ADDITIONAL MEASURES OF USE-EFFECTIVENESS OF CONTRACEPTION

Robert G. Potter, Jr.

Introduction

Whether the contraception of a sample is followed concurrently for a limited period, or retrospectively for an entire pregnancy interval, its use-effectiveness is conventionally measured by a summary pregnancy rate, i.e., the number of accidental pregnancies per 100 years of exposure. In a few recent analyses, to supplement this summary rate, the total exposure period has been segmented into successive periods and subinterval pregnancy rates also computed.[1] In the absence of postpartum amenorrhea, these subinterval rates typically decline, usually at a quite rapid rate. The sharpest decreases have been found among clinic patients whose use of a single prescribed method is followed for a limited period. Tietze's hypothesis regarding this steep decline is worth quoting in full:

> Human couples are markedly different not only in fecundity but also in ability and willingness to practice contraception con-

sistently. When a group of couples takes up birth control, accidental pregnancies tend to occur early among the less motivated or less skillful couples, and perhaps also among the more fecund couples. As the months go by, these types of couples are eliminated from the group while the more determined contraceptors remain, including among them an increasing proportion of subfecund and sterile pairs.

> Availability of several contraceptive methods further intensifies the process of self-selection. Couples who find a particular method objectionable and are therefore candidates for a contraceptive failure sooner or later change to a different method, leaving a high proportion of satisfied users among those who continue to rely on the same method.[2]

Declining pregnancy rates are also found among samples using a variety of methods and interrogated on a retrospective basis.

This paper attempts to extend methods for describing and testing the significance of differences between such declines of pregnancy risk. Any group's practice of contraception during a single pregnancy interval, or portion thereof, may be profitably viewed as a selective process wherein, with the passage of exposure, the more pregnancy-prone members are progressively eliminated by accidental pregnancy, and perhaps also by self-removal, and left behind is a more and more homogenously low-risk remainder. With adequate sample size, the intensity and speed of this selective process are most directly described by a series of subinterval pregnancy rates measuring the levels of pregnancy risk at the start of exposure and at specified durations thereafter. To supplement this description, one may adopt life-table methods and calculate the proportions of a hypothetical cohort who would remain protected for specified durations of exposure if (a) they were subject to the successive monthly rates of pregnancy observed in the sample and (b) all continued contraception until pregnancy. An additional advantage of this latter approach is that it is accompanied by formulas for estimating standard errors.

A Clinic Experience

The first application is made to a clinic experience published by Tietze and Lewit[3] to illustrate their methodology. This fictional experience incorporates two features characteristic of clinic data: an appreciable drop-out rate among patients and a series of subinterval pregnancy rates that decline rapidly with increasing exposure. The authors' procedures lead to two basic tables, reproduced below as Tables 1 and 2. In the first table, the hypothetical clinic patients are classified as to length of exposure and status at end of observation. It is assumed that exposure length is being measured in units of whole months. The records of 74 patients "not contacted" are set aside and another 95 patients are classified as having zero months of exposure with the prescribed method. Subtraction of these two groups leaves a sample of 1,112 patients practicing the prescribed method one month or longer, whose use-effectiveness is to be assessed.

Of the 1,112, 758, or 68 per cent, remain "active users" throughout an observation period ranging from 13 to 20 months depending on the calendar date of the patient's first visit. Another 11 per cent become accidentally pregnant. The exposures of the remaining 20 per cent are interrupted under other circumstances, mainly when they shift to other methods or abandon contraception. Moreover, half of these interruptions occur during the first three months of exposure.

