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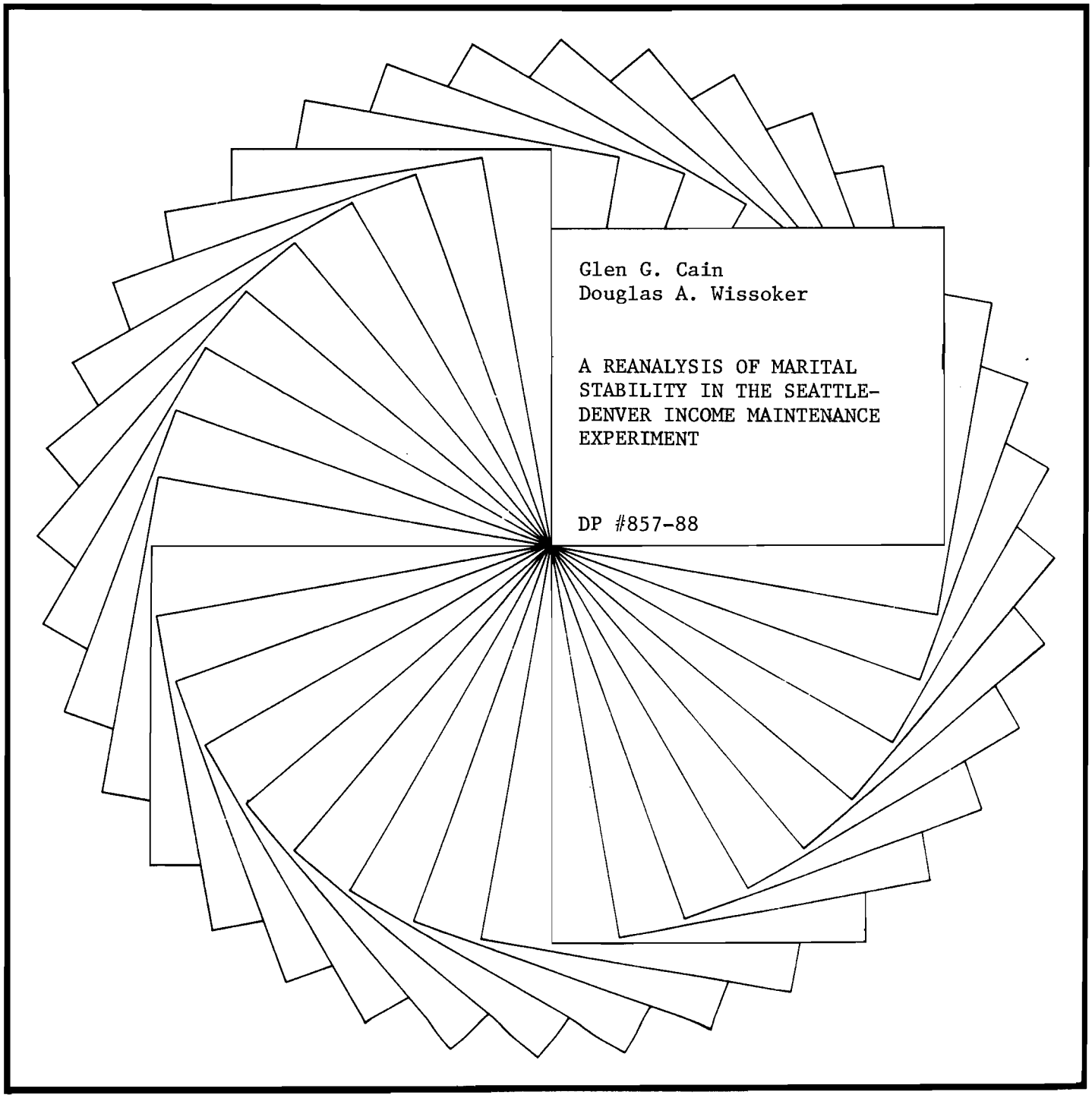

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A Reanalysis of Marital Stability in the Seattle-Denver Income Maintenance Experiment

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Abstract

This paper challenges the widely cited finding of Groeneveld, Hannan, and Tuma that the Seattle-Denver Income Maintenance Experiment provides evidence that guaranteed income plans for poor husband-wife families will increase marital dissolutions. The conclusion of this paper is that the plans (specifically, the negative income tax plans in the experiment) had no effect on the rate of marital dissolutions among the "treatment" couples relative to the control couples. The control couples were eligible for the existing program, Aid to Families with Dependent Children. Our reanalysis of the experimental data distinguishes between the experimental treatment in the form of the "pure" negative income tax and the treatment plans that involved an experimental training program. We use all the time periods of the experiment, allow for the timing of the marital dissolution in our inferences, and allow for attrition and reconciliations.

A Reanalysis of Marital Stability in the Seattle-Denver Income Maintenance Experiment

by Glen G. Cain and Douglas A. Wissoker

A LANDMARK STUDY IN POLICY RESEARCH

An article by Hannan, Tuma, and Groeneveld in 1977 in the *American Journal of Sociology* was the first published report that the Seattle-Denver Income Maintenance Experiment (SIME-DIME) had the effect of increasing marital instability among couples who were participants in the experiment.[1] The results startled and dismayed advocates of the welfare reforms being tested in the experiment, because they had expected that the reforms, which extended income transfer payments to poor husband-wife families, would stabilize marriages relative to the existing program, Aid to Families with Dependent Children (AFDC), which essentially provided benefits only to poor families without a father present. AFDC had itself been frequently blamed for contributing to the rising trends in marital breakups and female-headed families.

Although SIME-DIME, like three other social experiments with income maintenance plans sponsored by the (then) Department of Health, Education, and Welfare, was primarily designed to estimate labor supply responses, the find-

[1]Michael T. Hannan, Nancy B. Tuma, and Lyle P. Groeneveld, "Income and Marital Events: Evidence from an Income Maintenance Experiment," *American Journal of Sociology*, 82 (1977), 1186-1211. Subsequent articles in this journal by these authors reinforced their original findings. See "Income and Independence Effects on Marital Dissolution: Results from the Seattle and Denver Income-Maintenance Experiments," *84* (1978), 611-633; and "Dynamic Analysis of Event Histories," *84* (1979), 820-854.

ings on marital breakups have had the biggest impact.[2] Groeneveld, Hannan, and Tuma subsequently wrote over twenty research papers and articles on this subject, and their research was presented as testimony before Congress during debates on legislative proposals to reform the welfare system. In their final report, published in 1983, they claimed that "the negative income tax (NIT) plans tested in SIME/DIME dramatically increased the rate at which marriages dissolved among white and black couples," and reported that the rate of marital dissolution increased by "40 to 60 percent."[3] These findings were and continue to be an important source of opposition to such reforms in the welfare system as the negative income tax, which provides a guaranteed income and cash transfer payments to low-income married-couple families. Gilbert Steiner, who reviewed the testimony in congressional hearings on welfare reform, wrote that "the Seattle-Denver evidence has persuaded key politicians that a guaranteed-income plan at levels the leaders of the country think it can afford is incompatible with maximizing family stability in the affected population."[4] In this paper we present

[2]Citations to descriptions of all four experiments and a review of the labor supply responses in the experiments are provided by Robert A. Moffitt and Kenneth C. Kehrer, "The Effect of Tax and Transfer Programs on Labor Supply: The Evidence from the Income Maintenance Experiments," in *Research in Labor Economics*, Vol. 4, ed. R. G. Ehrenberg (Greenwich, Conn.: JAI Press, 1981), pp. 103-150.

[3]Lyle P. Groeneveld, Michael T. Hannan, and Nancy B. Tuma, "Marital Stability," in *Final Report of the Seattle-Denver Income Maintenance Experiment, Volume 1, Design and Results* (Menlo Park, Cal.: SRI International, May 1983), pp. 257 and 383. Hereafter, Volume 1 will be cited as *Final Report.*

[4]Gilbert Y. Steiner, *The Futility of Family Policy* (Washington, D.C.: The Brookings Institution, 1981), p. 110. The research findings of Groeneveld, Hannan, and Tuma were also cited by Martin Anderson, John Bishop, George

evidence to challenge the empirical findings of Groeneveld, Hannan, and Tuma.

The research of Groeneveld, Hannan, and Tuma was also influential theoretically, because it appeared to show serious inadequacies in the previous interpretation, particularly by economists, of the causal linkage between income maintenance programs and marital instability among low-income families. SIME-DIME appeared to show that "ungenerous" (or low-benefit) NIT plans caused increased marital breakups relative to AFDC even though (a) these NIT plans provided less income to a mother whose husband left her than did AFDC, and (b) NIT plans provide benefits to the husband-wife couple if they stay together, whereas AFDC does not. Also adding to the puzzle was their finding that the generous, high-payment NIT plans, which offered more income to a mother whose husband left her than did AFDC, had no destabilizing effect on marriages.

The theoretical framework used by Groeneveld, Hannan, and Tuma has two economic arguments. They hypothesize that welfare plans like AFDC or NIT (a) increase the economic independence of wives with children, which tends to destabilize marriages, and (b) provide income payments to intact husband-wife families, which tend to stabilize marriages. Groeneveld, Hannan, and Tuma depart from the conventional economic framework by hypothesizing that a stigma is attached to AFDC payments relative to NIT payments and that the "independence" and "income" effects of an NIT plan have complicated interactions for

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Gilder, Senator Russell Long, and Charles Murray as a reason for their opposition to welfare reforms that intended to expand assistance to husband-wife families. These citations are available from the authors.

plans of varying levels of generosity.[5] We refer to these theoretical ideas later.

