorced couples were perhaps not giving as much weight as they should have given to other characteristics in their partners, characteristics which are more functional in achieving marital compatibility. We can only surmise as to whether the lack of similarity in these physical traits led to the broken engagements. A more likely guess is that this group of couples were less firmly engaged, less deeply committed to one another. In view of their lack of similarity on these and other traits, they were perhaps more intrigued or infatuated than seriously in love.

Why a similar pattern of r 's did not occur for the last three anthropometric indices is not entirely clear. Note that for the Chest-Shoulder Index, the correlations are relatively high for all three groups.

It may be that similarity with respect to this index is related to the comfort which a man and woman experience when dancing or embracing. The fact that neither of the Sexual Constitution indices yielded a similar pattern of r's is even more interesting in view of the fact that Height, for which the degree of similarity was so markedly different for the three subgroups, is a component of each of these indices.

Physical Characteristics -- Qualitative Variables

1. Hair Color -- Do blonds tend to marry brunettes or blonds? At Time 1, the hair color of each of our subjects was recorded in one of six categories: black, brown, auburn, sandy, red, and blond. Because of the relatively-small numbers falling in some of these categories, the six were reduced to three for statistical analysis. The manner in which our couples paired off with respect to hair color is shown in Table 3-8.

Insert Table 3-8 about here

Table 3-8

Assertative Mating With Respect To Hair Color For 295 Couples

(~~The~~ chance frequency for each cell shown in parentheses)

Color	Women			Total	Men	Percent
	Blond	Auburn Red, Sandy	Black or Brown			
Black or Brown	45 (48.2)	15 (17.7)	177 (171.1)	237		80.3%
Auburn, Red, Sandy.	4 (3.5)	1 (1.3)	12 (12.3)	17		5.8%
Blond	11 (8.3)	6 (3.1)	24 (29.6)	41		13.9%
Total	60	22	213	295	Couples	100.0%
Percent	20.3%	7.5%	72.2%			

As will be noted, roughly three-fourths of both the men and the women were brunettes. There were 19 more blond women than blond men in this group; a difference of 20.3% and 13.9%. A difference this large (6.4%), for an N of 295, could arise by chance 3 or 4 times in 100. If we assume this sample difference to indicate a true difference in the population, it may represent a slight tendency for "Gentlemen to prefer blonds." Alternatively, it may also be that a few of the women with blond hair were not naturally blond (in spite of our plea that if they did use a bleach, they admit it to us for the purpose of this investigation!). In any event, the absolute size of this difference is small, and not deserving of any further comment.

Much more important is the fact that Table 3-8 shows no evidence of assortative mating on hair color. The numbers in parentheses in each cell indicate the number of couples which would be expected on the basis of purely random pairing. As will be seen, the actual cell frequencies are surprisingly close to chance expectancy for all cells. The χ^2 for this table is 5.6. For 4 degrees of freedom, a χ^2 this large is non-significant; in other words we have no basis for rejecting the null hypothesis that hair color is not operative in mate selection. Separate analysis of the three groups of couples also yielded non-significant values of χ^2 .

2. Hair. (Straight, Wavy, or Curly). There was a slight tendency for men and women to pair off with respect to this variable. χ^2 was significant at the .05 level for the SM group ($C = .19$), and not significant for the other two groups. Since wavy or curly hair is more apt to be found in some ethnic groups than others, and since we found ethnic background to be such a dominant variable in mate selection for our groups, it might have been assumed that this variable would have shown large C values than were found.

3. Eye Color. None of the χ^2 values were large enough to reject the hypothesis of random pairing with respect to eye color. Whatever the aspect of eyes that may attract a man or woman to each other, it is not color!

4. Handedness. Approximately 90% of all subjects were right-handed. Of the men, 7% were left-handed, of the women 5.2%, and 3.2% of both sexes were ambidextrous. Analysis revealed no tendency for research partners to pair off with respect to handedness; that is, all χ^2 were purely chance values.

5. Blood Groups. The proportions of both men and women in each of the major blood groups (A, B, O, and AB) were typical of those usually reported. The contingency tables yielded only chance χ^2 values. Had significant values been found, it would have been difficult to explain how men and women used this completely unobservable variable in choosing a mate. Since the χ^2 were not significant, there is no problem in interpretation.

6. Health History. Each subject was asked a series of questions regarding the presence or absence of a number of disorders in self or family. Analysis of these data did not show any evidence of assortative mating with respect to any of those disorders, which included: Heart disease, epilepsy, insanity, twitches, tuberculosis, and symptoms of breathing or lung disease.

Intelligence

Our data provide two completely independent estimates of the intellectual ability of all subjects -- as measured by the Otis S-A Test of Mental Ability, and as rated by associates. Pearson r's between the Otis IQ's and rated intelligence of our research partners are as shown in Table 3-9.

Insert Table 3-9 about here

In 1929, Jones criticized earlier studies of assortative mating for relying on unreliable ratings of intelligence, and reported a husband-wife r of +.55 for intelligence test scores. Reed and Reed (1965), analyzing data for 1866 couples, reported an r of only +.33 between the IQ's of husbands and their wives as measured when both were in the sixth grade. Cattell and Nesselroade (1967) report an r of +.31 for stably married, and +.21 for unstably married couples for intelligence as roughly assessed by the 16 PF personality inventory.

It appears, then, that marriages tend to be homogamous with respect to intellectual ability, and that the magnitude of the positive correlation obtained is a function of the reliability and validity of the measures used. Furthermore, the magnitude of the correlation is probably affected by the range of ability in the group studied. Reed and Reed report that the proportion of mentally retarded (IQ below 70) who marry other mentally retarded people is six times the expected frequency among normals. Our sample of course did not include any mentally retarded individuals. In fact, none of our subjects had an estimated IQ as low as 85, and

Table 3-9
Intelligence

	Still Married (SM)	Broken Engagement (BE)	Divorced (DIV)	All
Otis IQ	+.45**	-.07	.49**	.38**
Rated Intell	+.33**	.10	.40**	.30**

variability (standard deviation) of tested ability was considerably less than for an unselected sample. If it were possible to obtain reliable measures of ability for a truly random sample of the population, it is reasonable to suppose that the Husband-Wife correlation for intelligence would be at least +.50, and perhaps even higher.

Education. In an extremely valuable paper, Kiser (1968) presents definitive evidence for homogenous mating with respect to amount of formal education. In summarizing the evidence, Kiser states: "Despite their limitations, the data leave no doubt about the existence of assortative mating ~~by educational status among both white and nonwhite couples~~ (((((Lowell, this doesn't make sense--please fix))))))?"

The proportion of both wives and husbands choosing mates of similar educational attainment was consistently above the proportion expected under random mating at each age and educational level for both white and nonwhites.**

"On an absolute basis, assortative mating tended to be strongest at the extreme educational levels. It tended to be stronger for white

wives than for white husbands or nonwhite wives. At the college level it was stronger for nonwhite husbands than for white wives. At the elementary-under-8 level it was weaker for nonwhite husbands than for white husbands or nonwhite wives."

The educational level of our subjects at Time 1 was recorded on an eight point continuum; the resulting distribution is found in Table 3-10.

Insert Table 3-10 about here

Although the intervals on this crude scale are admittedly not equal, Pearson r's were computed for this variable to provide a rough indication of the degree of homogamy in the three groups of couples.

These were: SM, +.48**; BE, +.25; ~~All~~, ~~+.47*~~ ^{DIV, +.39} ~~coefficient of association.~~ ^{all, +.47**} ~~Div.~~

(These values are all considerably lower than the G coefficient of .68 and +.69, (G is the Goodman and Kruskal coefficient of association,) more appropriate for data expressed on a scale with unequal intervals.)

Reported by Garrison, Anderson, and Reed for large samples of Minnesota parents of babies born in 1965 and 1966.)

fn

Table 3-10
Education Level

LEVEL	No. Men	No. Women
More than four years of college.....	84	40
Four years of college.	88	67
Some college.....	50	73
High School plus, but not college.....	21	33
Some high school plus some other.....	11	9
High school only....	28	63
Some high school.....	12	23
Grade school only....	1	1
Total....	296	297

Again, it is likely that the selected nature of our sample with respect to ability and education result in lower statistical indices of assortative mating than would be found for a random sample. Even so, there was considerable homogamy of educational level both in our total group and in the subgroups. (For the very small sample of 12 couples whose marriage was broken by death, the r was +.69).

((((note: compute G for our data!!!!!!))))

yes!

PERSONALITY

As will be recalled, all subjects were assessed on a large number of personality variables. The battery of instruments included several personality inventories, a set of tailored attitude scales, and a personality rating scale used to assess the person as perceived by himself, by his partner, and by his peers. Unfortunately, then as now, there was neither a generally accepted taxonomy of personality traits, nor a recognized battery of devices for assessing them. Therefore, in planning the Time 1 assessment we used a shot-gun approach, yielding a large number of variables. We recognized that, because of the nature of some of the instruments, certain of the resulting measures might have low reliability and validity, yet it seemed preferable to obtain relatively crude estimates of some personality variables than not to assess them at all. We also recognized that there would be considerable overlap in many of the variables; that is, that there would be substantial correlation between some variables, even though they purported to measure different traits.

For these reasons, it is difficult to organize in a logical manner our findings on assortative mating with respect to personality variables. One possibility was that of constructing a massive table summarizing the correlations for the three groups of couples for all of the variables. Instead, we decided to present the findings

in a series of tables, each reporting the findings with respect to the instrument used to assess a sub-group of variables. The use of an identical format for each of the tables facilitates a comparison of similar variables assessed by different instruments or techniques.

Bell Adjustment Inventory. This self-report inventory consists of some 150 questions regarding the respondent's mode of adjusting in four domains: home, health, social, and emotional. Approximately one-fourth of the questions concern each of these four types of adjustments. The subject is asked to indicate his answer by marking "Yes, No, or ?", and his score is obtained by simply counting the number of times he gives the "poor" or "maladjustive" answer to the questions. The scores range from 0 to 30 for each of the four types of adjustment, with low scores presumably indicative of "good adjustment." The M-F correlations (correlations between research partners) are shown in Table 3-11.

Insert Table 3-11 about here

As will be seen, the significant r's are all positive, but they tend to be much smaller in magnitude than those reported for family

Table 3-11
Bell Adjustment Inventory Scores

Variable	SM	EE	DIV
Home	+.14**	-.04	+.16
Health	+.08	-.06	-.11
Social	+.20**	+.10	+.15
Emotional	+.16*	+.27	+.32*

background variables and for physical and intellectual variables.

Bernreuter Personality Inventory. This self-report inventory consists of 150 questions, also to be answered by marking "Yes, No, or ?". Bernreuter developed it in 1931 to assess four personality variables: Neuroticism, Self-Sufficiency--Dependency, Introversion-Extraversion, and Dominance-Submission. Each of these variables were conceptualized as a continuum on which all persons could be assessed on the basis of their answers. Unfortunately, it turned out that certain of these four variables correlated highly with others. Later, Flanagan, on the basis of a factor analysis, found that most of the information yielded by the inventory could be summarized by using only two scores which were but slightly correlated. He therefore developed scoring keys for these two variables, which were labeled self-confidence and sociability. The assortative mating r's for these scores are presented in Table 3-12.

Insert Table 3-12 about here

Table 3-12
Bernreuter Personality Inventory Scores

Variables	SM	BE	DIV
Neuroticism	+.12	.11	+.43**
Self-Sufficiency	.00	-.20	-.14
Introversion	+.15*	-.21	+.36*
Dominance	+.12	-.36	.26
Self-Confidence	+.09	.06	+.45**
Sociability	+.12	.04	-.06

Regardless of the names, these variables reflect but a slight tendency in the direction of homogamy for the large Still Married group of couples. Although all r's are positive, only one is statistically significant. Note, however, the very different pattern of r's for the DIV (divorced) couples, who tended to be much more similar on Neuroticism and Introversion, and the Flanagan variable which combines these into "Inferiority--Self-Confidence." One cannot but wonder if too much similarity on this variable was in part responsible for the failure of these marriages as compared with the almost random pairing of those couples whose marriage was successful.

Allport-Vernon Scale of Values. This instrument was designed to assess the relative strength of six basic values delineated by Spranger. It is an eight page form containing two parts. Part I consists of 30 statements or questions each of which has two alternative answers. Here is an example (Item 1):

The main object of scientific research should be the discovery of pure truth rather than its practical applications. (a) Yes (b) No.

The subject is asked to respond by one of four pairs of numbers: 3--0, or 0--3 if he definitely prefers (a) or (b), or

2--1 or 1--2, to indicate only a slight preference for the (a) or (b) alternative. Part II consists of fifteen similar items except that for each there are four alternative responses. The subject is asked to rank these four alternatives in order of his personal preference. Here is a sample item:

12. Should one guide one's conduct according to or develop one's chief loyalties toward --
 - (a) one's religious faith
 - (b) ideals of beauty
 - (c) one's business organization and associates
 - (d) society as a whole

On the basis of the subject's expressed preferences, he receives a score indicative of the relative strength of six values: Theoretical, Economic, Aesthetic, Social, Political, and Religious. Unlike any of the other measures discussed thus far, Allport-Vernon scores are what are known as "ipsative". An individual cannot score high on all of them, for a high score on one inevitably reduces his score on one or more of the other five. The MF r's for these six variables are as shown in Table 3-13.

Insert Table 3-13 about here

Table 3-13
Allport-Vernon Scores

Value	SM	PE	DIV
Theoretical	+.29**	+.16	+.36*
Economic	+.29**	-.09	+.37*
Aesthetic	+.38**	+.29	+.21
Social	+.30**	+.38	+.21
Political	+.29**	+.13	+.65**
Religious	+.54**	+.15	+.72**

Again we find a general pattern of homogamy; 17 of the 18 r's are positive. The r's for the BE group, none of which are significantly different from 0.00, tend to be lower than for the SM couples. In general, however, the DIV couples are more similar than the SM group. This is especially true for Political and Religious values, with r's of +.65 and +.72 for the DIV couples.

Because of the built-in interdependence of the scores for these six values, it seemed desirable to compare the profile of scores of research partners in each of the three groups. The statistical technique used was Kendall's tau. This statistic, like r, ranges from +1.00 for complete congruence of the rank orders of the six scores for the man and woman to -1.00 for completely dissimilar profiles. A tau of 0.00 is indicative of neither congruence or incongruence; it is thus the expected value for random pairing of two sets of scores. (For more information on this technique, see Siegel, 1956). A tau coefficient was computed for each couple: the distribution of these tau coefficients for the three groups of couples is presented in Table 3-14.

Insert Table 3-14 about here

Table 3-14
Frequency Distribution of Coefficients of Similarity (Tau) for
 Allport--Vernon Profiles of Couples Grouped by Outcome of Engagement

<u>Value of Tau</u>	<u>SP</u>	<u>BE</u>	<u>DIV</u>
+.80 and above	5	1	
+.60 to .79	22	3	3
+.40 to .59	32	4	2
+.20 to .39	42	3	8
.00 to .19	36	2	4
-.01 to -.20	23	2	4
-.21 to -.40	21	2	5
-.41 to -.60	14	2	3
-.61 to -.80	2	1	1
-.81 and below	9	2	3
<u>Totals</u>	<u>227</u>	<u>22</u>	<u>39</u>
<u>Median Tau</u>	<u>.08</u>	<u>+.20</u>	<u>+.17</u>

It is immediately apparent that congruence of M-F value profiles varies greatly from couple to couple within each of the three groups. The median value of tau is slightly above chance for all three groups, but there is no systematic difference among the three distributions of tau's. Because the value of tau for any one couple is determined by comparing only 15 slopes (segments of each profile), rather large positive and negative values of tau may be expected by pure chance. For profiles based on six variables only, tau's greater than $\pm .75$ are significant at the .05 level. Since only 15 of the 288 values in the above table exceed $\pm .80$, we must conclude that there is but a very slight tendency toward a greater than chance congruence in the Allport-Vernon profiles of engaged couples, regardless of the subsequent outcome of engagement.

Attitudes

Attitudes and values are closely related psychological constructs. If one values something highly, his attitude toward it is likely to be favorable. Values may fall at any point on a continuum, ranging from high to low; attitudes, on the other hand, are conceptualized as falling on a pro-con continuum, ranging from extremely favorable to extremely unfavorable. There is another important difference between these two closely related concepts: values tend to be more global (that is, to apply to a group of related objects, motivations, and practices,

all associated with a broadly defined social good or goal); by contrast, attitudes are directed toward a specific institution, practice or activity. Thus, two individuals may both value highly a social good, such as human welfare, but have markedly different attitudes regarding alternative institutions, for example, specific political parties, as agents for promoting their shared goal. Another distinction between value scales and attitude scales is that the pro-con attitude continuum has a neutral point near its middle; that is, an individual may have neither favorable or unfavorable attitude toward a thing or activity. His essentially neutral position may result from the fact that, in his judgment, the "good" and "bad" aspects of the object offset each other.... A neutral attitudinal position may also result from an individual's lack of familiarity with the object, or from his perception of the object as one which has little or no relevance to his life or to his value system.

Selected attitudes of our subjects were assessed at Time 1 with two generalized attitude scales (Remmers,). The first was designed to measure the Attitude toward Any Social Institution, the second, Attitude toward Any Home-Making Activity. Both consisted of 45 statements, ranging from extremely favorable to

extremely unfavorable. These statements had been selected from a much larger number on the basis of two criteria: (a) the consistency with which groups of judges agreed on their scale of value, and (b) their ability to cover the entire pro-con continuum.

Sample statements for Institutions were:

"The world could not exist without this institution."
"Has not yet found itself indispensable to society."
"Is the most hateful of institutions."

Sample statements for Home-Making Activities were:

"I like to do this better than anything else I can think of."
"I don't mind doing this if someone helps me"
"This is hard to do and I hate it"

Subjects were instructed to check each statement with which they agreed, in reference to three institutions: Marriage, Divorce, and Church, and five home-making activities: rearing children, housekeeping, entertaining, care of lawn, and gardening. A subject might check few, several, or many statements. His attitude score for any institution or activity was the median (that is, the middlemost) scale value of the statements which he checked. The possible range of scores

was from 11.9 (extremely favorable) to 1.0 . The actual range of scores was from 11.4 to 6.5 for Marriage, and 10.9 to 2.0 for the other institutions and activities.

With this background, we may now look in Table 3-15 to see the degree to which these attitudes were operative in the choice of a mate.

Insert Table 3-15 about here

First, we note that 23 of the 24 r's are positive, indicating homogamy of attitudes. For the Still Married (SM) group, the r's tend to be small, indicating that the ^{A NUMBER} members of these couples have only slightly more similar attitudes than had they paired off by chance. Curiously, the Divorced (DIV) group are more similar than the SM couples on 7 of the 8 attitudes. And the r's for Divorce, Church, and Gardening for the DIV group are significant at the .01 level, even though the sample is relatively small. In the case of Divorce and Church, the relatively large r's may be in part attributed to the fact that the DIV subjects had a wider range of scores. For Gardening, however, there was no difference in the variability of scores between the groups. The most surprising

Table 3-15

M-F Correlations of Attitude Scores for Three Groups of Couples

Attidue Toward	SM	DE	DIV
Marriage	+.17**	+.92**	+.22
Divorce	+.34**	+.21	+.53**
Church	+.29**	+.44*	+.51**
Rearing Children	+.35**	+.22	+.33*
Housekeening	+.15*	+.29	+.19
Entertaining	+.05	-.02	+.26
Care of Lawn	+.11	+.14	+.17
Gardening	+.20**	+.16	+.63**

of the r's is the +.92 for the small group of Broken Engagement (BE) couples. Even though these men and women did not marry each other, their attitudes toward marriage were remarkably similar. Again, this is probably due in part to the considerably greater variability in the BE group. In all, however, we must keep in mind that there was no difference in the average attitude scores among the three groups.

Opinions Regarding the Ideal Marriage.

One's opinion about anything -- another person, a habit such as smoking, or a way of behaving -- usually reflects both a value and an attitude. Each subject, at Time 1, was asked to indicate how essential, in his opinion, was each of 34 things or conditions for an ideal marriage. For each of 34 statements, he was asked to indicate his opinion on a simple 5-point scale:

- 1 Very Essential
- 2 Usually desirable
- 3 Makes little or no difference
- 4 Usually not desirable
- 5 Decidedly not desirable

As with all other variables, we computed the Male-Female correlation for each of these 34 variables to discover whether or not these relatively specific attitudes or opinions served as a basis for assortative mating. Thus, we had 34 additional opinions or attitude scores for each subject. However, as might have been expected, and as was confirmed by analyzing these scores, many of these opinions were highly inter-correlated. That is, most subjects who regarded Item 1 as "Very Essential" tended to believe certain other items were also "Very Essential" or at least "Desirable" for an ideal marriage. Therefore, the 511 correlations between all possible combinations of the 34 scores were computed separately for the men and women subjects. Each of the resulting correlation matrices was then analyzed by factor analysis. Factor analysis (see Harman, 1976) is a complex technique which is capable of identifying clusters of items which correlate highly among themselves, but which show relatively low correlations with items not in the cluster. This analysis revealed five distinct factors or clusters for men, but only four for women. However, the fourth female cluster included items which tended to fall into two separate clusters for the men.

Table 3-16 summarizes the resulting indices of assortative mating for all 34 items.

Insert Table 3-16 about here

Inspection of this table indicates a slight tendency of greater than chance M-F similarity of responses to these items in all three groups. This tendency toward homogamy is less for the Still Married (SM) than for the Broken Engagement (BE) and Divorced (DIV) groups of couples. Of greater interest, however, is the range of the r's for the three groups. To be significant at the .05 level, an r must be greater than $\pm .13$ for SM; $\pm .41$ for BE; and $\pm .31$ for DIV. For the SM group, 18 of the 34 r's are significant --all are positive. For the DIV group, 14 of the 34 r's are significant -- all are positive. For the small BE group, only 9 of the 34 r's are significant; of these, eight are positive, and one is negative. However, for this small group, three of the positive r's are above .60, and one is .82 !

We now present the findings for the individual items, first for the three factors or clusters of items which were essentially the same for men and women; this is to be found in Table 3-17.

Insert Table 3-17 about here

Table 3-16

Frequency Distributions of M-F Correlations for Thirty-Four Opinion
Items by Groups of Couples

Value of r	SM	BE	DIV
+.90 and above			
.80 to .89		1	
-.70 to -.79		1	
.60 to .69		1	1
.50 to .59			2
.40 to .49	1	7	3
.30 to .39	2	5	11
.20 to .29	7	4	2
.10 to .19	14	4	8
.00 to .09	5	6	1
-.01 to -.10	5	1	2
-.11 to -.20		1	2
-.21 to -.30		1	2
-.31 to -.40		1	
-.41 to -.50		1	
Total	34	34	34
Median	+.15	+.25	+.28

Table 3-17

Item	<u>Cluster A (M and F)</u>	<u>"Equality of the Sexes in Marriage"</u>		
		SM	BE	DIV
4	That the husband should be at least equal to his wife in intelligence	.13*	.11	.11
5	The the wife should be at least equal to her husband in intelligence	.17*	-.42*	.16
6	That husband and wife should have about the same amount of schooling	.20*	.23	.31*
7	That the husband should "wear the pants"	.12	.41*	.53**
8	That the husband and wife should be of the same religious faith	.30**	.42*	-.04

Although these 5 items are highly intercorrelated for the total samples of both men and women, the pattern of M-F r's reflects differences in the degree of similarity of opinion for the three sets of couples.

When interpreting these findings (and similar data on assortative mating), the reader is reminded that a significant positive correlation means that the paired scores for two members of each couple are more similar than if the scores had been paired by chance. The mere existence of "significance" says nothing about the magnitude of the score, or whether or not there was a difference in the mean score of men and women. It should also be remembered that the "scores" for these items had a maximum range of only from 1 to 5. In the case of many items, the actual range was even less. Since the value of r tends to decrease as the variability of scores is restricted, the relatively high correlations of +.41 and +.53 for the BE and DIV groups on Item 7 are of more than casual interest.

Table 3-18 shows the correlations for the "Sexual Morality" factor:

Insert Table 3-18 about here

Table 3-18

Item	Cluster B (M and F) Sexual Morality	SM	BE	DIV
29	That the husband should <u>not</u> have had sexual intercourse with any other woman before marriage	+.20**	+.34	+.32*
30	That after marriage the wife should be 100% faithful	+.19**	+.69**	+.12
31	That after marriage the husband should be 100% faithful	+.17 +.17**	+.78**	.00
34	That you yourself should have a mate who is sexually passionate	+.21**	+.33	+.18

For this group of items, all concerned with sexual behavior, the responses of the BE couples tend to be much more similar than for either the SM or DIV couples.

Table 3-19 shows correlations on the "Sharing & Respect" factor.

Insert Table 3-19 about here

Note that the SM couples show only chance similarity for the last three of these five items.

We now consider the r's for the items which did not fall into the three matched clusters, in Table 3-20.

Insert Table 3-20 about here

For this group of eleven items, it is of interest that the DIV couples tend to be more similar in their opinions than the SM group.

The remaining ten items (not clustered for either M or F) are presented in Table 3-21.