In their second table, Tietze and Lewit derive a summary pregnancy rate of 10.4 pregnancies per 100 years of exposure and also a set of subinterval pregnancy rates declining precipitously from a peak of 29.1 pregnancies per 100 years of exposure, during the first 3 months of exposure, to a rate of only 1.3 pregnancies per 100 years during exposure months 13–20. This decline in pregnancy rate is more rapid and proceeds farther than declines empirically observed, although sharp declines are characteristic of clinic experiences.[4]

TABLE 1. EXPOSURE LENGTH AND STATUS AT END OF OBSERVATION: DATA OF TIETZE AND LEWIT.[1]

Month of Exposure	Status at End of Observation[2] a	b	c	d	e	f	g	Total a–g[3]	h
0	—	—	—	—	66	23	6	95	65
1	—	34	—	—	44	11	1	90	4
2	—	26	—	2	14	18	5	65	—
3	—	15	—	2	6	7	4	34	—
4	—	8	2	1	6	4	2	23	—
5	—	8	—	—	4	4	2	18	2
6	—	10	—	—	3	5	6	24	—
7	—	7	2	4	8	3	1	25	—
8	—	5	1	—	—	2	—	8	—
9	—	—	—	1	—	—	4	5	—
10	—	2	1	2	4	—	1	10	1
11	—	3	1	2	2	2	—	10	1
12	—	2	4	1	—	2	—	9	—
13	72	—	6	1	—	1	—	80	—
14	105	1	2	1	—	—	1	110	—
15	89	1	2	—	—	—	—	92	—
16	104	—	—	—	1	—	—	105	—
17	102	—	—	—	—	—	2	104	—
18	106	2	4	1	—	—	—	113	1
19	96	—	3	—	1	—	—	100	—
20	84	—	2	—	1	—	—	87	—
	758	124	30	18	160	82	35	1207	74

[1] Source: adapted from Tietze, C. and Lewit, S.: Recommended Procedures for the Study of Use-Effectiveness of Contraceptive Methods, *op. cit.*, Table 1, p. 9.

[2] a- Active user; b- Accidentally pregnant; c- Planning a baby; d- Contraception not needed; e- Change of method; f- Contraception abandoned; g- Moved away; h- Not contacted.

[3] To be used in computing pregnancy or failure rates.

TABLE 2. SUBINTERVAL PREGNANCY RATES: DATA OF TIETZE AND LEWIT.[1]

Month of Exposure x	*Patient-months of Exposure* N_x		*Accidental Pregnancies* D_x		*Pregnancy Rate* $1200\Sigma D_x/\Sigma N_x$
1st	1112		34		
2nd	1022		26		$\frac{75 \times 1200}{3091} = 29.1$
3rd	957		15		
		3091		75	
4th	923		8		
5th	900		8		$\frac{26 \times 1200}{2705} = 11.5$
6th	882		10		
		2705		26	
7th	858		7		
8th	833		5		$\frac{12 \times 1200}{2516} = 5.7$
9th	825		..		
		2516		12	
10th	820		2		
11th	810		3		$\frac{7 \times 1200}{2430} = 3.5$
12th	800		2		
		2430		7	
13th	791		..		
14th	711		1		$\frac{4 \times 1200}{3590} = 1.3$
15th	601		1		
16th	509		..		
17th	404		..		
18th	300		2		
19th	187		..		
20th	87		..		
		3590		4	
Grand total		14332		124	$\frac{124 \times 1200}{14332} = 10.4$

[1] Source: adapted from Tietze, C. and Lewit, S.: *Ibid.*, Table 3, p. 11.

At any rate, an intense, rapid selective process is being simulated wherein a sample, which starts off with a fairly high rate of pregnancy is, within a year, screened down to a group of highly efficient users of the prescribed method. About one quarter of the original 1,112 patients are eliminated during the screening process.

Adjusted Proportions Remaining Protected. A supplementary way of depicting the speed and intensity of this selective process is to consider what might happen to a cohort of patients if none discontinued practice of the prescribed method during 20 months until pregnancy and if, during this period, they were subject to the schedule of monthly rates of accidental pregnancy calculated for the present clinic sample. In other words, it is being assumed that there are no interruptions of exposure, except by accidental pregnancy, for 20 months, and the estimated proportions remaining active users reflect this condition of no drop-out.