Finally, the research by Groeneveld, Hannan, and Tuma was influential in the methodology of the social sciences because their articles were among the first in sociology or economics to employ the statistical techniques of event history analysis to explain the duration and timing of social behavior in the context of a model with many explanatory variables. Specifically, the rate of marital breakups was the outcome of interest, and the experimental treatments were the principal causal variables. Estimating the rate of marital breakups rather than their incidence permits comparisons among groups that are observed for different lengths of time (different "exposures to risk") and permits the estimation of marriage durations, taking into account that some individuals will not have completed their spells when the period of observation ends ("right censoring").

It will be clear from our reanalysis of the SIME-DIME data that we are indebted to the authors for their methodological precedents. However, we disagree with their empirical conclusions and theoretical interpretations.

We claim that the NIT had no effect on the rate of marital breakups among the participants. We reach this conclusion by an analysis of basically the same data but using somewhat different models that we believe are more appropriate for answering the following central policy question: *If an NIT program were enacted, how would the rate or incidence of marital dissolutions in the affected population be changed in comparison with an AFDC program?*

A more important question, no doubt, is whether the impact of the program

[5]See Groeneveld, Hannan, and Tuma, *Final Report*, pp. 261-264 and 325-339.

on marital stability improved or worsened the lives of the parents and children in poor families, but answering this is beyond our capabilities. Our impression is that an increase in marital breakups is generally viewed as detrimental to the lives of children in poor families, and we are mindful of this concern in our approach to the analysis.

Given the central policy question posed above and the concern with children's well-being, we have reanalyzed the SIME-DIME data by making seven changes in procedures or emphasis from the previous analyses of Groeneveld, Hannan, and Tuma. These changes are discussed in the next section and are the reasons why we get different empirical results, which, in turn, lead us to different theoretical and policy conclusions. The methodology used in the original analysis is maintained, but the results from using it are shown to be quite sensitive to differences in model specifications.

CHANGES IN THE REANALYSIS

Excluding couples without children. The experiment began in the fall of 1970 in Seattle. The sample used by Groeneveld, Hannan, and Tuma consisted of 2,770 couples who were living together at the beginning of the experiment. An undetermined number were consensual unions. We exclude couples without children, about 10 percent of the sample, because we are interested in comparing the NIT with AFDC, and only poor families with dependent children are eligible to receive AFDC. Three additional advantages in excluding childless couples are that a legislated NIT plan would almost surely be restricted to families with children, that society's basic concern with welfare assistance to families is with

the well-being of children, and that we avoid a possible problem in dealing with tenuous unions if the union is consensual and there are no children present.

Dealing with different experimental treatments. SIME-DIME was complicated in its design by several features, including the use of the following four distinct experimental groups. One group of families was offered an NIT, with varying levels of guaranteed incomes (paid to the family if it had no earnings or other income) and varying benefit-reduction rates, defined as the percentage by which the plan's transfer payments are reduced with each additional dollar of earnings (or other income) by the recipient. The variations in the plans are discussed below.

A second group of families was offered a subsidized training, education, and job counseling program, subsidized at three different levels. In our reanalysis we do not examine the variation in subsidies and will refer to this treatment simply as the training program, abbreviated as TR. A third treatment group, consisting of the largest number of families, was offered a program that combined the training and the NIT plans. The sample design allowed for these three treatment groups to be compared with each other and with a fourth group of control families that received none of the treatments.

Distinguishing between the effects on marital stability of the training program from the effects attributable to the income transfers—the "pure" NIT—is essential, because the programs are distinct and have different expected effects on marital stability. Consider first that the training program was intended to raise the earnings of the participating husbands and wives; second, that about

the same number of wives took part in the program as husbands.[6] The theoretical framework used to analyze the NIT's expected effects on marriage suggest that the training program should have both stabilizing and destabilizing influences. By raising the income of the family, a training program could enhance the stability of the marriage. Alternatively, the marriage might be destabilized if the training program improved the earnings capacity of the wife and made her less economically dependent on her husband.

The NIT program also has both "income" and "independence" effects, but with an important difference. All NIT plans offer payments or income guarantees to husband-wife families and, therefore, may increase the stability of the marriage for this reason. However, only the relatively generous NIT plans that provide higher payments to the separated wife than AFDC offers carry "independence" effects, because she can become more economically independent than she could with AFDC.

The economic hypothesis about the expected effect of an NIT on marital breakups relative to the existing AFDC system may be concisely stated as follows. *NIT plans that are less generous than or equally generous as AFDC ought to promote marital stability relative to the current state in which AFDC exists and the NIT does not exist. NIT plans that are more generous than AFDC have both*

[6]Dickinson and West report that "despite the fact that nearly two-thirds of the wives were out of the labor force prior to enrollment [in the experiment], participation rates of wives were similar to those of husbands. For husbands and wives, the proportion attending counseling ranged from 40 percent to 60 percent, and the proportion receiving [training and education] subsidies ranged from 21 percent to 36 percent." Katherine P. Dickinson and Richard W. West, "Impacts of Counseling and Education Subsidy Programs," *Final Report*, pp. 211-212.

stabilizing and destabilizing influences relative to AFDC, and such plans may encourage marital dissolution.

The treatment that combined an NIT and a training program, which we abbreviate as TR/NIT, does not allow the economic hypothesis stated above to be tested, because no TR/NIT plan unambiguously supports marital stability; all provide the option for a training program for the wife and the consequent enhanced economic independence of the wife. In fact, the proportion of husbands and wives who actually took counseling, education, or training courses was somewhat higher in the experimental group eligible to receive NIT payments than in the experimental group eligible to receive only the training program.[7] It turns out that the distinction between the two treatments, NIT and TR/NIT, is one important source of the difference in our results compared to those of Groeneveld, Hannan, and Tuma. Their reported "NIT effect" on marital stability is actually an effect of the combination of the two treatments, because they used only one NIT variable in their models, although they controlled for a separate (or additive) effect of training.[8]

The NIT plans tested in SIME-DIME are shown in Table 1. The guarantee amounts are listed in column 2 and apply to a husband-wife family with two

[7]Jacob Benus, Harlan I. Halsey, and Robert G. Spiegelman, "The Seattle and Denver Income Maintenance Experiments' Counseling Program and Its Utilization," Research Memorandum 67, SRI International, Menlo Park, Cal., July 1979, p. 46.

[8]In Table 5.B.2 in Appendix B of the *Final Report* Groeneveld, Hannan, and Tuma report estimates of the effects of interactions between training and NIT treatments, but these results are not part of their main text (see footnote 5 on p. 291) or of their conclusions.

Table 1

Experimental NIT Plans in SIME-DIME for a Husband-Wife Family of Four and a Separated Wife with Two Children

NIT Plan[a] (1)	Husband-Wife Family			Separated Wife: Income (Payments)[b]				Sample Size[c]	
	Guarantee (2)	Tax (3)	Breakeven (4)	With $0 Earnings (5)	$2,000 Earnings (6)		$4,000 Earnings (7)	With Training Program (TR/NIT) (8)	No Training Program (NIT) (9)
1	$3,800	.8[d]	$5,802	$3,200	$3,700	($1,700)	$4,400 ($400)	53	39
2	3,800	.7	5,429	3,200	3,800	(1,800)	4,400 (400)	59	40
3	3,800	.7[d]	7,367	3,200	3,900	(1,900)	4,800 (800)	81	39
4	3,800	.5	7,600	3,200	4,200	(2,200)	5,200 (1,200)	102	74
5	4,800	.8[d]	8,000	4,200	4,700	(2,700)	5,400 (1,400)	85	49
6	4,800	.7	6,867	4,200	4,800	(2,800)	5,400 (1,400)	109	54
7	4,800	.7[d]	12,000	4,200	4,900	(2,900)	5,800 (1,800)	102	47
8	4,800	.5	9,600	4,200	5,200	(3,200)	6,200 (2,200)	104	58
9	5,600	.8[d]	10,360	5,000	5,500	(3,500)	6,200 (2,200)	119	59
10	5,600	.7	8,000	5,000	5,600	(3,600)	6,200 (2,200)	97	34
11	5,600	.5	11,200	5,000	6,000	(4,000)	7,000 (3,000)	52	25
TOTAL								963	518

[a]The plans are listed in order of increasing generosity, using the payments to the separated wife as the criterion.

[b]"Income" refers to the wife's annual income from the combined sources of the NIT payments and her earnings (if any). "Payments" refers to the NIT payments she would receive, depending on her earnings. The payment amounts are shown in parentheses.

[c]Sample sizes refer to husband-wife families at the beginning of the experiment. The number of control husband-wife families is 606. In addition there were 593 treatment families who were assigned to a training program without NIT payments.

[d]A declining tax rate, which increases the generosity of the plan by increasing payments if the recipient has earnings and by increasing the breakeven level of income for the recipient. (Compare plans 2 and 3 and plans 6 and 7.)

children. The three levels, $3,800, $4,800, and $5,600 are in 1971 dollars. In 1987 dollars these would amount to approximately $10,600, $13,400, and $15,700. The median income of families in the United States in 1971 was $10,300,[9] which amounts to $30,000 in 1987 dollars.

The benefit-reduction rates for the plans (column 3) are .5, .7, and .8, but five of the .7 and .8 rates declined as the recipient's earnings increased. Column 4 gives the breakeven level of income for each plan for the husband-wife family, defined as the amount of family earnings at which the NIT payments are reduced to zero. In the plans in which the benefit-reduction rate is a constant, the breakeven level of family income is calculated simply as the guarantee divided by the benefit-reduction rate. In 1987 dollars the lowest breakeven ($5,429) equals $15,300, and the highest breakeven ($12,000) equals $33,700. It is apparent that many of the experimental NIT plans were more generous than existing or proposed welfare plans.