Table 3-19

Item	Cluster C (M and F) Sharing & Respect	SM	BE	DIV
11	That the husband and wife should engage in the same outdoor sports	+.15*	-.03	+.30
12	That the husband and wife, if congenial, should take their vacations together	+.23**	-.26	+.13
13	That the husband and wife should be equally fond of social gatherings	+.03	+.27	+.11
14	That the husband and wife should respect the others religious, political or ethical connections and not strive to change them	-.03	+.37	+.30
15	That the husband and wife should not try to make each other over in habits, manners, dress, etc.	+.04	+.40	-.04

Table 3-20

Item	Cluster D (M and F) "Law and Order"	SM	BE	DIV
16	That marriage should be postponed until income permits a comfortable living without serious skimping	.10	-.02	.30
20	That the father should take an active interest in the discipline and training of the children	-.01	+.05	-.24
21	That children should be given religious instruction	+.40**	+.82**	+.62**
22	That children should be held to a strict discipline	+.12	+.36	+.55**
23	That parents should plan for two or more children, provided health, heredity or finance permit	-.04	+.44*	+.22
24	That household affairs should be run in a neat and orderly manner	+.09	+.16	+.34*
26	That the same standard of sexual morality should apply to both husband and wife	-.04	+.14	-.16
27	That husband and wife should not have had sexual intercourse with each other before marriage	+.30**	+.33	+.34*
28	That the wife should not have had intercourse with any other man before marriage	+.15*	.02	+.37*
32	That young people be trained never to engage in "petting" or "spooning"	+.26**	.00	+.42*
33	That husband and wife should be well mated sexually	+.22**	+.44*	+.23

Insert Table 3-21 about here

For only four of these ten items (9, 17, 19, and 33) is the r for the SM couples significantly greater than chance. Note, however, that on three of these four items the DIV couples are much more similar in their opinions than are the SM couples.

Table 3-21
The Remaining Items

Item		SM	EE	DIV
1	That the husband should be some years older than the wife	.11	.44*	-.29
2	That the husband should be the social equal of his wife	.12	-.19	+.18
3	That the wife should be the social equal of her husband	.02	-.33	+.10
9	That the husband and wife should have similar intellectual interests, such as scientific, literary, musical, etc.	.25**	-.02	+.32
10	That the husband and wife should like the same types of amusements (cards, games, theatre, etc.)	-.01	+.48*	+.31
17	That the wife should have money of her own, or should earn her own living by paid employment and not be financially dependent on her husband	.19**	+.29	+.17
18	That the wife should be allowed a definite budget for household and for her personal expenditures	.06	-.19	+.43**
19	That the wife should be kept fully informed of the family finances and of her husbands business	.12**	+.06	+.43**
25	That husband and wife should frequently express their love for each other in words	.06	.23	.34*
33	That husband and wife should be well mated sexually	.22**	.44*	.42**

In convention

Conformity of Opinion Regarding Ideal Marriage. As noted earlier in this section, there tended to be marked agreement among our subjects concerning the "correct answer" to most of these opinion items. For example, to the item, "Husband should be the social equal of his wife," 282 of the 297 men and 285 of the 297 women responded either (1) Very Essential or (2) Usually Desirable. This suggested the possibility of deriving from each subject another score based on all 34 items, which we called "conformity of opinions". The method of scoring was very simple: on the basis of the frequency distributions of responses of each sex to each item, we noted the modal (most frequently occurring) response, and made two cardboard scoring stencils by punching holes in the location corresponding to the modal responses. By counting the number of responses visible through the stencil, a "conformity" score was obtained for each subject.

The possible range of these scores was, of course, from 0 to 34. The actual range was 15 to 34, but over two thirds of both sexes had conformity scores of 31, 32, 33, or 34. Because the "correct responses were based on the modal responses of each sex, there was no difference in the average scores of the men and women. More importantly, there were no significant differences in the mean conformity scores of the SM, BE, or DIV subject for either sex.

Both the M and F subjects of the DIV couples tended to be slightly less conforming and their scores showed somewhat greater variability, but the differences were not significant.

It was, therefore, all the more interesting to examine the M-F correlations of those conformity scores for the three groups of couples: SM, +.03, BE, -.14; and DIV, +.42**. There was no significant tendency for the SM or BE men and women to select each other on the basis of conformity; for some reason, however, the men and women who married and were later divorced showed a significant and very marked tendency to select their mates on this variable.

Vocational Interests

Vocational interests were assessed with Strong's Vocational Interest Blank. In order to provide directly comparable measures for the men and women, the Men's form of the blank was used for all subjects. This eight page form consists of some 400 items grouped in eight parts:

- (1) A list of 100 occupations
- (2) A list of 54 amusements
- (3) A list of 39 school subjects
- (4) A list of 52 activities
- (5) A list of 53 kinds of people

[REDACTED]

Each of these items is followed by three letters: "L, I, and D".

The subject is asked to respond by marking one of these three letters depending on whether he likes, is indifferent to, or dislikes the occupation, amusement, school subject, activity, or type of person listed.

Part VI lists ten activities, ten factors "affecting your work," ten well known men, and ten types of positions in a club or society.

From each group of ten possibilities, the subject is asked to mark the three most and the three least preferable in importance to him.

Part VII is a list of forty-two pairs of items (occupations, activities, kinds of people). The subject is asked to indicate whether he prefers one or the other of each pair, or if he likes each about equally well.

Finally, Part VIII asks the subject to rate himself by checking one of three alternatives to each of 40 phrases descriptive of Present Abilities and Characteristics.

Obviously, there are no right or wrong responses to such items. The blank may be scored with any of a large number of scoring keys. Each Vocational Interest key was derived empirically by tabulating the proportion with which each alternative response to each item was given by a successful group of men in each of the vocations, as compared with the proportion of responses for Men-in-General,

as Strong called his "normal" group. Responses more typical of the men in the vocation are scored "+"; those less typical are scored "-". In addition to sign, scoring weights are assigned in accordance with the magnitude of the difference. An individual's score on a given scale is the algebraic sum of all the weights to~~l~~ all of his responses. Since the advent of electronic scoring machines, it is feasible to score each answer sheet for many vocations or occupations. Therefore, in 1955, we rescored all of the Time 1 answer sheets for 44 different occupations. Although most of these are occupations or vocations more often filled by men --Engineer, Policeman, Banker, Mortician, etc. -- the 44 also included many others often filled by women as well -- artist, social science teacher, musician, office manager, and author-journalist. Note also that the scoring process we have just described applies to the original Strong, circa 1930; the newer Strong blanks have a somewhat simplified scoring system.

Before presenting our findings on assortative mating for vocational interest, attention is called to the fact that Strong scores do not in any sense measure the aptitude, skills, or knowledge essential for success in an occupation or vocation. A high score for any vocation, say, musician, means only that the subject has a pattern of likes and dislikes very similar to that of the typical professional musician;

a low score indicates that his interest pattern is very unlike those in the vocation. Thus, a person may receive an "A" score for "Musician" even though he does not play any musical instrument. Similarly, an excellent violinist may earn a low score if his pattern of interests is not similar to that of professional musicians.

As with the other variables, each of these 44 vocational interest scores were examined to see if they were operative in assortative mating for out three groups of couples. The resulting distributions are shown in Table 3-22.

Insert Table 3-22 about here

Even a brief inspection of Table 3-22 indicates that all couples, regardless of the outcome of their engagement, tended to pair off in a manner which resulted in greater than chance similarity of interests. Note that for the large Still Married (SM) group, all r's are positive; for the group of 22 Broken Engagement (BE) couples, 37 of the 44 r's are positive, and for the group of 39 Divorced (DIV) couples, 42 of the 44 t's are positive. None of the nine negative r's are significantly different from 0.00 .

Table 3-22
Frequency Distribution Of M-F r's By Group of Couples

r	<u>Frequency</u>		
	SM	CE	DIV
+.60 and above		1	
+.50 to .59		1	4
+.40 to .49	2	8	6
+.30 to .39	11	4	10
+.20 to .29	19	9	14
+.10 to .19	10	9	4
.00 to .09	2	5	4
-.01 to -.10		5	1
-.11 to -.20		2	
-.21 to -.30			1
Total	44	44	44
Median	+.25	+.23	+.26
# Positive r's	44	37	42
# p < .01	33	1	7
# p < .05	7	8	13
# ns	4	35	24

The median value of r is about +.25 for all three groups; in other words, the groups are not differentiated in terms of degree of general similarity of interests of the partners. Greatest similarity of interests (r 's of +.40 and over, for at least one of the three groups of couples) were found for the following occupations, as shown in Table 3-23.

Insert Table 3-23 about here

For many of the scores, the relatively high M-F r is of about the same magnitude for all three groups (Psychologist, Math-Science Teacher, Office Manager, Purchasing Agent). Other scores yield substantial M-F r 's for only two of the groups (Farmer, Voc-Agric Teacher, Advertising Man). Still other scores yield a substantial M-F r for only one of the three groups (Architect, Veterinarian, Forest Service, Musician, and President Mfg. Concern).

* A further indication of the extent to which vocational interests are functional in mate selection is that forty of the forty-four scores yielded a positive M-F correlation, significant at the .05 level for at least one of the three groups of couples.

Table 3-23

MF Similarities in Occupations

Occupation	SM	BE	DIV
Artist	+.25**	+.50*	+.24
Psychologist	+.43**	+.65**	+.50**
Architect	+.16*	+.48*	+.10
Veterinarian	+.17*	-.04	+.44**
Mathematician	+.31**	+.43*	+.22
Physicist	+.35**	+.40	+.24
Farmer	+.27**	-.06	+.52**
Math-Science Teacher	+.30**	+.41*	+.37*
Voc. Agric. Teacher	+.24**	+.10	+.44**
Forest Service	+.17*	-.04	+.40*
Musician	+.17*	-.18	+.50*
CPA	+.23**	+.26	+.44**
Office Manager	+.39**	+.47*	+.41**
Purchasing Agent	+.41**	+.38	+.39*
Banker	+.35**	+.48*	+.25
Adv. Man	+.28**	+.18	+.40*
Lawyer	+.34**	+.43*	+.28
Author-Journalist	+.29**	+.49*	+.25
Pres. Mfg. Concern	+.17*	.08	+.50**

Interestingly, the four which did not are relatively specialized professions (see Table 3-24):

Insert Table 3-24 about here

Although the correlation is positive for all three groups of couples on each of these four scores (suggesting slightly greater than chance similarity of partners), none of the coefficient is large enough to achieve statistical significance. This finding is of considerable interest because the first three of these scores are substantially correlated with "Psychologist" scores. "Psychologist" yielded relatively high coefficients of assortative mating for all three groups of couples. Further, in spite of the considerable overlap in the interest patterns of Physicians and Psychologists (Strong reports the correlation to be +.74), it is apparently the unique aspects of the interest pattern of Psychologists which are operative, consciously and unconsciously, in assortative mating. Table 3-25 presents the r's for these two professions:

Insert Table 3-25 about here

Table 3-24
Specialized Professions

	SM	BE	DIV
Physician	.12	.36	.09
Osteopath	.06	.08	.25
Dentist	.09	.36	.23
Senior CPA	.12	.25	.25

Table 3-25

	SM	BE	DIV
Physicians Interest	.12	+.36	.09
Psychologist Interest	+.43**	+.65**	.50**

The Strong answer sheet was also scored to assess three more general personality variables: Occupational Level (OL), Interest Maturity (IM) and Masculinity-Femininity (MF).

The OL key was derived on the basis of the differences in item responses by two contrasting groups of men: Unskilled and business and professional men whose incomes placed them in the upper one-seventh of the population (over \$2500 in 1938!).

The IM key was derived on the basis of differences in item responses of groups of 25 year-old men and of 15 year-old boys.

The MF key was derived on the basis of differences in item responses by groups of males and females, including approximately equal numbers tested in high school, in college, and as adults.

The Male-Female correlation for these three scores for the three groups of couples is as shown in Table 3-26.

Insert Table 3-26 about here

In view of the earlier reported MF correlation for amount of education and socioeconomic status of parental home, it is not surprising that all three groups of couples were considerably more similar than chance in Occupational Level scores. Interest Maturity was about equally operative for the SM and BE couples, but not

Table 3-26

Variable	SM	BE	DIV
Occupational Level	+.29**	+.43	+.47**
Interest Maturity	+.30**	+.37	+.04
Masculinity-Femininity	+.18**	.42	.20

for the DIV group. The r's for Masculinity-Femininity indicate some tendency toward homogamy; that is, for men with more masculine interests to pair off with a woman also more masculine than the average woman in our sample.

Rated Personality Variables

We now come to the last group of variables. As will be recalled, in order to assess aspects of personality which had not been tapped by any of the several structured personality inventories, the author devised a 36 trait rating scale. This was used to obtain three operationally independent sets of ratings for each subject: the average of the ratings by five of the S's friends and/or associates, self ratings, and ratings by research partners.

As was anticipated, many of these 36 rated variables correlated highly with one or more of the others. In fact, rated "patience" and "breadth of interests" correlated +.83; "patience" and "temper" correlated +.82; "punctuality" and "dependability" correlated +.72. On the basis of a series of factor analyses, it became obvious that most of the information provided by the 36 separate ratings could be summarized by using not more than 10, and probably fewer factor scores.

In another place, the author will report the results of a series of 10 parallel factor analyses of the personality ratings. These include 5 separate analyses for each sex as follows: ratings by

(1) associates, (2) self, and (3) research partners at time of engagement, and ratings by (4) self and (5) research partner approximately 18 years later. The identical subjects, 166 men and 166 women for whom all five sets of ratings were available, were used in these analyses.

In order to avoid excessive redundancy in reporting the role of rated personality variables in assortative mating, we have selected 10 of the 36¹ traits as marker variables of relatively independent factors. Three criteria were used in selecting these 10 traits: a high loading on one factor, relatively high reliability (inter-judge correlation), and relatively low inter-correlation with any of the other 9 marker variables. Unfortunately, it was not possible to find 10 traits which met all of these criteria fully. The ten selected traits are relatively uncorrelated in that only 4 of the 45 intercorrelations are over .30.

With this background we now examine the degree to which the male and female members of our couples were similar on traits as rated by their associates. The data are shown in Table 3-27. The traits are

Insert Table 3-27 about here

Table 3-27

MF Similarity in Personality Traits as Rated by Associates.

Couples Grouped by Outcome of Engagement

TRAIT			SM	BE	DIV
1. Energy Level					
easily fatigued	vs	energetic	.18**	.14	-.11
sluggish	vs	peppy			
2. Intelligence					
dull	vs	brilliant	.36**	.05	.42**
13. Voice Quality					
unpleasant	vs	very pleasant	-.01	+.37	-.11
annoying	vs	melodious			
20. Dress					
careless, pays little attention to clothes	vs	very neat fastidious, "chic"	+.17*	+.08	.24
23. Breadth of Interests					
very narrow, interested in few things	vs	extremely wide very curious	+.28**	+.37	.30
25. Conventionality					
unconventional	vs	Very conventional	+.29**	+.60*	.24
27. Quiet	vs	Boisterous			
very quiet talks little		noisy, must be heard	+.04	.00	+.05
29. Temper					
decidedly ill-natured, uncivil, easily angered	vs	rarely angered good natured exceptional self control	+.09	.44*	-.06
31. Vanity - Modesty					
egotistic, very vain, conceited, proud	vs	unpretending unassuming, modest	.15*	+.06	-.15
32. Dependability					
unreliable, indifferent	vs	very dependable, conscientious	+.17*	+.06	.31*
MEDIAN r			.17	.11	between -.05 & +.24

(a) Marker variables to represent 100 values of the 36 trait Personality Rating Scale.

labeled and also described by the adjectives and phrases used to designate the extreme ends of each continuum. The numbers of the traits refer to the number of the original 36 trait scale.

The overall picture indicates that in the judgment of their associates, the two members of engaged couples are somewhat more similar than if paired by chance. For the SM couples, all r's are positive and 7 of the 10 are significantly different from 0.00. Similarity is greatest for rated intelligence. For the small BE group, the r is positive for all ten traits, but is significant at the .05 level for only two. One is for "Conventionality," which also shows considerable homogamy for the SM group; the other, however, is for "Temper", which yielded low, non-significant values for both the SM and DIV couples. Perhaps this similarity of tempers contributed to at least some of the broken engagements! For the DIV couples, 4 of the r's are positive and 6 are negative. Two of the 6 positive r's are significant at the .05 level. For "Intelligence" the r was of about the same magnitude as for the SM group. For "Dependability" it is somewhat higher for the DIV couples.

In summary, trait ratings show a general tendency toward homogamous mating, but the degree of similarity tends to be less than that found for the earlier reported personality variables, assessed by proven psychometric instruments. There are two possible explanations for this state of affairs.

(1) Ratings, even though based on the pooled judgment of five associates may be less reliable (contain more error variance, to speak technically) than scores derived from an inventory. Since errors in the ratings of the male could hardly be expected to correlate with the errors in the ratings of the female, the greater the proportion of error variance in the ratings, the more the actual correlation between the "true" ratings of our couples would be attenuated.

(2) The ratings may be less valid than objective scores. That is, to the extent that different raters may interpret differently the words and phrases used to describe a trait, the more the resulting average rating tends to be a mixture of two or more traits. Each subject was asked to name associates whom they felt knew him well enough to rate him and would be willing to do so. It is probable that the associates selected as raters tended to be friends of the subject, and hence similar to the subject in background, education, interests,

and so on. It is also likely that our subjects tended to select associates of the same sex. To the extent that there is any sex difference in the meanings associated with trait names, this would have lowered the validity of the trait for the research partners.

Unfortunately, our data do not permit an estimate of the extent to which these two conditions affected the MF r's for rated variables. It is, however, worth noting the relative value of the MF correlations for the one rated variable for which an objective measure was also available (See Table 3-28):

Insert Table 3-28 about here

The estimated reliability of the average rating of "intelligence" was estimated (from the inter-judge correlations) to be .86, higher than for any other trait. The reliability of the Otis is stated to be

. In other words, the proportion of error variance was relatively small in both of these measures of intelligence. However, the correlation between rated intelligence and Otis scores was only .49, indicating that while the ratings were in part based on intelligence, they were also influenced by aspects of the subject other than intelligence as objectively assessed. The proportion of common variance

Rec
Table 3-9

Table 3-28

Variable	SM	BE	DIV	ALL
Intelligence as Rated by Associate	+ .36 + .35**	+ .05 .10	+ .12 +.40*	
Intelligence as Measured by the Otis Test		+ .45** -.07	+ .38	

of any two variables is given by the squared correlation between them. The square of the r's for the SM couples, converted to a percentage, is 10.9% for rated intelligence, and 20.25% for measured intelligence. Since the reliability of the rated intelligence was relatively high, we must conclude that the lower MF for rated intelligence is primarily due to the lower validity of the ratings. In the case of the other rated variables, all of which showed considerably less reliability (inter-judge agreement, to be precise), it is probable that the smaller coefficients of assortative mating resulted from both lower reliability and less validity than characterized most of the objective personality measures.

Self-Rated Personality Variables

We also correlated the self ratings of two members of each couple. The resulting r's are shown in Table 3-29.

Insert Table 3-29 about here

While the fact that 26 of the 30 r's are positive indicates a slight overall tendency toward homogamy of self ratings, the magnitude of the correlations is generally lower than for these same traits as

Table 3-29

MF Similarity in Self Rated Personality Traits (a)
Couples Grouped by Outcome of Engagement

Trait	SM	BE	DIV
1. Energy Level	+.06	+.08	.00
2. Intelligence	+.12	+.21	+.28
13. Voice Quality	+.05	+.16	+.23
20. Dress	+.11	-.10	+.30
23. Breadth of Interest	-.02	+.16	+.11
O/C 25. Conventionality	+.26**	+.09	-.28
27. Quiet - Boisterous	.00	+.46*	+.16
29. Temper	+.67	+.37	-.03
31. Modesty	+.16	.00	+.02
R 35. Dependability	+.17*	-.15	+.16
Median -r	+.09	+.12	+.16

(a) Numbered variables represent the 10 clusters of the 16 variables.
 Trait Dimension, *Psychol.*

rated by others. There are at least two likely reasons for these low correlations: (1) low reliability--after all, each "score" was based on a single rating, and (2) any self rating is likely to be biased, that is, less than completely objective. A modest person may rate himself too low and a vain person may rate himself too high on any variable. In any event, these "self-perceptions" of our marriage partners were but slightly more similar than for chance pairings of men and women. However, this does not mean that our subjects did not perceive themselves and their partners to be similar. This is another issue, one to which we shall address ourselves in the next section.

Similarity of Couples as Perceived by the Subjects

At Time 1, each male subject was asked to indicate one or more possible ways in which he believed he and his fiancée differed; the female subjects were asked a parallel question with respect to their fiancée. The findings are shown in Table 3-30. Since there were no significant

Insert Table 3-30 about here

differences in the responses of the three groups of couples for any of the items, this table presents the findings for all couples,

Table 3-30

Percent of Engaged Men and Women Reporting Differences
Between Self and Research Partner in Each of Nine Domains

Domain	Male	Female
Education	36.6	30.5
Intellectual Interests	21.8	18.4
Religious Beliefs	19.8	22.1
Choice of Friends	11.8	9.4
Preference for Amusements and Recreation	9.4	8.4
Attitude toward Drinking	7.7	8.4
Attitude toward Smoking	11.4	6.4
Tastes in Food	20.8	20.1
Respect for Convention	19.5	17.4
Median Number of Possible Differences	1.4	1.3

but separately for male and female subjects.

As will be seen, most of our subjects, both men and women, tended to perceive themselves and their research partner as similar in all nine domains. The median number of differences checked was only 1.4 for the males and 1.3 for the females. Only 8% of the men and 4% of the women perceived more than three differences; whereas 18% of the men and 22% of the women reported no difference in any of the nine domains.

The most frequently perceived difference was in Education; as will be noted, approximately one-third of both men and women check this as a difference. Only one-fifth of both the men and women report differences in Intellectual Interests, Religious Beliefs, Tastes in Food, and Respect for Convention. For the other four domains, the proportion perceiving a difference is less than one-tenth. None of the sex differences is statistically significant.

It is obvious that most of the couples perceived their mating as extremely homogamous. Even though they may have recognized differences between themselves and their research partners in these domains, they tended to perceive them as too minor or too unimportant to report them. While we have no way of ascertaining the accuracy of these perceived similarities and differences, it is of interest that two of the most frequently perceived differences are for the domains of Education and Religious Beliefs, for which objective data are also available. As reported on page there was a real sex difference in the level of education of our subjects: that is, 57% of the men as compared with only 36% of

the women were college graduates. And, since the assortative mating correlation for level of education was a little less than .50, it is clear that many of our couples (considerably more than one-third) did in fact differ in Education. As for Religious Belief, it will be recalled (cf pages), 78% of our couples reported belonging to the same church and 22% to different churches. This is remarkably close to the percentage of subjects reporting differences in Religious Beliefs in Table 3- , above. These checks give reason for believing that the degree of perceived similarity has a basis in reality, at least for those behaviors which are objectively observable.

Appendix B
ELK Autobiography

Autobiography of E. Lowell Kelly

To be submitted to:

A History of Psychology in Autobiography
Akron, Ohio

E. Lowell Kelly
Professor Emeritus
University of Michigan

The decision to write this autobiography was stimulated primarily by the receipt of the delightful one by Quinn McNemar (1980). Although I had known Quinn well as a fellow graduate student at Stanford in 1930-32, I learned so many fascinating things about him as a person from his autobiography that I felt I owed it to my colleagues to relate my own varied career and to indicate the bases of my several but very different publications in psychology.

I was born in Kokomo, Indiana and spent the next 18 years either in Kokomo or on a farm in Howard County of which Kokomo was the county seat. My parents were poor but honest hardworking folks. Dad was one of a large family and completed only five grades of elementary school, but in going over his papers after his death I discovered a record of his elementary school performance and found that on a percentage basis his lowest grade in any year in any subject was 96. However, because of the large family and its generally impoverished condition, he was forced to drop out of school and begin contributing to the family income. Somehow, he managed to become an extremely skilled mechanic and by the time I was born was working in one of the two automobile plants then located in Kokomo.

Mother, on the other hand, was the older of only two sisters, her parents having died when both were young. They were reared by grandparents and an aunt.

She graduated from the local high school in 1899, played piano in the local church, and taught piano lessons for some outside income. Her high school grades were only average. Because my father had what at the time was a reasonably well paying job in the infant automobile industry, we lived comfortably and I began my elementary education in the Kokomo school system. Because I had apparently done so well in the first grade, I was given a semester promotion on entering the second grade. About that time my father's health failed precipitously, and he was advised that the only hope for his survival was to get out in the open air. His health problem had arisen because at that time the automobile exhaust of the cars being manufactured was not known to be hazardous and hence engines were run without benefit of proper exhaust of fumes in the factory where he had worked. Faced with this ultimatum in 1912, my father decided to try to find work on a farm even though he had absolutely no farming experience. Because of a lack of any capital he could not hope to begin sharecropping, and so accepted a job as a farmhand. His wages were small, \$30 a month, but fortunately his employer provided the family with a four room house (outdoor plumbing), and, more importantly, a milk cow and two hogs per year for butchering. Even so, the family survived, and within a couple of years, he had accumulated enough cash to become a tenant farmer, owning his own team of horses and a few rehabilitated farming implements, not to mention a cow!

When I was four years old, my first sibling arrived, a brother, Lester. About five years later, my only sister, Betty, was born and some six years later another brother, Charles, arrived. (I have often wondered, but never asked, my parents what method of birth control they used to space us so widely. My hunch is that it was the douche!)