For this purpose a very simple life table device may be used. In Table 2, one has, for each month of exposure x, the number of couples exposed, N_x, as well as the number of accidental pregnancies, D_x, from which one may compute a monthly pregnancy rate $q_x = D_x/N_x$. The complement of this rate, $p_x = 1 - q_x$, gives a monthly rate of protection. An accumulative product of the first x monthly protection rates, namely $P(x) = \pi p_i$, yields the desired probability of surviving the first x months of exposure without pregnancy. In life table terms, $P(x)$ is the same as l_x where l_0, the radix, has been set equal to 1.0.[5]

Results from this type of calculation are given in Table 3. A proportion of .87 could expect to remain protected during the 20-month exposure period. The standard error of this proportion, estimated by formulas discussed in a later section, is of the order of .01. Of the 13 per cent expected to become accidentally pregnant, 10 per cent would do so in the first 6 months, and only 3 per cent in the next 14 months. Among patients still protected at the end of 12 months, only about 1 per cent would lose that protection during the succeeding 8 months.

While such calculations might help to dramatize the high efficiency represented by patients still practicing the prescribed method after a year, they cannot be construed as furnishing an unbiased picture of what might have happened if none of the original sample had interrupted exposure for such reasons as shifting to other methods or abandoning contraception. Presumably if all patients had been constrained to continue practicing the prescribed method, the frequency of recorded accidents would have been higher and slower to decline.

Adjusted Pregnancy Rates. When comparing clinic experiences, one would like to be able to eliminate the biases resulting from differing schedules of patient drop-out. Interruptions of exposure for reasons other than accidental pregnancy affect subinterval pregnancy rates in two ways. First, if the couples shifting to other methods or abandoning contraception are relatively pregnancy-prone, then their removal causes subinterval pregnancy rates to decline more rapidly than they otherwise might. Secondly, interruptions of exposure for reasons other than accidental pregnancy affect subinterval pregnancy rates by increasing the relative weight given to experience occurring early in the subinterval while lessing the relative weight given to experience occurring late in the subinterval. To remove this second bias, one seeks a pregnancy rate in which these monthly experiences are weighted as they would be in the absence of drop-out. This desired adjustment is readily accomplished by extending the calculations of Table 3.

Consider adjusting the pregnancy rate relating to exposure months 4–6 inclusive. From the last column of Table 3 one has the adjusted proportions $P(3)$, $P(4)$, $P(5)$, and $P(6)$ still protected at the beginning of months 4, 5, 6, and 7. By taking first differences—i.e., $P(3)-P(4)$, $P(4)-P(5)$, and $P(5)-P(6)$—one obtains the proportions becoming pregnant during months 4, 5, and 6. The total proportion conceiving dur-

TABLE 3. PROPORTIONS REMAINING PROTECTED FOR SPECIFIED DURATIONS OF EXPOSURE, ADJUSTED FOR THE CONDITION OF NO DROP-OUT.

(1) *Month of Exposure*	(2) *Observed Monthly Conception Rate*	(3) *Observed Monthly Protection Rate*	(4) *Adjusted Proportion Remaining Protected During Initial x Months*
x	q_x	p_x	$P(x)$
1	.03058	.96942	.96942
2	.02544	.97456	.94476
3	.01567	.98433	.92996
4	.00867	.99133	.92190
5	.00889	.99111	.91370
6	.01134	.98866	.90334
7	.00816	.99184	.89597
8	.00600	.99400	.89059
9	.00000	1.0000	.89059
10	.00244	.99756	.88842
11	.00370	.99630	.88513
12	.00250	.99750	.88292
13	.00000	1.0000	.88292
14	.00141	.99859	.88168
15	.00166	.99834	.88022
16	.00000	1.0000	.88022
17	.00000	1.0000	.88022
18	.00667	.99333	.87435
19	.00000	1.0000	.87435
20	.00000	1.0000	.87435

TABLE 4. COMPARISON OF OBSERVED AND ADJUSTED PREGNANCY RATES: DATA OF TIETZE AND LEWIT.