Columns 5-7 show the income available to a wife (and her two children) who separates from her husband. Column 5 is the amount she would receive if she had no other income. The plans are listed in order of their generosity to the separated wife, assuming her earnings were $4,000 or less (in 1971 dollars). Column 6 shows what her income would be from each plan if she earned $2,000 (the amount in parentheses is the NIT payment she receives). Column 7 shows the two amounts, total income and the NIT payment, if her earnings were $4,000. The least generous plans, 1 and 2, providing the $3,800 guarantee and tax rates

[9]U. S. Bureau of the Census, *Statistical Abstract of the United States: 1984* (Washington, D.C.: U. S. Government Printing Office, 1983), p. 463.

of .8 and .7, were roughly equivalent to the AFDC programs in existence in the two states, Washington and Colorado, during the experiment.

Columns 8 and 9 show the numbers of husband-wife families assigned to each of the NIT plans for each of the two treatments, with and without the training program. An unfortunate consequence of using four experimental groups is smaller sample sizes for each group, a problem that is exacerbated when the outcome of interest is a relatively rare event, such as a divorce or separation. Clearly, the problem is even more acute in testing for differences among the eleven NIT plans shown in Table 1.

Dealing with the different durations of the experiment. Another complication in the design of SIME-DIME was the assignment of the participants to different durations of experimental treatments. The sample was divided into at first two and, later, three duration groups: 3 years, 5 years, and 20 years. The 20-year group was selected after the experiment was under way, from families already in the experiment. Only 6 percent of the sample was transferred into the 20-year plan, and their records were maintained through seven years of the experiment. Among the original couples enrolled in the treatment groups, 69 percent were assigned to the 3-year group and 31 percent to the 5-year group. The participants knew of these time limits.

Keeping in mind the central policy question to which the experiment is addressed, we seek to infer what the responses would be if the programs being tested were "permanent," or at least as permanent as enactment into law would imply to those affected. Did the experiment last long enough to permit valid

inferences about a legislated plan? This is a question that received considerable attention from Groeneveld, Hannan, and Tuma, who acknowledged most of the points we raise below.

One issue is whether the outcome itself involves short-term or long-term choices and arrangements. Many decisions about labor supply are short-term, although some, like changing one's occupation are not. Many demographic decisions, like having children, moving to another city, and changes in marital status, are long-term.

There are two major potential biases attributable to experiments of a short duration. One bias that understates the effect of an NIT on marital stability is that the total payments from a short-duration program are less than those from a permanent program. Whether the effect of these payments is to stabilize or destabilize the marriage, either effect might be understated. The wife, for example, might regard the short duration of the payments to the intact family as an insufficient source of support to preserve the marriage. On the other hand the wife might regard the short duration of the payments as an insufficient source of support to permit her and her children to live separately from her husband. (We assume here that the NIT payments are larger than the AFDC payments available to her.)

A second bias of the short duration of the experiment serves to overstate the effect, whether the effect is to stabilize or destabilize the marriage. For example, if the wife views the extra payments from the NIT as a subsidy to her divorce, she may make this choice sooner, rather than later, because the subsidy will

last only for the duration of the experiment. Thus, the timing of the divorce or separation is biased to occur sooner in a temporary experiment than it would under a permanent program.

If marital breakups occur earlier, two reasons for an upward bias in the NIT's estimated effect on increasing marital breakups are likely. The incidence of marital breakups will be higher for the treatment group if the incentive for an early separation increases the number of separations within the duration of the experiment, compared with the number in that same time period if the experimental plans (and the income payments) had extended past the termination date.[10] A second reason for an upward bias is that even if the incidence of marital breakups is the same for both treatment and control groups during the experiment, the *earlier* incidence among the treatment couples that a brief experiment induces will produce a higher *rate* of breakups. This is because the rate is defined in terms of the number (or incidence) of marital breakups divided by the number of time periods (years, months, etc.) for which the couples are at risk of a breakup.[11]

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[10]For example, assume that a couple participating in a three-year experimental NIT plan are in their third year and are intending to divorce. Assume further that if the couple were not in the experiment the divorce would occur in the fourth year. However, because the experimental plan subsidizes a divorce during the third year but not during the fourth and subsequent years, the couple decide to divorce in the third year. The period of observation for the research investigator is only three years, so the research shows a higher proportion of divorces in the experimental group relative to the controls that merely reflects the earlier occurrence of the divorce in the experimental group.

[11]As we discuss below, this bias can be avoided by allowing the rate to vary with time. This method of calculation allows the high rate of breakups in the early periods to be offset by low rates of breakup later.

A third source of upward bias in the NIT's effect on marital instability arises not from the duration of the experiment, but simply because the program is new, and a pent-up stock of potential marital breakups will be prompted to take place. This point was made in somewhat exuberant language by Jodie Allen, then a Labor Department official, in her testimony before the U.S. Senate:

> You expect in an experiment—or, indeed, in the real world—that when a program first comes in it will tend to have a higher initial impact than it might have in the long term because there may be a pent-up stock of discontented husbands and wives who say: "Whoopee, now we can get away from each other." After 2 or 3 years this potential for splitting will diminish, and that, indeed, has occurred in the experiment.[12]

These incipient breakups might well occur early, because the period when separations are subsidized is relatively brief, but it is the newness of the program that is the distinct source of the bias that Allen refers to.

The above three sources of bias all assume that the NIT payments to the separated wife exceed what she would receive from AFDC. As noted above in connection with Table 1, most of the NIT plans in SIME-DIME had higher payments than AFDC. Even if the NIT payments were the same or lower than those offered by AFDC, the immediacy of the receipt of payments and the ease of obtaining them by the already participating wives could prompt an earlier

[12] *Welfare Research and Experimentation, Hearings*, Subcommittee on Public Assistance, Committee on Finance, U.S. Senate, 95th Congress, November 15-17, 1978, testimony of Jodie Allen, p. 20.

separation than if the wife had only AFDC as an alternative source of income. Obtaining AFDC would require an application and a waiting period. Although we do not think these costs of obtaining AFDC benefits are so high that they would deter the eventual incidence of a separation, they would probably delay the incidence, if only for a few weeks or months.

We make three important changes in procedures and emphasis compared to Groeneveld, Hannan, and Tuma to deal with the different durations of the experiment and with the biases that are likely to occur because of the shortness of the experiment's duration. The first is to use all periods of experimental time: up to three, five, and seven years for those in the 3-year, 5-year, and 20-year plans, respectively. In their *Final Report*, Groeneveld, Hannan, and Tuma devoted almost all their attention to results for the first three years of the experiment, and they excluded the participants in the 20-year plan from their reported estimations.[13]

Our second change is to allow the 3-, 5-, and 20-year groups to contribute to the final results in proportion to the number of observations in each group. Groeneveld, Hannan, and Tuma emphasized the results for the 5-year NIT plan on grounds that it was more similar to a permanent plan. In support of this claim they cite their finding that the 5-year treatments had a larger destabilizing effect

[13]See *Final Report*, pp. 293-294 and Table 5.5 for the brief discussion by Groeneveld, Hannan, and Tuma of the experimental results for a five-year period. Note, however, that they use a different dependent variable here than in their analysis of the first three years of the experiment. In footnote 1 on p. 287 they mention excluding the 20-year participants.

on marriages.[14] We do not find any difference, statistical or practical, between the effects of the 3-year or 5-year treatments, and we will use the full sample in our calculations.

Finally, we allow the rate of marital breakups to vary over time and to vary differently for each of the four main experimental groups. As noted above, there are strong incentives for couples who intend to break up and who are eligible to receive NIT payments to separate sooner rather than later. Estimating the long-run or "permanent" response requires that time be taken into account, unless, contrary to what we find, the rate of marital breakups is constant over time. Groeneveld, Hannan, and Tuma report their results and conclusions on the basis of a model that imposes a constant rate. They do report trials with models that relax this imposition, but they state that they "found no significant variation over time in the effects of NIT treatments" during the experiment.[15] As shown below, we do find important time effects.

Adjusting for attrition bias. Another change in our analysis is to adjust for attrition biases that affect the treatment and control groups differently. The proportions of couples who dropped out of the sample were 20 percent in the control group and 12 percent of couples participating in the treatment groups. This difference was expected because families receiving benefits, especially NIT payments, have an obvious incentive to stay with the experiment. Moreover, the incentive is greater for more generous plans, and a lower attrition proportion was

[14]Groeneveld, Hannan, and Tuma, *Final Report*, pp. 358-359.

[15]Ibid., p. 360. See also their discussion of this issue in footnote 6 on pp. 292-293.

observed in the more generous plans.[16]

We expect that the control couples who drop out have a higher incidence of divorce or separation than the control couples who did not drop out, because attrition has been found to be associated with stressful situations, such as going on welfare, mental or physical health problems, moving from the community, and marital dissolution.[17] Wives in the control group who separate may receive AFDC benefits, but participating in AFDC does not give them any incentive to stay in the experiment and be interviewed every three months. (Our data pertain to the event histories of the wives and not the husbands after a separation occurs.)

Wives participating in the NIT plans have the opposite incentive, an incentive not to drop out, in response to an actual or impending marital breakup. The reason is that the NIT plan will provide the separated wives with immediate income support. If only the husband had been working, the usual situation among poor families, the wife would receive a substantial increase in NIT payments if her husband leaves. Thus, the NIT couples who break up will tend to stay with the experiment, and those who drop out are likely to have fewer breakups. However, the presumed stability of the NIT couples who drop out and the presumed instability of the control couples who drop out will not be observed.

Our adjustment for attrition bias is similar to that used by Groeneveld, Hannan, and Tuma in that we both assume different rates of marital breakup for

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[16]See Robert G. Spiegelman, "History and Design," in *Final Report*, pp. 30-32, for supporting evidence on these points.