In retrospect, it appears that we four children were ranked in academic aptitude by the order of our birth. As the first born, I found school work both appealing and remarkably easy. My next brother, Lester, made good grades, and completed his B.S. at Purdue with a B average. My sister, Betty, made only average grades in high school, and had no inclination to attend college. She did, however, have my Mother's aptitude for the piano and still plays it very well. My youngest sibling, Charles, had considerable difficulty even with high school subjects but did graduate and later became a very successful salesman and sales manager. It is not surprising that, as a psychologist, I have always been greatly interested in understanding individual differences! On his graduation from Purdue, my brother Lester joined the staff of the then young IRS in

Indianapolis, but a few years later had to resign because of a tragic illness -- Buerger's disease, a circulatory disorder. It is our best guess that this was instigated by the fact that he had experienced severe frostbite while helping a stranded motorist out of the ditch on a bitterly cold day. Severe cramping and swelling of the leg necessitated his giving up his position with the IRS. For several years he did tax accounting work at home, but as the disease progressed, involving gangrene and three successive amputations of the leg, he had to give up his work entirely. Fortunately, he had a most understanding and helpful wife who cared for him until his eventual death shortly before reaching 50. He had two able and well-adjusted children. My other two siblings are now retired and all have well-adjusted children and grandchildren.

With our move to the country, I again changed schools, riding a school bus to West Middleton, Indiana, elementary school. Because that system made no distinction between 2a and 2b, I was automatically placed in the third grade and this resulted in my graduating from high school at the age of 17. I found school work extremely easy, so much so that I had much time on my hands. As a result of strict parenting, I did not choose to get into mischief, but discovered the reference books at the school library, and began devouring them assiduously. In fact, I believe I read every entry in the encyclopedia then available in the school's very limited library.

In an effort to improve his opportunities for earning a better living my father arranged to do sharecropping with a succession of two or three different farm owners, always resulting in a move of family residence. We always remained, however, in the West Middleton school district. Although I had begun to work regularly on the farm chores after school, I found myself very much alone both intellectually and socially. Because of a lack of transportation and distance, I could not join the nearest Boy Scout troop, but about that time discovered an organization called the Lone Scouts, joined it, and began receiving their monthly newsletter. In one of these, I found a story about amateur radio which excited me greatly. Learning of my interest, a great uncle who was a conductor on the old Cloverleaf railroad running between Toledo and St. Louis, gave me a telegraph key and sounder which he had procured somehow from the railroad. I immediately began learning Morse code and enough radio theory to pass the FCC examination for an amateur license and station before World War I. Hostilities, however, closed all amateur radio activity, so I did not get my first amateur license until after the cessation of hostilities in 1918. Shortly thereafter I constructed a

rudimentary amateur radio station building my own transmitter and receiver. The transmitter consisted of a Ford spark coil, but as there was no electric power available in our home, I had to power it with dry cell batteries. Since I could not afford to buy new ones, I would regularly visit the automobile service stations of the day picking up discarded dry cell batteries (practically all automobiles of the period used dry cell batteries in their ignition systems, and they had to be replaced regularly). I found that by baking these in my mother's oven, I could resuscitate them enough to operate my amateur radio station satisfactorily. I mention this because, as will be seen later, amateur radio played many significant roles in my later life.

I still did well in school, but increasingly became aware of the limited talent of my teachers. I vividly remember having to help my fifth grade teacher solve arithmetic problems which he had attempted to do on the board. (In the evenings and weekends he served as the town barber.) During this period my parents attended Sunday School and the local Methodist Church regularly. At about the age of 12, I was "converted" and baptized. The next two years I was very active in the Epworth league, the young people's group of the Methodist Church. Like elementary school, high school courses were easy for me, so much so that when our general science teacher became ill with tuberculosis, I taught his general science course in my senior year (my first teaching experience!). During this period, I gave serious consideration to becoming a minister, primarily because one of the seminaries was offering to provide a scholarship to those who would promise to serve as a minister for a certain number of years after completing their A. B. degree. About this time, however, I was greatly disillusioned by the behavior of a new pastor who had arrived with a lovely wife to serve our local church in West Middleton. He was fired after a few months in the community, allegedly because he could not resist the wiles of the young ladies in his congregation!

Throughout my high school years I had been active in sports, although excelling in none. Because the school was small (only six in the senior class!) I played center on our basketball team, even though only 5'10". I also participated in track and field events, but never won any awards.

Although I had only tentatively decided on a career choice (electrical engineering) my decision to go to Purdue necessitated that I earn money to help pay costs, even though tuition was very low at the time. Graduating from high school in 1923, I obtained a job in the local steel mill, but worked there only one day. My job was

that of picking up heavy rolls of just-drawn, still-hot wire and loading them on a small trailer driven by another employee who not once during the ten hour day offered to help me in the slightest! Fortunately for me, an uncle in charge of maintenance at another local factory phoned that evening to say that he had found a job for me there. Officially, I was assigned to the shipping room, but functionally served as a "flunky" to the production manager. This factory produced cutting tools for the automotive industry, usually ordered in batches of several thousands to the specifications of the purchaser. The factory in which I worked not only smelted the materials for these tools, but then cast them to approximate size, after which they went through a process of rough grinding and then fine grinding. My assignment as flunky to the production manager consisted of trying to find out where an order had been held up or delayed and report the bottleneck back to him. Not surprisingly, not once during the entire summer did I successfully identify one of these bottlenecks. This was before the days of employee unions, and I found both employees and supervisors too frightened ever to admit that the problem fell within their department. Thus disillusioned by life in the industrial world, I decided that I would become a school teacher, especially since I learned that they earned \$5.00 a day!

In the fall of 1923, thanks to my savings from my summer employment, I enrolled in Purdue University. I had enough money to pay tuition and buy textbooks, but still had to worry about living expenses for the year. To economize on room rent, I rented a room in East Lafayette nearly two miles from the campus in West Lafayette, and, throughout that first year, my evening meal consisted of a loaf of bread and a quart of milk -- not very exciting, but nourishing. I also obtained two part-time jobs, the first at a commercially operated cafeteria in the campus area where I worked daily from 5 A.M. to 7:30 A.M. There I worked in the kitchen, frequently preparing several hams for baking, starting a five gallon pot of navy beans to boil, etc. I also did some short order cooking and when otherwise not occupied, washed dishes! For this, I was paid the magnificent sum of 25 cents an hour, but had to purchase my own breakfast. Fortunately, our cafeteria prices were so low I usually netted about 25 cents on the morning's work. Shortly thereafter, I also obtained a job in a lunch room in a local department store which served only lunches. There I acted as a soda jerk and a busboy. My pay consisted of a free lunch and thanks to a very kindly cook, my plate was usually heaped full of good food. Unfortunately, this department store was located in Lafayette, and I would frequently have to hurry from the job there back to afternoon classes at

the University.

At Purdue, I found all of my courses challenging and generally made superior grades in them. This pattern of life continued during my freshman and sophomore years. As a junior, I joined a new fraternity, but because it was composed largely of financially strapped persons like myself, it was in effect an early "co-op house". This provided a welcome change in opportunity for social interaction. Although my mother had tried valiantly to teach me to play the piano, I turned out to be a very poor student of that instrument. However, having learned to read the scales, I did learn to play both the saxaphone and the trumpet (both poorly) and enjoyed participating in the after dinner sing-a-long sessions at the fraternity.

Although I continued to work at my restaurant jobs, my financial condition was much improved as a result of returning home to help Dad with the harvest each summer. This was because he was good enough to allow me to use our wagon and our team of horses to haul gravel for the County Road Commission. Although the gravel bed would hold only one cubic yard, all of which had to be loaded by hand shovel, I could frequently haul four loads per day for each of which I received \$5.00. While dumping one of the loads of gravel on a road near our farm, I first met Naomi, the girl who was to become my first wife. While visiting her relatives on a nearby farm, she frequently borrowed one of the family's horses and went for a short horseback ride. It was on one of these rides that she came across me dumping a load of gravel and stopped to say, "Hi." I found her to be an engaging conversationalist, and learned that she was attending Indiana University Nursing School in Indianapolis. Before the conversation was over, I promised that I would come to Indianapolis to visit her some time and thus began a series of weekend journeys from Lafayette to Indianapolis.

Two very significant events occurred in the first semester of my junior year (1925-6). My fraternity brother, Nathan W. Shock, son of a mathematics professor at Purdue and I were both taking the same course in educational psychology with Herman H. Remmers, an extremely able and stimulating teacher, whom I regard as responsible for my eventual decision to become a psychologist. In the course of one of his lectures he described the Spearman-Brown formula, designed to predict the reliability of a test n times as long as that on which the reliability had been computed. Then almost as an afterthought, he scratched his head and said, "I wonder if that would also hold for the reliability of ratings with an increasing

number of raters?" Both Shock and I were very much intrigued with this remark, and soon hit upon a scheme for trying it out empirically. We would ask the 34 members of our fraternity, each of whom knew each other reasonably well, to rate each other on the Purdue Personality Rating Scale, then compute the intercorrelations of each rater against each other, the average of two raters against two others, three against three, etc., as far as we could go, eventually ending up with a single correlation of the mean of 17 raters against that of 17 others. The rating forms were all, of course, appropriately coded so that no member of the fraternity would ever learn how he had been rated by a fellow member. Because this work was done before the days of even a modern calculator, much less the computer, our computation of these several matrices of correlations proceeded slowly, but eventually was completed. All of the median values mentioned above turned out to fall fairly close to the points predicted by the Spearman-Brown formula for a test increased n times in length! Both Shock and I received two semester hours of credit for this project which was accepted as a Bachelor's thesis, and in 1927 it became our first publication with Professor Remmers as a joint author. (Bibliography #1).

The other significant event of that semester occurred shortly before Christmas. My roommate, an English major with a particular bent for literature and poetry, after poring over the projected class schedule for the second semester, suddenly exclaimed, "I do believe that if I attend summer school this summer (1926) I can graduate at the end of the summer." He had already attended one previous summer session and thus was ahead of me in terms of number of credits already earned. However, I found myself thinking how delightful it would be if I too could graduate at the end of the following summer, thus avoiding the worries of finding tuition and living expenses for my senior year. At that time, Purdue required 147 semester hour credits for graduation, so that this possibility did not look promising. Because I could earn but ten semester hours in the coming summer session, I found that I would have to complete 26 semester hours in the second semester in order to graduate in August. My first task was that of perusing the second semester schedule to discover if I could possibly schedule 26 credit hours in courses appropriate for my majors (mathematics and science). Fortunately, I located ten appropriate courses totalling 26 semester hours, which could be scheduled in the spring semester.

Because of my previous academic record, I did not

have the slightest doubt about my ability to handle such a heavy course load, but next came the problem of getting permission from the Dean to enroll for such a heavy load. I shall never forget my conference with the Dean on this issue: Fortunately, I knew him from a course in biology as a kindly, humane individual and outlined my proposed plan in some detail and hopefully effectively. Obviously, the Dean was somewhat taken aback at my proposal. His initial response was "Kelly, you're proposing to commit academic suicide." My retort was equally to the point, "Dean, it is my life and I'm willing to take the chance. Besides, I think I can make it." At that point, and with considerable reluctance, the Dean graciously wrote a short note and signed it stating that I had his permission to take the program of courses proposed! While I was obviously a busy lad during this semester, I completed the ten courses, all with an A, and thus was able to graduate at the end of the following summer session (August, 1926).

During one of my visits to see Naomi in Indianapolis that summer, I found her in a very fatigued and somewhat depressed condition. At first, I thought this was only because of the stressful combination of courses and nursing duties in her three-year training program. To our chagrin, we soon learned that she had contracted tuberculosis, at which point she was advised that she must drop out of nursing school and that her only hope of survival would be to move to a dry climate in the Southwest. Although neither she nor I had dated anyone else during our college years, we had never discussed marriage, and certainly had not set any date for such an event. However, because her father had died some time before and no one else in her family would be able to arrange for this change of climate, we decided to marry on the same day that I completed my B.S. at Purdue. And although I had no job prospects I felt reasonably sure that I could earn a living if we went to New Mexico.

Shortly thereafter we set out in a Model T Ford touring car, accompanied by her mother, who had agreed to go along to assist in nursing her back to health and doing the housework. The trip was long, arduous, and dusty, often punctuated two or three times a day by a blowout, which required patching an innertube and often a "boot" in the tire. Eventually however we arrived in Albuquerque and I stopped at the teacher's agency to inquire about the possibility of a teaching position for the following year. Since it was already late in August there was only one job available: a small school in the tiny village of Taiban, New Mexico, located on the arid eastern plains of that state, needed someone to teach Spanish and coach athletics. In no respect was it the position I would have wished for but under the

circumstances I had no other choice and agreed to accept a one year teaching position in Taiban. After all, I had had two years of Spanish at Purdue and with my modest high school athletic experience, felt I could handle the coaching job satisfactorily at least.

The next nine months in Taiban proved to be both a series of most unexpected and varied educational experiences. We found the community to be severely depressed economically because most of the cattle had frozen to death a few years before, with the result that the local bank was closed as well as all but one store and one filling station. The school was a six teacher affair, with three elementary school teachers in grades one through eight, and three high school teachers, of which I was the third. The other two consisted of an older couple who had recently completed their bachelor's degree at a state normal school at the age of about 45 to 50. The husband was designated Superintendent of the system, and had decided that he would teach all of the math courses and government. His wife was assigned to teach all of the English and history courses. And although I was given the technical title of Principal, I soon found that I would have to teach all of the other courses offered! These included general science, bookkeeping and typing, and some other essentially non-academic courses. The townspeople were kind, and to my amazement, I found myself being addressed as "Perfessor Kelly." We rented a small but pleasantly furnished house only a block east of the school. It had a windmill and a very large storage tank so we never had to worry about a supply of water. Plumbing, of course, was out-of-doors.

On the weekend preceding the opening of school, there was a small local rodeo to which I had been invited. As soon as I had arrived on the scene, I was asked, "Perfessor Kelly, would you like to ride a western pony?" Although I had qualms about doing so, I felt I could not afford to reject the challenge. After all, I had ridden farm horses occasionally while on the farm, but I did not know that the western ponies "steered" very differently from the farm horses of the midwest. Fortunately, I managed to stay on the horse and even learned something about how to steer it and stop it.

The following day I began my first day of teaching in the local school system. I soon learned that between a third and a half of the students were Mexican, and I could not understand their Spanish nor they mine. And, of course, I found them totally uninterested in learning the niceties of Spanish grammar or the Castilian pronunciation which I had learned at Purdue. A couple of weeks into the semester, the Superintendent

asked why I did not have the football squad out for practice, since we had a schedule of games coming up that fall! Since I had not even seen a football game at Purdue, much less ever played the games, I countered by saying that I thought we needed some equipment. The superintendent assured me we had plenty of money for such, and I looked through the sporting goods catalogue to make out an order. But the most important thing I ordered was a book on how to coach football! Since there were only 22 boys in the high school, it was difficult to arrange for scrimmage games with two full teams, but somehow or other we managed to complete the season by winning half of our games, all against larger schools.

About that time I began to realize the many other roles that I would be expected to play in the community during the year. While teaching the course in general science one morning, there was a sharp pounding on my door and somebody yelling, "Professor Kelly, Professor Kelly, come quick!" I opened the door and was asked whether I could do artificial respiration. I said that I could and the caller said, "Come quick, we need you!" Taiban was located on the Santa Fe railroad, and was a coaling and watering station for the engines on that line. On arriving at the station, I found a Mexican worker sprawled out on the ground and a small crowd all urging that I proceed with artificial respiration quickly. It seems that this unfortunate laborer had been hit by a crowbar while prying loose the side door of a coal car, and the crowbar had broken his neck. Although he had no pulse when I arrived, the crowd insisted that I carry out fifteen minutes of artificial respiration, at which time I said there was no hope and that the man was dead. At that point, I was approached by the proprietor of the only store in town still open, who asked whether I had a typewriter. I said, "Yes, why?" He replied, "We must make out a coroner's report." Thus, I found myself typing out a coroner's report on the individual's death, and mailing it to the appropriate authorities with my signature as acting coroner.

Shortly thereafter, I was aroused one Saturday morning with a call to come and help save the life of a girl who was choking to death. She lived with her family about a mile and a half from town and the wind was blowing so hard that to get there I had to drive my Model T Ford in low gear all the way! I found that she had swallowed a glass bottle stopper which had stuck in her throat, and she had suffocated before I arrived. These were but the beginnings of my unexpected career as a paramedic to the community throughout the year. The only physician within 25 miles lived in Fort Sumner, and on the occasions when we called him we

found him to be either under the influence of alcohol or dope and thus of relatively little assistance. Fortunately, because of her nurse's training, Naomi was extremely helpful in meeting such emergencies. Some time during the winter, the daughter of one of the elementary school teachers became quite ill, running a high fever and wheezing. The above-mentioned physician was called. He announced she had diphtheria, but she was so far gone that there was no hope of saving her life; but he insisted that each of those who had come in contact with her be given a shot of antitoxin. As he was leaving, my wife asked if he would leave some additional vials of antitoxin, so that we might try to save the patient. I shall never forget the following day and night: my wife administered the antitoxin intravenously, and after boiling the handle of a fly swatter, I wrapped a rag around it and used it as a swab to clear out her throat each time she started choking to death. (It was a rough night, but she survived!)

Two other events of that year stand out vividly. An older gentleman of the community had died in the middle of the night without benefit of medical attention, and the next morning I was approached by one of the local residents to explain that the community took care of its own under such circumstances: one of us would have to go to the nearby town to purchase a casket, two or three others would dig a grave in the local cemetery, and then somebody would lay him out and prepare him for the funeral. Knowing that the cemetery was located on a rocky, small hill, I elected to accept the latter task, and thus had my first experience as an undertaker. Embalming, of course, was out of the question, so he was buried the following day after a short funeral service.

Shortly after arriving in the community, I found that the local telephone line was completely nonfunctional. It was an old fashioned hand-cranked magneto type of ringer with a single line operating against ground. I decided that I would rehabilitate it, and succeeded in restoring communication between the store, the school, my own home, and that of a few other local residents. Shortly after my arrival, I was also declared superintendent of the local Sunday School, and taught a Sunday School class regularly. Fortunately, I never was asked to preach a single Sermon!

By midyear, Naomi's health had improved to the extent that we were able to permit her mother to return to her home in Indiana. Her mother was a very rigid individual; I do not believe I ever saw her smile during the entire period she had lived with us. Although we now had to do our housework and cooking, we

were pleased to be on our own.

I felt much more comfortable in coaching the basketball team, which, in spite of the small size of the school, managed to turn in a good record, sufficiently good to go to the state tournament held in Clovis some fifty miles east. There we won our games on Friday without difficulty against much larger schools, and also the morning game on Saturday. I pleaded with the squad not to overeat at lunch time, but they could not resist the lures of hamburgers and lush malted milks, and so were in rather groggy shape. Thus, we lost out in the semi-finals that afternoon.

During that year, for reasons not entirely clear to me, I succumbed to the lures of an advertisement to take a correspondence course in law, which assured me that I could obtain a license to practice law. Fortunately, I paid for it on an installment plan, and after the first half-dozen lessons found the memorization of ridiculously large amounts of law so boring that I cancelled the course. In addition to my teaching and coaching jobs, I found that I was expected to direct the local high school senior play, and managed to bring that to a performance which seemed to please the local populace. I was, however, quite unprepared to have the superintendent ask why I did not start a school orchestra. Somehow or other he had learned that I had a saxophone and cornet and so assumed that I was a musician. We managed to find a collection of a few students, all with more musical talent than I, and put on one or two performances, which again seemed to please the local residents.

One other event of that year stands out very vividly. Although I had little contact with the elementary school classes, I had noted that the first grade included a sixteen year old boy who had been retained in the first grade because he had never learned to read or write. Every year or so, the janitor would install a larger desk in the right rear of the first grade classroom, and there he sat all day long. One Saturday morning about midyear, I was awakened by a knock on the door, and found this so-called village dumb-bell standing at the door. Very politely, he asked, "Would the Perfessor care to ride my traps with me?" I looked over his shoulder and noticed two saddled horses waiting. While I had little interest in the proposed venture, I felt that I dare not reject this opportunity for a further learning experience. I quickly dressed, had a light breakfast, and we started riding across the plains to the south of the village. I inquired as to what he was trapping, and he said, "Coyotes, of course. I can make more money trapping them than anything else."

After riding several miles, we approached his first trap, and even at a distance it was obvious that a coyote was in the trap. Since the lad had a rifle hanging on the stock of his saddle, I assumed that he would shoot the animal, and we could proceed without any problem. Such was not to be the case. Immediately upon arriving at the trapped animal, clawing and snarling viciously, he deftly lassoed the animal by the neck and asked me to hold the rope so that he might proceed with further preparations for returning the animal alive to the village. He first slipped a wire noose around the coyote's mouth and wired it shut so that it could no longer bite, although it still snarled viciously. Similarly, he deftly lassoed first the front and then the hind feet, tying each of them up carefully with twine. At this point he calmly put the still snarling animal over the horn of his saddle, and we rode on the to next trap. You may be sure that I was doing some serious thinking as to what would happen if we found a coyote at the next trap. Of course I felt obligated to offer to take the next one over the horn of my saddle, but at the same time was still very uncertain about how my western pony would behave with a snarling coyote so close to its head. To make a long story short, during the next two hours we similarly tied up six coyotes and started back to the village, three on his pony and three on mine. During the ride he told me that he always took the coyotes home alive so that he could skin them while their bodies were still warm, and thus got a better price for the skins. As we arrived at my home, I asked if I could help with the skinning of the animals, but he thanked me profusely for my help and assured me that he could handle the skinning by himself without any problem. During the course of that ride back to the village, I discovered that he received a ten dollar bounty for each coyote trapped, and another ten to fifteen dollars for the skin. A quick calculation revealed to me that in this one day's trapping effort he had earned more than I earned in an entire month of teaching!

In thinking back over this year at Taiban, I cannot but be impressed with the ability of a very impoverished group of people to survive under extremely difficult circumstances. Furthermore, there was no juvenile delinquency, and no particular disciplinary problems at the school, and while practically everyone owned a rifle or shotgun for hunting purposes, I never saw a revolver during my entire year there. Furthermore, there seems to have been no criminal activity, at least none reported during the year, and I don't believe we ever locked our home during the entire year.

Shortly before the end of the school year and graduation ceremonies, I had received a letter from Professor Remmers of Purdue stating that he felt I was too able to spend my life in high school teaching and hoped that I did not mind that he had applied for and obtained a fellowship for me at the University of Iowa to pursue my doctorate in psychology there. While I was pleased at this opportunity to do graduate work with the aid of a fellowship, we were still sufficiently in need of cash that I applied for and obtained a six week teaching position at the New Mexico Normal School in Las Vegas (summer session). On completion of this brief instructorship, we headed back east in the old Model T Ford, arranging our route so that we might stop off at Iowa City and confer with those with whom I would be doing graduate work there. I remember only an interview with Professor Carl Seashore. While he assured me that they would be happy to have me do graduate work at Iowa, this brief visit to Iowa City convinced me that it was not the place I should do my graduate work. First of all, I was certain that I did not want to specialize in the psychology of music, and was unfamiliar with the other members of the staff. In any event, at that point I made a firm decision to reject the fellowship at Iowa and drove on east, first to Purdue to thank Professor Remmers for his efforts on my behalf and to apologize for not accepting the opportunity of doing graduate work at Iowa. We then went on east and visited with my family for a couple of weeks. Here I was again without a job and no definite plans. Although Naomi's health had improved so markedly in the high and dry climate, I felt however that we should go back west and so headed for Colorado thinking that we would stop first at Colorado Teachers College in Greeley and then go to Boulder to see what might be available.

At Greeley I had an excellent conversation with J. D. Heilmann who became my mentor for my master's work in psychology, and, more importantly, I received a research assistantship in the Division of Education Research, Frederick L. Whitney, Director. We obtained a small furnished apartment near the campus and so began my first graduate year. My work as a research assistant resulted in three unimportant publications (Bibliography #2, 3, and 4). I found Heilmann to be a superb teacher and a very stimulating mentor. My master's thesis was largely a replication of a study done the previous year by Professor Heilmann and published in the 27th Yearbook of the National Society for the Study of Education. It consisted of an effort to ascertain the relative influence of age, school attendance, socioeconomic status of the home, and mental age on school achievement and grade placement in

a population of 157 Greeley, Colorado ten year olds. Since my results were almost entirely congruent with those reported by Heilmann in the study of Denver children, I never considered publishing it. However, in the analyses of these data I used both the techniques of multiple and partial correlation and also a technique known as path coefficients and coefficients of determination proposed earlier by Sewall Wright. Thanks to the availability of a handcranked calculator, this rather elaborate series of statistical analyses were completed in time for me to receive my master's degree in June of 1928. Although at the time I was not sophisticated with respect to mathematical statistics, I had discovered in the course of completing this master's thesis that the two techniques, presumably different, gave identical results, to the fourth decimal place. I then prepared a paper on the relationship between the two techniques (Bibliography #5).

Naomi's health remained good throughout this year in Greeley and she was an invaluable unpaid research assistant and typed both my term papers and my master's thesis.

In the spring of that year, Dr. Heilmann told me that he had written to Terman at Stanford, praising my promise as a graduate student in psychology and called my attention to the fact that Professor Terman would soon be appearing at a state teacher's convention in Cheyenne. I hitchhiked to Cheyenne, had a delightful conference with Professor Terman in his hotel room, and even though I could not tell him my Stanford-Binet IQ (none had been administered in any of the school systems which I had attended), he offered me a research assistantship if I would come to Stanford for graduate study!

