SOCIAL AND PSYCHOLOGICAL FACTORS AFFECTING FERTILITY

XXI. AN EMPIRICAL RE-EXAMINATION AND INTERCORRELATION OF SELECTED HYPOTHESIS FACTORS[1]

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THE Indianapolis Study of Social and Psychological Factors Affecting Fertility has resulted thus far in the publication of twenty analytical reports, most of which have concentrated on specific hypotheses. Owing partly to the division of labor in the Study[2] and partly to time lags separating the publications, only a limited amount of synthesis and organization of the empirical results has been done. Although a final integrated report is planned after all the hypotheses have been analyzed, a brief evaluation should be useful at this time. This article is not intended to be a comprehensive summary of the Indianapolis Study to date; it consists rather in an attempt to interrelate some of the existing main findings of the Study in so far as they relate to our knowledge about contraceptive practice and size of planned families.

Also prompting this analysis is an interest in securing precise and uniform comparisons of the bearing of socio-economic status (SES) on the observed relationships between several hypothesis variables and fertility planning and the fertility of planned families.[3] The reports on specific hypotheses have rather consistently indicated that SES accounts, at least in part, for many of the relationships observed. The question of the comparative persistency of this influence will be examined

[1] This is the twenty-first of a series of reports on a study conducted by the Committee on Social and Psychological Factors Affecting Fertility, sponsored by the Milbank Memorial Fund with grants from the Carnegie Corporation of New York. The Committee consists of Lowell J. Reed, Chairman; Daniel Katz; E. Lowell Kelly; Clyde V. Kiser; Frank Lorimer; Frank W. Notestein; Frederick Osborn; S. A. Switzer; Warren S. Thompson; and P. K. Whelpton.

[2] Ten people other than the members of the original Committee have participated in the Study.

[3] This problem is discussed more fully and documented in Westoff, Charles F.: The Changing Focus of Differential Fertility Research: The Social Mobility Hypothesis. The Milbank Memorial Fund *Quarterly*, January, 1953, xxxi, No. 1, pp. 26–28.

here. For one hypothesis variable under consideration, namely marital adjustment, there has been no previous analysis of the influence of SES.[4] This particular question will be also partially answered. The final objective of this report is to indicate the extent to which our ability to predict variation in the dependent variables in question is improved by a joint consideration of several selected factors.

Measurement and Data

The hypothesis factors in addition to SES that have been selected for re-analysis are marital adjustment (more precisely, marital happiness), feeling of personal adequacy, tendency to plan in general, and feeling of economic security.[5] These particular factors were selected primarily because of their fairly pronounced individual relationships with fertility-planning status or size of planned family, or both. Although it is recognized that the measurement of these socio-economic and psychological variables might possibly be improved within the framework of the existing data by the newer techniques of scaling, as some recent experimentation has suggested,[6] this was not attempted in this present work. Instead, the summary indices originally constructed and used by the various authors

[4] This lack of information is emphasized by the author. *See* Reed, Robert B.: Social and Psychological Factors Affecting Fertility. VII. The Interrelationship of Marital Adjustment, Fertility Control, and Size of Family. The Milbank Memorial Fund *Quarterly,* October, 1947, XXV, No. 4, p. 425 (Reprint p. 301).

[5] For the original articles, *see Ibid.,* pp. 383–425; Westoff, Charles F. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility. XVII. The Interrelation of Fertility, Fertility Planning, and Feeling of Personal Inadequacy. The Milbank Memorial Fund *Quarterly,* July, 1952, XXX, No. 3, pp. 239–297 (Reprint pp. 741–799); Freedman, Ronald and Whelpton, P. K.: Social and Psychological Factors Affecting Fertility. XII. The Relationship of General Planning to Fertility Planning and Fertility Rates. The Milbank Memorial Fund *Quarterly,* April, 1951, XXIX, No. 2, pp. 218–243 (Reprint pp. 549–574); and Kiser, Clyde V. and Whelpton, P. K.: Social and Psychological Factors Affecting Fertility. XI. The Interrelation of Fertility, Fertility Planning, and Feeling of Economic Security. The Milbank Memorial Fund *Quarterly,* January, 1951, XXIX, No. 1, pp. 41–122 (Reprint pp. 467–548). The original analysis of socio-economic status appeared in Kiser, Clyde V. and Whelpton, P. K.: Social and Psychological Factors Affecting Fertility. IX. Fertility Planning and Fertility Rates by Socio-Economic Status. The Milbank Memorial Fund *Quarterly,* April, 1949, XXVII, No. 2, pp. 188–244 (Reprint pp. 359–415).

[6] Edgar F. Borgatta of the Laboratory of Social Relations, Harvard University, has done a considerable amount of experimentation on the Indianapolis data with the use of modern scaling techniques. This work is not yet complete.

have been retained. The main advantage of this is, of course, comparability with the previous articles. Thus for wives and husbands separately the summary indices of personal adequacy, general planning, and economic security will be used.[7] The summary index of SES is a composite for the couple; information on marital happiness of the wife and husband was derived from the answers to a single multiple-choice question.[8]

The measure of fertility employed here is the number of live births. The extent and success of the couples' planning of these births is measured through the classification by "fertility-planning status" used throughout the Indianapolis study. Based upon detailed pregnancy and contraceptive histories, this classification essentially orders the couples according to the extent to which they had succeeded in preventing unwanted pregnancies.[9] Only those couples classified as "relatively fecund" are included here.[10]

[7] For the details of the construction of these indices *see* Westoff and Kiser, *op. cit.*, pp. 286–297 (Reprint pp. 788–799); Freedman and Whelpton, *op. cit.*, pp. 221–224 (Reprint pp. 552–555); and, Kiser and Whelpton, XI. The Interrelation of Fertility, Fertility Planning, and Feeling of Economic Security. *Op. cit.*, pp. 112–114 (Reprint pp. 538–540).

[8] The summary index of SES appears in Kiser and Whelpton, IX. Fertility Planning and Fertility Rates by Socio-Economic Status. *Op. cit.*, pp. 213, 215, and 244 (Reprint pp. 385, 387, and 415).

The question on marital happiness is: "Everything considered how happy has your marriage been?" Seven multiple-choice responses were provided, ranging from "Extremely Happy" to "Extremely Unhappy." Although Reed utilized two additional criteria of marital adjustment, the desire to improve spouse and amount of disagreement between wife and husband, the responses to the single question on marital happiness were found to be more sensitive to the fertility variables and thus only these data are used here. The statistical analysis with the other two indices of marital adjustment was in fact done but the results will not be incorporated here.

[9] *See* Whelpton, P. K. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility. VI. The Planning of Fertility. The Milbank Memorial Fund *Quarterly*, January, 1947, XXV, No. 1, pp. 63–111 (Reprint pp. 209–257); Whelpton, P. K. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility. VIII. The Comparative Influence on Fertility of Contraception and Impairments of Fecundity. The Milbank Memorial Fund *Quarterly*, April, 1948, XXVI, No. 2. pp. 182–236 (Reprint pp. 303–357); and Westoff, Charles F.; Herrera, Lee F.; and Whelpton, P. K.: Social and Psychological Factors Affecting Fertility. XX. The Use, Effectiveness, and Acceptability of Methods of Fertility Control. The Milbank Memorial Fund *Quarterly*, July, 1953, XXXI, No. 3, pp. 291–357 (Reprint pp. 885–951).

[10] The "relatively fecund" group includes all couples reporting four or more

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Previous reports[11] have described at length the study design and the type of couples studied. Briefly, the intensive interview Study was restricted to couples meeting the following eligibility requirements: husband and wife native white, both Protestant, married during 1927–1929, neither previously married, husband under 40 and wife under 30 at marriage, both completed at least the eighth grade, and residents of a large city most of the time since marriage.

The choice of statistical techniques for an analysis of this nature is complicated by at least three problems. First, the indices of the psychological factors are based upon data which fall far short of the ideal requirements for quantification. At best, these data can be roughly classified as qualitatively ordered series of responses. A translation of qualitative responses into quantitative categories presents many complications not the least of which is the implicit assumption of equality of intervals. A second problem inheres in the reliance on correlational statistics employed in the major part of this analysis which necessitate certain assumptions resting on only weak foundations with these data, for example, those of normality and linearity. The third problem accrues from the use of the inflated sample and its implications for tests of statistical significance. The process of inflation by the duplication of punch cards would seem to negate the necessary requirement of statistical independence of units. This problem has been partly resolved in this analysis by a reduction in the chi square values obtained by a constant proportion which represents the

live births. It also includes couples with three or fewer live births unless they knew or had good reason for believing that conception was physiologically impossible during a period of at least 24 or 36 consecutive months since marriage (24 if never pregnant, 36 if ever pregnant). Failure to conceive when contraception was not practiced "always" or "usually" during periods of the above durations was considered good reason for such belief.

[11] *See* Whelpton, P. K. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility. IV. Developing the Schedules and Choosing the Type of Couples and the Area to be Studied. The Milbank Memorial Fund *Quarterly,* October, 1945, XXIII, No. 4, pp. 386–409 (Reprint pp. 139–162). Also *see* Whelpton, P. K. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility, V. The Sampling Plan, Selection, and Representativeness of Couples in the Inflated Sample. The Milbank Memorial Fund *Quarterly,* January, 1946, XXIV, No. 1, pp. 49–93 (Reprint pp. 163–207).

ratio of real cases to the total inflated sample. The probability values obtained from the chi squares in this work, consequently, are fairly rigorous. Needless to say, these problems do not exhaust the list.

This article will be divided into two sections: the first relates to fertility planning, and its correlates; and the second to size of the planned family and factors related to it.

Fertility Planning

A logical starting point for this entire analysis is to examine the intercorrelations of all the factors being considered. These coefficients[12] are presented in Table 1. It may be noted first of all that all relationships are positive.[13] As expected, the highest intercorrelations are those between the index of SES and the components of this index. More important, however, is the fact that the correlations between SES and the psychological factors tend to be higher than the intercorrelations of the psychological factors themselves. This holds true for both wives and husbands for every psychological factor except personal adequacy.

The total correlations of each of the factors with fertility-planning status are shown in the last column (wives) and bottom row (husbands). The values of these coefficients are very low with the highest value being .35 with SES. However, two points of interest may be noted. The index of SES, based upon eight variables, is more closely related to fertility planning status than is any one of the three separate components of SES considered here. Although the correlations between fertility planning status and the psychological factors are low, there also remains the important question of their comparative independence. In other words, are the relationships observed between the various psychological indices and fertility planning mainly the result of their mutual joint connection with SES?

[12] Coefficients of correlation were computed by use of the conventional Pearson formula.

[13] The signs throughout this analysis indicate the true direction of the relationships. The machine codes were adjusted for this purpose.

Factors	Index Socio-Economic Status	Husband's Average Annual Income	Husband's Longest Occupation	Education	Marital Happiness
Index Socio-Economic Status	x	*	*	.61	.29
Husband's Average Annual Income	.80	x	*	.44	.25
Husband's Longest Occupation	.69	.53	x	.42	.21
Education	.72	.51	.55	x	.22
Marital Happiness	.27	.26	.18	.18	x
Index General Planning	.34	.29	.26	.18	.21
Index Personal Adequacy	.27	.28	.20	.22	.35
Index Economic Security	.38	.39	.31	.26	.20
Fertility-Planning Status	.35	.26	.23	.23	.23

Table 1. Coefficients of correlation[1] showing the interrelationships among nine selected factors. The data for wives are shown in the upper right section of the table and those for husbands in the lower left section.

* Data relate only to husbands.

[1] All signs are plus. The number of cases for each correlational analysis is 1,444 except those involving the husband's longest occupation which are based on 1,402 cases. A coefficient of approximately .03 is required for a statistically significant departure from zero at the 5 per cent level and .05 for the 1 per cent level according to R. A. Fisher's t formula. The corresponding requirements for the deflated sample (N = 860) are .07 and .09.

Or, conversely, how much of the SES association with fertility planning can be explained by the psychological factors?