[17]See David N. Kershaw and Jerilyn Fair, *The New Jersey Income-Maintenance Experiment, Voume 1, Operations, Surveys, and Administration* (New York: Academic Press, 1976), pp. 119-227.

the couples who drop out of the experiment and then recalculate a full-sample estimate of breakups for the NIT and control groups.[18] We assume that the couples in the control group who dropped out are 25 percent more likely to become divorced or separated than those who remain in the experiment and continue to be interviewed. Groeneveld, Hannan, and Tuma tested the sensitivity of their results to attrition by assuming that the rates of marital breakups among dropouts were from two to ten times as large as the rate for those who remained in the sample. They assigned these high rates to the dropouts in both treatment and control groups. In contrast, our assumption is that the breakup rates for the NIT couples who drop out are *lower*—just half as large as for those who remain in the sample. Finally, we assume that the rates of marital breakups for the "pure" training group are the same for dropouts as for those who stay. Those eligible to receive only training have less incentive to stay in the experiment than those receiving cash benefits.

Allowing for reconciliations. There are four reasons for examining whether reconciliations and, for some purposes, remarriages differ between treatment and control groups.

- Most important, the children of separated parents who reconcile are likely to fare better economically and psychologically, particularly if the period

[18]See Groeneveld, Hannan, and Tuma, *Final Report*, pp. 305-310.

of separation is not long, compared to children whose parents remain separated.

- The costs to the taxpayers of an AFDC or NIT program will be higher if the separated mother remains unmarried, because the programs pay out more to low-income families. The incomes of families with a mother as the single parent are, on average, far below the incomes of two-parent families.

These considerations suggest that we examine how an NIT program, compared with AFDC, affects the proportion of time that children will be in two-parent families and the proportion of time that the family receives transfer payments. Just knowing that a marital separation has occurred does not tell us the proportion of time separated.

Allowing for reconciliations also offers some protection from two biases that are related to the way marital separations were reported in SIME-DIME; specifically, to differences in reporting between NIT and control families. A separation was recorded for both groups on the basis of an interview administered every four months to families in the experiment. In principle, a marital separation that lasted less than four months could go unreported. However, the NIT couples also reported their marital status every month as part of the information system for determining the amount of NIT payments they were to receive. Changes in marital status that were reported in the monthly reports of the NIT families were brought to the attention of the interviewers, who were instructed to verify the changes.[19] Thus, the NIT couples had more opportunities to report marital

[19]Gary Christophersen, "Implementation," *Final Report*, p. 80.

breakups, and they had a strong incentive to report even short-term separations because their NIT payments would generally increase if the wife and children were separated from the husband. The wife or husband was required to sign a statement testifying that the separation was permanent, but in practice the separation could be as short as one month.[20] These considerations suggest two more reasons for examining reconciliations.

- Some brief periods of separation among the NIT couples will be counted as an incidence of a breakup, whereas the same type of brief period of separation among the control group will not be counted. We do not know if the differences in reporting produced such a bias, but taking reconciliations into account reduces the potentiality of the bias.

- A few NIT families made fraudulent claims about their family composition to obtain more payments.[21] Groeneveld, Hannan, and Tuma discuss this issue and conclude that fraud was not an important source of bias in reports of marital breakups.[22] We make no adjustment for biases from reporting or

[20]Arlene Waksberg, "Overview of Master File System with Particular Attention to the Operational Flow of Family Composition Data," p. 24. This paper was originally printed by SRI International in January 1979 and is reprinted in the documentation for the data tapes for SIME-DIME available from the National Archives. Waksberg noted that obtaining "Affidavits of Separation" was "done in a nonrigorous fashion" (p. 24), and she suggested that the reporting differences between NIT and control groups led to a slight bias toward more reporting of marital breakups by NIT couples.

[21]Gary Christophersen, *Final Report of the Seattle-Denver Income Maintenance Experiment*, Volume 2, Administration (Princeton, N.J.: Mathematica Policy Research, May 1983), p. 148.

[22]Groeneveld, Hannan, and Tuma, *Final Report*, p. 313.

fraud, but, again, by allowing for reconciliations we can at least partially correct for separations that are relatively brief.

In summary. Our reanalysis differs from that of Groeneveld, Hannan, and Tuma in the following ways: we confine our analysis to couples with children, distinguish between the "pure" NIT plans and the plans that combine a training program, use all the time periods of experimental duration, give weight to the 3-year and 20-year treatments in accordance with the numbers of couples in these groups, and allow for the timing of the marital breakups, for attrition, and for reconciliations. Without allowing for timing, attrition, or reconciliation, we find that the rate of marital breakups was 13 percent higher among couples who were assigned to the "pure" NIT experimental plan, as compared to control couples. This percentage difference is not statistically significant, and its practical significance, discussed below, depends on whether the difference is transitory or permanent. An adjustment for attrition reduces the difference to 5 percent or less. An adjustment for timing shows that the long-run or "permanent" difference in marital stability between the treatment and control groups is 5 percent or less without any adjustment for attrition. Applying the attrition adjustment reduces the estimate of the long-run difference to zero. Finally, an allowance for reconciliation adds to the measure of family stability of the NIT group relative to the control group.

THE REANALYSIS

Replication. We obtained the SIME-DIME data that were used by Groeneveld, Hannan, and Tuma from the National Archives and from SRI Interna-

tional, which administered the design, operation, and evaluation of the experiment.[23] After essentially replicating their results, shown in Table 2, we make the changes discussed above and show new results in Tables 3-6.

The statistical method used by Groeneveld, Hannan, and Tuma for Table 2 is also used in our work. In Table 2 the effect of the experimental treatments on the rate of marital breakups (or on the duration of marriages) is estimated for couples who were already married when the experiment began. Maximum likelihood methods are used to estimate the rate that is most likely to obtain the marital outcomes in the observed sample, given the log-linear function that is specified below.

The marriage spell is defined to begin at the start of the experiment, but the length of time married before the start of the experiment is included as a control variable. In the simplest specification of the model the estimated duration is obtained by assuming that the rate at which couples divorce or separate is independent of time. Given this assumption, the estimated expected duration is the reciprocal of the rate of marital breakups. With time measured in discrete units, the rate is defined as the number of breakups divided by the number of couples at risk per unit of time. Given the rate of marital breakups, we can calculate the estimated proportion of marriages that survive for specified periods of time. A constant rate of breakups produces a proportion of survivors that declines uniformly with time. If the rate of marital breakups varies with time, then the functional form of the dependence on time must be specified to

[23]For help in getting the data we are grateful to Katherine Dickinson, Mario Lopez-Gomez, Philip Robins, Daniel Weinberg, and Richard West.

Table 2

Estimated Effects of Independent Variables on Rates of Marital Dissolution; Original Analysis of Groeneveld, Hannan, and Tuma (GHT) and Replications by Cain and Wissoker (CW) (for First 3 Years of Experiment)[a]

Independent Variable[b]	Whites				Blacks				Hispanics			
	GHT		CW		GHT		CW		GHT		CW	
	Coefficient	(Standard Error)	Coefficient	(Standard Error)	Coefficient	(Standard Error)	Coefficient	(Standard Error)	Coefficient	(Standard Error)	Coefficient	(Standard Error)
NIT	.43	(.22)	.27	(.22)	.45	(.21)	.44	(.19)	.01	(.34)	.00	(.31)
NIT, 3 yr.	-.33	(.24)	-.14	(.24)	-.30	(.23)	-.23	(.22)	.00	(.37)	-.02	(.37)
TR-1	.32	(.21)	.24	(.21)	.45	(.20)	.37	(.21)	.52	(.27)	.51	(.30)
TR-2	.14	(.20)	.06	(.20)	.30	(.20)	.27	(.20)	.13	(.29)	.05	(.30)
TR-3	.33	(.20)	.24	(.20)	.26	(.21)	.21	(.21)	.18	(.32)	.14	(.34)
TR, 5 yr.	-.29	(.27)	-.04	(.26)	-.38	(.26)	-.29	(.24)	-.04	(.40)	.00	(.37)
Children, n	.05	(.06)	.03	(.06)	.08	(.05)	.06	(.05)	.13	(.09)	.07	(.08)
Young child	-.29	(.16)	-.26	(.16)	-.29	(.16)	-.23	(.16)	-.43	(.28)	-.29	(.28)
AFDC, pre.	.50	(.29)	.46	(.18)	.05	(.19)	-.02	(.18)	.67	(.24)	.63	(.25)
Denver	-.20	(.14)	-.19	(.14)	.28	(.14)	.23	(.14)	...	...	...	...
Dur. marr.	-.10	(.03)	-.09	(.02)	-.05	(.01)	-.06	(.02)	-.03	(.03)	-.03	(.03)
Age-wife	.01	(.02)	.00	(.02)	-.01	(.02)	-.02	(.01)	-.06	(.03)	-.04	(.04)
Ed-wife	-.08	(.04)	-.06	(.04)	.01	(.05)	-.00	(.04)	-.03	(.05)	-.04	(.06)
Age-husb.	-.02	(.02)	-.01	(.02)	-.03	(.01)	-.02	(.01)	.00	(.02)	.00	(.03)
Ed-husb.	.02	(.03)	.00	(.03)	-.08	(.03)	-.11	(.03)	.03	(.04)	.00	(.05)
Normal earnings ($000's)												
0-1	1.01	(.44)	.92	(.43)	.40	(.36)	.29	(.33)	-.02	(.80)	-.08	(.84)
1-3	.89	(.31)	.74	(.29)	-.11	(.35)	.06	(.31)	-.19	(.44)	-.10	(.44)
3-5	.66	(.27)	.51	(.26)	-.10	(.22)	-.11	(.21)	.14	(.37)	.13	(.37)
5-7	.52	(.26)	.44	(.24)	-.28	(.20)	-.33	(.20)	.06	(.35)	.01	(.36)
7-9	.23	(.27)	.13	(.25)	-.34	(.20)	-.36	(.20)	-.01	(.37)	-.02	(.37)
Unclassified	1.37	(1.03)	1.27	(1.27)	-.59	(1.02)	-.64	(.97)	7.52	(70.22)	...	...
Constant	.73	(1.59)	-7.29	(.69)	.07	(.73)	-5.42	(.72)	-1.01	(1.06)	-6.98	(1.19)
Sample size[c]	1299		1260		939		901		538		492	

-Notes, Continued-

Table 2, Continued

[a]Source for estimates of Groeneveld, Hannan, and Tuma: Table 5.A.1 in Final Report, p. 367. The couples who are the units of analysis were already married at the beginning of the experiment. The length of time for the marriage spell is measured in days, beginning with the experiment. A continuous-time, log-linear rate model is used that assumes the rate of dissolution is constant over time.