Shortly thereafter, we packed our few belongings in our trusty old Ford touring car and headed for Palo Alto. After obtaining an apartment in that town, I had my first conference with Professor Terman and learned the nature of my duties as research assistant. It was at that conference that I also first met Quinn McNemar. During the course of that conference, Professor Terman explained that he and Dr. Catherine Miles were pleased with their success in developing an empirically scored masculinity-femininity inventory with the resulting distribution of the scores of ten year old boys and girls being almost non-overlapping. He said at that point that it was their desire to obtain data on the most masculine group of men and the most feminine group of men which could be located. He suggested that the most masculine group of men would be lumbermen, and the most feminine group, passive male homosexuals. At that

point, turning to McNemar and me, he said, "One of you is going to San Francisco, locate, test, and interview 100 passive male homosexuals, and the other will remain here and carry out the statistical analysis of our findings." Before I could respond, Quinn McNemar said loudly, "Kelly is going to San Francisco." At that time, homosexuality to me was a word only. During the next couple of weeks I avidly sought to learn all that I could from the meager existing literature on the subject and made my first trip to San Francisco beginning what I suppose was the first of my "clinical activities." Because I had learned that homosexuality was regarded as a deviant condition in the society of those days, I first approached the Chief of Police of San Francisco explaining the nature of the mission to which I had been assigned, and asked him for advice as to how to begin locating appropriate subjects. His advice was that of visiting the county jail where he said they always had a few "fairies" (his term) at any given time, and ask the county jailer to direct me to the most intelligent of those who might soon be released. Thus it was that I arranged with an admitted homosexual to serve as my guide and "research assistant" in locating subjects, interviewing and testing them. In my conversation with the police chief, I had also urged him to cease, for a while at least, the periodic raids and arrests of homosexuals. For my protection, I also obtained a letter from him stating that I was engaged in professional activities in case I should be rounded up along with some of my subjects!

My "research assistant" turned out to be an able lad, but unfortunately given to using dope so frequently that he was sometimes not able to keep the appointments that he had made with me. However, he proved to be not only a useful guide to the streets of San Francisco, but also an excellent teacher in helping me to spot prospective subjects, merely by observing their dress and gait as they walked down the street. With his help, we began arranging for regular meetings with small groups of potential subjects. In addition to a brief background interview, we administered the Terman-Miles M-F test and a medical student (related to Dr. Catherine Miles) also carried out certain physical measurements on the group. This process of data collection continued for about six or eight months when all at once our supply of potential subjects vanished. I read in the San Francisco papers about the round-up of a large number of such individuals and of course they assumed that I had tipped off the police to their whereabouts. Consequently, we had to stop with only 67 of the 100 subjects we had hoped to obtain. In retrospect, I realize that these subjects were in no sense a random sample of what today are known as "gays." Our sample was that of lower class, relatively

uneducated individuals, most of whom earned their meager livelihood as male prostitutes.

My course work at Stanford proceeded smoothly. As a formal teacher, Terman was a dismal failure. Typically, those enrolled in one of his courses would meet for one or two sessions, he would suggest a number of interesting topics to explore and ask us to prepare a term paper on the subject which we had selected. These he always read and returned with extensive editorial comments. The psychology staff at that time was small and the graduate courses spotty. I had little interest in learning about rat psychology from Calvin Stone, and while I found Walter Miles excellent at stimulating research ideas, he was greatly lacking as a formal teacher. My most valuable course experience consisted of those in the history and theories of psychology with Paul R. Farnsworth.

But, by all odds, the most valuable learning experience at Stanford in those days was the weekly seminar held in Professor Terman's home, regularly attended not only by the eight or nine graduate students but also by the entire staff of the department. Topics covered were wide-ranging -- all the way from topical issues to reviews of recently published books. Because of the small number of graduate students we appeared seriatim on the program, the topic always having been suggested and assigned by Terman. And naturally we made every effort to put on a good showing not only for him and our fellow graduate students, but also for all other members of the faculty present. I do not recall the topic of my first report at this seminar, but I distinctly recall the event which followed it. As I was preparing to leave with the others, Terman quietly asked me to remain behind, and when all others had departed we moved over and stood in front of the fireplace. He quietly spoke as follows: "Lowell, that was a pretty good seminar presentation, especially for a first one, but where in the name of God did you learn to speak the English language!" I responded, "In the same state where you were born, Professor Terman, Indiana." Without hesitation he responded, "Well, if you ever hope to obtain a university teaching position, you are going to have to learn to speak good English. Your accent, your intonation and your grammar are horrible."

Shocked by this unexpected admonition, I determined to seek to improve my use of the English language. I asked my fellow graduate students to correct errors of grammar which they noted in my speech and in the absence of any suitable recording devices at the time, I attempted to improve my pronunciation and enunciation by speaking to myself in the mirror while

Observing the positions of my lips. A few weeks later I made my second presentation at Terman's seminar. Again he quietly asked me to remain until after the others had left and this time again standing in front of the fireplace said, "Lowell, I detected a marked improvement in your speech, but you still have a hell of a long way to go! Keep it up."

My next serious hurdle was that of passing the reading examinations in French and German, in neither of which I had any formal training. However, I figured that with two years of high school Latin and two years of college Spanish, French should be the easier of the two to master first, and relatively early I presented myself for the reading examination. Unfortunately, I failed it flatly. The lead sentence of the first paragraph of the passage which I had been asked to translate contained one of the famous French double negatives, to which I had not been introduced. Failing to note it, I translated the remainder of the passage in accord with the incorrectly translated first sentence. Sometime thereafter, I was in Terman's office consulting with respect to my assignment as his research assistant when he suddenly asked me, "Lowell, what's this business about your not being able to read French?" I said that I thought I was fairly competent in translating French, and explained the cause of my earlier failure. At that point, he reached up on the shelf, chose one of the Binet volumes, opened it at random, and handed it to me and said, "translate aloud." I did so, and after reading a couple of pages in translation, Terman turned to me and said, "Lowell, you just passed your French reading examination and I will report it to the French department." Thus, I was able to concentrate on learning to translate written German, and while I found it difficult going, I successfully passed the reading examination in that language as well.

Some time late in that year, Terman's seminar was devoted to the review of three recently published books dealing with the general problem of marital compatibility. All had independently arrived at the same general conclusion: "marital compatibility is almost entirely a function of sexual compatibility." Although these conclusions had been reached by a competent psychiatrist, a physician-gynecologist, and a well-known social worker, I somehow or other could not bring myself to accept the validity of this conclusion. Later, in a conversation with Professor Terman, I told him that while my experience and opportunity for observation had been limited, it was my firm opinion that while sexual incompatibility was inevitably present in cases of marital incompatibility, I felt strongly that the basic problem had to do with basic

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personality incompatibility of husband and wife²¹⁹ and not their level of sophistication in matters of sex. It was at that point that I proposed to Professor Terman that I do my doctoral dissertation on the subject: i.e., assessing a large number of engaged couples before marriage on a wide variety of personality variables and then following them up to determine the adequacy of their marital adjustment as related to the variables assessed, individually for the man and woman, and jointly for the couple.

His response to my admittedly unrealistic proposal for a thesis problem was typical: he quietly stated, "Lowell, that is a very different testable hypothesis. However, to test it appropriately would require extensive research funds and involve you in a dissertation extending over many years. I recommend that you find a neat experimental psychology dissertation problem, complete your degree, and I promise I will assist you in securing appropriate research funds to carry out this proposal on personality and compatibility in marriage."

At that point, I consulted Professor Walter Miles, who immediately suggested a neat and promising project: the relationship between apparent movement (phi phenomenon) and eye movements. Miles had already designed and prepared equipment for recording eye movements (a pair of movie cameras) and I began constructing suitable apparatus for producing phi phenomena, i.e., two light going on and off with an appropriate time period between them (circa 300 to 20 milliseconds). Work on this thesis proceeded smoothly and within a couple of months I had already obtained very satisfactory records of eye movements and simultaneous recordings of the lights involved in the apparent movement. In the process of constructing this apparatus, to provide for the alternate switching on and off of the two light bulbs, I utilized a simple synchronous motor to switch the lights alternately. In the course of developing this equipment, I found it somewhat difficult to start the synchronous motor, and eventually devised an aid, a neon lamp. This resulted in 1929 in my publication of a description of this equipment (Bibliography #6).

Analysis of the data resulting from this experiment clearly revealed absolutely no relationship between eye movements as recorded on the movie film and the alternation of the light stimuli producing apparent movement. Although the results were negative, there was sufficient interest in the problem so that I believed it would be acceptable as a doctoral dissertation and I wrote it up accordingly. Just before submitting it to Miles and the committee for review, I decided to make a

ARCHIVAL REVIVAL ~~last~~ minute check of the more recent publications in the Stanford library, only to discover that someone else had decided to test the same hypothesis and only a couple of months before had published his results showing no relationship. Although I think I might have persuaded the committee to accept my dissertation even though it was a duplication of an already published research, I decided that I would rather do an original piece of research and therefore started looking for another suitable dissertation topic. During this period, however, I decided to work with Walter Miles in producing a simple demonstration unit for apparent movement which would clearly demonstrate the lack of relationship between eye movement and the phi phenomenon. It consisted of a sheet of black cardboard, with seven flashlight bulbs, six located at the corners of a hexagon and one in the center. The use of a simple switching arrangement permitted alternating the stimulus lights horizontally, vertically, or two pairs at a time in opposite directions. Also, one could switch the lights so that the center light would alternate with all six of those on the corners of the hexagon. This produced a rather dramatic spreading out of the light in six directions and its return to a central one. This apparatus was eventually described in a publication with Miles (Bibliography #9).

While searching for another potential doctoral thesis problem, I utilized the excellent shop in the department to construct an amateur radio transmitter and receiver. I renewed my amateur license and two over the air contacts with it before coming up with a thesis problem.

Somewhere in my courses at Stanford, I first learned about synesthesia, and was particularly fascinated by the concept of colored hearing. I began reviewing the literature bearing directly and indirectly on the subject of synesthesia, especially colored hearing, and found it to be very extensive but consisting almost exclusively of heresay evidence, portrayals of heresay evidence, and somewhat anecdotal and descriptive articles. Many theories had been proposed to account for the phenomenon, ranging from physiological bases all the way to association from conditioning, but no one seemed to have attempted experimentally to create the phenomenon or in the case of those already experiencing it, to change it by experimental intervention. I therefore decided to attempt to produce chromesthesia (colored hearing) by the technique of a conditioned response.

Thanks to a well equipped shop and considerable guidance from Walter Miles, I drew up a fairly elaborate set-up for this experimental investigation.

It was located in a dark room 11 by 15 feet. Presentation of the musical tones involved the use of a modified keyboard accordion and other devices to make it self-playing. Compressed air was provided by a vacuum cleaner located in an adjacent room to avoid the distraction of the noise. The simultaneous presentation of each tone and its paired color was accomplished by means of a revolving cylinder operating electrical contacts so that each time one of the eight keys of the accordion was depressed, the contact was closed. This action in turn operated a relay which in turn lighted the paired projection lantern located in the back of the room, thus producing a colored square on a light screen and directly in front of the tone unit.

Speed of presentation of the pairs of stimuli was controlled by an electric phonograph motor and the apparatus was made even more automatic by installing a counter to record the number of repetitions of the stimuli and also provide for the stopping of the apparatus at the end of anything from 1 to 30 revolutions of the cylinder. This relieved the experimenter of the responsibility of counting the numbers of stimuli reported and stopping the apparatus at the proper time. Most of the experiment consisted of presenting an entire chromatic scale with the simultaneous presentation of colors arranged in the same spectral order. However, throwing a few switches permitted the simultaneous presentation of a single tone and color pair for a specified number of times. Thirteen subjects participated in the part of the experiment involving multiple tones and colors and five with single tone color pairs. The total number of stimulations experienced by the subjects and the multiple pairings ranged from 380 to 3000, with a mean of 1283.

To determine the amount of conditioning (if any) and associative learning, subjects were "tested" using a mimeographed sheet containing 40 numbered blank spaces. Each subject was then presented with a series of forty tones chosen at random from the scale and after each tone was sounded, the subject was instructed to indicate: (1) whether he had seen the associated color, (2) whether he knew what the color was, and, if not, (3) to guess the color. The results of the experiment can be briefly summarized. When the tones were played in a dark room chromesthesia simply did not occur. On the other hand, learning to indicate the paired color did occur. After only 360 paired stimulations the subjects averaged 22.5 correct responses out of 40, a mean which increased to 31.7 after 2,000 presentations.

In order to determine whether or not any latent or subliminal conditioning had occurred, two simple experiments were conducted. Some previous writers had suggested that fatigue facilitated any tendency toward chromaesthesia, but the writer, who had sat through six sessions of 1,000 consecutive repetitions with the eight pairs of stimuli, and was obviously still in a state of fatigue, still noticed no tendency for the tone to elicit a color response. An earlier writer, Fere, had suggested colored hearing was due in part to a particular state of the nervous system, not only to fatigue, but also drugs and an unusual experience such as a bad fight or emotional disturbance. Midway in one of the later test sessions as the tone was being sounded in a dark room, a cap pistol was fired unexpectedly behind a screen. From the reactions and reports of the subjects, this was an emotionally disturbing experience, but in spite of it, none of the subjects reported any color sensations on hearing the tones presented.

However, before concluding that conditioning of chromaesthesia was impossible, I decided to try a more daring effort to elicit any latent chromaesthesia. From the literature I had learned that certain drugs, hashish and mescal, had a tendency to produce spontaneous synesthesia of various kinds in normal subjects when under the influence of the drugs. My review of the literature suggested that the most promising drug was mescal, also known as peyote. From the literature, I learned that when ingested in sufficient quantities, a series of interesting phenomena result. The chief physiological effects are lowering of pulse rate, indigestion and nausea, often accompanied by headaches, but the psychological effects were much more interesting and happily more pleasant. The most predominant of these, and also the most interesting, is the production of colored visions complex in nature and varied beyond description: all colors, forms, shapes and movements imaginable seen when the subject closes his eyes or when in a dark room. Another interesting psychological effect lies in the fact that small bits of faded color are often seen as brilliant splashes and that normally unnoticed kinaesthetic sensations are so marked as to be annoying. Because of the many unpleasant effects of the drug, the experimenter planned to take the mescal himself rather than ask any of his subjects to do so. However, four of the subjects, upon hearing of my plans, volunteered to take part in this final phase of the experiment.

The results can be described briefly. All but one of the five participating subjects were rewarded for

their pains and discomfort by gorgeous arrays of colored visions which rivalled those described by the previous investigators. Unfortunately, all of the subjects, except the experimenter, suffered severe attacks of nausea. And, the one subject who did not have colored visions of any kind seemed to suffer more than anyone else!

But once again there was no evidence of any kind that a connection had been established between the colors and tones used in the experiment. For those subjects experiencing the constant flow of colors in their imagery, the tonal stimuli not only failed to produce the paired colors but also failed to influence the visual imagery being observed at the time. In a word, the use of mescal only emphasized the negative nature of the experimental findings. My doctoral dissertation was published in 1934 (Bibliography #8). Because of the dramatic individual differences found in the response to mescal, a separate paper describing these differences appeared in another reference (Bibliography #7).

Having finally completed an acceptable doctoral dissertation, I was in the market for a job. The small but growing University of Hawaii was in need of a second man in the Department of Education and Psychology. I was offered the position of assistant professor and accepted the job. Thanks to Naomi's having done secretarial work (typing and mimeographing), we could afford to book passage to Honolulu and arrived there in time for the beginning of the fall semester. We rented a small furnished house only two blocks from the campus. The University at that time was still quite small and I was the second person in the Department of Education and Psychology. My colleague taught most of the courses in education, and I those in psychology. The population of the Territory at that time contained only 5 or 6 percent Haole (whites). The remainder of the population was composed of a wide array of races and linguistic backgrounds. There were only a few pure Hawaiians left, their number rapidly dwindling as they intermarried with the various groups that had emigrated to the Islands. The first to come were Chinese, who found work in the sugar cane fields not to their liking and by the time we arrived in 1930, most of the small stores in Hawaii were operated by the Chinese who had left the fields. The second group to arrive were Japanese, but they were soon replaced by Filipinos who worked in the fields. The student body was composed of a motley mixture of the races. I was intrigued by this interesting and yet to me unknown population, but to my surprise found that I could recognize and identify most of my students correctly, partly by appearance, partly by their names.

However, occasionally I found it difficult to guess the offspring of one of the interracial marriages.

I carried a fairly heavy teaching load, especially during the second year when my colleague in the department decided to return to a university in the states to complete his doctorate. Even so I felt it essential to do a certain amount of student counseling, both academic and personal. Because I was very busy I usually asked students to fill out a case history form and take a personality inventory before spending much time with them. One of my many counselees during that first year was eventually to become very well known. She was only a part-time student, the wife of a Naval Officer.

While I had absolutely no training in counseling psychology, I took these voluntary duties seriously and maintained a folder containing information about each counselee. Little did I realize what I was getting into when I agreed to counsel Mrs. Thalia Massie. Because my time for counseling was very limited, I asked her to fill out a Chassell's Personal Variables Record, a semi-standardized case history form, but with plenty of space to write in comments. I also had utilized one of the standard personality inventories of the day. On perusal of these instruments it was obvious that Mrs. Massie was a very unhappy lady, and that her marriage to her husband was anything but satisfactory. After two or three counseling sessions with her I concluded that she was so disturbed that I phoned her husband to suggest that she needed psychiatric help from a better qualified professional than myself. (At that time I did not know that there were no psychiatrists in Hawaii!) Her husband, Lt. Massie, said that she had been very pleased with the counseling she had been receiving from me, felt that it had been very helpful, and he urged me to continue to see her. I saw her on two or three more occasions and was delighted one day when she phoned to cancel an appointment, saying that my counseling had been so successful that she didn't feel she needed to see me any more. With some personal satisfaction, I immediately shifted her file to that of closed cases.

A couple of months later, I read in the newspaper of a seemingly tragic situation. According to the newspaper accounts, a white woman had left a Saturday night party at one of the Waikiki bars and started to walk home in her evening clothes. According to the press stories, based largely on her own testimony, she had been picked up by five local youths (non-white) and raped by all five of them. According to her story, she had also received a broken jaw from one of the attackers.

Frankly, I found this story a bit difficult to believe, but since the newspapers did not reveal the identity of the woman, I paid little more attention to it. I found it difficult to believe for two reasons. First, by and large, there was relatively little crime in Honolulu during that period, and, secondly, I had learned that in this lovely island paradise, sexual mores were such that rape was rare. But on the basis of her identification, five local youths had been charged with the gang rape and a long trial was held. The jury was unable to reach a verdict and the judge declared the case a mistrial, but required that the alleged assailants report to the court house daily pending another trial.

The next news regarding the case was even more shocking: one of the five accused youths had been murdered. According to the news accounts, he was picked up one morning on reporting to the courthouse as usual, taken to the Massie home, threatened with bodily harm if he did not confess to the crime of rape, and, upon refusing to do so, was shot. Present in the Massie home at the time of the murder were Lt. and Mrs. Massie, Mrs. Massie's mother, a socialite from New York City who had come out to defend her daughter's honor, and two Navy enlisted men who had assisted in the abduction. These white individuals then attempted to dispose of the body by driving it out to a cliff and throwing it into a blow hole on the assumption that the body would be lost at sea. To their surprise, the local police followed and stopped the touring car carrying the body. The result was the arrest of Lt. Massie, the mother-in-law, and the two enlisted men. All were charged with first degree murder. As may be imagined, the trial following received a great deal of publicity, not only in Honolulu, but in the mainland papers as well. The famous Clarence Darrow was hired to defend the accused murderers. The local Admiral and the Navy Department had insisted on keeping the accused in private quarters at a Navy base, and several members of Congress got into the act by proposing that since Hawaii was no longer safe for white women, the territory should become a military government.

Upon reading of these developments, I found myself wondering whether Mrs. Massie really had been raped as she had alleged. I reviewed her case file, especially those portions reporting altercations with her husband, including physical violence, her obviously disturbed psychological condition, and even went so far as to ask a friend to interview a nurse who was on duty at the time she was examined at the hospital after the alleged rape. Mrs. Massie's evening clothes were not in the slightest disheveled, there were no signs of semen on her underclothing, and the only injury they were able

to find was a broken jaw.

With this additional information, I found myself becoming increasingly doubtful that Mrs. Massie had ever been raped. While the four surviving accused young men were awaiting retrial, Honolulu, and for that matter the United States as a whole were deeply involved in this highly publicized pending murder trial. Frankly, I was disturbed, and wondered whether or not I had any responsibility to reveal the information available to me (This was long before the days of an ethical code for our profession.). So I decided to consult the University physician, as a member of a long-established profession. Upon telling him of the psychological condition in which I had found Mrs. Massie during counseling and of the report of the hospital nurse, I asked him whether or not under the circumstances I had any obligation to reveal the facts available to me. And I shall never forget his reply. It was, "Thank God, I don't know what you know!" Given his reaction, I replied, "Then, Doctor, you don't know that I know anything, do you? Please do not mention our conversation to anyone." At this point, I decided to forget the matter entirely and continued my university duties.

Shortly thereafter, while on the island of Maui in connection with my duties as Director of Admissions for the University of Hawaii, I picked up the Honolulu paper which had been flown in that day, and discovered that while on the witness stand, Mrs. Massie had been confronted with my confidential file regarding her. Her immediate reaction was, "Where did you get this? You have no right to have it. These papers are based on my confidential consultation with Dr. Kelly." She proceeded to tear the materials into small bits on the witness stand. Of course, I was in a state of consternation, and took the first seaplane back to Honolulu. From there, I went directly to the office of the President of the University, and demanded to know how my confidential file had appeared in the courtroom. At that point, the President told me that he had been approached by the prosecuting attorney, and advised that if he did not deliver the file, the prosecutor would have obtained a subpoena demanding it. Since he could not consult me directly, he went to my office, searched my files, found the Massie folder, and delivered it to the representative of the prosecuting attorney.

Reaction to this episode was immediate and vicious. Newspaper editorials and letters to the editor blamed me as having released this confidential information, and one member of the Board of Trustees was most insistent that I be fired immediately.

ARCHIVAL RE~~VIEW~~⁵²⁷ Finally, I wondered how the prosecuting attorney had learned of the existence of this file, but soon discovered that the university physician, in whom I had confided, was also the city and county physician and coroner. As such he worked closely with the other county officials and obviously told them of the existence of the file. During this furor, the County Medical Society prepared and published a statement that the matter of medical confidentiality had not been violated inasmuch as Dr. Kelly was not a physician!

In defense of the University President, I will say that he apologized to me for his actions, and defended me in a Board of Regents' meeting, assuring them that I had not released the confidential file that had been transferred to the prosecutor.

My two years in Hawaii had not been particularly productive either intellectually or in terms of research activity. I had collaborated with Terman in preparing the chapters on homosexuality for the book, *Sex and Personality* (1936), but Terman felt very strongly (and perhaps rightly) that I should not have my name publically associated with homosexuality so early in my professional career. Sensing that I was not being very productive, Terman urged me not to become a "lotus eater", and told me that I should get a job back in the States as soon as possible. Eventually, without my knowing about it, he applied for and obtained for me a postdoctoral fellowship year for me in the Social Science Research Council, a year which I decided to spend in Germany. During my second year in Hawaii I was also upset by the fact that Naomi began wondering whether she should not have married my Purdue roommate because she enjoyed his reading poetry to her. She decided to make a trip back to Indiana and be with him and discover to which of us she wished to be married. Two or three months later, she returned to Hawaii assuring me that she was no longer interested in my college roommate and we began preparations for our year in Germany.

The furor over the Massie case having subsided, I was granted a leave of absence from the University of Hawaii, and since I was already associate professor, presumably had tenure there.

Because of my total inability to speak and understand the German language, the Social Science Council urged me to go to Germany three months before my fellowship started in order to learn this difficult language. In June of 1932, then, we departed from Honolulu, stopped off briefly in the States to visit my parents, and boarded a ship in New York for Hamburg.

On arriving in Germany it became very obvious that educated German was much more anxious to learn English than to assist me in learning German. We therefore decided to seek a spot where no one knew any English! It turned out to be a small village on the North Sea where we took room and board with a retired World War I submarine commander who was an ardent Nazi, because he loved Hitler, but because he was sure the Nazis would bring the Kaiser back to the throne! No one in the village could speak any English, so we made little progress in learning German during these months but even so, were far from proficient in English. On arriving in Berlin in September, we enrolled in a course, Deutsch für Ausländer (foreigners). By the end of this course (three months) I was sufficiently fluent in English that I found myself talking to a chap from a non-European country. At one point, I paused to think German when he had learned English. To my surprise and delight, he replied that he did not know English!

During these early months in Berlin I had also visited several of their marriage counseling clinics, looking forward to learning more from them in preparation of my anticipated research on personality and marriage. Soon thereafter, however, the Reichstag burned. On reading the headlines in the paper the next morning, I commented to my landlady "Clever trick of the Nazis, wasn't it?" She cautioned me not to say such things, otherwise I might be in trouble. It was at this point that I decided that I had better leave Berlin and go to a pleasanter intellectual climate. Leaving Berlin, however, on a cold and foggy night, I received a very disturbing cable from the president of the University of Hawaii. He said that the university had finally reached Hawaii. The University had suffered a 40% budget cut which meant that the last few members employed could no longer be retained. Fortunately, this included me.