Month of Exposure	*Pregnancies Per 100 Years of Exposure*	
	Observed	*Adjusted*
1–3	29.1	28.8
4–6	11.5	11.6
7–9	5.7	5.7
10–12	3.5	3.5
13–20	1.3	1.5
1–20	10.4	8.3

TABLE 5. COMPARISON OF STATUSES AT END OF SPECIFIED DURATIONS OF EXPOSURE, FOLLOWING MARRIAGE.

Duration of Exposure (Months)	*Indianapolis Study (n = 593)*				*Princeton Fertility Study (n = 522)*			
	Accidentally Pregnant	*Planning Pregnancy*	*Active User*	*Total (Per Cent)*	*Accidentally Pregnant*	*Planning Pregnancy*	*Active User*	*Total (Per Cent)*
6	19	4	77	100	17	15	68	100
12	31	8	60	99	26	27	47	100
24	40	15	45	100	32	45	24	101
48	48	22	30	100	34	61	5	100

ing the 3-month interval, or $P(4,6)$, is $P(3)-P(6)$. To compute a pregnancy rate one also needs the total exposure experienced during the subinterval. The expression for this total exposure is:

$$E(4,6) = [P(3)-P(4)] + 2[P(4)-P(5)] + 3[P(5)-P(6)] + 3\,P(6) = P(3)+P(4)+P(5).$$

Note that because exposure is being measured in whole months, the same exposure length must be assigned to women conceiving in the last month of the subinterval as to those women surviving the subinterval without pregnancy.[6]

In general, the formula for an adjusted pregnancy rate spanning months $m, m+1, \ldots, n$ inclusive is:

$$F(m,n) = 1200\mathrm{P}(m,n)/E(m,n) = \frac{1200\,[\mathrm{P}(m)-\mathrm{P}(n)]}{P(m-1)+P(m)+\ldots+P(n-1)}$$

Adjusting pregnancy rates in this manner usually changes them little when the subinterval is narrow, as Table 4 illustrates. Summary pregnancy rates are more likely to be significantly affected.

Two Survey Samples

Attention now turns to two more general samples using a variety of contraceptive methods in the period following marriage. These data, collected retrospectively rather than concurrently, come from the Indianapolis[7] and Princeton Fertility Studies.[8] Here, when a couple shift their method they remain under observation, so that nearly the only circumstance for dropping out is when contraception is deliberately interrupted in order to have a planned pregnancy. The statuses of the two samples at the end of 6, 12, 24, and 48 months of exposure are compared in Table 5.

Evidently fewer Indianapolis couples are planning early pregnancies and perhaps mainly for this reason they show, at each duration, higher proportions classified either as accidentlly pregnant or still actively practicing contraception.

That this explanation does account for the contrasting incidence of accidental pregnancy is demonstrated in Table 6. Computed on a subinterval basis, pregnancy rates are almost identical in the two samples. The similarity extends to adjusted as well as to directly calculated subinterval pregnancy rates and also to adjusted proportions remaining protected for specified durations of exposure. Fairly wide subintervals are used because exposure bases of at least 1,200 months, and preferably 2,000 or more, are needed for reasonably stable pregnancy rates. Incidentally, it is unlikely that the declines in the two series of pregnancy rates are being seriously exaggerated by a tendency for couples desiring short postponements of pregnancy to be selectedly accident-prone. If such a correlation existed, one would not expect the two samples to exhibit such similar declines of pregnancy risk in view of the much higher percentage of Princeton Study couples planning an early first pregnancy.