[b]All variables refer to the beginning of the experiment. The following are dummy variables. The omitted dummy variable should be clear.

NIT = eligible to receive NIT payments.

NIT, 3 yr. = assigned to a three-year NIT plan.

TR-1, TR-2, TR-3 = eligible to receive the training, education, and job counseling treatments of varying levels of generosity. TR-1 is the least generous, and TR-3 is the most generous.

TR, 5 yr. = eligible for a training program for five years.

Young child = having at least one child at home who is less than six years old.

AFDC, pre. = received AFDC in the year prior to the experiment

Denver = living in Denver

Normal earnings (in thousands of dollars) = predicted category of normal incomes of the family, based on pre-experimental information, including the family's reported annual income in the year prior to the experiment.

The following variables are not dummy variables:

Dur. marr. = years married at the beginning of the experiment.

Age-wife, Age-husb. = age in years.

Ed.-wife, Ed.-husb. = years of schooling completed.

Children, n = number of children under age 19 who are living at home.

[c]The sample sizes of Cain and Wissoker that include spouses who died are 1295, 937, and 509 for whites, blacks, and Hispanics, and results for these samples show slightly smaller NIT effects. The samples that exclude spouses who died are shown in this table because they are closer to the results of Groeneveld, Hannan, and Tuma. For the discussion of their treatment of cases in which the spouse died, see Groeneveld, Hannan, and Tuma, Final Report, p. 305, footnote 1.

estimate the rate and to calculate the proportion of survivors at any point in time.

The smallest time unit for the marital records of SIME-DIME is a day, because the calendar date of the breakup is recorded. We follow Groeneveld, Hannan, and Tuma in using a model that assumes that the events are measured in continuous time, although we have also used discrete-time models. Specifically, we use a log-linear model of the instantaneous rate, r, of a marital dissolution of the following form:

$$\ln r_t = \mathbf{E}'\alpha + \mathbf{X}'\beta + \gamma t.$$

The three types of exogenous variables are: (1) $\mathbf{E}$, a vector of experimental treatment variables, usually specified as dummy variables; (2) $\mathbf{X}$, a vector of personal and family control variables, including the variables used to stratify the sample—site, income, and ethnicity—and a constant term; and (3) t, a scalar time variable. In some specifications of the model we allow interactions between $\mathbf{X}$ and $\mathbf{E}$ and between t and $\mathbf{E}$, and we sometimes use a vector of time variables, $\mathbf{T}$. The vectors, α and β, and the scalar γ are parameters to be estimated. The additive form of the model is shown, and subscripts denoting individual observations are deleted for brevity. We used the BMDP program for our maximum likelihood estimations of the model.

Table 2 shows the main results reported by Groeneveld, Hannan, and Tuma in the *Final Report* and our replications of this specification. Note that their specification assumes a constant rate of marital breakup over time; the time parameter, γ, is assumed to equal zero. The data include information on all cou-

ples, with and without children present, who were married when the experiment began. Data from the first three years of the experiment are used. The duration of marriage is measured as the time from the start of the experiment to the first observed marital breakup. Couples who drop out of the experiment are included up to the time (day) that they drop out, and the estimate of the instantaneous rate of marital breakups takes into account that they have not experienced a breakup for the period (in days) that they are in the sample. This procedure has the advantage of using the observed information on dropouts. The attrition biases that were discussed above, however, remain.

Table 2 shows the estimated effects of the NIT experimental plans by means of two coefficients: one for "NIT," the dummy variable for participating in any of the NIT plans, and one for being in a 3-year-duration plan ("NIT, 3 yr."). Thus, the coefficient for "NIT" represents the effect of being in a 5-year NIT plan. Recall that this NIT variable includes the "pure" NIT and the combined NIT-and-training treatments, and that the 20-year participants are excluded from the sample.

To illustrate the results from Table 2 that were emphasized by Groeneveld, Hannan, and Tuma, consider first the NIT coefficient of .45 for blacks. The interpretation of this coefficient is that the arithmetic value of the rate of marital breakups for the 5-year NIT group of black couples is larger by a factor of 1.57 [equal to exp(.45)], relative to the control group of black couples, given the presence of the other variables in the model, including four variables that control for participating in the training programs. The 5-year NIT plan for whites has a

multiplier of 1.53 [equal to exp(.43)]. A coefficient of zero on a dummy variable in these models implies no effect and yields a multiplier of unity. The results for Hispanics show such null effects of the NIT treatments.

These results were the basis for the conclusion by Groeneveld, Hannan, and Tuma that the NIT plans increased the rates of marital dissolution by 40 to 60 percent for white and black couples. The estimated impact of the 3-year NIT plan shows multipliers of 1.10, 1.16, and 1.01 for whites, blacks, and Hispanics, but these results were not emphasized by Groeneveld, Hannan, and Tuma. (To obtain the multipliers for the 3-year plans, add the coefficients for "NIT, 3-yr." and "NIT" and evaluate the exponential sum.)

Our replications show nearly the same coefficients for all the control variables and the same overall effect of the experimental treatment variables as those of Groeneveld, Hannan, and Tuma. A weighted average for our NIT multiplier (combining "NIT" and "NIT, 3 yr.") for all three ethnic groups is 1.20, which is identical to their corresponding multiplier. We have not yet determined, however, why our estimates of the treatment effects of the 3-year plans and for blacks are slightly larger than theirs, nor why our estimates of the treatment effects of the 5-year plans and for whites and Hispanics are slightly smaller than theirs.

The multipliers for "NIT" obtained from the coefficients in Table 2 can be interpreted as a ratio: the estimated rate of marital breakups for the 5-year NIT group divided by the estimated rate of marital breakups for the control group. The couples in the control group had, themselves, a high rate of marital breakups, so the large multipliers of "NIT" in Table 2 are not an artifact of a

very low denominator in the ratio. Among control couples who did not drop out, the proportions of whites, blacks, and Hispanics who divorced or separated in the first three years were 16, 24, and 20 percent. These percentages, which reflect the full three years of exposure to risk, are considerably higher than those reported by Sawhill et al. for poor couples in the Panel Study of Income Dynamics for a similar time period or for comparable controls in the New Jersey NIT experiment.[24] Over a three-year period, a marital breakup proportion of 16 percent for the white control couples implies a constant annual rate of breakups of 5.6 percent.[25] The multiplier of 1.53 from Table 2, applied to the 5.6 rate, implies that the annual rate of marital breakups for white couples in the 5-year NIT program is 8.6 percent.

Analysis with the full sample, couples with children, adjustments for attrition, and a distinction between NIT and TR/NIT. Table 3 is based on records of couples for their full tenure in the experiment. We exclude couples who were without a child when the experiment began, cases in which a spouse died during the experiment, and a small number of cases in which attrition

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[24]Isabel V. Sawhill, George Peabody, Carol A. Jones, and Steven B. Caldwell, "Income Transfers and Family Structure," Report no. 979-03, The Urban Institute, Washington, D.C., September 1975; Jon Helge Knudsen, Robert A. Scott, and Arnold R. Shore, "Household Composition," in *The New Jersey Income-Maintenance Experiment, Volume III, Expenditures, Health, and Social Behavior; and the Quality of the Evidence*, ed. Harold W. Watts and Albert Rees (New York: Academic Press, 1977), pp. 254-259.

[25]Let y_0 equal the number of couples at risk of a marital separation at the beginning of the experiment and y_3 equal the number who survive at the end of three years. If r is the annual rate of a marital breakup, then $r = .05646$ in the equation: $y_3 = y_0(1 - r)^3$, where $(y_0 - y_3)/y_0 = .16$.

Table 3

A Reanalysis of SIME-DIME: Estimated Effects on Marital Dissolution Rates for Original Marriages, with Children Present: "Continuous-Time" Models, with and without an Adjustment for Attrition[a]

Treatment Variable	Ratio of the Estimated Rate of Dissolutions among Treatment Couples to the Rate among Control Couples							
	Total		White		Black		Hispanic	
	Adjusted for Attrition?		Adjusted for Attrition?		Adjusted for Attrition?		Adjusted for Attrition?	
	No	Yes[b]	No	Yes[b]	No	Yes[b]	No	Yes[b]
NIT	1.13	1.04	1.17	1.10	1.29	1.18	.79	.73
TR/NIT	1.46*	1.36*	1.31	1.22	1.88**	1.74**	1.11	1.05
TR (training)	1.16	1.13	1.11	1.09	1.26	1.21	1.11	1.08
Sample size	2369		1100		784		485	

[a]The duration of the experiment for the analysis in this table is 3, 5, and 7 years for the 3-year, 5-year, and 20-year groups. The units of analysis are couples who were married or living together with one or more dependent children at the beginning of the experiment. The estimated treatment effects are derived from the log-linear rate model like those used in Table 2, with the following changes. (1) For this table the following dummy variables specify the treatments: NIT-3 yr., NIT-5 yr., NIT-20 yr., TR/NIT-3 yr., TR/NIT-5 yr., TR-3 yr., TR-5 yr. See Appendix Table A.1 for coefficients and standard errors of these treatment variables. (2) Other changes compared to Table 2 are that two dummy variables for the educational attainment of the wife are included instead of the two linear variables for the wife's and husband's education, and that the variable for the number of children is dropped.