Early in 1933 then we moved on to Vienna, and, amazingly enough, managed to sublet an apartment from Frau Rosenthal who had at one time been a secretary to Sigmund Freud. Since I was busy translating Freud in the original German, I began asking her as to what he had really meant in some passages, only to discover that she had not had the best comprehension of the material she had read. During the next few months I attended psychoanalytic seminars, both Freudian and Adlerian, for some reason never felt the need to have a training in psychoanalysis even though it could have been

Unfortunately, after a few months in lovely Vienna, the Nazis took over Austria. At this point we decided to move on to Switzerland for the rest of our fellowship year. In Zurich I read and tried to understand Jung, but with little success. I also had an opportunity to interact with several intellectuals in Zurich. One event stands out vividly in my memory. On a Sunday evening I was being entertained at dinner by a group of professors at the University of Zurich. To my amazement although all present were highly educated Swiss gentlemen, the dinner table conversation was entirely in Schweizerdeutsch. Although the conversation was a very animated one, I was totally unable to understand even the topic of conversation much less its details. After about 20 minutes, when I had not expressed an opinion my host turned to me and said, "Herr Doktor Professor Kelly, was denken sie?" in high German. I replied, "Gentlemen, I have no idea of what you have been talking about. Your speech is completely unintelligible to me." At that point, both the host and all of his guests apologized profusely and from that time on the conversation was carried out in high German. In their apologies they had complimented me by saying that my German was so good that they simply assumed that I had been able to handle their particular dialect so popular in Zurich.

Throughout the spring of 1933, I had been deeply concerned about how I would earn a living after completing the fellowship year. Because of the effects of the Depression, there were practically no teaching jobs available in any of the colleges and universities in the States. From Terman and Miles, I learned that they had recommended me very highly for a position at Connecticut State College. Whereas its enrollment had been relatively small until that time, economic exigencies made it impossible for the elite to send their sons and daughters to the Ivy League schools, and they were applying in large numbers to the Connecticut State College. The result was that the college needed a second man in the then one-man department.

I shall never forget the letter I received from Dean Gentry of the College stating in essence that he had never had a prospective candidate for a position come so strongly recommended by such distinguished persons, but that in spite of the strong recommendations he did not feel that he dare offer me the position because he, in his long experience as an administrator, had never hired a staff member without a personal interview. Since this was before the days of transoceanic flights, and interview was totally impossible, I replied by letter, probably one of the

most important letters I ever wrote. As I recall,²³⁰ I first stated that I understood the Dean's desire to interview any individual whom he was considering appointing to his staff, but in view of the very strong recommendations and my successful record at the University of Hawaii, I thought that he might consider waiving this requirement in my case. Next I bluntly stated that, after all, I was physically presentable, dressed reasonably conventionally, spoke passable English, had a record of successful teaching experience in Hawaii, and really believed that I deserved to be considered without an interview. To my delight about three weeks later I received a response saying that my letter had persuaded him and he was going to violate his long-established rule regarding a personal interview. In other words, I had a job!

In August, Naomi and I left Zurich and went to visit some friends in Munich, where we purchased bicycles and teamed up with a delightful German lad for a trip down the Rhine. In fact, we rode our bicycles all the way to Hamburg. Although an Aryan, our perceptive young friend was extremely anti-Hitler and was worried about future developments in a Nazi Germany. By that time, however, things were so tense in Germany that we discussed such matters only while riding our bicycles in open space, or quietly in our sleeping bags in a meadow alongside the river. Regretably, I have no idea what happened to our friend during World War II.

We sold our bicycles in Hamburg and departed on a German ship which made a brief stopover in London, thus giving us an opportunity to see a few sights in the city there. One incident served to remind me how thoroughly I had been Germanized in the last fourteen months: when searching for a particular place we wished to visit, I casually approached a London Bobby and in my best German asked him for instructions. Of course, he was totally nonplussed and I hastily apologized and made the same request in English.

The ocean voyage to New York was uneventful and on arrival, we proceeded to Storrs, Connecticut. There I immediately reported to Dean Gentry so that he might see the person that he had hired without the benefit of an interview. I found the Dean a charming gentleman, paralyzed from the waist down by an earlier attack of polio. On introducing myself, he flung around in his swivel chair and looked me over carefully. His first words were, "Thank God, you're presentable!"

The fall term started shortly thereafter and I found myself with an extremely heavy teaching load, as also had been the case at Hawaii. I also found myself

ARCHIVAL REMOVALently sought out by students for academic year
personal counseling. One of my early counselees there
was a somewhat emotionally disturbed freshman girl who
never seemed quite able to tell me what was bothering
her. It was only several weeks later that I discovered
the source of her problem in a most dramatic episode.
But, before relating it, I must relate an equally
surprising turn of events.

A few weeks into the first semester of 1933 at
Connecticut I received a letter from Professor Terman
stating that during the spring and summer of 1933, he
had been so worried that I would not find an academic
position in the States that he had submitted a research
proposal to the National Research Council, Committee
for Research on Problems of Sex, to support the
longitudinal study of personality and marriage which I
had contemplated. Should I fail to obtain a position in
the States, he had planned to invite me to Stanford to
carry out my planned marriage research there as a
research associate. He further stated that he was
certain that for my own professional career it was
better that I accept almost any academic position
rather than become a research associate. But because he
knew how much I wanted to carry out the marriage
research, he had not told me about the research grant,
lest I make the wrong decision! He then admitted that
he was in the embarrassing position of having obtained
a research grant to carry out my proposed research
program and asked for my advice as to how we should
proceed from there. He said that he did not want to do
the research which I had planned, but was obligated to
do something with the research grant related to the
program proposed. He again indicated his willingness to
support my application for a research grant from the
same source (Professor Robert M. Yerkes, his close
friend, was chairman of the committee).

My response was simple and straightforward. I
suggested he use the grant already awarded to pursue a
cross-sectional study comparing the personalities of
happily married and divorced couples and I would
prepare an application for a research grant to carry
out my long-planned longitudinal study. Terman seems to
have been delighted with this proposal, and wrote a
very strong letter to Professor Yerkes urging him to
give favorable consideration to the proposal which I
would submit shortly. Because I had already given a
great deal of thought to the overall research design of
this longitudinal study, I immediately began preparing
a research proposal and submitted it that fall to
Professor Yerkes. Shortly thereafter, I received a
phone call from him asking if I would come to New Haven
(about 60 miles) that afternoon to confer regarding the
research proposal. Although I was responsible for a

laboratory in elementary psychology that afternoon, I met the group, gave them instructions as to how to proceed with the experiment, told them that I was sure they could get along fine without me, and excused myself as having an important obligation in New Haven. As I went to the door of the laboratory, the unhappy young freshman girl whom I had been counseling, followed me outside and, to my astonishment, asked if I would see her mother while in New Haven. I told her that I could not imagine why I should see her mother, but if it was important I would be glad to do so. Her reply was that "mother wants very much to talk with you." Following a very pleasant and apparently successful conference with Professor Yerkes, I then decided to look up the young lady's mother. When I introduced myself at the door, instead of being invited into the living room, I was greeted with a torrent of words, the gist of which was "your Professor G. has seduced my daughter. What are you going to do about it?"

Although much shaken by this turn of events, as calmly as possible I replied, "My dear lady, may I come in and sit down?" She acceded to my request and then, in as calm a voice as I could muster under the circumstances, I explained that this was a very serious accusation and wondered how she could be so certain of its validity. Her reply was, "My daughter is an honest girl and wouldn't lie to her mother." (I then discovered that she was an only child.) I did not argue the issue, but pointed out that I was hardly the person to do anything about the situation as she had demanded since Professor G. was my superior. However, I did advise her that if she felt that her daughter had been seduced, she should report the matter to the President or Dean of the University. I then gave her both names and telephone numbers where she could reach them for an appointment. A couple of days later I noticed that the President's door was closed (we all lived in one single building at the University of Connecticut in those days!) The following day I was called into Dean Gentry's office and advised that Professor G. would be leaving the University at the end of the year, and that I would become Chairman of the Department, and should go about finding a second man for the department. The Dean further asked that I not speak to Professor G. about this situation for the remainder of the academic year. I replied that this would be difficult, but that I would try. I also bit my tongue not to remind the Dean that Professor G. had been hired with the benefit of a personal interview. This experience, however, was no doubt responsible for my repeated efforts to evaluate the validity of the personal interview as a selection device, and resulted in several publications on the topic in later years (Bibliography #20, #21,

Thus began a very eventful and busy academic year. Within a couple of months I was advised that my application for a research grant had been favorably acted upon and began making detailed plans to initiate the collection of data on 500 engaged couples. This involved not only purchasing and numbering personality tests, but developing a personal data sheet, a thirty-six item personality rating scale, etc. The following five years, 1933 to 1938, were extremely busy ones. My wife, Naomi, became a dedicated and invaluable research assistant on the marriage project and by the time I left Connecticut early in 1939 we had succeeded in enlisting the cooperation of and collected data for 300 engaged couples.

During these years, I was also busy not only with teaching courses and collecting data for the marriage research project, but also carried out two other research projects, neither of which resulted in a publication. The first of these concerned the validity of graphology. At that time the leading Hartford newspaper was carrying a daily column on graphology written by a young lady whose name I have completely forgotten. Not surprisingly, many of my students read these columns, and asked about the validity of graphological interpretations which were available from the young lady for ten cents apiece! I suggested to one of my classes that we carry out an experiment: if each of them would provide me with a sample of their handwriting, I would submit them to the young lady for graphological analysis at no cost to the students. I chose one of the handwriting samples, sent it to the newspaper with the appropriate ten cents enclosure, and soon thereafter received a mimeographed sheet containing about 50 statements with about half of them checked off as applicable to the personality and character of the writer of the handwriting sample. I then simply reproduced this mimeographed sheet and distributed it to each member of the class as if it were an interpretation of his or her own handwriting. In response to a questionnaire as to the accuracy of the interpretation, a large proportion of the class thought that the analysis was remarkably correct and astute! I then explained the deception to the class and discussed with them why they had been taken in and why each had been so impressed with the validity of what they thought was an individual interpretation. The answer, of course, was that the statements on the mimeographed sheet were so general, ambiguous, or complimentary that each person was led to accept it as applicable to him or her. Sometime thereafter, in a public meeting on the topic of graphology in Hartford, I reported the results of this experiment. The young

lady graphologist was present in the audience.²³⁴ Her defense was that one could hardly expect a detailed individual analysis for ten cents, but that she could do much better if they would pay her a considerably higher fee for a true individual analysis.

Because she was so sincere in her belief in her own competency as a graphologist, I then challenged her to a definitive experiment. By this time I had collected both handwriting samples and personality ratings from associates on more than a hundred of the engaged couples. I invited the young lady to come to Storrs, study each sample of handwriting as long as she wished, to rate the subject on the 36 item personality scale, and we would then correlate her ratings with those of associates of the subject. Somewhat to my surprise she accepted this challenge, came to Storrs, and spent a couple of weeks on the project. Although she had promised to provide personality ratings for 100 subjects, by the time she had completed 50 she said that she was so exhausted that she simply could not proceed with the experiment. With the help of several undergraduate volunteer assistants, we then computed the correlations between her ratings and those of friends of the subjects for each of the 36 traits. The resulting correlations were randomly distributed around zero, and not one of them was more than three times its standard error! On receiving this information, the young lady who had remained in Storrs acknowledged that she now knew that she could read the personality for only about half of the 36 traits, namely those for which her ratings correlated positively with those of acquaintances! Of course, I was not able to persuade her that even the positive ratings were only random, chance variations from zero. At this point she became very depressed and I made arrangements for her to see a physician at my expense. Although I think it was a worthwhile experiment, it hardly seemed one worth publishing.

Although I was extremely busily involved during these years, I decided to again become involved in amateur radio, and built and assembled an amateur station. I had also read with much interest (and, in retrospect, naivete) the reports of the experiments in extrasensory perception being carried out by J. B. Rhine at Duke University. Naively, because they were published in journals, and I was thus willing to accept the existence of ESP. After purchasing a deck of ESP cards, I began searching for a subject who could perform consistently at a better than chance level.

ARCHIVAL REVIVAL My goal was, once having found such a 235 subject, to see if his or her performance would be impaired by encompassing him in a magnetic shield or in a magnetic field. Unfortunately, even after weeks of searching and the cooperation of many would-be subjects, I was never able to locate one that could perform in an ESP situation at better than chance level.

Just about the time that I had given up all hope of finding a subject capable of reading ESP cards in a better than chance fashion, I happened to contact the Duke radio station one morning on my amateur radio station and I told the operator of my efforts to find a subject capable of performing successfully in ESP experiments. He in turn told me that one of Rhine's best subjects happened to be his roommate, one who could correctly call cards at any distance. I asked him if he would be willing to get his roommate to participate in an experiment in which I would look at the cards seriatim in Storrs, Connecticut, and by number ask him to respond to the symbol on the card at which I was staring. And while I had no illusion that I could perform the same task I said that I would be willing to attempt to read the cards that he was looking at Duke University. Fortunately, he proved agreeable to the proposition and we systematically began a series of long distance experiments on ESP. I do not recall how long this experiment continued, but I recall very distinctly that neither he nor I had been able to perform in a better than chance fashion and thus we abandoned the experiment.

About the same time I made what I considered a important discovery: that one could "read" the ESP cards with a high degree of accuracy from the reflections of light off the back side of the cards. The rather large and distinctive symbols had been printed in such a way that a distinct impression of the symbol could be seen by the differential reflection of light from the back side. I found that others as well as I could frequently "guess" the correct symbols for 20 to 23 cards out of the deck of 25 as contrasted to the chance score of 5! At the next APA meeting (I believe in 1936 in Ann Arbor) I reported this finding to a group discussing ESP experiments at Duke, at which J. B. Rhine was present. Of course, I suggested that proper experimental controls would necessitate throwing out all of the previously accumulated data in which the subject had been permitted to see the back of the cards. Rhine was incensed by this suggestion and retorted loudly that he was not to blame since he had not printed the cards! As a result of this shocking experience, I decided that I had wasted a lot of time in pursuing ESP experiments at Storrs and completely

ARCHIVAL REVIVAL abandoned the project as well as losing all confidence in further reports from the Rhine laboratory. To this day, I have not seen any evidence based on carefully controlled experiments which would lead me to believe in the existence of ESP.

During this same period, Naomi and I were heavily involved in collecting data on the three hundred engaged couples and although our own marriage was obviously anything but a compatible one, she was an extremely faithful research associate throughout the period and contributed greatly to our eventual collection of the original data on the engaged couples. But because it was obvious to both of us that our marriage was not going well, we eventually decided to cease pretending both to ourselves and friends and to move to separate quarters. She began sharing an apartment with a research associate in animal genetics in what had earlier been the Storrs Agricultural Experimental Station, and I rented a home on a lake about ten miles from the campus and set up bachelor quarters. Even though we were living apart, we still cooperated very effectively in completing the data collection for the marriage project.

In discussions with Naomi, we agreed that we should be divorced but, since there was no such thing as no-fault divorce in those days, I agreed that she should file a complaint for a divorce which she did in October of 1937. Since we had already divided such little property as we had, and as there were no children, I made no effort to contest the divorce even though the formal complaint was that of intolerable cruelty to the complainant. Our divorce was finalized on the 20th of December, 1938.

Shortly after moving into my new bachelor home on the lake, I installed an even better amateur radio antenna, and spent many pleasant hours conversing with amateur friends all over the world. Practically every morning while having breakfast, I would engage in "chit-chat" with a number of friends who regularly appeared at that time of day on a specific frequency. The group included the owner of a radio station in Tennessee, a very superior radio technician in Texarkana, Arkansas, and several others scattered throughout the midwest. Included also was one foreign station in Haiti, the operator of which was a young lady named Lillian with an extremely broad British accent. From our conversations I learned that she had been born in Jamaica but had lived in several Central American countries, and was operating the station belonging to her father, also an old time "ham" operator. Near the end of one of these early morning chit-chat sessions, she announced that we would not be

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hearing her station for some time, as they were leaving for an extended trip. Since I knew her father was British, and she spoke with such a broad English accent, I asked if they were going to London. She replied in the negative, stating that her father, who worked for a shipping company, was being transferred from Haiti to Baranquilla, Columbia. Since they would need many things not available in that city, her mother, her sister, and she were coming to New York for a shopping tour during the summer of 1937. Because we had talked on so many occasions, but had never met personally, I suggested that she let me have her address in New York City, and I would look her up on one of my week-end trips to see a play in New York. I took her to a number of plays that summer, often accompanied by one of the more mature psychology students at the University of Connecticut. On one of these visits, both the student and I suggested that perhaps they would like to visit my home in Connecticut and we arranged to transport Lillian and her sister to Storrs for a weekend. Because I was in the middle of a divorce and very doubtful that I ever wanted to marry again, I paid but relatively little attention to either of the two young ladies, but tried to provide them with an interesting weekend. I found Lillian the more attractive and pleasant of the two sisters, but had no inkling that she would eventually become my second wife.

After becoming ensconced in their new home in Baranquilla, her father again erected an appropriate antenna, and we continued our daily chats via amateur radio. From these I found her an increasingly interesting individual and eventually we began holding our conversations in Morse code lest short wave listeners become too aware of the feeling developing between us. Eventually, and after much soul searching, I wrote her a very serious letter suggesting the possibility of our marriage, but carefully pointing out that there were many potential obstacles to its success. Because of our very differing backgrounds and education I suggested that she might not find it pleasant to be the wife of a college professor, and that I was certain a college professor's income would not provide the retinue of servants to which she had become accustomed while living in the tropics. However, such warnings on my part apparently had no effect and shortly after receiving the letter, she assured me by amateur radio that her answer was "yes." Later that week, I conversed with her father in Morse code and formally asked for her hand in marriage. He was a bit surprised by the development, but gave the proposed marriage his blessing. Even though this conversation was in Morse code, the family received several cablegrams the following day from short wave listeners

throughout the Caribbean who had been listening in!

Lillian and I were married on Christmas day, 1938, in Riverside Church, New York City. In spite of the dire warnings by many of my friends and colleagues who felt that I was taking a long chance in marrying a young lady with whom I had had so little personal contact, I refused to take their warnings seriously. And it is a pleasure to report that after nearly fifty years of marriage, I do not believe that there has ever been a more compatible marriage in spite of the fact that most of the courting was done by international amateur radio.

At the time of our marriage, I had already agreed to take a leave of absence from the University of Connecticut, and spend the spring semester of 1939 at Purdue as a visiting associate professor of psychology. On a very cold and snowy day in late January, we drove from Storrs to Indiana, carrying along our personal belongings, and thus began my career at Purdue. Of course, we had taken along our amateur radio station, but Lillian was not permitted to get a license, because, although under British law she assumed the citizenship of her husband, under American law she had to wait three years and become a naturalized citizen before she could obtain an amateur license.

At Purdue I taught several courses in psychology and found the academic situation there so pleasant that I accepted the offer of a permanent position as associate professor and resigned from my previous one at Connecticut. Before leaving there I had already learned to fly and had acquired a private pilot's license, and in the summer of 1939 I purchased an airplane which Lillian and I used for many cross country trips. She, too, took flying instruction, but because she was pregnant with our first child, she was not permitted to solo, and so never obtained her private pilot's license.

It was about this time that it became increasingly apparent that the United States would likely become involved in World War II and the Civil Aeronautics Administration began a program of training student pilots in what was called a Civilian Pilot Training Program. As a psychologist, I was much interested in the possibility of developing a pilot aptitude test, one which might be used to reduce the unfortunately large number of failures in flight training. Rather than seeking to test motor coordination and other such potential aspects of aptitude for flying, I decided to rely on an empirically scored inventory based on the differential responses of persons who succeeded and those who failed in flight training. This Biographical

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Inventory, as we called it, was in no sense a test, but rather a statement of preference for a large number of activities. Subjects were merely asked to indicate whether or not they had engaged in such an activity in the past and indicate the degree to which they liked or disliked the activity in question. Even though the empirically derived scoring weights were based on a relatively small sample of successful and unsuccessful trainees, the Inventory appeared to have considerable validity in predicting the likely outcome of flight training.

This test was administered to each successive incoming group of trainees at the Purdue Airport (mostly Purdue students). I was shocked to find that some of the persons scoring very high on the Biographical Inventory did not succeed in flight training. While I had no illusion that the Inventory was 100 per cent valid, I began to wonder why students whose aptitude appeared to be so promising were failing so often in training. It was at that time that I again resorted to amateur radio to investigate the actual flight instruction carried on between the instructor and student in the two-seat training planes of the day. Since it was totally impossible to observe this instruction directly, I decided to utilize radio communication instead of direct observation. With the assistance of a very highly qualified amateur technician, we designed and built an electrical intercom (microphone and earphones), which was in turn used to modulate a low-powered lightweight battery-operated transmitter in the training plane. Because of the much improved intelligibility of the electrical intercom versus the speaking tubes which had been used up until that time, both the instructors and students were very happy to participate in this experiment. But because of the impossibility of analyzing flight instruction in detail as it occurred, we decided to record these instructional hours and transcribe them for detailed later analysis. This was before the days of tape recorders, so we were forced to use an old fashioned Ediphone, in which the cylinder had to be changed every six or seven minutes, but thanks to the assistance of my wife, Lillian, we managed to record and eventually transcribe 100 hours of "in the air" flight instruction.

Although we had casually "listened in" to portions of the flight instruction as it was recorded, it was only when we made a detailed analysis of actual transcriptions of these instructional hours that we began to realize how very inadequate the instruction was in many instances. Perhaps the most striking finding was the absence of standardization of the terms used by the various instructors. This analysis showed

ARCHIVAL REVIVAL that four instructors had used 500 technical terms in their flight instruction, but only 70 were common to all four instructors. One instructor used 265 terms which were never used by the other three. Among them there were 14 different expressions to instruct students to increase and 20 to reduce power, 33 for turns, and 18 for describing only two or three control motions of the stick and rudder.

Even more shocking was the discovery that none of the flight instructors really understood the basic principles of aerodynamics with the result that their instructions, while well intentioned, were often absolutely incorrect. This is not too surprising when it is remembered that most instructors of the day had learned to fly with older pilots similarly uninformed with respect to aerodynamics, and all had learned to fly simply by "the seat of their pants." While it is possible to learn to fly with such an instructor acting primarily as a safety pilot, much as a child learned to ride a bicycle by his mistakes, such instruction was totally inadequate for students who would soon have to fly in the dark and on instruments through conditions of poor visibility. Obviously, something had to be done to remedy this situation, so we decided to create a clearly written manual in standardized language for some 60 basic maneuvers. In preparing these "lesson sheets" we had the assistance of a bright, knowledgeable, and well-informed instructor. These 60 standardized maneuver sheets were then published in the form of a manual for instructors entitled "Patter", published by the Civil Aeronautics Administration.

As it became more obvious that the United States would become involved in World War II, "Patter" was translated into several other languages, including one edition in Chinese for use in teaching Chinese student pilots. By late 1942, it was clear that the Civilian Flight Training Program, and our research activities at Purdue could not longer continue, and I decided to apply for a Reserve Commission in the Navy. I was commissioned as a Lieutenant USNR. As a result of some fluke in the paperwork, I was commissioned, not as a psychologist in the hospital corps, but as a deck officer with a star on my arm. This odd turn of events turned out to be advantageous in view of the extremely varied assignments which I had while in the Navy. When I wanted people to listen to me professionally, I would remind them that I was Dr. Kelly, and when I encountered problems with line officers also wearing a star, I would also be a "line officer."

Although assigned originally to the Division of Medicine and Surgery which included the other psychologists in the Navy, I was soon given additional

assignments in the training division of the Office of Naval Operations, and actually spent most of my Navy career on this "additional duty." The Navy decided to use my Biographical Inventory in connection with the selection of naval aviation cadets and soon produced its version of "Patter" for use by primary flight instructors. One of my early assignments was to the Primary Flight Training Command, with the job of observing flight training at several of the primary flight training bases throughout the United States. Not surprisingly, I found that the primary flight training instructors in the Navy were no better informed regarding the aerodynamics of flight than those in the civilian training program back at Purdue. Because of my age (I was 37 at the time of commissioning), the Navy refused to permit me to take flight training and thus acquire a set of wings on my left breast, symbolic of a naval aviator. It was, therefore, not surprising that I had much difficulty in trying to convince the primary flight training instructors in the Navy to use the Patter booklet. They would call attention to the fact that I did not have a set of wings. And while I might know how a light plane flew, I was totally unfamiliar with the characteristic of the planes used in naval flight training.

Initially, I was extremely frustrated at this situation, but soon developed a gimmick which gave my opinions more credibility. On finding myself in a seemingly hopeless argument with a group of flight instructors at a primary base, I would find a topic on which we disagreed markedly about their current instructional practices, typically one resulting from the fact that they did not understand the aerodynamics of flight. Upon being challenged that I did not know how the Navy training planes really flew, I would then challenge the instructor to go for a ride in one, and suggested that we would carry out a simple experiment of, say, adjusting the throttle. The instructors of that day often claimed that the throttle controlled air speed, while I maintained that it controlled whether or not the plane descended or climbed. Quite typically, I would challenge them by betting a bottle of the best Scotch on the Base as to the correctness of my position. This experience was repeated many times, and I won a bottle of Scotch on each occasion!