In order to provide some answers to these questions, coefficients of partial correlation are presented in Table 2. The dominating influence of SES is rather clearly in evidence. The partial correlations of SES with fertility-planning status with the several psychological factors individually held constant are only slightly lower than the original value of r (.35). When feeling of economic security is held constant in the partial analysis, the zero order r remains unchanged for the wives and is decreased only to .33 for husbands. The inversion of this comparison results in perhaps an even more pronounced manifestation of the overriding influence of SES. The zero order r between economic security and fertility-planning status is .09 for wives and .13 for husbands. With SES controlled, the first order r changes sign for wives (–.07) and is reduced to .00 for husbands.

The same associations are shown by a completely different mode of analysis in Table 3. This table reflects the results of subdividing the couples into various numbers of categories of

Index General Planning	Index Personal Adequacy	Index Economic Security	Fertility-Planning Status
.41	.24	.43	.35
.34	.25	.45	.26
.28	.15	.33	.23
.27	.17	.24	.24
.28	.36	.22	.24
x	.33	.23	.20
.22	x	.27	.17
.19	.32	x	.09
.19	.20	.13	x

marital happiness, personal adequacy, etc., and testing the statistical significance of the resulting distributions in which fertility-planning status is cross-tabulated with another factor. For example, the cell in the row of SES and column on marital happiness in the upper deck of the table means that out of eight subclasses of marital happiness (four each for wives and husbands) six distributions of fertility-planning status by SES are significant at the 1 per cent level of probability. Conversely (cell in second row of column on SES) out of the ten subclasses of SES only two distributions of fertility-planning status by marital happiness are significant at this level. When fertility-planning status is varied with SES within economic security classes eight out of eight distributions are significant as compared with zero out of ten when SES classes are held constant and economic security is varied with fertility planning.

Table 2. Total and partial correlations of five selected factors with fertility-planning status.

Factors Correlated with Fertility-Planning Status	Total Correlations	Controls for Partial Correlations				
		Socio-Economic Status	Marital Happiness	General Planning	Personal Adequacy	Economic Security
	WIVES					
Socio-Economic Status	.35	x	.30	.30	.32	.35
Marital Happiness	.24	.15	x	.20	.19	.23
General Planning	.20	.07	.14	x	.15	.19
Personal Adequacy	.17	.09	.09	.11	x	.15
Economic Security	.09	−.07	.04	.05	.05	x
	HUSBANDS					
Socio-Economic Status	.35	x	.31	.31	.31	.33
Marital Happiness	.23	.15	x	.20	.17	.21
General Planning	.19	.08	.15	x	.15	.17
Personal Adequacy	.20	.12	.13	.17	x	.17
Economic Security	.13	.00	.09	.10	.07	x

We can conclude from Tables 2 and 3 that SES is by far the most dominant factor of those studied in association with fertility planning status. Further partial correlations of the second and third order were computed between SES and fertility planning with various combinations of the psychological factors held constant but the general picture remained unchanged. Marital happiness exhibits a somewhat higher association with fertility planning with SES constant than does either general planning or personal adequacy. As far as the data in this form indicate, the psychological factors, especially feeling of economic security, are of negligible importance in relation to fertility planning. Further corroboration of this is evidenced in Table 4 where various multiple correlation combinations are presented. The effect of adding the three psychological factors

Table 3. The number of distributions[1] out of the designated totals that are significant at the 1 per cent and 5 per cent levels. Each distribution is a cross classification between fertility-planning status and a given factor with another factor controlled. (The numbers in the bottom line indicate the total number of distributions utilized for each particular interrelation in the column above.)

Factors Cross-Classified with Fertility-Planning Status	Factors Controlled				
	Socio-Economic Status	Marital Happiness	General Planning	Personal Adequacy	Economic Security
	One per cent level of significance				
Socio-Economic Status	x	6	5	7	8
Marital Happiness	2	x	3	3	3
General Planning	1	2	x	1	1
Personal Adequacy	0	1	2	x	1
Economic Security	0	1	0	0	x
	Five per cent level of significance				
Socio-Economic Status	x	6	5	7	8
Marital Happiness	4	x	3	6	3
General Planning	1	3	x	3	4
Personal Adequacy	1	2	3	x	2
Economic Security	2	1	0	1	x
	Total number of distributions utilized				
	10	8	6	8	8

[1] The number of degrees of freedom in these distributions varies from one to twelve.

Factor Combinations Correlated with Fertility-Planning Status	Wives	Husbands
General Planning and Personal Adequacy	.23	.25
Marital Happiness and Personal Adequacy	.26	.26
General Planning and Marital Happiness	.28	.27
SES and General Planning	.36	.36
SES and Personal Adequacy	.36	.37
SES and Marital Happiness	.38	.38
General Planning, Personal Adequacy and Marital Happiness	.29	.29
SES, Personal Adequacy and General Planning	.36	.37
SES, Personal Adequacy and Marital Happiness	.38	.38
SES, General Planning and Marital Happiness	.38	.38
SES, Marital Happiness, Personal Adequacy and General Planning	.39	.39

Table 4. Multiple correlations of combinations of two, three, and four selected factors with fertility-planning status.

to SES increases the original correlation of SES with fertility planning (.35) only to .39, hardly a substantial improvement.

In view of the apparent paramount importance of SES in this relationship, it is desirable to try to disentangle the important components of SES in order to narrow down the source of its influence. The four factors of husband's average annual income since marriage, husband's longest occupation since marriage, education of wife, and education of husband were selected from the total of the eight components constituting the

Table 5. Total and partial correlations of three components of socio-economic status with fertility-planning status.

Factors Correlated with Fertility-Planning Status	Total Correlations	Wives			Total Correlations	Husbands		
		Controls for Partial Correlations				Controls for Partial Correlations		
		Income	Occup.	Educ.		Income	Occup.	Educ.
Income	.26	x	*	.18	.26	x	.17	.17
Occupation	.23	*	x	.15	.23	.11	x	.13
Education	.24	.14	.16	x	.23	.12	.13	x

* Data relate only to husbands.

SES index.[14] Partial analysis[15] of these three factors (Table 5) reveals that income is slightly more important than education and occupation but the observed differences between the three offer little basis for assessing any real priorities of importance.[16] The coefficient of multiple correlation between these three factors and fertility-planning status is slightly lower than the original value of .35 for the SES index.

Planned Fertility

Turning now to the subject of factors associated with planned fertility, we will raise the same questions as we did in the analysis of fertility-planning status. Is SES again the dominant factor? What are the maximum predictive values that can be obtained for size of the planned family? One disadvantage of the distribution of completely planned family sizes in this study is its somewhat narrow range.[17] The bulk of the couples fall within the 0, 1, and 2 parity groups and thus require a high level of discrimination as far as prediction is concerned.

The correlation coefficients presented in Table 6 are generally lower than those observed for fertility-planning status.[18] There is, however, a radical change in the importance of some of our factors. Whereas the correlation of economic security to

[14] The remaining four components are net worth, rental value of the home, rating on Chapin's Social Status Scale, and purchase price of car.

[15] *See* Table 1 for the zero order intercorrelations.

[16] The restriction of the sample to wives and husbands with at least complete grammar school education might be expected to have a stronger lowering effect on the relation of fertility-planning status to education than to the other socioeconomic variables on which no sampling restriction was made.

[17] Because of the inconsistencies, irregularities and extremely low correlational values obtained from the analysis of the "number planned" groups (205 couples who interrupted contraception in order to conceive their *last* pregnancy but who stated that they had had one or more previous pregnancies under other circumstances) the results of this group are not included in this article. The analysis is thus restricted to the more homogeneous group of 403 couples classified as "number and spacing planned" who either practiced contraception continuously and never had a child or who deliberately interrupted contraception in order to have every pregnancy. For convenience these couples are labeled "completely planned" in the present article.

[18] The correlation of fertility-planning status with the fertility of all couples (1,444) is −.58.

fertility-planning status was very low (.09 for wives and .13 for husbands, Table 2), it presents the highest of all correlation values with *size* of *planned* families (.32 for wives and .31 for husbands, Table 6). Moreover, this relationship is largely independent of SES. The values are reduced only to .28 for wives and to .27 for husbands when the index of SES is controlled. Of the three SES components listed, only income succeeds in partially reducing these values (.23 and .24 for wives and husbands, respectively). General planning and personal adequacy (not included in these tables) are almost completely negligible. For general planning the correlations are .01 for wives and –.04 for husbands; for personal adequacy, the values are .02 for wives and .09 for husbands. The slight relationship between marital happiness of wives and size of planned family appears to be largely a function of their joint associations with

Table 6. Total and partial correlations of six selected factors with size of completely planned families.

Factors Correlated with Size of Completely Planned Families	Total Correlations	Controls for Partial Correlations					
		SES	Mar. Happ.	Econ. Sec.	Income	Educ.	Occup.
		WIVES					
Socio-Economic Status	.16	x	.14	.03	*	*	*
Marital Happiness	.11	.07	x	.05	.04	.09	.10
Economic Security	.32	.28	.31	x	.23	.31	.31
Income	.24	*	.22	.09	x	.22	**
Education	.10	*	.08	.02	–.01	x	.07
Occupation	.09	*	.07	.00	**	.05	x
		HUSBANDS					
Socio-Economic Status	.16	x	.16	.05	*	*	*
Marital Happiness	–.01		x				
Economic Security	.31	.27	.31	x	.24	.28	.30
Income	.24	*	.25	.13	x	.18	.23
Education	.17	*	.17	.10	.04	x	.15
Occupation	.09	*	.09	–.01	–.06	–.02	x

* Indicates that correlation analysis was not performed because income, education, and occupation are components of the summary index of socio-economic status. A coefficient of approximately .10 is required for a statistically significant departure from zero at the 5 per cent level and a coefficient of .13 for the 1 per cent level of significance. The corresponding requirements for the deflated sample (N = 240) are .13 and .17.

** Data relate only to husbands.

income and economic security. The low zero order correlations between either education or occupation and size of planned family are virtually eliminated when either income or economic security is held constant.

The multiple correlations in Table 7 indicate that the zero order correlations between economic security and size of planned families (.32 and .31) are hardly improved to any degree by the addition of other factors. The maximum values obtained are .34 for wives and .35 for husbands which indicate the relation of planned fertility to economic security, income, education, and occupation simultaneously considered. The combination of economic security and income produces coefficients of .33 and .34 with planned size of family.

In an attempt to ascertain the role of childlessness in these relationships, 126 couples who had experienced no live births were eliminated,[19] leaving a group of 277 couples classified as completely planned *fertile* families. As indicated in Table 8, when the analysis is restricted to fertile couples, the relation of economic security to size of planned family is lowered substantially (.20 for wives and .14 for husbands). Holding income constant serves to reduce these coefficients even further

Table 7. Multiple correlations of combinations of two, three, and four selected factors with size of completely planned families.

Factor Combinations Correlated with Size of Completely Planned Families	Wives	Husbands
Education and Occupation	.11	.17
Education and Income	.24	.24
Income and Occupation	*	.25
Income, Occupation, and Education	.25	.26
Economic Security and Socio-Economic Status	.32	.31
Economic Security and Occupation	.32	.31
Economic Security and Education	.32	.32
Economic Security and Income	.33	.34
Economic Security, Income, Education, and Occupation	.34	.35

* Data relate only to husbands.

[19] It will be recalled that childless couples in the Study are mainly voluntarily childless in that they had always practiced contraception. The few exceptions are those having pregnancies that terminated in unintentional wastage.

Factors Correlated with Size of Completely Planned Fertile Families	Total Correlations	Controls for Partial Correlations					
		SES	Mar. Happ.	Econ. Sec.	Income	Educ.	Occup.
		wives					
Socio-Economic Status	.14	x	.14	.05	*	*	*
Marital Happiness[2]	.02		x				
Economic Security	.20	.15	.20	x	.12	.18	.17
Income	.19	*	.20	.10	x	.18	**
Education	.08	*	.08	.02	−.03	x	.03
Occupation	.11	*	.11	.05	**	.08	x
		husbands					
Socio-Economic Status	.14	x	.15	.09	*	*	*
Marital Happiness[2]	−.06		x				
Economic Security	.14	.09	.14	x	.07	.11	.10
Income	.19	*	.20	.15	x	.13	.16
Education	.14	*	.14	.11	.03	x	.09
Occupation	.11	*	.12	.05	−.02	.03	x

Table 8. Total and partial correlations between six selected factors and size of completely planned fertile[1] families.