[b]The adjustment for attrition assumes (1) the rate of marital breakups for dropouts in the control group is 25 percent larger than the rate of marital breakups of controls who did not drop out; (2) the rate among dropouts in the NIT and TR/NIT groups is 50 percent smaller than the rate of those who did not drop out; (3) the rate for dropouts in the training group, TR, is the same as the rate for those who did not drop out.

*Statistically significant at the 10 percent level (two-tailed test). The test is conducted by estimating the log-linear model with and without the two (or three) dummy variables specifying the treatment being tested, and then determining whether the change in the log-likelihood ratio is statistically significant. The individual coefficients of the treatment variables and their standard errors are reported in Appendix Table A.1.

**Statistically significant at the 5 percent level (two-tailed test).

occurred on the first day. The sample in Table 2 includes childless couples, cases in which the husband died, and all cases of attrition.

Two important objectives in our reanalysis in Table 3 are to estimate the effect of the "pure" NIT treatment separately from the treatment that combined training and NIT, and to use all experimental observations and periods. We point out that even when Groeneveld, Hannan, and Tuma combined the NIT and TR/NIT groups, their main conclusion, and the public's perception of their conclusion, of a "dramatically" large destabilizing effect of an NIT on marriages was based on only 25 percent of the observations in the NIT and TR/NIT groups. That is, the white and black couples in the 5-year NIT and TR/NIT plans constituted only 25 percent of the NIT and TR/NIT sample.

In Table 3 we summarize the first part of our reanalysis and show 12 estimates of the experimental treatment effects, consisting of a summary estimate for each of the three types of experimental treatment—NIT, TR/NIT, and TR—for the total sample and for each of the three ethnic groups. These 12 estimates are shown in the columns without an adjustment for attrition. Our method of summarizing the estimates and our adjustment for attrition are discussed below.

The estimation model for Table 3 includes the personal and family control variables used in Table 2, with minor changes that are mentioned in the notes to Table 3. As in Table 2, no time variables are included. The experimental variables are seven dummy treatment variables for TR, 3 yr.; TR, 5 yr.; NIT, 3 yr.; NIT, 5 yr.; NIT, 20 yr.; TR/NIT, 3 yr.; and TR/NIT, 5 yr. Controls are the omitted group. The "pure" NIT was the only treatment that included a 20-year

subsample that was extended into a sixth and seventh year of the experiment.

The coefficients of the personal and family variables are not shown, but they are very similar to the values in Table 2. The seven experimental coefficients for the total sample and for each of the three ethnic groups produce 28 coefficients, too many to summarize unless, contrary to fact, they fell into a systematic pattern. In particular, the coefficients for the duration groups, 3-year and 5-year, varied in their relative sizes. We show below, in connection with Table 4, that the assigned duration of the plan has no effect on the rates of marital breakups. Therefore, for each ethnicity and the total sample, we summarize just three treatment effects in terms of multipliers, which express the ratio of the estimated rate of marital breakups of the treatment groups to the estimated rate of marital breakups of the control group.

A summary measure of each treatment's effect on the breakup rate is derived from a weighted average of the two (or three) coefficients of the treatment-by-duration variables, using as weights the proportion of observations in each duration group. For example, the coefficients for the TR, 3 yr. and TR, 5 yr. treatments among black couples are .354 and -.060. The 3-year group has 70 percent and the 5-year group has 30 percent of the families that are in the TR treatment, so the weighted average of the coefficients is .23, which yields the multiplier of 1.26. In a similar way we calculate a treatment effect for the total sample as a weighted average of the effects for the three ethnic groups, using as weights the proportion of observations in each ethnic group. Our presentation offers an easily interpretable summary of the experimental results, which preserves

the a priori assumptions that the model should allow for distinct (interactive) treatment effects for the three ethnic groups and for the different durations of the plan. An alternative way of summarizing the impact of the treatments is to impose zero effects of the assigned plan duration and to estimate the model with these restrictions. Another specification, which measures summary effects of the treatments for the total sample, imposes zero effects of the assigned durations of plans and merges all ethnic groups, controlling for additive effects of ethnicity. Our results are similar when we estimate these alternative models,

For the sample as a whole the estimated rate of marital breakups among couples in the "pure" NIT group is 13 percent larger than that for the controls. This is the single summary statistic that most succinctly informs us of the "NIT effect" in the experiment with this particular model. It is not statistically significantly different from zero. Whether it is practically significant depends on its being a transitory or permanent difference in marital breakup rates. Consider that the ratio reflects the following approximate rates of marital breakup per year: .06 for controls and .0678 for NIT couples (.0678/.06 = 1.13). In the first year 60 control couples and 68 NIT couples out of 1,000 couples in each group would be predicted to separate or divorce. If these rates were constant over time, as is assumed in the model used, then after 10 years, 461 marital breakups from among 1,000 control couples are projected compared with 504 marital breakups among 1,000 NIT couples. The difference is neither large nor trivial, but the assumption of constancy over time will be shown below to be rejected.

The "pure" NIT's effect for whites is similar to the effect for the total sample.

The NIT's effect on marital instability among black couples is positive (destabilizing) and large and that for Hispanics is negative (stabilizing) and large, although neither is statistically significant at conventional levels. The "pure" training program has an effect on marital breakups that is positive and about the same size as that for the "pure" NIT program. The impact of TR/NIT, the combined training and NIT program, is significantly destabilizing, especially among black couples.

Our adjustment for attrition is shown in the adjoining columns of Table 3. To obtain the adjustment we use one-year discrete periods and calculate the rate of marital breakups for the three treatments and the three ethnic groups. The first incidence of a breakup ends the marriage spell, so any subsequent attrition creates no problem of bias, because the full duration of marriage from the beginning of the experiment is known. An attrition that occurs during the year is assumed to have occurred at the half-way point of six months. The observed rate of marital breakups is calculated in the usual way as the number of breakups divided by the number of periods of exposure to risk. A couple that drops out in the first year is assumed to "contribute" only half of one period of exposure to risk. A couple that remains in the experiment for seven years contributes seven periods. The rates of marital breakup for these observed periods are calculated for each ethnic group for each of the four experimental groups, including the control group.

Now consider the couples that drop out and have not reported a marital breakup. Each couple represents a number of unobserved periods of exposure to risk. We assign rates of marital breakups to the unobserved periods of the

dropouts in accordance with the assumptions discussed earlier. The breakup rate of the control dropouts is assumed to be 25 percent larger than that of the observed controls. For example, if the observed breakup rate for a particular ethnic group of control couples is 5 percent, we assign a rate of 6.5 percent to the dropout couples. For the NIT and TR/NIT groups the breakup rate for dropouts is assumed to be 50 percent less than that of couples who did not drop out. A 6 percent breakup rate among those who did not drop out, for example, leads to an assumed 3 percent rate among the intact couples who dropped out. Recall that NIT payments to a wife after she separates would generally increase substantially, so she would be unlikely to drop out of the experiment. Finally, intact couples in the "pure" training group who drop out are assumed to have the same rate of marital breakups as the trainees whose full duration of marriage is observed.

These adjustments for attrition reduce the unadjusted ratios for the "pure" NIT group by 8 percent for the total sample. The unadjusted ratio of 1.13 is reduced to 1.04. The reduction is less for whites (6 percent) than for blacks (8.5 percent), because the attrition proportion is smaller among whites. These adjustments, which are very close to those recommended by Groeneveld, Hannan, and Tuma, serve to lower the breakup effect of the NIT plan to an inconsequential 4 percent for the total sample.[26] Black couples continue to show a moderately large destabilizing effect of the NIT treatment, but this effect is actually smaller

[26]In discussing their analysis of attrition bias, Groeneveld, Hannan, and Tuma stated that "reasonable adjustments for attrition bias are on the order of 10 percent for blacks and 5 percent for whites" (*Final Report*, p. 310).

than the size of the stabilizing effect among Hispanics. The destabilizing effect of the TR/NIT program remains relatively large.

Allowing the rate of marital breakups to vary over time. Table 4 shows the results of a log-linear rate model in which "time" and "time interacted with treatments" are added to the model used in Table 3. We drop the assumption that the rates of marital breakups are constant over time and test the hypothesis that more frequent early breakups by treatment couples are offset by fewer breakups later.

The coefficients (or effects) of the time variables are shown in Panel A of Table 4. The underlying unit of time is one day, but the coefficients in the table are accompanied by the percentage declines for a one-year period. In a model without interactions with the treatment variables, a single linear time variable has a significantly negative coefficient (-3.61×10^{-4} in column 1), which indicates that the hazard of a marital breakup declines with time. This decline in the rate of marital breakups amounts to 12.3 percent per year, implying that an 8 percent rate in the first year would decline to a rate of 4 percent in the fifth year and to 2 percent in the tenth year.