Because flight instructors tended to move about the country rather frequently, word soon spread that perhaps this guy Kelly knew what he was talking about after all, and they should pay attention to the Patter book. My concern was not that of proving myself right, but

ARCHIVAL REVIVAL rather than of showing that the Navy was losing a lot of promising young pilots on their first night flight,²⁴² or when they undertook instrument flight training without an understanding of aerodynamics. My determination was enhanced when, visiting a primary base one evening, I observed a group of cadets taking their first night flight. One of the young cadets flying alone flew directly into the tail of another plane with the result that both student pilots were killed. At that time, navigation lights on the plane consisted of a red and green light on each wingtip and a single white light on the tail. Realizing that it is practically impossible for depth perception to be operative when staring at a single light, I urged that the tail be equipped with two red lights with a standard distance between them, making it possible to judge the distance much more accurately. This was eventually done, and such equipment is now standard on all aircraft.

Because of the lack of proper coordination between primary and secondary operational training in the three separate commands of the Navy, one which was obvious to others as well as to me, the Navy eventually decided to create a "super" flight training command to coordinate the activities of all three other training commands. This was to be located at Pensacola, Florida, and Admiral George Murray had been named to head up this new command. I was visiting Pensacola on other business, but my Admiral in Washington asked me to pass on his regards to Admiral Murray, which I was glad to do as I was anxious to meet with him. Our initial conversation turned out to be an extremely pleasant one. Since his staff had not yet been assembled, he was anxious to have my opinions regarding both the problems and the possible solutions to the problems of coordinating flight training in the three training commands. To my surprise, before our conference had ended, he asked me whether I would be willing to join his training command as a special assistant. I was flattered by this invitation and request for a transfer was soon approved in Washington.

My year at Pensacola proved to be a very pleasant one because I found Admiral Murray to be very sincere in his efforts to improve flight training throughout the Navy. Unfortunately, his open mind was not shared by many other members of his staff, with the result that there was much argument amongst us. But because it was necessary for the Admiral to attend frequent training conferences in other locations, he soon began the practice of asking me to ride alongside him in his DC-3 to help plan the agenda and to suggest the positions which he should take on issues to be discussed. I was gratified because he told me that he

felt that I was the only one on his staff that would disagree with him if I thought his opinion was wrong. The result was that I had a delightful and very productive year as a member of Admiral Murray's staff.

During this year, working closely with a highly qualified instrument flight instructor and an exceptionally able enlisted man who was an artist, we produced one of the best instrument flight training manuals ever written, "Flight Through Instruments." As with all other Navy publications, names of the authors did not appear on the publication, but even so, I still treasure the single copy of it still in my library. It is of course now outdated because of technological developments in aircraft instruments, e.g., radar, glide path indicators, etc.

As I was a member of the Admiral's staff, Lillian and I were assigned a spacious furnished apartment in the Officers' Quarters. We purchased very good food and very cheap liquor at the Navy stores, and always enjoyed having meals out at the Officers' Mess. The only unpleasant aspect of the Officers' Quarters was the presence of hoardes of cockroaches, but Navy personnel did a pretty good job of keeping them cleaned out of our kitchen.

One of the more interesting and educational experiences involved my participation in conferences held by the combined air training commands of the U.S. Air Force, the U.S. Navy, the Royal Air Force, and the Canadian Air Force. The last three took turns in acting as host for these conferences depending on the time of year and the climate. We always selected a place which would provide for a couple of days of hard work on a well-planned agenda and a couple of days of recreation. Except for the pilots of the aircraft which transported us, I was invariably the lowest ranking officer in the group, which typically included several generals and admirals. Even so, I was ordinarily listened to respectfully by these superiors.

As a Naval Reserve officer without wings, I was indeed a notorious person among all those involved in flight training. Although I had won many bottles of Scotch and had never lost a single bet, it seems that the word got around that wherever possible the officers with wings figured out ways and means of putting my knowledge and flight skills to critical test. The first of these occurred when I was visiting the primary flight training command. The Commanding Officer was a chief pilot for one of the airlines. While I was visiting this command in Kansas City, a trip was scheduled to take certain members of the command, including an admiral from Kansas City, to Corpus

Christi, and I was invited to go along. The pilot was the Commanding Officer, and we travelled in a Navy DC-3. We had scarcely gained altitude when he came back to the cabin and asked if I wouldn't like to fly the DC-3. Although I had never even been in the cockpit of a DC-3, I said I would very much enjoy the opportunity at which point he took my seat in the cabin and invited me to take the pilot's seat in the cockpit. Without any instruction whatever, I was merely told to take us to Corpus Christi, Texas! There was a young copilot in the right hand seat, but I soon found that by agreement with the Chief Pilot, I was to get no help whatsoever! I asked for the compass course and was told that I'd find some maps and I could figure it out for myself. I then asked for the frequencies of the appropriate radio beam which would take us in the proper direction and was again told that I would find references in some of the booklets in the cockpit. In brief they were doing everything possible to make me cry "Turkey," but I refused. Fortunately, the weather was good, and I finally found the appropriate references, compass courses, and radio beams, and believe it or not, they made me fly the plane practically all the way to Corpus Christi. I will say that they had the good judgment not to insist that I land it! My last similar experience occurred on one of the combined air training command trips. We had been transported to Montreal in a B-24 Liberator four-engine bomber. On the return trip, again after gaining altitude, the pilot asked me if I wouldn't like to fly a Liberator. Again, without even giving me an opportunity to locate the instruments, he left saying that he wanted to have a nap. And, believe it or not, there was not a copilot or a flight engineer in the cockpit. Thus I found myself alone at the controls of by all odds the largest plane of its day. As the pilot left me, I said it looked like there was some nasty weather ahead, to which he casually replied, "You know about instrument flying. Take it through on instruments!" Fortunately, I managed to do so, and the pilot returned to the cockpit to land the plane in Washington. He was a very promising young pilot well-known for his acrobatic performances, but was unfortunately killed a couple of weeks later when putting on an air show in Washington.

I won my last bottle of Scotch as a result of telling my efforts to instruct flight instructors in a conversation with a good friend and fellow resident of the Officers' Quarters with whom I had played frequent bridge and poker. When I told him of my problems in convincing flight instructors that I knew what I was talking about he said, "Well, you may know about light planes and training planes, but you certainly don't know how our big flying boats operate." He was commanding officer of the flying boat squadron at

Pensacola. Of course I replied that I thought a PBY-5A
ARCHIVAL REVIEW according to the same aerodynamic principles²⁴⁵ and
made a bet with him which we settled the next day.
Since we shared the bottle of Scotch I suppose he did
not feel too badly about losing the bet, but I think
perhaps he did learn something about how airplanes fly
aerodynamically.

But near the end of 1945, it was obvious that my
delightful tour of duty with Admiral Murray was coming
to an end. He had just been designated as Commander in
Chief of the Pacific Fleet and very kindly invited me
to go along as a member of his staff. I told him that I
would be delighted to do so, and I couldn't imagine any
place safer than being on the Admiral's flagship, but
reminded him that I had absolutely no experience in sea
duty or in fighting a naval and aerial war, and thought
that I could not be particularly useful. He replied,
"Kelly, you're probably right as usual, but I would
like to have you along." About that time, the Admiral
due to replace him at Pensacola arrived and it soon
became obvious that I could not have a satisfactory
working relationship with him. Shortly after my arrival
at Pensacola, I had told the Admiral that I felt our
"super" training command needed a different kind of
organizational chart than the hierarchical one standard
in the Navy. I said that I thought that we could make
the most useful contribution if everybody who needed to
talk to everybody else in the Command could do so
without going through the organizational chart to the
Admiral and then back down the person he wanted to
communicate with in another division. At that point I
suggested an entirely different type of organizational
chart for the Command: the Admiral and his chief of
staff were located at the center and the lower levels
were arranged in concentric circles around the center.
Much to my surprise, Admiral Murray accepted my
suggestion and we utilized this organizational chart
throughout my tour of duty at Pensacola. When the new
Admiral arrived, he took one look at the chart, and
asked "Where in the hell did this organizational chart
come from?" When told that I was responsible for it, he
proceeded to tear it up and say that we would go back
to the standard organizational chart for the command.
At that point, I decided that I should ask for a change
of assignment.

Somewhere along the line (I don't recall the exact
date) I was promoted from Lieutenant Commander to
Commander, U.S. Naval Reserve, which entitled me to
wear not only three stripes, but also have a cap with
"scrambled eggs." And I also don't recall how it came
about that my next assignment was with the Office of
Naval Research in Washington. By that time, the war was
over in Europe, and we were obviously winning in the

Pacific and the Navy decided to use some of its unspent appropriations to support unclassified research in universities. This included not only the natural and physical sciences, but also the social sciences. Jack Darley and I were in charge of the unit involved in reviewing proposals for ONR grants to support social sciences in the universities. I had been at this assignment for only a few weeks, however, when the Admiral in charge of ONR called me in and said, "Kelly, I would like to give you an unusual assignment: one for which I dare not write orders. There are three bills pending in the U.S. Senate to create a National Science Foundation, and naturally ONR, as sort of a forerunner of a National Science Foundation, has an interest in what kind of legislation is passed. Now, I can't tell you what to do when you get there, but I would like to tell you what kind of a National Science Foundation ONR would like to have. I am asking you to go to the Hill, become acquainted with the committee responsible for these hearings, and do anything you can to contribute to the passage of a good National Science Foundation bill."

With these instructions I went to the Hill, introduced myself to the persons responsible for arranging the hearings, and offered my services. Shortly after I was joined in this role by Barry Commoner, also a naval officer, and two or three other federal employees on loan to the Committee. I was told that our task was to help secure testimony from respected scientists, but I discovered that this included many other responsibilities. I therefore familiarized myself with the details of each of the three proposed Senate bills, noting where they agreed and where they disagreed. I soon found this to be invaluable information as I briefed prospective witnesses before they testified. The typical scientist-witness, I discovered, was totally unfamiliar with the similarities and differences between the bills and so as I explained to these prospective witnesses that each might be questioned on these issues, I asked "What is your position?" When I found that the witness did not have a position, I proceeded to tell them what in my best judgment was the position most likely to be beneficial to the country in the long run. This was a fairly heady experience, because the scientists called in to provide testimony included some of the outstanding scientific names of the day: Robert Oppenheimer, Edward Teller, etc.

Shortly after I began these duties on the Hill, I was called by the Admiral in charge of the Bureau of Medicine and Surgery, who said, "I hear that Admiral 'X' has you up on the Hill in connection with the proposed National Science Foundation. Now, I'd like to

ARCHIVAL REVIVAL tell you what the Bureau of Medicine and Surgery would like to see come out of this legislation. I listened politely only to discover that many of this admiral's desires did not coincide with those of my own admiral. And a couple of weeks later I was called by the admiral in charge of the entire naval operations and went through the same routine only to find that his wishes did not coincide completely with those of either of the two admirals. Since each of them assured me they did not want to tell me what positions to support or promote, I decided that this left me completely free to influence the legislation according to my best judgment.

I also soon discovered that there were many mechanical details involved in arranging for and holding these hearings. I found that Senators are very busy people, but learned that, for a hearing to be official, it must be chaired by a member of the appropriate committee. I recall that on several occasions I had a great deal of difficulty in finding even one member of the committee after searching the Senate, the Senate Office Building, and the Senate Dining Room.

Even though only one or occasionally two members of the Senate Committee were present to hear the testimony of our usually well-briefed scientist-witnesses, good stenographic services and the Government Printing Office soon provided copies of the statements of those who expressed their opinions on the proposed science legislation. But, because many of these tended to be long, and because the Senators were very busy people, we soon learned that the testimony was neither heard nor read by all of the committee and certainly not by the other members of the Senate, who would eventually have to vote on a bill!

It was at this point that the several "assistants" on loan to the Committee's staff began preparing summary condensations of the testimony, which hopefully the senators would find time to read. And we discovered that if such summaries arrived at the Government Printing Office by midnight, and were accompanied with a requisition and Senator's official stamp, they would be set in type, printed, and delivered by 8 A.M. the next morning! Thus, once again, I found myself writing what would become "publications" without any indication of authorship. And because those of us doing this work were also very busy, the manuscripts of these condensations were often delivered to the printing office only a few minutes before midnight! But none of us complained about the long hours and often hectic schedule of our work. This was because we were all excited about the probability of our work ending up

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With a National Science Foundation which would²⁴⁸ best serve scientists, universities, and the nation -- in the creation of which we could all be proud to have made even a small contribution.

Fortunately, the task was not too difficult. There was general agreement among all witnesses that an NSF was neededt. There were no critical disagreements between the Democratic and Republican senators (One asked me to explain the difference between social science and socialism!). Of course there were many specific issues on which there were marked differences of opinion: whether or not the NSF should support classified research, who would profit from new discoveries and potential patents which might emerge, etc. But finally, acceptable compromises were reached on all such issues, and Congress passed the bill in 1946. My personal rewards for this year "on the Hill" were several: in addition to receiving my regular pay as a commander, USNR, I felt that I had made a definite contribution to the creation of the NSF, I had the opportunity of learning a great deal about the federal legislative process, I had gotten to meet and in many cases became friendly with several senators, with eminent scientists who participated in the hearings, with other scientists who served as "loaners" to the committee -- all-in-all a somewhat heady experience for a college professor. And I received a handsome and sturdily bound volume of the hearings. At one point, I recall thinking briefly about the possibility of becoming a senator myself, but quickly realized the ridiculousness of this idea!.

Because of my dedication to the job of creating the NSF, I remained on active duty status several months after I could have returned to civilian life. Since I had been on leave from a tenured position at Purdue since late 1942, I had not given serious thought to any alternative but returning to Purdue. However, during my year "on the Hill" I began receiving offers of other academic appointments: the University of Maryland, Ohio State University, and others. Because I was so engrossed with the NSF legislation, I had refused to commit myself to any institution. Finally, my wife Lillian insisted that I either accept or reject each of these job offers, and asked me where I really wanted to go. I told her that I'd really like to go to cthe University of Michigan and help my old friend, Don Marquis, who had recently accepted the chairmanship of the Department of Psychology there, to develop what had been a relatively weak department into one of the best in the country. She asked me if I had mentioned this

desire to Marquis. My response was, "No, I am^{not} going to ask for a position there, but I hope he will invite me to join him."

A couple of weeks after this conversation, I attended an AAAS convention in St. Louis. Among the first people I met on arrival were Don and Dorothy Marquis, and they invited me to have dinner with them the same evening. Early in our dinner, Don told me that he would very much like us to visit Ann Arbor, because he wanted me to assist in building a large and good department there! Of course I accepted his invitation, and shortly thereafter, Lillian and I went to Ann Arbor. After conferences with other staff members of the Department and the college Dean, Don offered me an appointment as Professor of Psychology with primary responsibility for developing a strong program in Clinical Psychology. Naturally, I accepted this offer at once and even before we returned to Washington to prepare to move to Ann Arbor, my appointment was approved by the University of Michigan administration and the Board of Regents: salary, \$6,400 for the academic year.

But before leaving Ann Arbor, I had another conference with Marquis. He told me that although my appointment was now firm, and that he was delighted that I had agreed to accept it, he was sorry to have to inform me that the budget of the Department was so tight that it would cover only half of my salary -- that I would have to earn the other half as a consultant to the Veteran's Administration! Naturally, I was shocked by this information, but assured Don that I would join his staff anyhow.

In order to comprehend the nature of my role at the University of Michigan, it is essential that the reader know a little more about Don Marquis, its chairman, whom I had joined. Instead of attempting to do so in my own words, I quote the following two paragraphs by Robert Sears (1973), who prepared a memorial statement for Don:

By 1945 the war was over. Don was tired, but Washington had been a heady experience. He was restless. The University of Michigan needed a vigorous young entrepreneur to revitalize its department, to build up the faculty, to provide the facilities for the massive host of returning veterans. Marquis was an obvious choice and he accepted the challenge.

The next dozen years were a golden age

for Michigan. With incredible ingenuity, Marquis put together half-salaries, research grants, VA stipends, training grants, industrial contracts, to bring together a faculty to satisfy the pressing needs of the university. He brought them in individually, many of them youngsters with promise, as well as in virtually wholesale lots. There was the Survey Research Center and the Center for Group Dynamics, and later there was the Mental Health Research Institute. The story of Michigan's growth, of its development of cross-disciplinary graduate programs, of its emphasis on training Ph.D.s to teach as well as to do research, is not a one-man story. Characteristic of Don Marquis was the quiet way in which he stimulated others to do all the things that had to be done -- and were.

From the very beginning, Marquis and I collaborated in building the new Department of Psychology at the University of Michigan. Although I was never officially designated associate chairman of the department, in reality I functioned in that role. Marquis and I conferred often and closely in making all of the major decisions regarding staff, curriculum, admissions, etc. He was far more able than I in dealing with the high levels of administration, with research granting organizations, and in attracting the several large groups of staff members who joined the University and held appointments in the Department of Psychology and related departments. I, on the other hand, found myself charged with administering most of the internal, day-to-day affairs of the Department. This, of course, included, with the help of a graduate committee, the selection of graduate students from amongst the large number of applicants at the time. I also taught my share of the heavy teaching load, in spite of the fact that, technically, I was only employed on a half-time basis.

James G. Miller had been closely associated with the OSS assessment program employed during World War II to select people for hazardous overseas and often behind the lines assignments (OSS Assessment Staff, 1948). This program employed many unique and as yet untested assessment procedures, which unfortunately could not be validated or invalidated because of a lack of suitable criterion measures. He was very much interested in an assessment program which would put these measures through extensive and critical evaluations. As Chief Clinical Psychologist of the Veterans Administration in 1946, Miller and the University of Michigan negotiated a major research

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TRACT WHICH WOULD PERMIT AN EVALUATION OF THE OSS PROCEDURES AS WELL AS OTHER SELECTION TECHNIQUES IN SELECTING THE LARGE NUMBERS OF CLINICAL PSYCHOLOGISTS THEN BEING TRAINED BY THE VETERANS ADMINISTRATION IN GRADUATE DEPARTMENTS OF MANY UNIVERSITIES. I BECAME DIRECTOR OF THIS FIVE YEAR RESEARCH PROGRAM, BUT WAS VERY ABLY ASSISTED BY DON FISKE, WHO HAD COME TO THE UNIVERSITY OF MICHIGAN TO COMPLETE HIS OWN PH.D. DEGREE, ALTHOUGH HE HAD BEEN A GRADUATE STUDENT AT HARVARD BEFORE WORLD WAR II.

I FIRST KNEW DON IN THE FALL OF 1942 AS A YOUNG ENSIGN IN THE PSYCHOLOGY UNIT OF THE BUREAU OF MEDICINE AND SURGERY OF THE U.S. NAVY. SHORTLY AFTERWARD, HOWEVER, HIS SERVICES WERE REQUESTED BY THE OSS ASSESSMENT STAFF, AND HE SPENT TWO OF THE WAR YEARS AT STATION S (TO THIS DAY, I DON'T KNOW WHETHER THE "S" STOOD FOR "SECRET", OR HAD SOME OTHER MEANING!). THUS, DON FISKE WAS AN INVALUABLE RESOURCE TO ME IN PLANNING OUR FIVE YEAR ASSESSMENT PROGRAM AND ALTHOUGH HE DID NOT RECEIVE HIS PH.D. UNTIL 1948, HE SERVED AS ASSOCIATE PROJECT DIRECTOR AND LATER AS CO-AUTHOR OF OUR MAJOR REPORT ON THESE STUDIES (BIBLIOGRAPHY #21).

I CALL ATTENTION TO THE FOLLOWING FEW FACTS. SIXTEEN CONSULTANTS WERE UTILIZED IN THE EXPERIMENTAL DESIGN AND PLANNING OF THE DETAILS OF THIS PROJECT. THE PROFESSIONAL AND TECHNICAL STAFF INVOLVED TWENTY-FOUR INDIVIDUALS, SEVEN OF WHOM SERVED MAJOR ROLES. SIX ASSESSMENT PROGRAMS WERE HELD BETWEEN THE FALL OF 1946 AND THE SUMMER OF 1948, AND INVOLVED A STAFF TOTALLING 99 DIFFERENT INDIVIDUALS.

AS THE PRINCIPAL INVESTIGATOR OF THE ASSESSMENT PROJECT AND SENIOR AUTHOR OF THE BOOK REPORTING ITS FINDINGS, I FIND IT DIFFICULT TO EVALUATE THE IMPACT AND THE VALUE OF THIS FIVE-YEAR PROGRAM. BUT AFTER RECENTLY REREADING THE KELLY-FISKE VOLUME, THIRTY-FOUR YEARS AFTER ITS PUBLICATION, AND AT THE RISK OF APPEARING TO SEEM IMMODEST, I NOW JUDGE BOTH THE PROJECT AND THE BOOK TO HAVE BEEN A VERY SIGNIFICANT CONTRIBUTION TO THE DISCIPLINE OF PSYCHOLOGY FOR THE FOLLOWING REASONS: (1) THE BROAD AND ECLECTIC APPROACH IN CHOOSING ASSESSMENT PREDICTOR TECHNIQUES, UTILIZING OBJECTIVE, SUBJECTIVE, PROJECTIVE, CLINICAL, AND QUALITATIVE VARIABLES, (2) ITS CLEAR RECOGNITION OF THE COMPLEXITY OF ALTERNATIVE CRITERIA OF PROFESSIONAL COMPETENCE AND THE POSSIBLE LACK OF CORRELATION AMONG THEM, (3) ROUTINE USE OF RATINGS WHICH PERMITTED THE STATISTICAL EVALUATION OF CLINICAL JUDGMENTS, THUS MAKING POSSIBLE DIRECT COMPARISON OF THEIR PREDICTIVE VALUE AS COMPARED WITH THAT OF OBJECTIVE TEST SCORES, AND (4) THE ACTUAL FINDINGS, WHICH REVEALED THAT THE ALLEGEDLY SUPERIOR VALIDITY OF SEVERAL TECHNIQUES HIGHLY VALUED BY CLINICIANS WAS FOR THE MOST PART

illusory, and hence did not justify their cost in professional time and effort.

I realize that my evaluations are not wholly shared by some of those who served as members of the several assessment staffs and many of the procedures which were found by us to be of dubious value are still widely used by many clinicians, and are still taught in some graduate schools. Unfortunately, strongly held belief systems are not likely to be modified by research data, no matter how well the research was executed. But even so, I would argue strenuously that the VA Assessment Project on Clinical Psychologists was far more productive and had more impact than did the parallel program on the assessment of psychiatrists, carried out at the same time at the Menninger Foundation by Holt. Frankly, I found the publications resulting from that project were so lacking in value that I did not retain them in my file, and therefore do not have a bibliographic reference for them.

In 1946, I became a member of the APA Executive Committee on Training in Clinical Psychology (David Shakow, Chairman), and participated in visits to many universities to evaluate and accredit programs. In 1948 I became a member of the Training Committee of the NIMH. At a meeting of this committee in December of 1948 it was decided to arrange for a conference on graduate training in clinical psychology, to be funded by NIMH through a grant to APA. The APA Board of Directors, in turn, created a conference executive committee responsible for the conference. It was composed of two members of the Shakow committee, two representatives of training universities, and one to represent the committee of university departmental chairmen. Eventually, the committee was expanded to include a representative of the U.S. Public Health Service, and one from the APA Central Office. And, after it was decided to hold a conference on the campus of the University of Colorado in August, 1949, Victor Raimy was added to the committee to serve as an administrative officer. For reasons not entirely clear to me, I was named chairman of this executive committee, and later served as chairman at all sessions of the conference itself.

During our preliminary meetings, the executive committee decided that participation in conference meetings should be limited to those who were immediately concerned with problems of training in the universities and in the field agencies, particularly at the doctoral level, the only level accredited by the American Psychological Association. Forty-two universities had received Association approval in some degree by that time for their training programs, and

representatives of 41 universities attended the conference. In addition to the university representatives, clinical psychologists were selected to represent supervisors in typical field training installations such as the VA, state hospitals, community agencies, and medical school clinics. Because of the many and varied relationships with government agencies, an official was also invited from the Veterans Administration Training Program, the Surgeon General's Office of the Department of the Army, and the United States Public Health Service. Finally, because clinical psychologists were only one arm of the mental health team, representatives were also invited from the American Psychiatric Association, the American Association of Psychiatric Social Workers, Psychiatric and Public Welfare Nursing, and the Division of Counseling and Guidance of the APA.

By circulating lists of questions related to problems of training, the executive committee eventually settled upon a preliminary agenda that began with discussions of fundamental problems and later took up more specific fields of training. As was anticipated, the original program was revised several times during the conference, as the need for more time on particular questions became apparent. The very pleasant climate of Boulder and the extremely comfortable facilities provided by the University of Colorado contributed greatly to a successful conference. As this was to be primarily a working conference, the 71 participants were divided into subgroups of 17 or 18 persons which met for a session each morning, afternoon, and evening. These subgroups were decided by shuffling a deck of cards, thus permitting each of the participants to get to work directly with all of the others. Each participant was designated once as chairman and once as recorder for one of the subgroups. Each subgroup met for two hours to discuss the same general topic, and to prepare a written report covering its deliberations and major conclusions. The chairman and recorder for each particular topic prepared a general report for that topic, reports which were duplicated and distributed to all participants as preparation for the general session devoted to each of the topics. All of the general sessions were recorded and typescripts of the recordings were later made available.

During the last three days of the conference, the general sessions were extended to four hours in order to permit more discussion, revision, and voting on the many resolutions which had been proposed by the subgroups.

In addition to his extremely valuable services in

coordinating the facilities of the University of Colorado, Victor Raimy assumed the responsibility for compiling and editing the book *Training in Clinical Psychology*, published by Prentice-Hall in 1950.