* Indicates that correlational analysis was not performed because income, education and occupation are components of the summary index of socio-economic status. A coefficient of approximately .11 is required for a statistically significant departure from zero at the five per cent level and .14 for the one per cent level where N = 277. The corresponding requirements for the deflated sample (N = 165) are .15 and .20.

** Data relate only to husbands.

[1] Excludes childless couples.

[2] Total r considered too low for partial analysis.

to .12 and .07. The relations of income to size of these fertile families, on the other hand, is reduced only to .19 from the previous value of .24 with the childless families included and it is apparent that this relationship (.19) is slightly less dependent on economic security than it is for all completely planned families. The associations with the other factors remain approximately the same[20] except martial happiness which is now almost completely unrelated.

Summary and Evaluation

The chief aim of this interruption of the reports on the analyses of the Indianapolis Study hypotheses is to provide some in-

[20] Again general planning and personal adequacy exhibit negligible relationships. The coefficients are −.04 for wives and −.07 for husbands for general planning and −.03 and .00 for personal adequacy.

tegration and synthesis of the existing empirical findings as they relate to success in planning family size and size of the planned family. The particular hypothesis factors selected for this reanalysis are: socio-economic status, marital happiness, feeling of personal adequacy, tendency to plan in general, and feeling of economic security. Total, partial, and multiple correlations are the primary statistical techniques used.

The analysis of these factors in relation to fertility-planning status indicates quite clearly (for these data and summary indices at least) that SES exerts the predominant influence. The relationships of fertility-planning status with the "psychological" factors appear to be largely functions of SES. Some partial independence of SES is retained by marital happiness. On the whole, however, SES is the dominant factor of those studied in relationship to fertility-planning status.

The relationships of these various factors to size of completely planned family presents a different pattern. Feeling of economic security, in this instance, contributes the highest relationship. This relationship is largely independent of the SES index but is reduced somewhat when income is held constant. The exclusion of childless couples from the completely planned family group results in decreased relationship of completely planned fertility with economic security and a relative increase in the importance of income.

Analysis of the three major components of the SES index (income, occupation, and education) reveals that for both fertility-planning status and planned fertility, income has the greatest predictive sensitivity.

All of the correlation coefficients between the hypothesis factors and the two dependent variables of fertility planning and completely planned fertility are fairly low. The maximum proportion of the variation in the dependent variables that can be accounted for by multiple combinations of various factors is 15 per cent for fertility planning and 12 per cent for size of planned family. There are numerous possible explanations for the limited predictive values obtained. One possibility, of

course, is that the measurement of the various independent variables, particularly the personality characteristics, may have been inadequate. The Indianapolis Study was planned and designed in 1938–1940 before the comparatively recent innovations in scaling techniques. A more serious problem derives from the *ex post facto* nature of the Study design which results in the relation of answers to psychological questions asked in 1941 to reproductive behavior occurring at any time up to 15 years prior to that date. This same problem applies to a lesser degree to the socio-economic factors. There is an absence of control over the time sequence dynamics of all these relationships which only a longitudinal study could overcome. There is, of course, always the possibility that the psychological variables studied here may be less important for fertility behavior than others not included in the present analysis.

On the other hand, in view of the complexity and diversity of individual values and motivations associated with reproductive behavior, it may be somewhat futile to expect much higher prediction from a statistical analysis utilizing crude indices of certain sociological and psychological phenomena. It should be emphasized that such factors as SES and feeling of economic security are, at best, only predisposing conditions in their effect on fertility behavior. Feelings of economic security or insecurity, for instance, only facilitate or retard the operation of other more complex attitudes surrounding the having and rearing of children. In other words, our current predictive instruments only *indirectly* approach the problem and are successful in predicting fertility planning and fertility only to the extent of their correlation with the direct but more subtle factors involved. How common these latter factors are among different couples is a research question certainly deserving consideration in any future studies of this type.

EXPLORATION OF POSSIBILITIES FOR NEW STUDIES OF FACTORS AFFECTING SIZE OF FAMILY[1]

CLYDE V. KISER

TWO developments have conspired to create an interest in starting a new field study of factors affecting size of family. One is the fact that the Indianapolis Study of Social and Psychological Factors Affecting Fertility is nearing completion. The other is the dramatic increase in birth rates during the past decade. These situations prompted the Milbank Memorial Fund to devote one of its discussion groups at its 1952 Annual Conference to the topic Exploration of Possibilities for New Studies of Factors Affecting Size of Family. Three half-day sessions were devoted respectively to the subtopics: I. The Indianapolis Study; findings, weaknesses, and implications for future studies; II. New Techniques and Methods Available From Related Fields; and III. Suggestions Regarding Aims, Scope, and Methods in New Studies.

I. THE INDIANAPOLIS STUDY: FINDINGS, WEAKNESSES, AND IMPLICATIONS FOR FUTURE STUDIES

As a point of departure, Mr. Kiser presented a resumé of the Indianapolis Study and discussed some of the weaknesses of the data. His report on findings was restricted to those concerning the twenty-three hypotheses and since this is being published elsewhere[2] only the summary results (Table 1) and a few general statements will be presented here.

One of the most publicized findings of the Indianapolis Study is that of the direct relation between socio-economic status and fertility within the "number and spacing planned" group. The

[1] A report on the Round Table on Exploration of Possibilities for New Studies of Factors Affecting Size of Family, held in connection with the 1952 Annual Conference of the Milbank Memorial Fund at The New York Academy of Medicine, November 19–20, 1952. The participants are listed in the Appendix.

[2] Kiser, Clyde V. and Whelpton, P. K.: Resumé of the Indianapolis Study of Social and Psychological Factors Affecting Fertility. To be published in the November, 1953 issue of *Population Studies*.

average fertility of the total group of "number and spacing planned" families is low, but within the group fertility is directly rather than inversely associated with socio-economic status. Equally important, however, is the fact that within this "number and spacing planned" group, fertility is also directly associated with feeling of economic security of the couple. This relationship persists in slightly reduced form when socio-economic status is held constant.

The Study also yields some interesting relationships of fertility planning and size of planned family to other social and psychological characteristics. However, a fair generalization is that in most cases the relation of the other psychological characteristics to size of planned family tends to be weak or to be much reduced when socio-economic status is held constant. This situation is pointed up rather strikingly in the number of cases in Table 1 in which it was necessary to use the word "partially" in reference to the question "Has the hypothesis been supported?"

The failure of the Study to yield more in the way of psychological correlates of planned fertility may simply be the result of attempting to relate rather small differences in sizes of planned families to psychological attributes. The lack of sharper relationships could arise from (a) the actual absence of much variation in psychological attributes by fertility within the narrow range of family sizes considered; (b) the inadequacy or crudeness of the measures or scaling of the psychological attributes; and (c) the possible methodological weakness inherent in dealing with one hypothesis variable at a time. It may be noted, however, that a beginning has been made in "across the board" analysis of the data.[3]

Other or related weaknesses of the Indianapolis Study are:

1. *Inadequate Preparation.* This includes (a) the lack of an integrating theory or organizing principle for the twenty-three

[3] Westoff, Charles F. and Kiser, Clyde V.: Social and Psychological Factors Affecting Fertility. XXI. An Empirical Reexamination and Intercorrelation of Selected Hypothesis Factors. The Milbank Memorial Fund *Quarterly*, October, 1953, xxxi, No. 4, pp. 421–435.

Table 1. Indianapolis Study hypotheses classification and results of analyses.[1]

Class and Subject of Hypothesis	Hypothesis Number *See* Vol. II pp. 147–149	Published Article Number	Direction of Relation Found		Has the Hypothesis Been Supported	
			Fertility-Planning Status	Size of Completely Pl. Fam.	Fertility-Planning Status	Size of Completely Planned Family
(1)	(2)	(3)	(4)	(5)	(6)	(7)
I. *Status and Security*						
Socio-Economic Status	3	IX	+	+	Yes	No
Economic Insecurity	2	XI	–	–	No	Yes
Economic Tension	1	*	–	+	No[2]	No[2]
II. *Community and Family Background*						
Family and Childhood Situations	12	*	±	±	Partially	Partially
Residence and Migration History	11	XVI	±	±	Partially	Partially
Doubling-Up of Families	4	*				
Health of Wife and Husband	21	XIII	+	–	No[3]	No[3]
Health of Children	22	XIII	+	–	No[3]	No[3]
III. *Interest in Home and Children*						
Liking for Children	5	*	0	+	No	Partially
Children Wanting Brothers and Sisters	6	*	DNA	+	DNA	Partially
Parental Preference Re Sex of Children	10	XIV	DNA	±	DNA	Partially

Reasons for Second Child:						
Belief "Only Child" Handicapped	8	*	DNA	+	DNA	Partially
Desire to Insure Against Childlessness	9	*	DNA	+	DNA	Partially
IV. *Personality Characteristics*						
Personal Inadequacy	16	XVII	−	−	No	Partially
Feeling Children Interfere With Personal Freedom	7	*	−	+	No	No
Ego-Centered Interest in Children	18	XVIII	+	−	Partially	Partially
Fear of Pregnancy	23	XIX	+	−	Partially	Partially
Rationality of Behavior:						
Tendency to Plan	17	XII	+	−	Partially	Partially
Interest in Religion	15	X	−	+	Partially	Partially
Adherence to Traditions	14	XV	−	+	Partially	Partially
Conformity to Group Patterns	13	*				
V. *Marital Adjustment and Husband-Wife Dominance*						
Marital Adjustment	20	VII	+	+	Yes	Partially
Husband-Wife Dominance	19	VII	0	0	No[4]	No[4]

[1] Symbols:
* = Unpublished data—results preliminary (Col. 3).
+ = Direct relation with hypothesis variable (Col. 4–5).
− = Inverse relation with hypothesis variable (Col. 4–5).
0 = No relation with hypothesis variable (Col. 4–5).
DNA = Does not apply (Col. 4 and 6).
[2] Results believed to be spurious because of selective factors.
[3] Data on health very inadequate.
[4] Some results found on reformulated hypothesis.

hypotheses; and (b) inadequate conceptualization of some of the specific hypotheses; and (c) the lack of sufficient pre-tests of the validity and reliability of the psychological data.

2. *Deficiencies in Sampling.* This includes (a) the small size of the sample (even the inflated sample is too small to afford sufficient numbers in some of the crucial cells); (b) the limited possibilities of generalizing on the basis of a highly homogeneous sample in one area; and (c) the inadequacy of a uniform sampling scheme for all twenty-three hypotheses, some of which require specialized samples.

3. *Deficiencies in the Data.* This includes (a) the *ex post facto* nature of the data and the resulting difficulty of differentiating between cause and effect or between determinative and selective factors; and (b) the failure of the Study to provide any hint of the forthcoming baby boom, presumably due largely to the fact that it was restricted to women married 12–15 years at the time of interview (1941) and hence gave little or no attention to factors affecting marriage.

The listing of these weaknesses furnished points of departure for Ronald Freedman's discussion of implications of the Indianapolis experience for future investigations. He expressed the belief that some of the most significant findings from the Indianapolis Study might yet develop and he recommended that a small team of investigators be given freedom to range across the list of formal hypotheses and to combine and interrelate sets of data which have thus far been treated separately.

Dr. Freedman believed that one of the significant contributions of the Indianapolis Study is its provision of criterion data on the extent and effectiveness of contraceptive practice. The data are very convincing on the overwhelming influence of contraception as against other immediate factors in the general reduction of fertility. However, the data are limited in time and space and there is urgent need for similar data for a sample of the general population. Dr. Freedman urged that we should have on a national basis time-series data about the fertility norms and practices in order to establish bench marks

both for historical analysis and to provide a frame of reference for more intensive studies of the Indianapolis type.

Regarding the key-role of socio-economic status in fertility behavior, Dr. Freedman suggested that this variable should be one of the important take-off points for future studies. He thought that an important research objective should be that of determining what there is about style of life in different socio-economic classes that connects with different fertility patterns.