The main point of Table 4, however, is not that there is a declining trend in marital breakups for the sample as a whole, but that there are differences in the trends among the four treatment groups. Column 5, for the "Model with Interactions," shows a 2.6 percent decline per year in the control group's rate of marital breakup. The decline for the "pure" NIT group is 14.3 percent, which is 5.5 times as rapid as that for the controls. The decline for the TR/NIT group

Table 4

A Reanalysis of SIME-DIME: Estimated Effects of Treatments on Marital Dissolutions, Allowing for Time Dependence and Time-Treatment Interactions in Panel A, and Simulated (Projected) Comparisons of Proportions of Dissolved Marriages for Treatment and Control Groups for Periods of 3, 5, and 7 Years in Panel B

Panel A: Coefficients and Effects on Marital Dissolution Rates of Time in a Log-Linear Rate Model. (Time is measured in units of one day.)[a]

	Coefficients, Standard Errors (in Parentheses) and Annual Percentage Change				
	Model with Time[b]		Model with Interactions[c]		
Variable	(1) Coefficient (Stand. Err.) x 10^{-4}	(2) Annual Percentage Change in Rate	(3) Coefficient (Stand. Err.) x 10^{-4}	(4) Time + Time x Treatment x 10^{-4}	(5) Annual Percentage Change in Rate
Time	-3.61** (1.04)	-12.3	-0.71 (1.98)	-0.71	-2.6
Time X NIT			-3.53 (2.84)	-4.24	-14.3
Time X TR/NIT			-5.63** (2.74)	-6.34	-20.7
Time X TR			-1.24 (2.84)	-1.95	-6.9

Panel B: Ratio of the Estimated Proportion of Marital Dissolutions among Treatment Groups to the Estimated Proportion of Marital Dissolutions among Control Couples, Using "Model with Interactions."[d]

	After 3 Years		After 5 Years		After 7 Years	
	Adjusted for Attrition?		Adjusted for Attrition?		Adjusted for Attrition	
	No	Yes[e]	No	Yes[e]	No	Yes[e]
NIT	1.17	1.04	1.05	.94	.97	.86
TR/NIT	1.36	1.24	1.16	1.05	1.02	.93
TR	1.13	1.08	1.09	1.04	1.05	1.00

- Notes, Continued -

Table 4, Continued

[a]The coefficients shown in Panel A are obtained from the log-linear rate model for the total sample (n = 2365), with time (linearly), time x treatment (as three dummy variables), seven dummy variables for the duration-of-plan treatments, two dummy variables for ethnicity, and the standard set of personal and family control variables. See notes b and c.

[b]"Model with Time" has time (in days) as the only variable measuring time dependence.

[c]"Model with Interactions" has time (in days) and the three time-treatment dummy variables. The collection of all four time variables is statistically significant at the 1 percent level. The collection of the three time-treatment interactions is statistically significant at the 13 percent level, given the presence of time and the treatments as additive variables. See text for further discussion of the statistical significance of the time-treatment interactions.

[d]Survivor proportions are estimated assuming an elapse of 3, 5, and 7 years, using the "Model with Interactions" and evaluating the hazard at the sample means of the personal and family control variables. The estimated proportion of marital dissolutions is one minus the proportion of survivors.

[e]The adjustment for attrition assigns a survivor proportion that is 25 percent lower among control dropouts than the proportions of survivors among control couples who did not drop out. A 50 percent larger proportion of survivors is assumed for the NIT and TR/NIT dropouts. The survivor proportion of trainee (TR) dropouts is assumed to be the same as among trainee couples who did not drop out.

**Statistically significant at the 5 percent level (two-tailed test).

is very large, 20.7 percent per year, which is 8 times as rapid as the controls' decline. The decline for the "pure" training group is 2.7 times as rapid as the controls' decline.

The three terms that specify the interaction between the experimental treatment and time are marginally significant, being statistically significant at a 13 percent level. Now consider our hypotheses that the time effects for the three treatment groups are (a) not merely different but *negative*, as compared to the controls; and (b) increasingly negative as the generosity of the treatment increases. Let a linear ranking of the generosity of the experimental groups be $E*$, defined by assigning 0 to controls, 1 to TR, 2 to NIT, and 3 to TR/NIT. We test our hypotheses about the sign of the time effects by adding the interaction term $t \times E*$ to the model that includes additive terms for t and for the three treatment dummy-variables. We find that the variable $t \times E*$ is negative and statistically significant at the 1.5 percent level (one-tail test).

Accepting the negative sign of the point estimate for the control group at face value, an explanation is that the negative trend results from heterogeneity among the control couples in their traits of stability and commitment to their current marriage. Either earlier marital breakups or earlier attrition on the part of the "less stable" couples could produce a declining trend in the control group's rate of marital breakups even though the true time effect for each couple is zero. As regards attrition, the longer the experiment continues, the more selective is the remaining control group of "stable" families. As stated above, however, the NIT couples who are experiencing a dissolution in their marriage have a financial

incentive not to drop out. If we assume that the declining trend for the control group reflects the heterogeneity factor, the *differentially* large (negative) trends for the experimental groups reflect the effects of the treatments in prompting earlier separations.

Another approach in dealing with heterogeneity in the population is to impose an assumption about the distribution of the unobserved component of "stability" in the estimation. ("Stability" is partially controlled by such observable variables as the age of the wife and the duration of the marriage when the experiment began.) Unfortunately, when attempting to model unobserved heterogeneity by one or another distributional assumption, the results obtained can vary widely.[27] Our attempts, which we can make available to interested readers, have not yielded stable results.

Panel B of Table 4 shows ratios of the estimated (or projected) proportions of marital breakups among treatment couples to the estimated proportions among controls after periods of three, five, and seven years elapse. These ratios imply no practical effect of the experimental treatments on marital stability after a period of five or seven years. The predicted proportions of breakups are derived from applying the log-linear rate model to the total sample of 2,365 couples. Specifically, the model's coefficients of time (shown in Panel A) and the coefficients of the personal and family variables (which are evaluated at the sample means of the personal and family variables) are used to predict the proportion of breakups

[27]This problem and citations to the literature are discussed in Paul D. Allison, *Event History Analysis* (Beverly Hills, Cal.: Sage Publications, 1984), pp. 32-33.

after selected periods of time elapse.

The ratios for the five- and seven-year periods, unadjusted for attrition, range from .97 to 1.16 and reflect the accumulated numbers of marital breakups after five (or seven) years. Referring again to the negative coefficients of the time-interaction terms in Panel A and comparing the seven-year projection with the five- (or three-) year projection in Panel B, we see a pronounced tendency for the early breakups among treatment groups to be offset by fewer breakups later on. Neither the five- nor seven-year periods displayed in Table 4 is any longer than the actual experiment, but only 30 percent of the couples in the experiment were in it for five years and only 6 percent were in it for seven years. Extrapolating the outcomes beyond seven years, which would soon produce ratios that are less than one for all treatment groups, does not seem warranted.

The same type of adjustment for attrition used in Table 3 is shown in Table 4. The control couples who drop out are assigned a 25 percent larger proportion of marital breakups (which is the same as assigning a 25 percent smaller proportion of survivors). In the NIT and TR/NIT groups, the couples who drop out are assigned a 50 percent larger proportion of marital breakups than those in these groups who did not drop out. For example, the projected percentage of marital breakups among controls after five years is 27.9 percent, implying that out of 447 couples who did not drop out, 125 experienced a divorce or separation. (Couples who drop out after they report a marital breakup are not counted as dropouts in these calculations.) The 99 control couples who dropped out (18 percent) are assigned a marital breakup proportion of 34.9 percent (1.25×27.9),

and the adjusted proportion for the full sample of 546 couples is 29.2 percent. The TR/NIT group had a projected proportion of marital breakups of 32.5 percent after five years elapsed. The attrition proportion was only 11 percent, and the weighted average of the estimated proportion of marital breakups for the combined sample of those who did and did not drop out is 30.8 percent. For the TR/NIT and control groups the original ratio of 1.16 (32.5/27.9) is reduced to 1.06 (30.8/29.2). All ratios for the "pure" NIT group after five (or seven) years elapse are reduced to below unity after the adjustment for attrition.

The log-linear rate model that allows for time dependency is useful for determining the effects of being assigned to a 3-year, 5-year, or 20-year plan. Estimating these duration effects is not feasible with the constant-rate model. Consider the effect on marital stability of being in the 5-year NIT plan relative to being in the 3-year NIT plan. Without controlling for time, this effect is a mixture of two influences: one is the longer duration of the NIT plan, per se, including the fact that the financial benefits of the longer plan are larger; the second influence is attributable to being in the experiment for the fourth and fifth years. As we have seen from estimating time dependence among control couples, merely being in the experiment longer has a negative effect on the rate of marital breakups. By controlling for time directly, we can isolate the effect of being assigned to the 5-year treatment plan relative to being assigned to the 3-year treatment plan.

When we test for the difference between the 3-year and 5-year plans for the treatment groups in a model with time included, we find that a dummy variable for the 5-year duration-of-treatment *reduces* marital breakups relative to the 3-

year duration-of-treatment by 5 percent. This small difference is statistically insignificant. In this model the linear effect of time is negative, statistically significant, and about the same size as shown in column 1 in Table 4. These results, like those discussed below concerning the 20-year groups, are not shown in a table for reasons of brevity. The table is available from the authors.

In testing for the difference between being in a 20-year NIT plan and being a 20-year control, we must deal with the fact that the 20-year couples are not a random selection of all couples, because they were assigned to their 20-year status after the experiment had been running for about 30 months. All the assigned couples, therefore, met the criterion of being "stable" in the sense of not having dropped out of the experiment throughout the first 30 months. Both groups of 20-year couples, NIT and control, may be considered similarly stable, however. We find that the 20-year NIT plan has a positive effect on marital breakups relative to the 20-year controls, which, although large, is not statistically significant at a 10 percent level. The insignificance is not surprising, given the small samples—87 couples in the 20-year control group and 93 in the 20-year NIT group.

To anticipate a question that is likely to arise, we should mention that couples in the 5-year (or 3-year) treatment groups should not be directly compared to the corresponding 5-year (or 3-year) control groups. The control couples assigned to the 5-year duration group were not randomly assigned to this group at the beginning of the experiment, as were those in the three treatment groups. Instead, the controls were assigned to the 5-year group after the experiment had been

running for a certain number of months.[28] By the time of the assignment to a 5-year control group, a number of control couples had dropped out, and all these dropouts were considered to be in the 3-year control group. The average annual rate of attrition is, indeed, substantially higher among the 3-year controls than among the 5-year controls: 8.8 percent compared to 2.0 percent. Remarkably, only 9.5 percent of the 5-year control couples dropped out in *five* years, while 24.2 percent of the 3-year couples dropped out in *three* years! In our analyses we have simply pooled the 3-year and 5-year control couples into a single control group. Together they constitute a random sample.