Although the flyleaf of this volume clearly states that Raimy was its author, for some reason my name appeared on the spine of the bound volume, and I insisted that the publisher provide a red gummed stripe to cover my name on all of the books we could trace.

Probably the most significant of the many resolutions of the Boulder Conference was that stating that clinicians should be trained as both scientists and practitioners. Although not supported strongly by all of the 71 participants, it was overwhelmingly adopted because of the generally recognized fact that none of the other mental health professions made any effort to train their members to conduct the kinds of investigations crucially needed to answer a host of unresolved questions in the field -- theoretical, methodological, and practical. In brief, it was agreed that much research was needed to evaluate the validity, efficiency, and efficacy of then currently used techniques of diagnosis and therapy, so that at least one of the members of the mental health team would be selected and trained to pursue research on such issues. But the Boulder Conference recommendation seems to have had relatively little impact for a vast majority of clinical psychologists. Even for those trained in superior programs, their Ph.D. dissertations constitutes the only research effort, and often even that has not been published. Regretfully, I must admit that this is true even for those trained at the University of Michigan, where, over the years, I supervised 42 doctoral dissertations. But the absence of research has not served to slow down the growth of the field!

Because many of us felt strongly that well-trained psychologists deserved public recognition of their competence, we proposed and succeeded in promoting legislation providing for the certification of qualified psychologists. As President of the Michigan Psychological Association in 1948, I was successful in securing a certification bill in Michigan -- in spite of vigorous, and sometimes nasty opposition from organized medical and psychiatric groups. But certification was not to endure. With the increasing number of psychologists engaged in private practice (full- or part-time), and the possibility of third-party reimbursement for services, psychologists both in Michigan and elsewhere decided that psychologists should be licensed rather than merely certified. Both in Michigan and at the national level, I argued strongly against the licensing of psychologists. My sole reason

for doing so was in no way personally motivated, but based on the firm conviction that licensing should continue to mean exactly what all dictionaries say it should mean: "the authorization by law limiting specific acts to those who have been evaluated and judged qualified to perform specific acts." Thus, only licensed barbers may cut hair, give shampoos, beauticians may cut hair, give permanents, etc. Only licensed electricians may install wiring and electrical fixtures in buildings, and only licensed physicians may prescribe drugs and do surgery. It is important to note that these are all specific acts and all are potentially harmful to society if not carried out by properly qualified personnel. Note also that psychiatrists are licensed only as physicians, and nothing in their license specifies that they may carry out psychotherapy.

In their mad scramble, psychologists either didn't bother to read the dictionary definition of licensing, or were so concerned about achieving what they thought would be equal status with the medical profession, that in most states they raised war chests, hired attorneys, lobbyists, and proceeded to persuade legislators to create a licensing bill for psychologists. But, still, in my opinion, the victory was a hollow one since it turns out that the only specific activity that they were able to prohibit to non-licensed persons was to hold oneself forth to the public as a psychologist! When licensing was accomplished in Michigan, I was offered a license, but on conscientious grounds refused to accept it. As a result, I, in spite of a somewhat illustrious career in the field of psychology, am not legally allowed to hold myself forth as a psychologist to the public. Throughout my long professional career, I have engaged in educational and vocational counseling, learned and occasionally used hypnotherapy, and supervised many clinical students in doing psychotherapy. The fact that I do not have a license does not bother me in the slightest, because, like teachers, educational and vocational counselors, social workers, pastoral counselors, and others, I may engage in any of the activities I wish as long as I do not hold myself forth to be a psychologist! But since I am now too old and frail to undertake clinical activities, it matters little to me. I do worry somewhat, however, lest the apparent success of psychologists in achieving legal status apparently somewhat comparable to that of psychiatrists may lead to similar efforts on the part of other professional groups -- social workers, counselors, etc. If these other groups are successful in achieving licensing, I am afraid that it will be licensing in name only, rather than the limiting of specifically damaging activities to individuals properly qualified.

This problem is further complicated by the fact that third party reimbursement for psychotherapy was originally designed to apply only to psychotherapy conducted to cure or ameliorate mental disease. I am totally unwilling to assume that all persons who seek and deserve counseling in modern society are suffering from disease, and while I would argue to the hilt that many of the persons engaging in counseling and psychotherapy are as effective (or as ineffective!) as psychiatrists in such therapies, I am not at all sure that all persons who can profit from counseling or psychotherapy should have their expenses in securing professional help reimbursed by third party insurance. To summarize, the situation is indeed a messy one and I am less than proud of my colleagues in psychology who in their scramble to achieve licensing for themselves acted as something less than public-minded citizens in our democracy.

In reviewing the records of my activities during my first few years at Michigan, I find it difficult to believe that I was so involved in so many roles. Fortunately, the missing half of my salary at the University had to be earned but for one year as Branch Chief Clinical Psychologist of the VA Regional Office. The research contract for the VA Assessment project was generous enough to cover my half salary in 1947-48 and in 1949 I began earning it as half time Chief of the Psychological Clinic of the Institute for Human Adjustment, which is supported by Rackham funds administered by the Graduate School. In this role I supervised several other members of the clinical staff as well as clinical trainees obtaining practical experience as interns in a clinic. Even so, I found time to do some work with clinic patients myself. I continued in this role for three or four years when, upon the retirement of the Director of the Institute, I assumed that role, a position which I held until my retirement from the University in 1974.

But, in addition to my work in the Department teaching, chairing the graduate committee with responsibility for selection of our incoming graduate students, and my unofficial role as an "associate chairman", I found myself involved with many other responsibilities. I served as consultant to the Office of Naval Research (1946-1951), to both the Central and Regional Offices of the VA from 1947-1956, and the Selective Service System from 1948-1953. I was also a member of the NIMH Training Committee from 1948-1954 and of the Research Study Section of NIMH from 1948-1962. These responsibilities entailed many time-

consuming trips to Washington (and Bethesda). I enjoyed these roles, especially getting to know and work with other psychologists. In 1950 I was elected to a three-year term as a member of the APA Board of Directors.

I was, of course, extremely frustrated by my inability to pick up and continue my research on personality and marriage which I had shelved on entering the Navy in 1942. But finally, with help of a small research grant from the Faculty Research Fund of the University of Michigan, I was able to turn my attention to this longitudinal study of 300 engaged couples first assessed during 1935-38. With much help from my second wife, Lillian, 1952-53 was spent in digging out the files regarding all previously collected data, and planning a large-scale follow-up study to be carried out in 1953-54.

This follow-up study consisted of recontacting as many as possible of the original 600 subjects, securing a minimum report on the present outcome of the marriage or engagement, and inviting all to participate in the final stage of the study, which included (a) retesting on five of the seven psychological tests used in the original battery, and (b) reporting in detail on the marriage between research partners, and other intervening life experiences.

In spite of the fact that 16 to 18 years had elapsed since the original testing, we were successful in securing definitive information regarding the outcome of all 300 original engagements: 278 of the original 300 engagements had resulted in marriage of the partners. There were 22 broken engagements, and by 1954, of the 278 marriages, 12 were terminated by death and 39 by divorce. So it was that after nearly 20 years, 454 of the original 600 persons were still living as husband and wife in 227 marriages. Although the time demands on the subjects for this retest program were considerable, 86% of the subjects participated in this follow-up phase of the study, and of course, this resulted in massive amounts of new data on the 521 subjects responding.

Thanks to a generous research grant from the Foundation's fund for Research in Psychiatry, I was able to employ several very able research assistants to score the tests, code responses to the questionnaires, and punch the data on the cards. Even so, the thought of carrying out the necessarily complex statistical analyses of both the original and new data using IBM mechanical card equipment was an appalling one. Fortunately, about that time IBM brought out its first electronic computer, the 650. Thanks to the help of a good friend and colleague, Jim Miller, I was given

permission to use the only 650 in the area free of charge, if we agreed to prepare the necessary programs, and to use the 650 in the Detroit office only at night! Even this appeared to be a real opportunity to speed up the data analysis, and also to learn to use an electronic computer, so we accepted the offer. Since I could not take the time to learn computer programming, I used a portion of my research grant to send a brilliant young man to IBM school to prepare the most essential programs needed in our statistical analysis. Little did I realize the problems that would ensue!

Early in 1954 I was one of the five nominees for APA President. I was not greatly surprised by my nomination, since my many activities had resulted in notoriety and name recognition, but not necessarily fame. Thus, although I was not surprised at having been nominated as a candidate for the APA presidency, I had no illusions that I would be elected. In fact, I even offered to bet high odds against the possibility of such an event. My reasons were as follows: (1) while originally identified with experimental psychology, and a Fellow of Division 3, I felt that members of that Division would regard me as having "left the field" with my forays into selection and training, not only of aircraft pilots, but -- horror of horrors -- clinical psychologists! (2) While I had been elected President of Division 13, the number of consulting psychologists in that division was relatively small. And (3), most importantly, I felt that I had probably offended a significant portion of the much larger group of clinical psychologists (Division 12) by the critical evaluation of popular clinical techniques of the VA Assessment Project, and by having been so clearly identified with the scientist-practitioner model for the clinical psychologist at the Boulder Conference.

But my evaluation of the likelihood of my election turned out to be wrong. To my amazement (and I suspect that of most of my colleagues), I led the ballot, and in 1954 found myself President-Elect of the APA and destined for another three year term on its Board of Directors. Even more importantly, I was faced with the necessity of giving serious attention to my Presidential Address for September, 1955, the title of which had to be submitted early in the year to appear in the convention program. Because I had never been seriously interested in psychological theories, I was not about to propose a paper on psychological theory. As a methodologist, I might have discussed the things we learned in the VA Assessment Program, but that had already been published. Consequently, in anticipation of the results of the complete statistical analysis of the marriage data, both the original and reassessment data, I submitted the title "Consistency of the Adult

"Personality." This meant that the major data analyses had to be completed during the summer of 1955. This, in turn, meant many long night sessions by Lillian and me with the 650 computer in Detroit.

It was then that we began to learn of the host of problems confronting us in our first use of the computer: (1) we found that both the untested programs and all data had to be entered using IBM punch cards. (2) The computer had a very limited magnetic drum memory with only 4,000 locations. This necessitated analyzing the data in rather small batches; a 20x20 correlation matrix was the largest possible output, and it required several hours of computation. (3) The output of the computer was in the form of punch cards, which then had to be put through a mechanical tabulator to obtain a readable printout. (4) Since we were working at night during the summer months, we encountered serious problems with high humidity, often resulting in cards being destroyed both when entered into the machine, and when the results were being punched out. (5) We found that any momentary loss of AC power (as in an electrical storm) resulted in complete erasure of all information on the magnetic memory drum! This often occurred after a specific block of analysis was 90% complete, and necessitated our doing it all over again with our fingers crossed. (6) Since this primitive computer utilized hundreds of vacuum tubes in its circuitry, the failure of any one would necessitate our ceasing operation until an IBM technician could locate and replace the defective tube. (7) Finally, we found that, because of a less than accurate and complete programming manual, one of our most used programs gave what appeared to be correct results about half the time, and during the other half the results were totally impossible values. We finally identified the problem as follows: The programming manual provided two instructions for "divide". Eventually we learned that one of them instructed the computer to divide the number at one location by that at another specified location and clear the accumulator of any remainder. The other divide instruction instructed the computer to divide and retain any remainder in the accumulator! So, depending on the magnitude of this remainder, it made a real difference in the subsequent computations.

But in spite of the many time-consuming delays, enough of the analyses were completed so that I was able to prepare my APA presidential address (Bibliography #28) in time to permit the Kelly family to drive to San Francisco by way of Yellowstone and Yosemite Parks, camping out at nights. One of the most memorable of these was the night we camped out in the redwood forest north of San Francisco.

As President, my family and I were assigned a lovely guest suite in the famous St. Francis Hotel in San Francisco. As APA president I was, of course, busy with board and business meetings leading up to my address on the night of September 4, 1955. While getting on my tuxedo in preparation for that session, San Francisco experienced one of its frequent earthquakes. While this one resulted in no serious damage, I recall remarking to my family, "Why on earth couldn't nature have waited a couple of hours and arranged for this event at the time I was just completing my earth-shaking presidential address?" I have no illusions that my presidential address was indeed "earth-shaking", but after having listened to many, and read many others, I now regard it as falling somewhere in the midrange of such presentations. On rereading it 30 years later, I realize that it was too long, and that I had attempted to present too much data in such an address. I guess I was tempted to do so by virtue of the fact that this was the first publication of my extended longitudinal study, and could not resist including more data than was really necessary. Perhaps it is well that I did so, since the next published report based on the longitudinal study of personality and marriage would not be published until thirty years later (for reasons to be narrated shortly).

The principal results reported in the 1955 paper were these: (1) The personalities of adults are neither so changeable as some theories would have predicted, nor as fixed as would have been expected from other theories. (2) Of the variables assessed the most consistent over time were intelligence, values, and interest patterns. The self-concept as indicated by self-ratings was fairly consistent, but the least consistent of all were attitudes. (3) Differences over time in the mean values of most variables were few and generally small in value. (4) Sex differences in these changes were few and minor.

At the time of the analysis, there were widely held yet conflicting opinions regarding the nature of assortative mating. One was that opposites attract; the other was that likes attract. I had already analyzed the data with respect to assortative mating, and found both beliefs to be justified, but in a somewhat surprising fashion. For the 22 engaged couples who later broke their engagement, there was a distinct tendency for opposites to attract, e.g., tall pairing with short, bright with dull, and even those with marked religious differences. However, such pairings during the engagement period, while intriguing and somewhat exotic, do not appear to have led to marriage. Likes attracting likes, however, seem to have been especially characteristic for the 39 couples who were

later divorced; in fact, the husband-wife correlations for many variables were in the same range as those for identical twins! However, for the large majority of couples, i.e., those who married and remained married, the husband-wife correlation was positive for practically all variables, but only of about the same magnitude as those characterizing the correlation of scores for siblings.

While it is commonly believed that persons married to each other tend with the passing years to become more and more similar (in fact, it has even been written by some authors that the principle holds for physical appearance), obviously our data provided no direct test of this hypothesis.

In 1937 I had prepared a preliminary report on psychological factors in assortative mating (Kelly, 1937). In that analysis, I had found that the husband-wife correlations were positive for practically all variables ranging from -.02 to .58. In other words, we found that for married couples there was no evidence to support the opinion that opposites attract.

What about the time 2 correlations? To our surprise we found that in general they proved to be no different than those at the time of original testing. Actually, they were slightly smaller for 21 of the 38 variables, and the few statistically significant shifts were in the direction of couples becoming slightly less similar after living together as husbands and wives for nearly 20 years. Apparently the initial similarity between them was adequate to establish and maintain a cohesive relationship without the need to become more alike. While one can think of many environmental pressures tending to promote increasing congruence between mates, one must not overlook the apparently equal impact of centrifugal forces associated with maintaining the many kinds of role differentiations expected of husbands and wives in our culture.

In addition to analyzing the marriage data, and preparing my 1955 presidential address, I also found the time to prepare a review of the current theories and techniques of assessment for the Annual Review of Psychology (Bibliography #25). And in spite of the many difficulties we'd encountered in using the IBM 650 electronic computer, I began an effort to persuade the University of Michigan that a computer was essential for scholarly research in the University. In this effort I encountered considerable resistance from the Dean of the Graduate School, a highly competent physicist. Eventually we acquired a 650 at the University, which of course has been regularly updated since that time with newer and newer models of

computers until now we have a computer network linking the University of Michigan computer with those of many other institutions, as well as computer terminals in all major research offices in the University.

About the time Fiske and I completed the book length report on the Assessment of Clinical Psychologists, I was approached by the late Wayne Whittaker, Assistant Dean of the University of Michigan Medical School, and chairman of its admissions committee. Whittaker was Professor of Anatomy, held a Ph.D., and was highly respected for his efforts to improve the selection of students for the medical school. My immediate response to his request was to say that if his only interest was in predicting medical school grades, I was not interested in collaboration. In the course of a luncheon conversation, Whittaker convinced me that his concern was much broader and more socially responsible than I had anticipated. I distinctly recall his statement, "You know, when the Medical Admissions Committee says 'yes' to Jones and 'no' to Smith, they are making a decision that has to do with the allocation of a considerable proportion of society's financial resources. Furthermore, they are saying to one youngster, 'you may have a medical education largely at society's expense', and, to the other one, 'you may not, you don't deserve it'." Perhaps more importantly, Wayne added, "They are saying to the first one, by virtue of the small proportion of failures, you have an opportunity, in fact, you are almost guaranteed a chance of earning thousands of dollars more annually than if we say no."

He then went on to say that they would like to know how to select the kind of persons who might be inclined to return at least a part of society's investment. To me this was a challenging conception of an important problem, and shortly thereafter, with the aid of two small research grants from the Medical School, we went to work, assisted by one of my graduate students, Leonard Uhr. We began by testing two classes of medical students, the class of 1952 as seniors, and the class of 1956 tested as applicants in 1952. Of equal importance was the fact that we agreed to develop as many alternative criterion measures of professional competence as was feasible. In carrying out these projects, we received much help from Gordon Bechtel, another graduate student, my wife Lillian, and G. W. Dickinson of the University of Illinois, whose high speed computer and programs made possible certain analyses which were unable to complete at the University of Michigan at the time.

Eventually, we developed measures of 100 potential predictor variables and 54 criterion variables; hence

the need for the high speed electronic computer. These two related projects resulted in two of what I regard as my most significant publications (Bibliography #30 and #46).

On the whole, the findings of these two projects on the selection and performance of medical students confirmed and extended the findings of the assessment project on clinical psychologists: (1) Intellectual performance in academic coursework is best predicted by premedical grades and objective tests such as the MCAT. (2) A large number of logically and socially defensible alternative criterion measures are relatively uncorrelated with academic performance. (3) Many of these alternative criterion measures are better predicted by non-cognitive, i.e., personality variables than by the usual type of aptitude tests. And (4) the personal interviews carried out by the five member selection committee added practically nothing to the prediction of any of the criterion measures. This latter finding resulted in two additional publications (Bibliography #24 and #31).

During the last few years of the medical student project, I was asked to serve as a member of the selection committee. Upon reviewing the results of our research, and in view of the relatively large number of applicants as compared with open places in the medical school freshman class, the committee as a whole concluded that the selection interview was no longer justified. We recommended to the Dean of the Medical School that, after elimination of the least promising applicants on the basis of grades and test scores, the actual selection of students from that point on be conducted on the basis of a lottery. The Dean reacted very negatively to this proposal, and assured us that if the admissions committee did not know how to select good medical students, the Dean's office did! At that point, I resigned from the committee. Since I have had no contact with the selection procedures since that time, I do not know what present practices are.

I do know that our findings, based on these two classes of medical students at the University of Michigan, forced me to some unpleasant conclusions regarding the nature of today's physicians. As a young man I had perhaps an over-idealized conception of the profession, believing that most physicians were primarily devoted to taking care of the sick, doing good work, helping out in community services, and promoting medical research. But after examining and analyzing the data obtained on our subjects, I was

forced to very different conclusions. Based on their scores on the Strong Vocational Interest Blank (VIB), our medical students were found to be individuals who, if not planning to become physicians, would be more likely to become manufacturers, big business men, production managers and engineers, certainly not the kind of people who would become teachers, ministers, social workers, etc. As a group, the medical students revealed remarkably little interest in the welfare of human beings. One of their highest secondary scores was on the Farmer key on the Strong VIB, a key derived from the modal interest patterns of highly successful graduates of scientific agricultural schools, whose primary interests and values were that of increasing production by the application of science. Such persons are not scientific in the sense that they wish to discover new truths, but rather with the application of science toward the goal of increasing production. Our medical students also scored relatively high on the Aviator's scale of the Strong, based on the modal interest of commercial pilots, military pilots, etc. The one thing that they have in common is maleness and the lack of interest in anything cultural. I can only hope that the situation has changed with the increasing numbers of young women entering the medical profession today.

From the time he was eight years old, our only son had expressed a continuing interest in becoming a physician, and enrolled as an undergraduate at the University of Michigan in the four year premedical program. At no time during these research projects had I discussed any of my findings with him, and hence I was somewhat shocked at a conversation with him during the middle of his sophomore year. During his first two years of college, he lived away from home in a fraternity heavily populated with other pre-med students. One evening he returned home to have dinner with his parents, and was extremely glum. When I inquired as to the nature of his current problem, he blurted out, "Dad, Mother, would you mind terribly if I don't want to become a doctor?" I assured him that, as a counseling psychologist I wanted him to do whatever was best for him, but wondered why, since he had been so persistent in his expectations of becoming a physician, he had now changed his mind. His immediate response was, "I can't stand my fellow pre-meds!" Of course, I asked why, and again his response was very firm. He had concluded that his fellow pre-med students were not the slightest bit interested in the science courses they were taking in their pre-med years, but, being bright and studious, they were bound to make good grades in them in order to get into medical school. But what troubled him the most, he said, was that most of them were not in the slightest interested in science

for science's sake, nor in serving their fellow men, but only in figuring out a way to make the highest possible income in medicine.

Of course I was not too surprised to learn of his conclusions, based on his association with pre-med students, since for the most part they conformed to the facts which we had been discovering in our five-year research project. At that point I asked him what he wanted to do with his life, and he said that he proposed to transfer to Western Michigan University, major in biology, and eventually become a research scientist in a medically related field. Parenthetically, it is a pleasure to report that he was eminently successful in this endeavor and a few years later received his Ph.D. in endocrinology at the University of Wisconsin. His record there was so outstanding that he was awarded a Medical Research Council fellowship in Canada, and has been an extremely productive and well-recognized investigator in that field ever since. And, in spite of the fact that he has a Ph.D., rather than an M.D., he has the title of Professor of Medicine at McGill University, and receives continued substantial research support from both the Medical Research Council in Canada and from other foundations.

In 1956 one of my abler graduate students, Lewis Goldberg and I began a follow-up study of the clinical psychologists earlier assessed by Kelly and Fiske. The results of this follow-up study, supported in part by a grant from the Research budget of the VA Central Office, was published as a Psychological Monograph in 1959 (Bibliography #39).

During the next few years I continued to serve as a consultant to several federal and national agencies. This included a Research Study Section of NIMH (1948-1962); the Accident Prevention Study Committee of NIMH (1960-1962); the Armed Forces Epidemiological Board, Committee on Accident Trauma (1958-1962); the FAA Aeromedical Research Committee (1962-1964); as a member of the Research Committee of the Educational Testing Service (1959-1967); a member of the Board of Directors of the Psychological Corporation (1962-1969); National Merit Scholarship Corporation (1962-1967); the Agency for International Development (1966-1968); and, finally, in 1957 I was elected President of the Division of Clinical Psychology of the APA. My presidential address, entitled "Clinical Psychology, 1960", was a report of survey findings of the field (Bibliography #40).

During the years 1946-1957 Don Marquis and I had worked very closely and effectively together in

building one of the best departments of psychology in the world. He and I were not only close professional colleagues, but my wife and I were also good personal friends of both Marquis and his wife, also a psychologist, typically visiting each other one or two times per week. During the summer of 1957, Marquis invited a younger master's level psychologist to become an assistant on one of his many projects, and from that time on, I began to notice marked changes in his behavior. Our interpersonal relationship became cooler because he knew that I detected a growing affection between him and the young lady recently arrived in the Department. As I personally found her to be relatively unattractive both physically and personality-wise, I could not understand Marquis' fascination with her, but early in the fall of 1957 he suddenly announced that he was resigning as Chairman of the Department, and so advised the Dean of the College. Thus it was that I found myself appointed as Chairman in 1957 for a five-year term. As I have stated, I had been acting pretty much unofficially as associate Chairman, so I fitted rather naturally into the new role but with markedly increased responsibilities, since I was still serving on a half-time basis as director of the Institute of Human Adjustment. Together Marquis and I had assembled a superb staff, and I am happy to say that none of them resigned on his departure. In addition to my administrative duties from 1957-1961, I collaborated with James Miller, Ralph Gerard, and others at the Mental Health Research Institute in a series of studies on psychopharmacology, and we published a few unimportant papers. I also published a few chapters in edited texts (Bibliography #42, #44, and #45).

During the presidential campaign of 1960, my wife and I had waited until two o'clock in the morning to hear John F. Kennedy speak on the steps of the Michigan Union. It was in that brief speech that he first proposed the idea of a Peace Corps, an idea which to me as a continuing idealist seemed most attractive. As is generally known, the Peace Corps was established in 1961, and in the fall of that year I received a telephone call from Sargent Shriver, who had been appointed as its director. He said that they were getting thousands of applications from people to become Peace Corps volunteers, but were able to send only small groups of selected volunteers to carry out specific functions in certain countries with which cooperation had been arranged. He said that, in view of this situation, it was terribly important that we select those volunteers best qualified to carry out the duties proposed for them in each country, and further stated that, on inquiry, he had found that I was best qualified to become Chief of the Selection Division in the new Peace Corps. I told him that while I found the

idea most appealing, I could not possibly consider accepting his offer since I was Chairman of the largest department of psychology in the world. Shriver said that he was sorry to hear that, but urged me to come to Washington to discuss with him the possibility of who might best serve as Chief of Selection.

A few days later I visited the Peace Corps and conferred with Mr. Shriver, who turned out to be a most personable and persuasive gentleman. Before the day ended, I had agreed to accept the position of Chief of Selection for one year only, phoned the Dean of the College to ask him to appoint an acting chairman of the Department of Psychology, and, of course, my wife to tell her that I was renting a small apartment in Georgetown! My year in the Peace Corps turned out to have been extremely rewarding personally, but also extremely fatiguing. Because of the large number of applications which had to be processed and the urgency of getting Peace Corps groups selected, assigned, and trained for their duties in the host countries, the hours were long and often somewhat frenetic. In fact, in retrospect, I do not believe I ever worked harder or such long hours at any time either in graduate school or in my varied professional involvements.