Dr. Freedman stated that in any study of fertility one encounters a mixture of factors that represent the residual influence of earlier historical forces and those that are indigenous to the current situation. He believed that the design of the Indianapolis Study served to minimize the influence of the "historical forces" and to emphasize the factors in the "current situation." This was indeed the case in that one rationale for concentrating the Study on an urban native-white Protestant group with at least a grammar school education was the belief that a group of this type, with knowledge of contraception, was setting the fertility pattern to which other groups would eventually conform. Furthermore, the emphasis on "recent" factors is evident in all of the hypotheses which are concerned with the relation of given variables to size of planned family.

Dr. Freedman believed that the sample design described above had been very productive for certain purposes. Thus, one of the most significant findings of the Study is that whereas fertility is inversely related to socio-economic status in the general population, it is directly related among those who plan family size most effectively. Nevertheless, he felt that the sample design was not appropriate to test some of the hypotheses which are really of an historical character, such as those concerning the roles of religion and tradition. Actually, of course, this is but one aspect of a general deficiency of having only one sampling scheme for the testing of twenty-three separate hypotheses—many of which would require specialized sampling schemes for adequate testing.

Dr. Freedman thought that in the Indianapolis Study the

measures of fertility phenomena were much better than the measures of the social and psychological variables. He regarded this as a natural consequence in a pioneering study which attempted to encompass a very wide range of variables. It was in fact frequently called an exploratory study designed to screen out factors of importance for future studies. It was not possible to experiment extensively with scales for each variable.

With the Indianapolis Study providing leads as to the substantive areas in which more intensive analysis might be most fruitful, future studies should concentrate on a smaller number of more generic hypotheses and much attention should be given to the development of concepts and measuring scales before going into the field for final data on the relation of fertility to the variables selected. He believed that a series of small-scale studies with extreme criterion groups might well precede any new large-scale study. He wished finally to say that students owed a considerable debt to the group that initiated and conducted the Indianapolis Study. Their work is notable not only for the significant substantive results obtained but also for the humility with which they have exposed each step to public examination. Their willingness to experiment and to reveal their problems as well as their successes makes the next steps much easier to take.

Discussion. Dr. Frank W. Notestein asked what would have been lost if the Study had dealt with wives only or with husbands only. Mr. Kiser was of the opinion that for most of the hypotheses relatively little would be lost. The extra expense and time required for including the husband was pretty large. It meant evening appointments for the interview in which the husband was included, so the field work was slowed down considerably. He agreed that for testing certain hypotheses such as marital adjustment, husband-wife dominance, and parental preferences regarding sex of children, data for both spouses are important. Dr. Samuel A. Stouffer and Dr. Reuben Hill were of the opinion that the husband-wife similarities and

discrepancies in replies to the same questions should be valuable classifications.

Apropos of Dr. Freedman's suggestion about the need for national data, Dr. Notestein thought it would be well to make a systematic analysis of the discrepancies between findings based upon indexes of certain variables and findings based upon short cuts such as self-ratings on the hypothesis question, interviewers' ratings, etc. He thought the results of such an analysis might shed some light on the value of national opinion polls regarding the influence of certain factors on family size.

Dr. E. Lowell Kelly expressed the view that before any new study is launched it would be well to follow Freedman's suggestion about "across the board" analysis of the Indianapolis Study materials. He suggested, in effect, some type of cluster analysis or factor analysis of the data with as many variables as possible included simultaneously. He thought that some such analysis might help to clear up the problem posed by the overriding influence of socio-economic status. What goes with socio-economic status? How many things are so tied up with this that we have had to treat it as a kind of single variable?

Dr. Burton R. Fisher was dubious about Kelly's proposal regarding factor analysis. In the first place, he thought it likely that there are many U-shaped relationships, especially with many of the socio-economic variables. In the second place, the assumption that "some milk and perhaps some cream" still remains in the Indianapolis data might be questioned. He preferred to see one choose from the first study those hypotheses that seem most profitable to retest and to proceed with a new study or a series of studies designed to get at given sets of variables. He thought that interpenetrating samples might enable one to relate a whole host of things that could not be accomplished in a single study.

Dr. Kelly replied that he was well aware of the possibility of curvilinear relationships and of the inadequacy of correlation techniques based upon linear relationships. He assumed that

a sophisticated analyst would not confine his efforts to a single global approach but would cut the data in some way to make it meet the criteria of the technique in question. Thus there might be a succession of analyses in which one would group data amenable to like treatment.

Dr. Balfour thought the comparative financial cost of reworking the existing data and undertaking a new study might have some bearing on the choice between these alternatives. The over-all costs of the Indianapolis Study and subsequent analyses, if the time and expense of all personnel are included, may show that the survey itself was by far the lesser part of the total cost. The setting up of a new study under current conditions, and possibly in a new setting, might be advisable rather than to continue the reworking of old data. Dr. Balfour, therefore, asked whether anyone could state the cost of the preliminary organization and collection of data in the Indianapolis Study as compared with the cost or expenditure for the analysis.

Mr. Kiser replied that a great deal of free service went into the planning and conduct of the Indianapolis Study and in the analysis of the data. He stated that the total grant from the Carnegie Corporation was $75,500 and that about $45,500 had been paid out by the time the field work was finished. Dr. Lowell J. Reed stated that in estimating the cost of further analysis of the data one should bear in mind that all materials are available on punch cards and that they can be mechanically transferred to new cards under desired arrangements at little expense.

Dr. Morton Deutsch thought the Indianapolis Study had suffered by the lack of an integrating theory. The twenty-three hypotheses which form the basis of the Study do not have any systematic relationships with each other; apparently no attempt was made to link the hypotheses explicitly to core socio-psychological conceptions. However, examination of the twenty-three hypotheses investigated by the Study and of the questionnaires suggests that implicitly the Study is centrally

concerned with areas of considerable theoretical significance: the social psychology of goal-setting and of planned action. Considerable research and theoretical writing has been done in these areas and it is likely that the Indianapolis Study could profit by applying to its own data some of the more theoretically-oriented questions arising from the research in these areas. Thus, it would be of interest to see if the Indianapolis data can provide answers to such questions as: Under what conditions does explicit goal-setting with respect to having children occur? What factors affect the goals which are set? What factors influence the stability of the goals which are set? What factors influence the effectiveness of action to achieve fertility goals?

The questionnaires employed in the Study do not provide the data necessary to a full answer to these questions. They do, however, give much relevant information, particularly about the personal and experiential characteristics of the respondent, which may determine the respondent's perceived causal structure with respect to certain kinds of events—i.e., the perceived events which lead to having or not having children and the perceived consequences of having or not having children. More explicit analysis of the different patterns of causal expectations which exist among the various respondents would provide an opportunity to link these patterns with personality, group membership, and experiential characteristics, on the one hand, and to goal-setting behavior and to the effectiveness of planned action, on the other hand. Once these linkages exist, it should be possible to link socio-economic variables, such as prosperity or depression, peace or war, political stability or instability to fertility behavior in a rational way rather than by hunch.

Dr. Wellman J. Warner thought the Indianapolis Study had made a distinct contribution simply by helping to clear the way for studies of motivation in relation to fertility. He believed there was some tendency, however, to expect of this Study something that it was not intended to be. It was not

set up as a longitudinal study and it could not be expected to yield classifications or results for which a longitudinal study would be required. However, some of the variables appear to be inadequately defined and hence poorly measured. He singled out "conformity to group patterns" which might mean a number of things. Actually, he stated, a social pattern is likely to be a more generalized norm which is then taken by the individual and synthesized into a range of situational factors in which the conformity may emerge as exactly nonconformity to what we have assumed the pattern to be originally. He emphasized the necessity of precision and specificity in definition and conceptualization of the variables to be measured.

Dr. Leonard S. Cottrell asked whether the Committee responsible for the Study had been called upon to make, or had found it appropriate to make, recommendations regarding policy. Dr. Thompson, Dr. Reed, and others replied that the answer was definitely "No." The belief that a Study of this type might furnish information of value to policy makers has been voiced as one of the reasons for making the Study but there was never any thought that the Committee would formulate a set of policy recommendations.

Dr. Thompson further stated that despite the shortcomings of the Indianapolis Study he, as a member of the Committee, was not disappointed in the results. He thought the Study had yielded much information of value and that it had helped to delineate the pitfalls to be avoided in future investigations. He recalled that in the early planning of the Study there was constant recognition of the fact that mistakes would be made, that the first attempt was regarded as an exploratory one, and that its function was to help prepare the way for future investigations in the field.

II. New Techniques and Measures Available from Related Fields

Sociology. Dr. Samuel A. Stouffer stated that he would not attempt to present a systematic review of techniques developed

by sociologists or anthropologists which could be used in any future study of social and psychological factors affecting fertility. However, as a sociologist, he was quite surprised to find that Hypothesis 13, Conformity to Group Patterns, was the only one listed in Table 1 on which no statement of results could be made. He suggested that when he and his wife were married some 25 years ago they were very conscious of the surrounding power and pressures of the social groups in which they were living. They knew that if they had had a child during the first year of marriage or as many as three children during the first four or five years of marriage they would have been subjected to strong controls of ridicule. It would have been taken for granted that they did not know any better. Nowadays, perhaps partly because knowledge of contraception is more widespread and partly because of changing values and sanctions, if a couple in an equivalent social position has three children during the first four or five years of marriage it tends to be regarded approvingly as happening because the couple wants the children.

Dr. Stouffer believed that if students are to get at the heart of social and psychological factors affecting fertility they must understand the values and sanctions, and particularly the sanctions of gossip and ridicule, that are prevalent in the immediate environments in which a particular family lives. He acknowledged the difficulty of studying these intangibles but he said that during the past ten years sociologists have been giving much attention to the problem of assaying values.

Dr. Stouffer believed that the efforts to determine the values of a particular social group had been helped a great deal by the new developments in scaling, most of which have come since 1941. In his opinion the important thing about the scaling technique is that it pretty well guarantees the uni-dimensionality of what is being measured.[4] He reported that he and other

[4] For a description of the H-technique in scaling, a modification of the scaling technique developed by Louis Guttman, *see* Stouffer, Samuel A., *et al.*: A Technique for Improving Cumulative Scales. *Public Opinion Quarterly*, xvi, No. 2, Summer, 1952, pp. 273–291.

sociologists had not been concerned too much with the study of a particular value as such but rather with conflicts of values and the extent to which people put one value or set of values higher than another value or set of values. As members of society we belong to different groups and some of these groups have conflicting norms. For this type of analysis the new scaling devices hold considerable promise.

Dr. Stouffer stated that one of the uncertainties in studies of the above type is our ignorance about the amount of confidence that can be placed in ordinary verbal responses that a person gives when he is interviewed quickly or when he is given a pencil and paper test. He reported that Dr. Borgatta had been cross-checking verbal responses of soldiers with their written responses to identical questions in order to secure some test of validity. Dr. Stouffer thought that a systematic cross-check of replies of wives and husbands to the same questions in the Indianapolis Study would be useful. Particularly valuable for indications of validity of conflicts are the replies to the questions requiring statements of wife's opinion, husband's opinion, wife's statement of husband's opinion, and husband's statement of wife's opinion.

In conclusion, Dr. Stouffer stated that he felt confident that with the newer techniques one could identify the major group memberships of respondents and determine the extent to which they feel identified; and that we could go further than the Indianapolis Study did in establishing the relation of "conformity to group patterns" to fertility behavior. He also believed that the application of scaling techniques to other variables such as "liking for children" might yield sharper relationships with fertility behavior than those observed.

Dr. J. F. Kantner stated that since he is responsible for analyzing the data on conformity, he felt impelled to comment on Dr. Stouffer's remarks. He noted that the hypothesis is something of a misnomer since most of the items were not intended as measures of the influence of group membership on fertility. They related rather to the more limited question of

how attitudes toward certain fertility-related matters, e.g., attitudes toward birth control ads, birth control clinics, proper spacing of children, the ideal number of children for people in moderate and well-to-do circumstances, etc., are associated with fertility. Items such as these show only a moderate degree of association with fertility and fertility planning. One item, however, is available that can be considered as measuring the influence of conformity to group norms. This is the average number of children of a couple's three best friends. Here, in line with Dr. Stouffer's expectations, a higher degree of relationship is found. For the relationship to fertility, $r = +.36$ among couples having no unwanted births. For the relationship to effectiveness in fertility planning the coefficient of contingency is $-.42$ ($P < .001$) with consistent differences to be observed with five socio-economic groups.