Though appreciable, the declines of pregnancy risk observed for the two survey samples are less rapid and less far-reaching than is typical for clinic experiences. The contrasting speeds and intensity of selection between the fictional clinic experience analyzed earlier and the two survey experiences are dramatized when one considers the hypothetical proportion that might remain protected for specified durations if made subject to the observed monthly rates of pregnancy and if all continued contraception until pregnancy. Theoretically, in the clinic sample only 13 per cent would experience pregnancy during 20 months, and most of these would fail in the first 6 months. Among couples from the Indianapolis and Princeton samples, 18 per cent would fail in the first 6 months, and another 15 per cent in the succeeding 14 months. In fact, only about half would remain protected as long as 4 years, and about 60 per cent for 3 years.

The subinterval pregnancy rates of the Indianapolis and

Princeton Fertility Study couples do not go below 10 pregnancies per 100 years of exposure. Two factors may be operating to limit the selective process and to prevent it from taking the pregnancy rates to a lower level. First of all, the survey couples still practicing contraception a year after marriage are using a mixture of methods, not solely an efficient, prescribed method. Secondly, as compared to the clinic patients, a larger percentage of the survey couples are practicing contraception to postpone rather than to prevent pregnancy, and for that reason have less to fear from an accidental pregnancy.

In both surveys the decline in subinterval pregnancy rates (Table 6) is irregular. During the first 12 months, decline is slow; then occurs a plunge, followed by slow decline. The fact that the plunges occur at about the same time in the two studies greatly reduces, though it does not wholly eliminate, the possibilty of an extreme sampling fluctuation. But if the plunges are accepted as real, then one has to posit additional mechanisms besides merely the progressive elimination of pregnancy-prone couples by conception.

Many of the clinic patients who dislike the prescribed method and shift to another method, or who abandon contraception, remove themselves thereby from observed exposure early enough so that an accidental pregnancy is not charged

TABLE 6. COMPARISON OF PREGNANCY RATES AND PROPORTION REMAINING PROTECTED FOR SPECIFIED DURATIONS FOLLOWING MARRIAGE.

	Indianapolis Study			*Princeton Fertility Study*		
Interval of Exposure	*Observed Pregnancy Rate*	*Adjusted Pregnancy Rate*	*Adjusted Proporation Remaining Protected*	*Observed Pregnancy Rate*	*Adjusted Pregnancy Rate*	*Adjusted Proporation Remaining Protected*
1–6	42.0	41.9	.81	39.1	39.2	.82
7–12	36.8	36.6	.67	32.4	32.4	.70
13–24	15.8	15.8	.57	15.1	15.4	.60
25–48	10.7	10.6	.46	11.4	9.8	.49
1–48	23.1	21.5	.46	25.9	19.9	.49

against them. Matters are quite different in the two surveys. Respondents are asked whether they used contraception at all during the interval following marriage. Then they are asked whether they planned their first pregnancy in the sense of deliberately interrupting contraception in order to have it. Respondents answering *yes* to the first question (did use contraception) and *no* to the second (pregnancy occurred under another circumstance) are classified as experiencing a contraceptive failure. Thus the couples who practice irregular contraception or soon abandon it because they find it uncongenial are typically charged with accidental pregnancy. The same outcome is likely for pairs practicing irregular contraception or abandoning it because they cannot agree about how long they want to delay their first pregnancy. In this manner a relatively high pregnancy rate could be maintained for several months longer than is typical for clinic samples.[9] Indeed, it is not wholly implausible to suppose that in retrospective studies at least a few respondents who experience pregnancy within a year of marriage on account of not practicing contraception at all will mistakenly recall unsuccessful contraception.

Estimating Standard Errors

One limitation of present methodology for measuring use-effectiveness of contraception is its poverty of formulas for estimating standard errors.[10] An approximate formula is available for the adjusted proportions remaining protected during specified durations of exposure, utilized in Tables 3, 4, and 6. In some situations, illustrated below by a comparison of Indianapolis and Princeton samples, this standard error affords the basis for a more useful test of significance than do the one or two other formulas currently available.