Because the Peace Corps was such a successful venture, by late 1962 the decision was made to hold a major conference reporting on the organization's plans for the future. This was held in Puerto Rico, in a lovely hotel built by one of the Rockefellers. Invitations to the conference were extended to representatives of all the host countries as well as to those of other countries considering the possibility of establishing their own Peace Corps. As a relatively senior member of the Peace Corps staff, both Lillian and I were invited to attend this conference, and we did so in high style. We flew from Washington to San Juan in Air Force II, in the company of then Vice-President Johnson in his shirt sleeves! At San Juan, we were ferried by helicopter to the hotel somewhere in the foothills (I forgot the exact location), and of course both the food and the service were superb. It was indeed a delightful bit of relaxation in an otherwise very hectic year in the Peace Corps.

Just after falling asleep after a long, hard day in the Peace Corps office, I received a telephone call from Sargent Shriver stating that he wanted me to accompany the second group of volunteers scheduled to leave for Chile the following Monday. Although somewhat groggy, I asked why they needed a baby sitter to accompany them on this trip, since their training was designed to make them self-reliant both as individuals and as groups. Shriver told me that there were rumors

of a revolution in Chile, and he would feel more comfortable if a senior Peace Corps official accompanied them. At that point, I asked him just what a senior Peace Corps official should do if the shooting started. His response was, "Act Wisely!" I also recalled that my passport had expired, but Shriver assured me that was no problem (after all, he was the brother-in-law of the President!). I also remember telling him that, while I didn't regard my presence on the charter flight as essential, if I were going to South America I would like to visit our already present Peace Corps groups in Chile, Peru, and Bolivia to see if I could learn how to improve the selection process. Shriver replied, "That's fine with me. Just write your own orders."

Thus, on the following Monday morning, the Chile II group and I departed for Santiago. Fortunately, there was no evidence of a revolution in Santiago or Valparaiso, where I also visited a Peace Corps group. My visits to these several groups, however, revealed certain very real problems for some of the working Peace Corps groups. For example, a group of nurses assigned to Sucre to help modernize nursing techniques in the local hospital was not warmly received by the Catholic sisters in charge. In fact, their duties were limited to one hour a day in the hospital! However, because of their adaptability, these six girls had found other important jobs to be done in the community, and were highly regarded by the local citizenry for their many services.

On the flight from Peru to La Paz I had an opportunity to see lovely Lake Titicaca, high in the Andes. After a few days in La Paz, I decided that I should really visit a group of six volunteers in a high Andean village but 100 miles west. Knowing that the runway of the airport in that village was nestled in a mountain valley, and was somewhat dangerous, especially at the high altitude, I was about to buy a bus ticket for this trip, only to discover that on the previous day the bus had rolled over a mountain cliff and killed all of its occupants. Not surprisingly, I decided to use a DC-3 for the trip.

As I recall, there were only four members of the Peace Corps group in this Andean village, all men, and, I believe, assigned to work with the peasants of the area in an effort to improve both their agriculture and their sheep and goat production. Even though there was a small airport near the village, it was so tiny that there were practically no hotels or shops. In fact, the Peace Corps boys lived and ate at the only pension in town. I found them to be reasonably well-satisfied with the work which they were doing, and remained

overnight in the pension where they were staying. I do not recall the menu of that evening meal, but I do recall that there was no choice of items on the menu, so I ate what was set before me. In particular, I remember enjoying a fresh green salad topped with a cheese dressing.

The next day I returned to Sucre, said farewell to the nurses whom I had gotten to know well there, and returned to Washington via La Paz sometime in late November, 1962. At the Peace Corps I, of course, reported my observations not only to Shriver but to the officers in charge of volunteers in each of the countries in which I had visited. Shriver was good enough to ask me to stay on beyond the year I had promised, but I said that if I had not been able to establish a satisfactory selection program by that time I had failed, and one of my assistants was named Chief of Selection.

Shortly thereafter, I returned to Ann Arbor in an extremely fatigued state, so fatigued, in fact, that I did not even pretend to report to my office at the University, thinking that a rest from the first of December to the first of February would permit me to regain my strength. Since that did not happen, I asked the Dean to appoint Wilbert McKeachie, who had been acting as chairman of the Department, to complete my unfinished term. Even though still tired, I returned to the Department in February of 1963. Somehow I managed to find the energy to teach an undergraduate honors course in elementary psychology, and continued to teach statistics throughout the next ten years. I was also continuing the statistical analysis of the marriage data and drafted several chapters for the proposed book reporting on that longitudinal study. Progress, however, was extremely slow, and I was greatly discouraged by my failure to get more accomplished.

Sometime in 1963, Shriver asked me to accompany Bill Moyers of the Peace Corps staff for a short trip to the Netherlands, which at the time was considering establishment of a Peace Corps of its own. Unfortunately, Moyers became ill, so that I had to function both in policy discussions with members of the government of the Netherlands as well as discuss selection procedures with lower level staff members. It was an extremely pleasant trip, and I enjoyed it greatly in spite of my overall continuing fatigue.

Back in Ann Arbor, my health continued to be poor, my chief symptoms being aches and fevers, and, even more frightening, spells of dizziness which I later recognized as auras. At that point I asked my personal physician to hospitalize me for a thorough neurological

checkup to be supervised by a Board Certified neurologist. During this week in the hospital I told him my history and emphasized that I thought that my problem was related to a bug which I had picked up in South America. After a week of hospitalization and a few casual laboratory tests, the neurologist in charge declared my health to be excellent, and that I should return to work (I later discovered that he is perhaps not the most competent physician in Ann Arbor, in spite of his Board certification).

Even though I had been given a clean bill of health by the neurologist, and managed to carry out my duties at the University -- after a fashion -- I found that I was extremely fatigued and needed much sleep. About two years later I experienced a major seizure about 6 A.M. while in bed. This seizure was so violent that I ended up with a badly broken back and was hospitalized to have a back brace made. My personal physician of course advised me to begin taking phenobarbital regularly in order to reduce the probability of future seizures. But while I was lying in the hospital bed awaiting the construction of my back brace, I asked my daughter to get me books on tropical medicine from the University of Michigan Medical Library. After only a couple of hours of reading I was pretty certain that I was suffering from Brucellosis Melitensis, also known as Malta fever. I then asked my daughter to get me the latest books on Brucellosis and with a couple of more hours was absolutely certain of my self-diagnosis. I was fascinated by the fact that, in addition to Brucellosis Melitensis, which affects sheep and goats, there is another form called Brucellosis Abortus which affects cows and in humans results in undulant fever, and also Brucellosis Suis, which affects hogs and frequently affects human beings who eat poorly cooked pork. I was even more fascinated to discover that these three forms of Brucellosis are limited to the animal species indicated above. For example, a pig can drink the milk of an infected cow and not get the disease. Humans, on the other hand, can acquire all three varieties and most serious of the three is Brucellosis Melitensis. It was at that point that I recalled eating the delightful goat cheese dressing on my salad in the Andean village in Bolivia!

More importantly, in my reading of these texts, I discovered that there is a simple blood test which not only permits a clear diagnosis, but also indicates the level of infection. In brief, it involves taking a small sample of blood and mixing it with a serum from

the particular organism. The severity of the infection is indicated by the number of times the blood can be diluted and still result in an agglutination of the red corpuscles. A "titre" of 1 in 100 is regarded as acute.

On my discharge from the hospital, I asked my personal physician to write me a prescription so that I might obtain a brucellergin titre at a local laboratory. It turned out to be 1 in 640! In other words, I was not only suffering from Malta fever, but was hot as a firecracker. During the next couple of weeks I attempted to persuade one of several local physicians to take my diagnosis and titre seriously, but none would do so. It was at that point that I discovered that if one wants to be treated by most physicians one should not diagnose himself!

Since I was unable to obtain help from any physicians in this great medical center of Ann Arbor, I decided to call one of my public health physician friends in the Peace Corps and ask his advice. His response was that he knew nothing about the disease in question, but would find out who at the NIH in Bethesda was an expert on the disease. Within an hour he called me back and recommended that I consult Dr. Norman McCullough. I immediately phoned Dr. McCullough and arranged for an appointment to visit him within the next couple of days.

Unlike my physician friends in Ann Arbor, I found Dr. McCullough completely accepting of my self-diagnosis, and soon learned the basis of his sympathy. Originally, Dr. McCullough had been a veterinarian studying Brucellosis in the Chicago stockyards. At one point in his research, he had expressed a desire to extend his research into the effects of the disease on human beings, and was quickly told by the medical profession that in order to do so he would first have to obtain an M.D. degree. At that point he entered the University of Chicago Medical School, but as a true scientist continued to study Brucellosis and maintained cultures of the bug in his laboratory. During his internship year, Dr. McCullough began suffering a severe back pain, and, upon consulting orthopedic surgeons on the staff, was advised that he would have to take surgery on his back to repair a slipped disc. At that point he said, "Gentlemen, I was a medical student only a year ago, and remember reading that if one has a slipped lumbar disc, he also has a pain down one of his legs. I have no pain in my legs, so perhaps I have developed Brucellosis, which, among other symptoms, sometimes produced nodules on joints." At that point he decided to determine his titre and also found it to be 1 in 640.

Since he was obviously now very healthy, I said, "Marvelous, you found a way to cure yours. Now tell me what the proper drugs are." At that point, he frowned and said, "I'm sorry, Professor Kelly, there is only one drug that will kill Brucella, and that is streptomycin, and you are too old to take it. The necessary dosage of streptomycin in anyone over the age of 50 will result in complete deafness in both ears." Naturally, I was disappointed since I was already 58 at the time and asked what else I might do. His advice was very simple, "Try to keep your Brucellosis under control by taking four tetracycline and four sulfadiazine tablets daily for the rest of your life." Upon his advice, I obtained appropriate prescriptions from my local physician and have taken both of these antibiotics daily since 1963. The price, has, however, been somewhat heavy, since these drugs will also destroy the "friendly bacteria" which produce the flora on the lining of the intestines. The result is that I have suffered from bowel trouble throughout this period, but continue to take the drugs believing that the bowel discomfort is less serious than the potential effects of the Brucellosis.

In 1970, I became involved in with my first political venture. The township in which I lived just west of Ann Arbor had been controlled for four years by a Board of Trustees representing the one-party system characteristic of the township. In my judgment, as well as that of some of my neighbors, they were not functioning in the best interests of the township. Realizing that it would be totally impossible to run a slate on the minority party with any chance of being elected, I and a small group formed the Scio Township Citizens Committee, and proposed an alternative slate of candidates for Trustees. Thanks to an effective educational campaign, this alternative primary slate received the majority of votes in the primary election which automatically assured that we would be elected in November. Thus it was that I became, not a United States senator, as I had once hoped, but a trustee of this township. I served as trustee of the township in 1970-1973, but because of continued poor health, and my impending retirement from the University, I decided not to run for reelection.

In 1972, Lillian and I made a trip to Honolulu to attend the APA convention there. I enjoyed showing her the islands, but shortly after our return to Ann Arbor she suffered a serious heart attack, requiring several hospitalizations following the major attack. During this time I plead with her cardiologist to see if she was a possible candidate for open-heart surgery. He was very resistant to the idea, pointing out that an archarteriogram necessary for an appropriate evaluation

of the feasibility of open-heart surgery was in itself hazardous. Even so, I finally persuaded him to order this procedure, and an eminent heart surgeon invited me to view the movie in the dark room with him. Although I understood little of the presentation, he pointed out that Lillian's lower ventricle had been severely damaged by the heart attack, with the result that it was not emptying itself of blood. This examination also indicated that her veins were too small to expect successful results from by-pass surgery. At that point we retired to Lillian's hospital room and he proposed to do surgery as follows: "First of all", he said, "you have a very inefficient heart, so I propose to take a 'French pleat' in it. You will end up with a much smaller pump, but a much more efficient one!" Next he said that although bypass surgery was not indicated, he thought that she would profit greatly from a Vineberg operation. We, of course, asked what that was and he explained that it involves dissecting out one of the mammary arteries, opening the chest cavity, and inserting the mammary artery, pierced in numerous places, into the sinusoid circulatory system within the heart. He explained that it acted just like a soaker hose on a dry lawn. "More importantly," he said, "from each of the small cuts that we make in the mammary artery a new capillary will grow, with the result that the heart muscle will be completely revascularized."

Since Lillian had essentially been a cardiac cripple for several months, we were delighted with this proposition, and after waiting three or four months for time in the operating room, Dr. Morris performed the surgery described above. Although Lillian was on the table for better than six hours, her recovery was quick. In fact, she was discharged from the hospital eleven days after the surgery. Far more important was the fact that she was completely free of anginal pains, and her heart was still functioning well twelve years later! We were, of course, most grateful to Dr. Joseph Morris for this very successful open-heart surgery, which he assured us was the type that every heart surgeon would want performed on himself should he ever need it. Unfortunately, a couple of years ago, Dr. Morris fainted and was found by his partner in his office one night. His condition was so serious that his partner performed a quadruple bypass, feeling that time did not permit Dr. Morris to get the kind of open-heart surgery that he really wanted. To our amazement and consternation, we have never yet found another physician or nurse who has even heard of the Vineberg operation, developed by Dr. Vineberg of McGill University after extensive experimentation on sheep and goats. Apparently, however, practically all open-heart surgery involves bypass procedures, the benefits of which are of limited duration. The only reason I can

find for failure to use the Vineberg procedure is that it is far more time consuming, both for the surgeon and the operating room.

At the expiration of my term as trustee of the township, I chose not to run for office again both because of my health, and also because I was expecting to retire from the University of Michigan at the end of 1974. I did, however, attend a party celebrating the victory of my successor later. Early the next morning I had to go to the little bathroom adjacent to the bedroom, and in my sleepiness did not put on my "popbottle" glasses, which I needed because of cataract surgery many years before. As a result I managed to fall over the threshold of the shower in this bathroom, and suffered severe back pains. Rather than call for help in the middle of the night, I took several aspirin, and went to bed with the expectation of getting my back checked at the hospital the following morning. That morning, however, I was totally unable to get out of bed, but fortunately was able to call my daughter, who in turn called an ambulance and took me to the hospital. X-rays of my back showed a severe fracture in the lumbar region, and I was immediately hospitalized and put on painkilling drugs. All that afternoon and evening, however, I was visited by what seemed to be every white-coated individual in the hospital, all insisting on palpating my right side just below the rib cage. None of these physicians said anything to me, but I tried to explain to them that the pain was in my back, and asked for additional painkiller.

Early the next morning, my personal physician visited and in a quiet voice asked, "How long have you had this aneurysm?" I replied, "Dr. Clague, you know that an aneurysm is asymptomatic, and one does not ordinarily discover it until it bursts. And death follows immediately even though one is in the hospital at the time." My doctor explained the x-rays of my broken back had revealed an orange-sized aneurysm on my descending aorta just below the rib cage, and demonstrated to me how one could feel the pulse beating in it by placing a hand lightly over my stomach just below the rib cage. It was then that I realized what all the other physicians had been palpating the night before. Dr. Clague explained that, with an aneurysm of the size indicated in the x-ray, predicted life expectancy is less than fifteen months, and he asked me if I could possibly think of any way we could determine how long it had existed. At that point I recalled that I had had my last faculty physical at the University of Michigan Health Service about a year before, and suggested that x-rays there might provide the answer. Dr. Clague examined them, and reported that the large

aneurysm had been clearly visible a year before. A team of surgeons was consulted and all agreed that I should have an aneurysmectomy as soon as possible. However, because of my seriously broken back, it was decided that I should remain in bed, receive blood pressure reducing medicine in quantities sufficient to reduce the probability of a rupture of the aneurysm until surgery could be performed.

Naturally, I was furious with my internist, who had not reported the presence of the aneurysm to me. I assumed that either he was so incompetent in reading the x-rays that he had not seen it, or, alternatively, that he had not felt that I could survive the massive surgery involved in replacing the aorta with a plastic one. About a month later, after I was able to walk around the hospital corridors, I encountered this internist just as he was getting off the elevator. Of course, I was most anxious to ascertain whether or not he had seen the aneurysm in the x-ray, and, if he had seen it, why he had not told me about it. I questioned him bluntly in front of many persons in the elevator lobby. His response was, "I told Dr. D., to whom I had sent you for a prosthetectomy." I said that was very kind of him, but wondered why he had not told me about it personally. At that point, he stuttered more than usual, and lamely explained that he had intended to tell me about it sometime! I asked, "When? On the autopsy table?" I can only assume that he thought that I was too old and my health was too frail to undergo the massive surgery involved in an aneurysmectomy.

After assuring myself that my internist had seen the aneurysm in the x-rays a year before, in a loud voice audible to all in the lobby, and in no uncertain terms, I told him that I thought he had been guilty of gross malpractice. However, instead of suing him, and thereby increasing local insurance rates, I was going to tell everyone that I knew what a lousy physician he was. Although I have seen him on a number of occasions since, he has never apologized for his failure to advise me of the presence of this large aneurysm.

I underwent surgery for the aneurysm some three months later. This involved laying practically all of my internal organs to one side in order to replace the aorta which runs alongside the right side of the spinal column, so I now have a plastic aorta and plastic tubes replacing the arteries going down through each groin. Although this surgery was very massive for a person of my age, after some ten days in intensive care I was returned to a regular hospital bed and visited by the team of surgeons. At this time I was running a fever, and so they called in Dr. Clague, the neurologist who had become personal physician to both Lillian and me.

Dr. Clague is a short, mild-mannered individual. But when in my presence the tall cocky surgeon announced that I was suffering from a relapse of my Brucellosis, thus causing the fever, Dr. Clague shot back immediately: "Gentlemen, you know I do not like surgery. In fact, I hate the sight of blood, but I recall that we all studied surgery under the same great professor at the University of Michigan, and if I recall correctly, many times in the course of his lectures he emphasized that fever following surgery almost always indicated infection in the lower part of the wound." Raising his voice, he then asked loudly, "Have you gentlemen examined your handiwork?" With that he turned on his heel and left the room. Much stunned, the team of surgeons hastily consulted with each other, and decided to return me to surgery. The following day, I learned that they had opened up the lower eight inches of the 24 inch incision, cut some four inches deep, and discovered the infection. At that point I was advised that the wound would have to be permitted to heal from the inside out in order to avoid the possibility of re-infection. The surgeons asked whether, if I returned home, there was anyone who could dress the wound daily. Because Lillian was still in the hospital with problems of her own, I moved in with my daughter, Pat, who cared for the wound during the next two months required for its healing. Fortunately, she had had some nurse's training, and, even more fortunately, did not hesitate to remove the proud flesh that accumulated, something that the visiting nurse said that she would not be permitted to do. About this time, we decided to sell our large home and build an apartment on the back side of our daughter's home.

In 1974, at the age of 69, I retired from active duty at the University, which was kind enough to provide a final sabbatical furlough with salary that year. My friends and colleagues gave me an extremely pleasant retirement party. While receiving many congratulatory remarks from my colleagues for my services to the University, I also experienced considerable ribbing that night for not having yet published the research of the marital compatibility study.

Both because of my retirement from the Department and my continuing poor health, the next few years were not intellectually very productive. I continued to prepare a manuscript for a book reporting the results of the marriage research. Further, with Goldberg, Fiske, and Kilkowski, I published a follow-up study of the VA assessees twenty-five years later (Bibliography #54). From 1982 to 1984 I served as consulting editor for the Encyclopedia of Psychology, a four volume set, published in 1984. In addition to conferring with

the editor, Ray Corsini, I also prepared three entries for the Encyclopedia as well as a brief biography (Bibliography #55). Also during this period, I came to the conclusion that I was never going to be able to complete the study on personality and marital compatibility to my satisfaction. Fortunately, a brilliant Ph.D. in psychology at the University of Michigan, James J. Conley, had developed much interest in this longitudinal investigation, and offered to take all of my original, as well as coded data with him to his new position at Wesleyan University in Middletown, Connecticut. With the aid of another of my Ph.D.'s, Jerry O'Dell, this transfer of data was carried out smoothly, at which time Conley and I jointly planned a third follow-up of these originally engaged couples now in their later maturity. With the aid of a substantial research grant from the National Council on Aging, Conley carried out this follow-up very successfully during the next two years, and has already published one paper extending my findings on the consistency of personality over a much longer time period. And, finally, in press to be published in the spring of 1986 is a paper of which I will be the senior author, reporting the essential findings of this unusually long longitudinal study of personality and marital satisfaction (Bibliography #56).

In order that we would not become a burden on any of our three youngsters, Lillian and I decided to move into the Glacier Hills Retirement Home on July 1, 1983. This is a facility which has an excellent nursing home, and the terms of our contract provide for lifetime care. We have a very comfortable two-bedroom apartment with kitchenette and are generally very pleased with the many services provided for us retirees. Somewhat to my surprise, I was elected president of the Glacier Hills Residence Association, a very active group with some twenty committees which helps keep people intellectually alert and keeps Glacier Hills from merely being an "old folk's" home. My many duties as president-elect and current president of the Association have kept me intellectually alert during these last two years.

Unfortunately, a nasty accident on August 27, 1984 forced a marked change in my life style. In getting up from the dinner table in our dining room, I managed to fall over a chair and suffered a badly broken hip joint. I have little memory of the ten days immediately following, which were spent in the local hospital where I had my hip pinned; I was then returned to the Glacier Hills nursing center for a thirty-day period of physical rehabilitation and learning to use a walker.

As if the broken hip were not an adequate reason

for slowing down my activities, I had another serious but fortunately brief illness in late November of 1984. Shortly after awakening on November 17, I found that I had little appetite, and what appeared to be indigestion. This was not relieved by the usual antacid pills, and I spent most of the day in bed. During the day, I found it increasingly difficult to breathe, but really had no idea of the nature of my problem. By the evening of that day I was so uncomfortable that I called a nurse from the nursing center, who in no uncertain terms said that I either had pneumonia or congestive heart failure! She recommended that I be transferred immediately by ambulance to the local hospital, and to my surprise, I was given an IV in the ambulance as well as oxygen. During the next three or four hours in emergency, I was firmly diagnosed as having an attack of congestive heart failure. I was transferred to an intensive care room, so that my heart could be monitored while I continued on IV, which included a piggy-back solution of amphylline. This procedure proved to be very effective in reducing the congestion in my lungs. After only four days in the hospital, I was discharged and EKG studies showed that no serious heart damage had resulted. On the last night in the hospital, I was visited by not only my personal physician, but by a cardiac specialist. I was amazed to discover from them that the condition I had experienced, "congestive heart failure", was one frequently not recognized by physicians themselves when they suffered from it. More importantly, I learned that a key prodromal symptom of the developing condition is edema, particularly in the lower extremities, so I have naturally been alert for any reoccurrence.

In mid-May, 1985, I received a very pleasant surprise, namely that I had been named as the American Foundation of Psychology's Gold Medal recipient for my contributions to professional psychology. Not only does this include the gold medal, but a \$2,000 check and payment of all travel expenses for two to the APA Convention in Los Angeles on August 25.

Of course, I am pleased to have been so honored, but, in looking back over my varied and very busy professional career, I must admit that perhaps I really was deserving of the award.

Unfortunately, 1985 turned out to be a miserable year for Lillian, my wife and helpmate for nearly half a century. Early in the year, she began experiencing severe discomfort just below the rib cage. She first had many diagnostic tests as an outpatient, but was hospitalized on two occasions for nearly a month each time, and underwent repeated surgeries, none of which was effective in reducing her pain. Eventually, by mid-

autumn, on the basis of ultra-sound and CT scan data, her problem was diagnosed as inoperable cancer of the pancreas, with a life expectancy of three to four months. While this finding was a shock to both of us, we accepted the inevitable, and spent many hours reviewing the highlights of our exceptionally happy marriage. Eventually, because of an inability to tolerate food, Lillian became extremely emaciated, and as her pain became more severe, was given injections of Dilaudid every four hours by a nurse from our Nursing Center. Finally, her physical as well as mental condition had deteriorated so markedly that on December 15th she was transferred to our Glacier Hills Nursing Center, where she died about 1:30 A.M. on December 21st, just four days short of our 47th wedding anniversary.

At almost the same time, I experienced a second attack of congestive heart failure. Fortunately, an ambulance and two paramedics were in Glacier Hills at the time. They started an IV and rushed me to the emergency room of the local hospital. Thanks to prompt and effective treatment, I was in the hospital only four days, and was able to return to my daughter's home in Ann Arbor to share Christmas time with my three children. And, perhaps more important, my personal physician had sent me home with prescriptions for several drugs designed to prevent future congestion of my lungs.

So, as I have reached the age of 80, I complete this long and rambling autobiography. It is much longer than I had anticipated when I began to make notes for it, but I found myself continually surprised at the many details that I felt worth recording. I have no idea whether it will ever be published, but at least I am sure that my children will be glad to learn why they saw so little of me during their period of growing up, and perhaps, also, it will be interesting reading to at least a few of my colleagues.

I have no idea how many years are left to me but as long as I remain intellectually alert, I look forward to enjoying them. Fortunately, both Lillian and I maintained an active interest in amateur radio. We have operated stations on practically all of the amateur frequencies from Glacier Hills, and have even constructed an amateur television station. Since both of my parents lived nearly to the age of 90, I hope that, in spite of my health, I can continue to enjoy whatever time is left to me.

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