Mrs. Pratt stated that she had in fact experimented with scaling techniques in the data on liking for children without very good results. Similar reports had been given by other analysts—Ruth Riemer and Marianne DeGraff Swain.

Dr. Robin Williams wished to make three comments regarding Stouffer's remarks. He stated that during the past ten years sociologists had made much progress in operationalizing their notions about the importance of membership groups and reference groups, in the determination of human behavior. He stated that when we talk about norms we should mean not merely what people say *they* believe but also what judgments other individuals in the group have imparted to them. His second comment was that although the scaling devices had been used mainly for attitude scaling they might also be used for scaling objective behavior. The third comment was that in their attempt to study values, he and Edward Suchman feel that there are some real possibilities of getting directly at people's effective goals by the use of scaling techniques which involve putting people into choice situations and having them make their choices and justify them. He stated that this approach needs to be supplemented by behavioral evidence but

it has been used to advantage in field studies of inter-group relations in a number of communities throughout the country.

Dr. Freedman thought that if research workers are to get at situations of the type mentioned by Dr. Stouffer, it would be necessary to study the group context in addition to data on the individual. Thus one should not only interview the individuals but try to ascertain something of the character of the groups in which the lives of the respondents are anchored. Dr. Stouffer added that one should try to learn how much stake the individual has in the group and how relevant the group is to him.

Dr. Edgar F. Borgatta stated that the Indianapolis Study Committee and the several analysts had agreed to his experimental application of the H-technique of scaling to several groupings of the Indianapolis data. He listed thirteen areas (hypotheses or grouping of hypotheses) that he desired to test for scales. He felt that whereas this approach is the opposite of the one suggested by Dr. Kelly, it is for that reason complementary to it.[5]

Simplified Intelligence Tests. Dr. Norman Frederiksen discussed the possibility of using a short intelligence test in a survey situation. He stated that for a long time psychologists and people in psychiatric work have been looking for short cuts in the measurement of intelligence. The Kent Emergency test has been used rather widely and so have short sections of the vocabulary tests of Wechsler-Bellevue and Stanford-Binet. The principal objection to the use of these tests is that they contain free-response items and therefore considerable training might be required to bring the interviewers to a satisfactory standard of scoring accuracy.

Dr. Frederiksen listed the following as criteria for choosing an intelligence test for survey purposes:

(a) Appropriate content. (Probably verbal items should be used since attitudes are mediated verbally in large part.)

[5] Dr. Borgatta has subsequently carried out preliminary analyses of the thirteen areas and plans to extend the analysis further.

(b) Brevity. (The test should be as short as is consistent with acceptable reliability.)

(c) Fairness to older as well as to younger members of society. (This implies, for one thing, that the test should be unspeeded.)

(d) Freedom from influence of fortuitous factors. (Inclusion of a variety of content is one way to guard against any one person having an advantage because of unusual experiences.)

(e) Reasonable reliability. (A reliability coefficient in the range of .5 or .6 would perhaps be satisfactory for the present purpose.)

(f) A score distribution well above the chance score range.

(g) Objectivity in scoring.

After listing the above he gave his specifications for a test as follows: It should be a paper-and-pencil vocabulary test composed of about twenty multiple-choice items, administered without a time limit.

Dr. Frederiksen stated that after writing out the above criteria and specifications, he discovered an article by Robert L. Thorndike entitled "Two Screening Tests of Verbal Intelligence" published in a 1942 issue of the *Journal of Applied Psychology*.[6] This article describes two tests that were developed for use in a Gallup Poll for the purpose of investigating the relation of scores on the intelligence test to various voting preferences. The test that Thorndike recommended was a twenty-item multiple-choice vocabulary test; most respondents could finish the test in about ten minutes. Thorndike suggested that the reliability of the test for a population of wide range in ability was about .83. Dr. Frederiksen thought permission could be secured easily to use this test if desired.

Mr. Frederick Osborn stated that he had heard the opinion expressed that it would be possible in any questionnaire containing two or three hundred questions to word or arrange the questions in a manner that the replies would afford a pretty fair indication of the I.Q. of the respondent. He wondered

[6] Thorndike, R. L.: Two Screening Tests of Verbal Intelligence. *Journal of Applied Psychology*, 1942, 26, pp. 128–135.

whether that was based upon anything that had been done or whether it was just a presumption.

Dr. Kelly stated that a few years ago one of his students developed an intelligence key on the basis of strong-interest item responses in which the person never said more than "like," "indifferent," and "dislike." The key correlated about .60 with standard intelligence measures and it predicted school grade slightly better than did these same intelligence measures. Dr. Kelly didn't believe a person could respond to all of the questions in the Indianapolis Study without revealing a lot about himself, including intelligence.

Dr. Frederiksen stated, however, that definite hazards were involved in trying to ascertain intelligence by the indirect method suggested. Many of the measures thus derived are not independent, so one would be unable to use ordinary multiple correlation methods in the analysis. In general, he believed the direct approach with a short intelligence test would be more satisfactory.

Screening Test for Neurosis. Dr. Allister M. Macmillan, who has been experimenting with an amplification of a screening test used during the war to detect neuroses among inductees, emphasized at the outset that the technique is not one that is now "available for use." He explained that as a psychologist on Dr. Alexander H. Leighton's social research team studying mental health in the Stirling County area of Nova Scotia, his assignment was that of trying to develop a quick method of spotting neuroses in the community population as an adjunct to the psychiatric case-finding. The need was for a test which could be given rather quickly in a survey-like situation by relatively inexperienced interviewers. Furthermore, it was important that the test items be of a "non-offensive" nature when asked of a rural maritime population, of little psychiatric sophistication, without jeopardizing the rapport of the whole mental health study.

The fifteen "psychosomatic" questions developed and used by the United States during World War II, i.e., the NSA

(Neuropsychiatric Screening Adjunct) questions reported in the fourth volume of *The American Soldier* series,[7] appeared to be the type of items which answered the requirements fairly well since they were of the "forced-choice" variety favored by some leading clinicians (if one were to use a questionnaire rather than a projective technique). Also, the NSA items had formed a quasi-Guttman Scale in the Services situation giving some measure apparently related to neuroticism. Using these fifteen NSA items intact as a core, he added sixty other items with a health or social orientation to form a "Health Opinion Survey" (HOS), which was used in a community situation, similar to Stirling, and in a hospital situation in an experiment to make comparisons between the two in order to develop a scale before attempting measurements in the research area. In planning the experiment, it was realized that the "community situation" is considerably different from the "military situation" since the draftees had been torn from their normal surroundings and dumped into an induction center, while on the other hand it was felt that no particular problem would be encountered in asking the questions of people taking treatment in a hospital for neurosis. Like the soldiers, these people were in an unfamiliar situation. It was therefore believed that the situation for the community people should also be structured in an unfamiliar way. It was also realized that some concrete and meaningful inducement to take the test would be necessary in the community situation.

The solution was to use large mobile testing stations consisting of house-type trailers for all the community interviewing. These trailers were divided into cubicles. In the forward part was set up a Keystone telebinocular, a set of scales, and a weight measuring device. The people of the various communities sampled were told that they could get certain tests free in the trailer, including an eye-test. A cubicle at the other end of the trailer was used for the HOS interview, which lasted

[7] Stouffer, Samuel A., *et al.*: MEASUREMENT AND PREDICTION. Princeton, Princeton University Press, 1950, pp. 492–493.

approximately twenty-five minutes. The provision of height, weight, and eye test services not only served as an inducement to the people to cooperate, but also added to the unfamiliarity of the situation. The respondents were thus taken away from their home milieu where the presence of other members of the family might have influenced their replies to the questions. Also the "clinic situation" created in the trailer to some extent approximated the "Hospital situation" and permitted some degree of standardization.

The sample data were being analyzed at the time Dr. Macmillan gave his report. He could say, however, the comparison of the community and hospital series indicated that fourteen of the NSA questions that were used and twenty-nine additional ones stood up at the 1 per cent level of significance when done on a "whole-group" basis. The community sampling for this original standardization included a mountain area where farming conditions were poor, a valley area where the farming conditions were very good, and another area which was once a prosperous ship-building center but is now regarded as a poor farming area by the local agricultural people. In each case proportionate sampling of contiguous urban areas was done.

Because it was felt that there were differing modes of response to the "Health Opinion Survey" by these differing population strata, the preliminary analysis included statistical comparisons of these separate strata with the hospital neurotics and it was found that, of the forty-odd questions which showed statistical significance for combined groups, twenty were significant at the 1 per cent level in all the strata, and might therefore be termed "universal" for that region. Dr. Macmillan did not yet know whether it would be possible to develop actual scales for use in the comprehensive study. Further statistical analysis is planned with the help of Dr. Garnet McCreary, the project's statistician. If it proves impossible to develop scales or indices from the material, the twenty questions may simply be used operationally in the Stirling area as a rough screening device.

Measures of Personality. Dr. Angus Campbell emphasized at the outset that he knew of no standardized and validated personality measuring device which is now suitable and ready for use in a survey such as the one being considered. He thought the Indianapolis Study itself had been exceptional in its use of many questions regarding attitudes and values. However, if one thinks of personality traits as pervasive styles of behavior which characterize an individual in many kinds of life situations, one will be disappointed if he hopes to find a measuring device suitable for a survey situation. He emphasized that his remarks related specifically to the format of a population survey and not to possibilities of measuring personality of college sophomores, army recruits, or members of any other type of captive audience in which lengthy tests could be applied.

Several reasons for the infrequency of previous efforts to measure personality in surveys were mentioned. (a) The standardized and validated personality tests that are available are long and tedious. It is seldom that multiple interviews can be taken in population surveys and those who design population surveys generally find it necessary to cram everything into a single questionnaire that can be taken within the tolerance limits of the people being interviewed in their homes. (b) The necessary questions in a personality test are rather personal. (c) Few of the people designing surveys in the past have thought that the introduction of personality measurements would contribute enough in accounting for the variance in their dependent variables to make it worth while to include the questions. (d) Many of the personality tests that have been developed have been standardized for college students and would have to be rewritten for application to a general population.

For the above reasons, Dr. Campbell thought that anyone contemplating the use of personality measurements in a survey would be confronted with a whole separate problem. There must be a great investment in time and thought before the

questions are ready for inclusion in the survey questionnaire.

Dr. Campbell emphasized that he did not want to seem entirely negative in the matter and he described some of the experimental work now being done. He and his colleagues at Michigan have been experimenting with the F-Scale of the authoritarian scales. Leo Srole has developed a scale intended to measure "anomie." A number of projective tests have been devised in which the intent of the tester is not obvious to the one being tested. Recently Dr. Campbell and his colleagues attempted an experiment of this type in connection with a survey for the Civil Defense Administration. The objective was to try to find some means of measuring the degree of fear or insecurity which people felt regarding atomic bombing. Four small pictures were devised and there was variation in the order in which they were shown. Sometimes the two most drastic pictures were shown first; sometimes the other two. The individual was asked four questions: What is this picture about? What is the person thinking? What do you think will happen next? What do you think the other people might say it is about? The materials are in the process of analysis.

Dr. Campbell's conclusion was that much so-called projective interviewing [of the above type] is possible in the format of a survey. It requires careful planning and a certain level of ability in the interviewers. Measures of more generalized personality traits are still more difficult. In taking a population survey one must pay attention to the practical facts of the format of a sample survey, the tolerance of the respondent, the imposition on him, and so on. He stated, finally, that while there may be great returns yet to be realized in the area of personality measurement surveys, this area at the moment is still highly developmental.