Examining the subinterval rates of Table 6, especially the adjusted pregnancy rates and the adjusted proportions remaining protected, one finds that the recorded use-effectiveness of the Indianapolis couples is barely lower than that of the Princeton Study couples throughout the 48-month exposure period. At the same time the folly of resting the comparison on unadjusted summary pregnancy rates is manifest; this pair of rates, because of their sensitivity to the contrasting schedules of dropout,[11] produce a difference in the opposite direction.

The question at issue is whether the pregnancy rate of Indianapolis couples is enough higher than that of the other sample so that the difference may be considered as statistically significant. One would not want to base a test of significance on the biased difference yielded by the two unadjusted summary pregnancy rates. Formulas are not presently available for estimating the sampling errors of adjusted summary pregnancy rates. Procedures for expoliting subinterval pregnancy rates are also lacking. Individually, these subinterval pregnancy rates command only small exposure bases. Together they present the problem of statistical dependence. However, the standard error of the adjusted proportion remaining protected for x months may be estimated by:

$$S.E._{P(x)} = P(x)\left[\sum_{i=1}^{x} \frac{q_i}{n_i p_i}\right]^{\frac{1}{2}} \qquad [1]$$

where p_i, q_i, N_i, and $P(x)$ have the same meanings as before.[13] Formula [1] is approximate inasmuch as the monthly conception rates, q_i, are not independent. The formula becomes exact only asymptotically as sample size increases. Given moderate or small samples, formula [1] tends to be biased downward owing to the neglect of a host of positive covariance terms. However, as Goodman notes,[14] this bias is usually small when all q_i are small, which typically they are in a contraceptive context provided that N_x-values are not allowed to become too small by extending calculations over too long a duration of exposure x.[15]

Calculation with Formula [1] is straightforward but tedious. To estimate a standard error for $P(48)$ calls for computing and summing 48 terms. To lighten computation, one may adopt a further approximation in which the monthly functions N, q, and p are replaced by analogous functions relating to subintervals of arbitrary width. For instance, one might elect to use 6-month intervals. Applied to an exposure-span of 48 months, the alternative formula would be:

$$(S.E.)'_{P(48)} = P(48)\left[\sum_{i=0}^{7} \frac{q'_{1+6i}}{N'_{1+6i}\, p'_{1+6i}}\right]^{\frac{1}{2}} \qquad [2]$$

where

$$p'_{1+6i} = P(6i+6)/P(6i)$$
$$q'_{1+6i} = 1 - p'_{1+6i}$$
$$N'_{1+6i} = N_{1+6i} - W'_{1+6i}/2$$

The symbol W'_{1+6i} designates the number of dropouts—i.e., terminations of exposure for reasons other than accidental pregnancy—occurring during the 6-month interval including months $6i+1, 6i+2, \ldots, 6(i+1)$. It is assumed that $P(x)$-values have already been computed so that they may be used to derive p'_x-values in the convenient fashion indicated.

Estimates resulting from [2] may be higher or lower than those based on [1]; more often than not they will be lower; but judging from applications so far, they will be close enough for practical purposes. (See Appendix for further discussion).

Among Indianapolis couples, the adjusted proportion remaining protected for 48 months is .027 less than in the Princeton Study sample. By formula [2], the standard errors of the individual proportions are .0226 and .0341. These statistics yield a nonsignificant critical ratio of:

$$\frac{P'(48) - P''(48)}{(S^2_{P'(48)} + S^2_{P''(48)})^{\frac{1}{2}}} = \frac{.027}{.041}$$

Thus the observed difference between samples is not large enough to justify rejecting the hypothesis that use-effectiveness of contraception is the same in the two groups, at least during the 48-month span of exposure considered.