Dr. Conrad Taeuber stated that the Census Bureau is sometimes asked to include certain questions, particularly in the Current Population Survey, that are outside the normal scope of census interests. For example, it could not consider asking any questions relating to personality until someone develops

a technique suitable for interviewers who have not received prolonged training for work of that type. However, about a year ago the Census Bureau was asked to conduct a survey on a series of mechanical aptitude and verbal aptitude tests in which the technique was that of having the interviewer appear at a door, introduce himself, list household members and designate a respondent. Using flash cards containing pictures or words, he would try to get the respondent to tell whether a particular screw shown in a diagram moved to the right or to the left, and to identify words, and that sort of thing. The whole purpose was to see whether a test of this type could be applied in the circumstances under which census operations are normally conducted. The results were remarkably good. In fact, the response rates were much better than those secured by one of the more sophisticated research organizations that had tried the same thing in the general population.

Dr. Stouffer wished to agree with Dr. Campbell's remarks regarding the impossibility of using elaborate personality tests in a study of the type considered. Short cuts are needed. But it is probably not the function of groups skilled in making surveys to develop suitable briefer devices. The briefer devices must be developed experimentally in university laboratories with available captive audiences in order to see how much relationship there is between the more elaborate procedures and the simple short cuts. There would still remain problems incident to the use of the short cut in a general population. The survey people should try them out in the field and send the results back to the laboratory for further refinement and adjustment. Dr. Stouffer insisted, however, that full-scale substantive studies of the type considered should not themselves be used primarily as testing grounds of the tools. The tools should be field-tested thoroughly before effort is made to use them in any large study of social and psychological factors affecting fertility.

Economics of Family Consumption and Standards of Living. Dr. Dorothy S. Brady discussed two findings that she thought

might have some bearing on problems of measuring the economic level of families in a survey. The first of these had to do with the relation between consumption and size of family. Two measures of this relation were briefly described. In the first plan, historical data tend to show with remarkable consistency that among families of similar income, the proportionate increase in consumption expenditures with increasing size of family tends to be only about one-sixth the proportionate increase in size of family itself. In other words, when income is held constant there is a definite down-grading of consumption expenditures per individual as size of family increases. The other measure, and one which needs further testing, is concerned with proportionate increases in income with increasing size of family that would be necessary to support a given level of consumption. Dr. Brady's data suggest that if a given level of consumption is to be maintained, the proportionate increase in income must be about two-thirds the proportionate increase in size of family.

The other general finding had to do with group standard of living. Dr. Brady drew a chart to indicate the manner in which broad occupational classes of the census type differed with respect to relation between average expenditures and average income. When consumer behavior in relation to income was thus traced from one broad occupational group to another the consumer expenditures were found to have an income elasticity of about eight-tenths. However, when the occupational groups were sub-classified by income, the correlations that applied to the individual occupational group yielded an income elasticity of only about six-tenths.

Dr. Brady suggested that the above finding might be relevant to some of the difficulties experienced in the analysis of Indianapolis Study data. If one has an hypothesis to the effect that the behavior of a family is in relation to its group standard, and if the group standards do vary, it is essential to eliminate the effects of the group standards first.

Dr. Brady stated that some of her current research indicates

that cities and communities differ widely with respect to interaction between the whole community level of living and the socio-economic groups within the community. We cannot say that wage-earner families against clerical families against professional families have any particular differences that are constant over all cities. Dr. Brady was of the opinion that there are variations in the group effect of the city on the individual and variations in the group effect of occupational classes on the individual and that in order to understand figures on income it may be necessary to place the families in relation to their groups.

Dr. Hill asked whether any of the consumption studies ever treated children as means of consumption satisfaction rather than as ends. Dr. Brady stated that she had sometimes considered this approach and that she once had an hypothesis that the difference between consumption patterns in 1918 and 1936 might have been due to the more frequent substitution of an automobile for a child in the later year. None of the analyses with which she experimented seemed to support that hypothesis but Dr. Brady emphasized the impossibility of making statistical comparisons of this type because this would necessitate putting a value on a child.

Dr. Stouffer was hopeful that the quantitative values of costs of children could be derived from the first set of data exhibited by Dr. Brady. Recognizing that certain complications are involved, he nevertheless thought that the cost of the child could be treated in rather strictly comparable terms with the cost of other purchasable commodities. He thought this approach would make sense, particularly with respect to families using contraception and free to choose between, say, an automobile and a child.

Dr. Joseph Spengler thought that the problem was more complex than Stouffer's suggestion would imply. For instance, the wife's disposition to work is not independent of number of children. Furthermore, some of the children may contribute to the family income in later years. The problem is not a

straight exchange of a child for a motor car unless income is standardized.

Sampling and Field Procedures. Professor Frederick F. Stephan explained that sampling and field procedures have developed very rapidly since the Indianapolis Study was planned and a whole week could be devoted to a review of them. In the limited time available he could only touch a few of the high spots. These are summarized as follows:

(a) To derive maximum benefit from the new techniques in sampling and field procedures, it is necessary to make up our minds more definitely today than in 1940 as to what we want in a survey, and the reasons for, and the contemplated uses of, the data that are to be collected. Until we know what analyses are to be made we do not know what type of data to get. Until we know what data to get we do not know whether we should get a few families or many, whether we should try to get a sample enriched with certain types of families or a simple cross-section of the population, and in considering various other possibilities whether to use one type of sampling procedure or another in attempting to obtain the maximum yield of information out of the whole enterprise.

(b) Usually we have several objectives and we want more than we can get with our powers and resources. Hence to apportion our effort wisely it is necessary not only to determine in advance rather precisely what we want but also to arrive at some judgment about the relative values of the principal results we would like to obtain. Thus in connection with the proposed study it is essential to decide whether it is more important to have the study concerned with the determination of *causal relationships* or with finding out what is the *actual distribution* in our population of certain factors that are related to childbearing. The choice between these objectives will depend greatly upon the particular interests of the group that is responsible for the survey. However, a discussion like ours should attempt to assess the relative importance of the two types of objectives and this should be done within the context of the deeper problem as to which type of result is more likely to move us along fastest

toward the ultimate goal of having a well-rounded understanding of the questions that concern us.

(c) Before undertaking another study of the type we are considering we might give some thought to the relation of this study to the whole field in terms of the "strategy of research." Thus if it can be assumed that other research groups are going to undertake certain studies in this field, what can we do that will most advance the whole common effort toward comprehensive and thorough understanding?

Once these decisions are made we begin to know pretty definitely what it is we want to seek in the way of data and for what purpose. Assuming that we also have some notion about the level of accuracy of different types of data desired we are then in position to raise questions about the tools and techniques available for collection and analysis of the data. A few of these are listed below.

(d) A powerful method of arranging complex experiments by "factorial design," originally developed in agricultural research, is being extended into psychological experimentation and other fields. In the classical method of setting up an experiment only one variable is manipulated at a time. Thus seeds of a given variety may be planted in several plots and each plot treated with a different fertilizer. In such a comparison of the effects of fertilizers, the use of a factorial design permits one to compare simultaneously and on the same plots the effects of insecticides, the results of different methods of cultivation, and other factors. To accomplish this it is necessary to subdivide the plots or increase their number but all plots may contribute to each comparison. Experiments of this type require careful planning and in their early history they taught the lesson that the help of the statistician should be enlisted in designing as well as analyzing the experiment.

One consequence of factorial design is that it frees one of the necessity of having a highly homogeneous sample in order to control the major variables. If properly done, a considerable range of variation may be incorporated in the design to accomplish this end. The relationships that are discovered may then have validity over a wider range of populations.

(e) Scientific principles are being applied to the operation as well as the design of surveys. The whole survey operation is considered to be a system directed to the production of specific results as economically and effectively as possible. Previous surveys are analyzed and preliminary trials are run to obtain information about cost, accuracy, and other criteria needed to determine the optimum size and distribution of the sample, the optimum length and procedure for the interviews, the best apportionment of time between training and field work, and the best answers to many other technical problems. In this respect the scientific approach to conducting a research survey serves a function similar to that of studying the production process in a factory in order to ascertain what number of workers and machines and what level of quality of materials are needed to give the greatest yield of end product. Similar approaches to complex operations have been developed in industry as well as agriculture and science. They were applied to many military problems during World War II. The Operations Research Society was organized recently to foster their development and wider application.

(f) There have been important advances in the study of change. Longitudinal studies offer a means of observing changes as they occur over a period of time. Cross-section studies observe at a single time a succession of groups at different stages of development. Both approaches present difficulties. In a longitudinal study the panel of individuals may be affected by the progress of the study itself. In a cross-section study of change, groups in different stages of development may not have had comparable experience at each stage and hence cannot be regarded as strictly equivalent to a single cohort passing through life. There have been a number of significant studies conducted by repeated interviewing and also theoretical advances in analyzing processes of change psychologically and mathematically, all of which could contribute to the planning of future studies of fertility.

(g) Methods have been developed for increasing the accuracy of estimates derived from surveys and also contributing to their analysis and interpretation by utilizing information from other sources. This utilization of collateral data may affect the design

of the sampling, the content of the schedules, and other aspects of a survey operation. Hence it should be considered in the original planning.

(h) Improvements in the means of observation and the measuring instruments employed in surveys are of great importance. The training of interviewers should emphasize the possible biasing effects of failure to contact clients or to get their full cooperation or to conduct the questioning effectively. The use of the mails, telephones, and inquiry from neighbors should be increased in order to reduce the number of "not founds." Interviewers can easily invite refusal to give information about income, contraception, etc. by betraying their fear of a refusal in the manner in which they ask the question. The selection of interviewers is highly important for their abilities, motivation, and attitudes may determine the value of the results more than any other part of the survey.

(i) Sampling can be applied within the interview as well as in selection of respondents. The possibility of refusals and the risk of securing abbreviated and inaccurate replies tend to increase with the length of the questionnaire. One technique is that of rotating questions and not asking all of the questions of all of the people. This plan was followed, for example, in the 1950 Census of Population and Housing.

(j) The practice of assessing the accuracy of surveys, so notably pioneered in the Indianapolis Study, is becoming an essential part of all survey reports. Not only must surveys be designed to give accurate results but the degree of accuracy actually attained must be estimated for the guidance of all who make use of the results. The use of parallel sub-samples provides a useful way of getting approximate estimates of sampling error or tests of significance. Thus in the analysis the total sample might be divided into ten sub-samples that are alike in all respects except size. It is then possible to secure a set of estimates for each of the sub-samples in order to yield a frequency distribution of sub-sample results. From these an estimate of sampling variance may be secured. Methods of estimating or allowing for survey bias are equally important but still inadequately developed.

(k) A more sophisticated attitude toward tests of significance has spread through the ranks of research workers. Chance alone will account for about 5 per cent of all tests that are found to be "significant" at the 5 per cent level. Tests for significance must be applied with due allowance for the fact that one has accepted a certain risk of having non-significant items turning out as significant.

(l) Professor Stephan emphasized the importance of making quantitative estimates of qualitative variables and conducting quantitative experiments so that one may increase the precision of results and so "creep up on the truth." This involves the development of mathematical models and the deduction of results from them for empirical testing. It is particularly important if the chief objective is that of studying causal relationships rather than to determine the prevalence of given characteristics in the population.

(m) Noting that some of the hypotheses in the Indianapolis Study when taken two or more at a time do not lead to any definite conclusion, Professor Stephan emphasized the need for a more general theory or organizing principle incorporating the separate hypotheses that will specify the results of differences (or changes) in two or more conditions affecting the same population. Ordinarily this requires a replacement of the present elementary statements of inequality by a set of quantized relationships. When models are devised to express such a set of hypotheses, the results should have more meaning in terms of causal relationships than the present formulations have. Then we will be able to design empirical studies that will move us ahead more rapidly toward realizing the ultimate objective, whether that ultimate objective is making recommendations for policy, giving the data to people who will make the recommendations, or merely adding another chapter to the growing body of basic social science and knowledge about human behavior.

Discussion. Dr. Stouffer thought it well to issue a warning that the system of rotating questions might easily lead to heartbreaks in the analysis. Professor Stephan agreed that one might easily regret it later if he made a judgment in advance

that certain questions would be asked of only some of the people. However, that principle was applied to the fertility questions in the 1940 and 1950 Censuses. The questions on fertility probably would not have been asked at all if they had to be asked of all the married women. Sampling of this type is often a compromise which may lead to heartbreaks.