Summary

Any group's practice of contraception during a single pregnancy interval may be viewed as a selective process wherein, as exposure time elapses, the more pregnancy-prone are progressively removed by accidental conception, and perhaps also by self-removal, leaving behind a more and more homogenously low-risk remainder. Consequently, when a series of pregnancy rates are calculated for successive segments of the total exposure period, these subinterval rates typically decline. This paper attempts to extend methods for describing and testing the significance of differences between such declines. Applications are made to a clinic sample that exhibits an exaggeratedly sharp decline of subinterval pregnancy rates as well as to two survey samples displaying less steep, though still appreciable, declines of pregnancy risk.

As an additional way of describing a decline of pregnancy risk, one may consider the proportions of a hypothetical cohort that would remain protected for specified durations of exposure if (a) they were subject to the successive monthly risks of pregnancy empirically observed and if (b) all continued contraception until pregnant. One advantage of this life-table approach is that it is accompanied by formulas for estimating standard errors. This asset becomes important in situations where, because of differing schedules of drop-out, the conventional pregnancy rate yields a biased comparison, and one has no other comparative statistic on which to base a significance test.

Regarding clinic experience, patient drop-out affects subinterval pregnancy rates in two ways. First, if the couples shifting away from the prescribed method or abandoning contraception are relatively pregnancy-prone, then their removal causes subinterval rates to decline more rapidly than they otherwise would. Secondly, interruptions of exposure for reasons other than accidental pregnancy affects subinterval pregnacy rates by increasing the relative weight given to experience

occurring early in the subinterval while lessening the relative weight given to experience occurring late in the subinterval. This second bias is rather easily removed. The effect of the adjustment is usually small for subinterval pregnancy rates, but may be quite significant for a summary pregnancy rate.

Appendix

COMPARISON OF FORMULAS [1] AND [2] FOR ESTIMATING STANDARD ERRORS OF P(X)

In applications so far, the standard errors estimated by formulas [1] and [2] usually differ only in the fourth decimal place. Table A-1 is illustrative. The risk of more serious deviations increases when subintervals wider than 6 months are used in [2], when the frequency of dropout is higher, and especially when sample size becomes small since then both formulas become inefficient.

TABLE A-1. COMPARISON OF ESTIMATED STANDARD ERRORS OF $P(x)$ BY FORMULAS [1] AND [2]: DATA FROM PRINCETON FERTILITY STUDY.

Length of Exposure Period x	*Adjusted Proportion Remaining Protected* $P(x)$	*Standard Error of P(x) As Estimated By:* Formula [1]	Formula [2]
6	.819	.0176	.0175
12	.695	.0221	.0221
24	.595	.0261	.0257
36	.523	.0304	.0300
48	.492	.0336	.0341

REFERENCES

[1] Examples are Tietze, C.: Differential Fecundity and Effectiveness of Contraception, *The Eugenics Review*, January, 1959, 50: 231–237; Potter, R. G., Jr.: Contraceptive Practice and Birth Intervals among Two-Child White Couples in Metropolitan America. THIRTY YEARS OF RESEARCH IN HUMAN FERTILITY: RETROSPECT AND PROSPECT. New York, Milbank Memorial Fund, 1958, pp. 74–92; Tietze, C., and Alleyne, C.: A Family Planning Service in the West Indies, *Fertility and Sterility*, May–June, 1959, 10: 265–266; and Tietze, C.: The Use-Effectiveness of Contraceptive Methods. *In* Clyde V. Kiser (ed), RESEARCH IN FAMILY PLANNING. Princeton, Princeton University Press, 1962, pp. 358–361.

[2] Tietze, C.:The Use-Effectiveness of Contraceptive Methods, *op. cit.*, p. 359.

[3] Tietze, C., and Lewit, S.: Recommended Procedures for the Study of Use-Effectiveness of Contraceptive Methods, reprinted from INTERNATIONAL PLANNED PARENTHOOD FEDERATION MEDICAL HANDBOOK, Part I, pp. 1–14.

[4] Two series of subinterval pregnancy rates cited by Tietze, (The Use-Effectiveness of Contraceptive Methods, pp. 360–61) display declines from 15 to 3 pregnancies per 100 years and 29 to 7. A decline from 22 to 5 is observed in a third series: Tietze, C., and Alleyne, C.: *op. cit.*, p. 266.

[5] The D_x, N_x, P_x, q_x notation are taken from C. L. Chiang and his treatment of the current life table *in* Stochastic Study of the Life Table and Its Applications: II. Sample Variance of the Observed Expectation of Life and Other Biometric Functions, *Human Biology*, September, 1960, 32: 227–30.

[6] To avoid this convention, and to strengthen the life table analogy, one might introduce additional assumptions about the manner in which accidental conceptions and other withdrawals are distributed over the typical exposure month, thereby providing for fractional lengths of exposure. However, it is not certain that any additional precision is achieved by this provision. Furthermore, the life table analogy cannot be extended to T_x (total months of protection remaining to survivors) or to e_x (expectation of protected life) since not all of the sample, and often only a minority, become accidentally pregnant during the period of observation.

[7] Dr. C. V. Kiser kindly made available the contraceptive records of the Indianapolis couples. A thorough report of these data is contained in Westoff, C. F., Herrera, L. F., and Whelpton, P. K.: The Use, Effectiveness, and Acceptability of Methods of Fertility Control, *in* Whelpton, P. K .and Kiser, C. V. (eds.), SOCIAL AND PSYCHOLOGICAL FACTORS AFFECTING FERTILITY, Vol. 4. New York, Milbank Memorial Fund, 1954, pp. 885–892.

[8] See: Westoff, C. F., *et al.*, FAMILY GROWTH IN METROPOLITAN AMERICA. Princeton, Princeton University Press, 1961. 433 pp.

[9] Many couples belonging to the two surveys may have become progressively more irregular in their practice of contraception before finally stopping altogether or becoming accidentally pregnant. Any such tendency for the monthly pregnancy risks, of individual couples to increase, rather than to remain fixed or fluctuate around a fixed value, would help to explain why the pregnancy rate descends so slowly during the first year of marriage. In contrast, most of the pairs still protected after a year of exposure have remained protected so long because of fairly regular contraception. For most of these couples, the desire to have a pregnancy will involve an abrupt transition from relatively regular practice to nonpractice of contraception.

[10] Two approximate, and as yet inadequately investigated formulas for estimating the standard error of the conventional pregnancy rate are treated *in* Potter, R. G., Jr., and Sagi, P. C.: Some Procedures for Estimating the Sampling Fluctuations of a Contraceptive Failure Rate, *in* Kiser, C. V. (ed.), RESEARCH IN FAMILY PLANNING. Princeton, Princeton University Press, 1962, pp. 389–405.

[11] This sensitivity of the conventional pregnancy rate to extraneous factors is discussed *in* Potter, R. G., Jr.: Length of the Observation Period as a Factor Affecting the Contraceptive Failure Rate, Milbank Memorial Fund *Quarterly,* April, 1960, XXXVIII: 140–152.

[12] Formula [1], usually ascribed to M. Greenwood, is widely used in studies of cancer survivorship. *See,* for example, S. J. Cutler and F. Ederer: Maximum Utilization of the Life Table Method in Analyzing Survival, *Journal of Chronic Disease* December, 1958, 699–712. *See also,* Chiang: *op. cit.,* pp. 229–30.

[13] Goodman, L.: The Variance of the Product *k* Random Variables, *Journal of the American Statistical Association,* March, 1962, 57: 55.

[14] Elveback recommends that calculations be stopped before N_x reaches a value below 25. Cf.: Elveback, L.: Estimation of Survivorship in Chronic Disease: The 'Actuarial' Method, *Journal of the American Statistical Association,* June, 1958, 53: 436.

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