Mrs. Lee F. Herrera stated that the factorial design was quite applicable in agricultural studies but she had difficulty in visualizing its extensive use in social science, particularly in surveys of the type considered. New variables corresponding to fertilizer and insecticides cannot be introduced—we must take the variables that exist in the population.

Professor Stephan agreed about the difficulties. He stated that certain modifications in factorial design probably would be needed for application to social science. However, he pointed out that certain aspects of factorial design were in fact introduced in the Indianapolis Study when it was decided to sample on the basis of fertility-planning status and size of family. He also pointed out that the demonstrated importance of socio-economic status indicated the wisdom of making further studies, either in a new survey or from the materials in the Indianapolis Study, with a view toward discovering what aspects of this vaguely defined term are really important.

Professor Stephan acknowledged that the interrelationship of the components of socio-economic status (such as education, occupation, and income) imposes a limitation on the application of factorial design. However, it is possible to design a sort of "staircase analysis" in which adjacent occupational or income groups are compared. He emphasized that more knowledge of the specific problems involved was necessary before maximum use could be made of factorial design in the social sciences.

III. Suggested Aims, Scope, and Methods of a New Study

Mr. Frederick Osborn thought that, as in the Indianapolis Study, the general aim of any new study would be an increase

of knowledge of factors affecting fertility. He doubted the necessity of being concerned over whether or not the factors investigated will affect policy. We are not living in a vacuum and we might well be motivated by the feeling that the prospective study should be of great importance to problems of policy. However, we can rest assured that any important factors affecting fertility will be of interest to policy makers. That matter will take care of itself.

As to scope, Mr. Osborn thought the new study should include provision for investigating the relation of personal characteristics to fertility. In view of Dr. Campbell's remarks he realized the difficulty of incorporating personality tests in surveys. However, he hoped that certain types of personality characteristics might be amenable to measurement in a survey. He thought that some type of neuro-psychiatric screening test such as that described by Dr. Macmillan might be tried. He presumed that some means of testing general intelligence, either with a short test such as that described by Dr. Frederiksen or by using the replies to other questions as described by Dr. Kelly, might be used.

As for type of couples to be studied, Mr. Osborn thought that the new study should concentrate, as before, on those using contraception but that special efforts should be made to secure more large families that are planned as to size.

Dr. Jessie Bernard hoped that at least some consideration would be given to the possibility of a fertility study in a country where the population problem is really acute. She thought that a very useful job could be done in India, studying and comparing couples that do and do not limit births.[8]

Dr. Bernard also felt, however, that new studies of factors affecting size of family are needed in the United States. She

[8] Attention may be called to the fact that the United Nations and the government of India are currently sponsoring a field study of population in relation to economic and social factors in the State of Mysore.

See Durand, John D.: United Nations Population Study in India, a paper in Approaches to Problems of High Fertility in Agrarian Societies. The Milbank Memorial Fund, New York, 1952, pp. 153–158. Some of the questions asked in the Indianapolis Study were incorporated in the schedules for the study in India.

believed that the whole climate of thought regarding marriage and the family had changed greatly since the time when the Indianapolis Study was planned. At that time the public concern was over the low birth rate. We are now in the midst of a high birth rate and a study valid for the conditions of the 'thirties may not be valid for those of the 'fifties.

Dr. Bernard emphasized the fact that people are members of both publics and groups and that we cannot hope to understand them unless we know what publics they belong to and are being subjected to and what groups they are identified with. She, therefore, suggested that in any new study the units of investigation be changed in certain respects. Some of the Indianapolis hypotheses were concerned with the impact of the group but the Study was designed in terms of the individual.

Individuals are affected by gossip and by group norms and conflicts of norms of the type that Dr. Stouffer had mentioned. Dr. Bernard has long been much convinced, for example, about the role of fashion in fertility. She has noted a definite upward trend since the 'thirties in the number of children that girls in colleges say they would like to have. In recent years the women's magazines such as *Glamour, Charm,* and *The Ladies Home Journal* have helped to glorify maternity by showing modern fashions in maternity clothing. There is now a fashion in natural childbirth.

A study of the influences of gossip, group norms, and fashions on fertility requires an interdisciplinary approach. A communications analyst might be called in to assess the impact of the mass media. What filters through to the people? How do fashions and other collective norms start and how are they propagated?

Dr. Bernard believed that in any group there are people who become models for others to follow. Thus Mary and Helen might belong to the same bridge club. When Mary has a baby all of the others want babies. If Helen has the baby the idea is not so contagious.

As for methods of study, Dr. Bernard noted that the same Indianapolis Study sample was used for testing different hypotheses that really required different types of samples. She suggested that a "sample bank" might be considered. Thus a brief preliminary survey of a total city such as the Household Survey of Indianapolis might give some indication of the general characteristics of the population. Then for specific hypotheses one might select a separate sample for study pinpointed to the specific requirements of that hypothesis. This would multiply the number of families in the total sample but it would reduce the load of questions on the respondents in each specific sample.

Dr. Bernard also endorsed the idea of the longitudinal study and suggested linking it with some type of experimentation. Thus it might be possible to set up two matched samples of young couples, supply one group with some type of family allowance for children, and observe the patterns of family growth in the two groups over a period of ten years.[9]

Dr. Reuben Hill stated that his effort to make suggestions for a new study would partake of his current experiences in Puerto Rico. The situations were parallel in that a previous study of broad scope had been made in each case. Thus Paul Hatt's *Backgrounds of Human Fertility in Puerto Rico* was the counterpart of the Indianapolis Study and in each case a new study was desired. In the current study in Puerto Rico, which Dr. Hill is directing, the emphasis is placed on the factors affecting the planning and controlling of family size rather than size of family.

Dr. Hill stated that he definitely agreed with Dr. Cottrell about the importance of framing a study in terms of its potential value to policy makers. In view of the pressing problems

[9] An experimental study of this type has been carried out by Flanagan with the help of the Pioneer Fund which sponsored it. The Pioneer Fund offered educational scholarships to children born to regular officers in the Army Air Corps during the calendar year 1940, provided there were already at least three other living children in the family. A brief report on the accomplishments of this study is included in Flanagan, John C.: A Study of Factors Determining Family Size in a Selected Professional Group, *Genetic Psychology Monographs,* 1942, xxv, pp. 3–99.

in Puerto Rico one is not justified in undertaking social research in that area simply for the purpose of satisfying scientific curiosity or for the purpose of contributing to methodology. It should be framed with reference to its possible use not only by those who will make new studies but also by those in social work and by those concerned with policy.

The family as an interacting group was taken as the unit of study in Puerto Rico. Dr. Hill acknowledged that the husband-wife pair are the major actors in family planning but he emphasized that the offspring exercise significant influences at later stages of the family cycle. Moreover, in a real sense the husband and wife act as agents of the family in their thinking and planning.

Dr. Hill stated that he and his colleagues had looked in vain for a conceptual system which might have rendered theoretically relevant the twenty-three hypotheses in the Indianapolis Study. To provide such an organizing framework in the Puerto Rican Study they agreed upon "the interactional frame of reference" which lends itself well to the study of the family as a planning and decision-making association.

Some of the component concepts of the "interactional frame of reference" are (a) status and inter-status relations, which become the bases for authority patterns and initiative-taking; (b) role, role conceptions, role taking, role playing, and role organization, with parents viewed in role-playing and children viewed in role-taking terms; and (c) processes of communication, consultation, conflict, compromise, and consensus.

Dr. Hill believed that the interactional approach provides not only the tools for observation but also a pool of theory that can be drawn upon in the formulation of diagnostic study questions. Thus from family-interaction theory and its close relative, structure-function theory, come questions which may be used in the quest for the social-psychological antecedents of success in fertility planning and control. These antecedents differ in quality from the psychological and socio-economic correlates of fertility in the Indianapolis Study. They partake

of the dynamic quality of interaction systems and are oriented to intra-group processes rather than to traits, characteristics, and status categories.

Dr. Hill stated that the family is intermittently concerned with what Robert B. Reed called negative and postive aspects of fertility planning among the Indianapolis couples.[10] The family is thus perceived to be the chief decision-making unit with respect to control of family size and the Puerto Rican Study is concentrating on intra-family communication in the decision-making process. Conditions favoring and impeding communication will receive major attention.

Questions designed to explore this dimension of family life were tried out in a preliminary study by Stycos and Hill.[11] The January, 1953 issue of *The Annals* presents some of the possibilities of birth control in Puerto Rico as determined by preliminary application of the study design described above to this problem.[12] A quantitative verification phase of research into the problem is being undertaken by study of 1,000 families during 1953–54.

As to research design, Dr. Hill strongly recommended that in a new study in the United States reconnaissance precede definitive study. He recommended as a guide the descriptions of exploratory research in Jahoda, Deutsch, and Cook: *Methods in Social Research.* In conclusion, he stated that he had tried to illustrate with the Puerto Rican study the use of family-interaction theory to high-light the empirical situations that need exploring by probing questions. Answers to the diagnostic

[10] Reed, Robert B.: The Interrelationship of Marital Adjustment, Fertility Control, and Size of Family. A paper in Whelpton P. K. and Kiser, Clyde V. (Editors): Social and Psychological Factors Affecting Fertility, Vol. II. The Milbank Memorial Fund, New York, 1950, pp. 270–276.

[11] For a description of this preliminary study, *see:*

Hansen, Millard: The Family in Puerto Rico Research Project. A paper in APPROACHES TO PROBLEMS OF HIGH FERTILITY IN AGRARIAN SOCIETIES. The Milbank Memorial Fund, New York, 1952, pp. 50–61.

Stycos, J. Mayone: Family and Fertility in Puerto Rico. *The American Sociological Review,* October, 1952, XVII, No. 5, pp. 572–580.

[12] Stycos, J. Mayone and Hill, Reuben: The Prospects of Birth Control in Puerto Rico. *The Annals* of the American Academy of Political and Social Science, January, 1953, *285:* pp. 137–144.

questions, phrased in terms that the respondents can understand, provide hypotheses of a practical and applied dimension in social psychology which, if tested and confirmed, would add to knowledge that can be used by agencies working directly with families in their fertility problems.

Dr. Leonard S. Cottrell listed several possible criteria or bases for selection of questions to be investigated in any new study of fertility. In the first place he wanted to make it clear that his previous comments about gearing studies to policy implications did not mean that the long-held and cherished rights of scientists to follow the bent of their curiosities should in any way be impaired. Therefore, he would say that whatever those responsible for any new study are most curious about should be a good guide to the formulation of aims of the study.

The chief problems or interests that have emerged from the Indianapolis Study might be good guides for future planning. Thus the tremendous importance of socio-economic status might be used as a point of departure. The dynamics of status have hardly been touched upon and the conditions and processes involved in the placement of a person, both in his own mind and in the minds of others, on any kind of value or status scale, would seem to meet the highly relevant salient as to the selection of problems and what determines the decisions with respect to having or not having another child.

Dr. Cottrell stated that one of his students and also Louis Guttman had once done some research on the relation of personal adequacy to socio-economic status and social mobility. Their results were similar in that they found at least enough evidence for an hypothesis that as individuals approached a critical boundary between their class and the next higher step in the social level their feelings of inadequacy or inferiority develop quite markedly. Once they get safely beyond the critical boundary their feelings of adequacy and superiority mount sharply. Dr. Cottrell suggested that this hypothesis in relation to fertility might well be tested. It would afford classifications

on the basis of dynamic phenomenon and it would take into account the reference group orientations, the value and goal aspiration levels, and more relevantly the factors in the field of perception, such as the having or not having of children, which enhance or inhibit the chances of realizing these goals.

Dr. Cottrell also endorsed the suggestion that a more general type of theoretical structure be developed and that specific problems or hypotheses for testing be derived from this more general theory.

Finally, Dr. Cottrell referred again to the matter of policy. He stated that he was particularly interested in this matter because one of his current professional activities is that of determining how social-science activities can be made more available and more usable in the field of practical action. He thought it would be quite valuable and proper from a scientific standpoint, for instance, to try to ascertain for a given segment of society the processes, dynamics, or interventions that might be required to encourage higher or lower birth rates.

Dr. Frank W. Notestein thought that any consideration of whether a study should be concerned with policy should take into account the many and subtle relationships that condition the scale and nature of childbearing. Many of these probably are tied up inextricably with the whole process of social change and there is the question as to whether fertility habits can be changed without manipulating the whole environment. However, if usefulness of a study for policy makers is an important consideration, some special emphasis might be given to exploring the effectiveness of those things that might be done by sharp intervention on a limited scale. This might simply dictate some selection of interests of the investigators.

One of the specific problems that might be dictated by desire to have a new study useful to policy makers is that of the reasons for the increase in the marriage rate during the past decade. Dr. Notestein pointed out that only about 11 per cent of the females 25–29 years of age were reported as single in 1951. This was once the residual rock-bottom proportion of

spinsters at the end of the childbearing period. Since the trend toward earlier marriage has been an important component of the increase in the birth rate, the conditions affecting marriage as well as those affecting fertility might well be included in a future study.

Dr. Notestein also emphasized the importance of trying to ascertain the reasons for the very sharp relationships of fertility behavior to socio-economic status as compared with its weak relationship to attitudinal responses. In this connection he thought it would be strategic to give special attention to those aspects of the Indianapolis Study that did yield fairly rich results, such as economic security, personal adequacy, and marital adjustment.

Dr. Notestein was of the opinion that something rather unpredictable accounted for much of the change in the fertility situation during the past decade. Marriage and fertility behavior appeared to be very little oriented to the long-run position and the present came greatly into immediate importance. He would like to see a study grapple with the reasons for this apparent change. He agreed that fashion might be an important factor. However, he emphasized that the problems involved are complex and difficult.

As for method, Dr. Notestein agreed about the need for sharper scaling but he believed that scaling deficiencies were by no means the fundamental weakness of the Indianapolis Study. If the characteristic is sharply defined the results will show up with only crude scaling. He doubted that one could devise scales of socio-economic status sufficiently poor to obscure the relation of fertility to this variable. Dr. Notestein did suggest, however, that the next study might give more attention to inter-pregnancy intervals than had been done thus far in the analyses of Indianapolis data.

Dr. Notestein thought that the preceding discussions had emphasized the complexities and difficulties that would be encountered in a new study. He believed that the development of plans for a new study should proceed with great care and

that the materials in the Indianapolis Study should be exploited to the full in planning the next study. This adds up, he thought, to the desirability of having a one-year seminar with membership drawn from a variety of skills and institutions and staffed with at least one or two full-time people whose business it would be to see what could be gotten out of the Indianapolis materials as guides for the next study. The analytical jobs required might well be farmed out to different members of the seminar willing to take them. Following this, another year or two might be devoted to detailed planning of a series of next steps. These next steps themselves might be farmed out to persons or institutions having the particular skills needed. The tooling up stage and perhaps any final study might be decentralized in this manner.

He suggested finally that the proposed seminar might be concerned with ways and means of linking special studies to the more general and conventional information available from the Census and National Office of Vital Statistics and, alternatively, to see how further analyses of the more general types of data might be directed in ways that will make the special studies more fruitful.

Discussion. Dr. Freedman thought the proposed seminar might well afford a means for testing some of the specific suggestions that had been made at the present meeting. A relatively small group might undertake to develop a theoretical construct and perhaps some of the derived specific problems. Some of these specific problems might be tested in part from the Indianapolis materials. Others might require small-scale studies for which some funds should be available. There are certain lines of experimental work that could be done for testing the instruments and procedures. The small-scale experiments might be an economical way of telling us what is practical for a large field study.

Regarding the linking of special studies with official data, Dr. Dunn stated that it might be possible to have the Census Bureau include certain questions of relevance to the special

study in one or more of the Current Population Surveys. For instance, through this device it might be possible to identify families having births during a particular month. Then through a collaborative means of follow-up it might be possible to secure additional information from these families. A bridge between the specialized studies and official statistics might help to interpret secular trends in marriage and fertility. It might broaden the interpretive base of the official data and also provide a wider bench-mark base of official data to which one could tie a considerable number of the special studies that are now cross-sectional in nature and rather isolated from their broader significance.

Dr. Daniel O. Price suggested that the establishment of some sort of coordinating committee on research in fertility also might help to broaden the interpretive base of specialized studies. He was hoping to start some type of longitudinal study of selective migration at the University of North Carolina. He thought it possible that one or two specific hypotheses on the relation of migration to fertility might be tested in such a study. There may be other hypotheses that could be farmed out to other studies in prospect.

Regarding Dr. Dunn's comments, Dr. Conrad Taeuber stated that it might also be possible to use the 1950 Census fertility sample to put the findings of the Indianapolis Study in a broader framework. Thus it might be possible to replicate the Indianapolis sample in several important respects such as nativity, color, age, duration of marriage, and education, and ascertain whether the Indianapolis Study really failed to predict the fertility of couples of the type that were studied or whether the baby boom arose from the different behavior of population groups with other characteristics.

Dr. Hill suggested that if the interviewers had gotten closer to the families studied they might have picked up some indication of a forthcoming increase in the birth rate. Dr. DeVinney doubted that this was necessarily true. He recalled Whelpton's statement that women of the same age as those in

the Indianapolis Study had contributed little to the increase of the birth rate. If the study had dealt with newly married couples, it might have given some evidence of what was to come. However, among the couples studied (married 12–15 years) approximately one-half of the conceptions were not wanted, at least at the time that they occurred. Therefore, the closer the interviewers got to these couples, the closer they might have come to the assumption that birth rates would be still further depressed. The notion that the future trend will be revealed just by getting close to a group of families may be a little treacherous unless one is making a very sophisticated analysis of the actual forces and causal relationships that are involved.

Dr. Thompson urged caution in acceptance of the belief that a fundamental change in reproductive behavior had occurred. He stated that the evidence is not yet adequate to justify the conclusion that there has been any actual increase in size of family. He thought one should await the cohort studies and tests of the type that Dr. Taeuber mentioned before concluding that an increase in average size of completed family has occurred. It is possible that the average size of completed family is unchanged despite the earlier marriage and change in timing of births. He agreed, however, that apart from size, change in timing is an important change in fertility behavior.

Dr. Wilbert E. Moore noted that several speakers had emphasized the importance of broad social changes. At the risk of reading himself out of the sociological profession he would urge the planners of the next study not to abdicate their responsibilities to the central purpose of the study. The ultimate aim, as he saw it, was to predict intimate individual or paired behavior within a given social context. Thus the study should be focused on motivation. He further suggested that if one is interested in potential changes in a situation, the deviants become of great importance.

Dr. Moore believed that the previous discussions regarding

tensions, security, questions of whether or not cognitive planning is undertaken, the time dimensions of the planning, etc. all pointed rather clearly to the need for concentrating the study on tensions of the individual as he fits himself into the social system. He thought that this type of approach would enable one to get further with reference to the circumstances necessary to get people to take a rational or at least a cognitive orientation toward their behavior in marital relationships and childbearing. He agreed with previous speakers about the need for further tests of the relation of verbal response to actual behavior.

Dr. Joseph J. Spengler stated that the child could be regarded as a package of utility and that fertility could therefore be analyzed in terms of the family's consumption pattern. He suspected that much of the change in the birth rate could be interpreted in terms of changes in consumption patterns. He therefore suggested that the next study provide for an investigation of the relation of changes in family income through time to changes in consumption patterns (including the having of children). Retrospective data of this type might be collected in a single survey but the ideal would be a longitudinal study in which the various changes are recorded as they occur.

Dr. George J. Stolnitz said that some of the remarks that had been made about socio-economic variables on the one hand and attitudinal and personality variables on the other, pointed up the need for making an important distinction regarding objectives of any future study. This is quite apart from the relative success in measuring the two types of variables. If the major objective is that of predicting fertility trends for the general population of the United States it is perhaps more important to have good data by socio-economic status than by attitudes. This arises from the fact that Census data provide a basis for weighting the rates by socio-economic status according to the importance of these classes in the general population. In contrast, there is no direct basis for weighting fer-

tility rates that are distributed by attitudes or personality traits in the general population. However, if the chief objective is that of studying the motivations and personality characteristics in fertility, the attitudinal data are obviously needed.

Dr. Bernard liked the idea of farming out separate parts of a study to individuals or agencies having the special skills required. She would extend this principle to any final study. She thought it unfortunate that in this country the tendency to regard something big as good had extended even to attitudes towards research. Dr. Macmillan agreed in terms of the development of techniques and stated that during the past hundred years much of the outstanding research of this developmental character had been of small scope, which then could be applied with some measure of assurance to the larger scale study.

Dr. Stouffer doubted that the question of the relative merits of small-scale and large-scale research was an important one. Both types are and should be used by large research organizations. Small studies are valuable for refining techniques and for securing leads, as well as for furnishing limited substantive data. Professor Stephan agreed and said that judgment was necessary in deciding when it is important to have intensive observation of a few individuals and when it is important to use broader statistical surveys. The advantages feed back and forth and each type of study is better for having the other.

In closing the meeting the Chairman stated that he would not undertake to summarize the discussion. The purpose of the session was not to secure a set of formal recommendations but rather to have the benefit of free discussion and exchange of ideas on the possibilities of a new study in fertility among people engaged in this and related fields. He expressed the hope, however, that the suggestion regarding the formation of a seminar or group to proceed with next steps might materialize. He was sure that the suggestions offered at the present meeting would constitute a stimulus for further re-

search in the field and that they would be of much help in shaping the preparations and plans for such research.[13]

[13] Addendum. A work session on preparation for new studies of fertility, attended by twenty-eight people, was held at Princeton University June 18–19, 1953, under the sponsorship of the Milbank Memorial Fund. Further preparations and plans for research in this field will be developed under a grant recently made for this purpose by The Population Council.

Appendix

Participants at the Round Table on Exploration of Possibilities for New Studies of Factors Affecting Size of Family

November 19–20, 1952

Twenty-ninth Annual Conference of the Milbank Memorial Fund

Chairman: Lowell J. Reed, *Vice President*
The Johns Hopkins University

Balfour, Marshall C., M.D., *Assistant Director, Division of Medicine and Public Health, The Rockefeller Foundation*

Bernard, Jessie, *Department of Sociology, The Pennsylvania State College*

Borgatta, Edgar F., *Lecturer and Research Associate, Laboratory of Social Relations, Harvard University*

Boudreau, Frank G., M.D., *Executive Director, Milbank Memorial Fund*

Brady, Dorothy S., *Consultant on Costs and Standards of Living, Bureau of Labor Statistics, Department of Labor*

Campbell, Angus, *Director, Survey Research Center, University of Michigan*

Cottrell, Leonard S., Jr., *Social Psychologist, Russell Sage Foundation*

Deutsch, Morton, *Assistant Professor of Psychology, Graduate School of Arts and Science, New York University*

DeVinney, Leland C., *Associate Director, Division of Social Sciences, The Rockefeller Foundation*

Dunn, Halbert L., M.D., *Chief, National Office of Vital Statistics*

Fisher, Burton R., *Professor of Sociology, University of Wisconsin*

Frederiksen, Norman, *Head, Research Department, Educational Testing Service*

Freedman, Ronald, *Department of Sociology, University of Michigan*

Herrera, Lee F., *Division of Biostatistics, New York University School of Medicine*

Hill, Reuben, *Research Professor in Family Life, Institute for Research in Social Science, University of North Carolina*

Kantner, John F., *Department of Sociology, College of William and Mary*

Kelly, E. Lowell, *Professor of Psychology, University of Michigan*

Kiser, Clyde V., *Milbank Memorial Fund*

Macmillan, Allister M., *Programme of Studies in Community Development, Cornell University*

Moore, Wilbert E., *Professor of Sociology, Princeton University*

Notestein, Frank W., *Director, Office of Population Research, Princeton University*

Osborn, Frederick, *President, American Eugenics Society*

Pratt, Lois V., *Bureau of Applied Social Research, Columbia University*