Assignments to the 3-year and 5-year treatment groups were random. The average annual attrition rates for the NIT, TR/NIT, and TR groups are 4.5, 3.7, and 4.5 percent for the 3-year duration groups; and 3.6, 2.3, and 3.4 percent for the 5-year durations. The slightly lower dropout rate for the 5-year plans is explainable by the extra financial and training benefits these plans provided.

The main conclusion of this section is found in Table 4, where we see no effect of the NIT on the rate of marital breakups in a model that allows for time dependence. Furthermore, time dependence is so strong in the TR/NIT plan that its previously measured destabilizing effect on marriage dwindles to a near-zero effect when projected for a period of seven years. A second important

[28] Waksberg states only that "later, a sample of the control families was selected to be interviewed for the same length of time as 5-year financials [5-year NIT experimentals]." See Waksberg, "Overview of Master File System with Particular Attention to the Operational Flow of Family Composition Data," in the microfiche provided by the National Archives along with the SIME-DIME tapes, p. 239.

conclusion of this section is the finding of no difference between the 3-year and 5-year duration-of-treatments.

Taking reconciliations and remarriages into account. We would like to know two proportions related to the marital durations of the treatment and control groups. One is the proportion of time spent together by couples who were already married when the experiment began. We will refer to these marriages as "original" marriages. A second concern is the proportion of time that the wife is married, whether to her "original" husband or to a new husband. We focus on the wife because she is most likely to maintain custody of the children and to be at risk of going on welfare.

The proportions cannot be satisfactorily estimated, however, because the duration of observation is too short, three years for most of the sample, and because the sample is too small. In particular, the numbers of wives who reconcile, remarry, and in some cases have second and third marital breakups and second and third reconciliations and remarriages are too small to permit reliable estimates of the duration of time in all these states. Nevertheless, a measure of the duration of time in an original marriage (or in any marriage) is needed to avoid relying only on the initial separation as the experimental outcome.

Groeneveld, Hannan, and Tuma examined reconciliations and remarriages in an unpublished paper,[29] and they also examined remarriages among women who

[29]Groeneveld, Hannan, and Tuma, "Topics in the Analyses of the Effects of SIME/DIME on Marital Stability," Technical Memorandum SD.12, SRI International, Menlo Park, Cal., June 1980.

were single parents when the experiment began.[30] In the *Final Report* (p. 311) they provide an informative diagram and statistics on several stages of transition from the original marriage to subsequent states that show a substantial number of reconciliations and remarriages and a small number of repetitions of these changes in marital status. From the total sample of 2,770 couples (including childless couples) there were 624 initial separations by wives who did not subsequently drop out of the experiment. Of the 624 separations, 184 (29 percent) reconciled and another 83 (13 percent) remarried. Thus, reconciliations and remarriage were relatively common. As we show below, the percentages would be even higher if we examined the treatment group separately. Among those who reconciled or remarried, subsequent marital breakups and, then, subsequent reunions were also *relatively* frequent, but the absolute numbers are too small to analyze reliably.

Realistically, we cannot obtain reliable estimates of the probabilities of being in these different marital states and of the durations in these states to obtain lifetime projections. Instead, we construct two measures of marriage duration for the "original" wife. One is the duration of time with the original husband, which we calculate as the sum of the time in the first spell of marriage and the time (if any) in the reconciliation spell. Our second measure is the duration of time married, which is a similar sum: the time in the first spell of marriage plus the time (if any) in a subsequent spell of either a reconciliation or a remarriage. The estimation method for the duration of these "summed" spells is the same

[30]See Groeneveld, Hannan, and Tuma, *Final Report*, pp. 341-344.

as that used in Tables 2-4, when the first spell of marriage was being examined, except that, when a reconciliation (or remarriage) occurs, the event that ends the summed spell is the second breakup. If there is only one spell of marriage, then of course the event that ends it, if it does end, is the first breakup. As before, the log-linear rate model takes account of whether the wife is with her original (or a second) husband in the last period of our observation.

We also estimate the duration of (or rate of ending) the wife's first period of separation. The event that can end the separation is a reconciliation (or remarriage), and the estimation method takes account of whether the wife is in a state of separation or marriage in the last period of our observation.

If the treatment and control groups are very different in the incidence of a *second* reconciliation or a *second* remarriage, we will misestimate their differences in the duration of time spent together with the original husband or of time in marriage. Similarly, if the treatment and control wives are very different in the rates at which a second period of separation ends, we will misestimate their differences in time separated. We assume that these repeat spells are sufficiently uncommon and sufficiently similar for the treatment and control wives when the repeat spells occur to ensure that our estimates give valid comparisons for the treatment and control groups. It is reassuring to know that marital breakups that occurred a "second time" were uncommon. Of the original 2,770 couples, there were 25 known cases (1 percent) of a second marital breakup following a remarriage and 80 known cases (3 percent) of a second marital breakup following a reconciliation.

Table 5 shows a summary of the results of estimating the log-linear model of the rate of ending the wives' spells of (a) marriage to the original husband (adding the time in first spells of reconciliation), (b) marriage to any husband (adding the time in either a first spell of reconciliation or remarriage), (c) separations, defined as separated from the original husband when focusing on reconciliations only, and (d) separations, defined as being unmarried when focusing on being married (to any husband).

Rows 1 and 3 in Table 5 show that adding reconciliations and remarriages scarcely changes the treatment-control comparison of the rate of marital breakups relative to the results shown in Table 3. However, rows 2a, 2b, and 4 show that the rates at which wives end periods of separation are, compared to control wives, higher for the "pure" NIT group, about the same for the TR/NIT group, and lower for the TR group.[31] We use a constant-rate model because our estimates of time dependence for the rates of reconciliation and remarriage had very large standard errors. Define r_{mu} as the rate of separation and r_{um} as the rate of reuniting (reconciling or remarrying). The subscripts refer to the transitions from married to unmarried (or separated), and vice versa. Having obtained the estimated rates from the model used for Table 5, we calculate the expected duration of time married, $E(m)$, equal to $1/r_{mu}$, and the expected duration of

[31] We note in passing that if the wife ends her spell of separation by marrying a new husband, it is unclear whether she remains "at risk of reconciling." We deal with this ambiguity simply by defining her period at risk in both possible ways. Row 2a shows our results when we allow a new marriage to end her period at risk, and row 2b is the result when her period at risk of reconciling with her original husband continues during the time that she is remarried.

Table 5

Estimated Effects of Treatments on the Sum of the First Spell of Marriage and First Spell of Reconciliation (or Remarriage) and on First Spell of Separation[a]

Type of Spell	Ratio of the Rate of Ending the Spell among Treatment Couples to the Rate of Ending the Spell among Control Couples		
	NIT	TR/NIT	TR
Only Reconciliations Added to Marriage Spell			
1. First spell of marriage plus the spell of reconciliation (n = 2363)[b]	1.14	1.45**	1.16
2. First spell of separation, defined as separated from original husband: (n = 553)[c]			
2a. Assumes a remarriage ends the risk of reconciling	1.20	.88	.88
2b. Assumes a remarriage does not end the risk of reconciling	1.27	.90	.95
Reconciliation or Remarriage Added to Marriage Spell			
3. First spell of marriage plus the spell of either reconciliation or remarriage (n = 2363)[b]	1.19	1.41**	1.22**
4. First spell of separation, defined as being unmarried (n = 553)[c]	1.12	1.03	.83

[a]A log-linear rate model is used that is the same as that in Table 3, except that all three ethnic groups are aggregated and two dummy variables for ethnicity are included among the independent variables. As in Table 3, the treatment effects in Table 5 are based on weighted averages of the coefficients of the following dummy variables specifying the treatments: NIT-3 yr., NIT-5 yr., NIT-20 yr., TR/NIT-3 yr., TR/NIT-5 yr., TR-3 yr., and TR-5 yr. The coefficients and their standard errors are reported in Appendix Table A.2

[b]As in Table 3, a ratio greater than 1 in rows 1 and 3 implies a higher rate of ending the spell of marriage for the treatment group than in the control group. A higher rate implies a shorter estimated duration of being married (by either definition of being married--to original husband or to any husband).

[c]A ratio greater than 1 in rows 2a, 2b, and 4 implies a higher rate of ending the spell of separation for the treatment group than the control group. A higher rate implies, therefore, a shorter duration of the period of separation. The "pure" NIT group is estimated to have a shorter period of separation than the control group. See footnote 31 in the text for comments about the distinction between rows 2a and 2b.

**Treatment effects are statistically significant at the 5 percent level (two-tailed test). See Appendix Table A.2 for the statistical significance of the coefficients of each treatment variable as defined in note a above.

time unmarried, $E(u)$, equal to $1/r_{um}$. The proportion of interest to us is

$$E(m)/[E(m) + E(u)],$$

where m can refer either to the wife's being married to her original husband or, alternatively, to any husband. The two proportions are shown in Table 6.

Regarding reconciliations only, column (1) shows for the four experimental groups the estimated proportions of time that the wife is with her original husband. The striking finding, shown in column (2), is that couples in the "pure" NIT program show a slightly larger estimated proportion of time together than the controls. The difference of 5 percentage points is too small to be considered statistically or practically significant, but note that there is no adjustment for attrition nor for time dependence. Columns (3) and (4) show that the proportion of time married is slightly larger for controls than for the "pure" NIT group, but again, a difference of 6 percentage points is unimportant.

The training program, TR, shows for the first time a relatively large destabilizing effect on marriage. The combined program, TR/NIT, has an effect on the two proportions that is about the same size as it was in Table 3, when there was no adjustment for attrition and no allowance for time dependence.

The emphasis we have placed on estimating the effect on marital stability of the "pure" NIT program leads us to view the allowance for reconciliations and remarriage, shown in Tables 5 and 6, as further refutation of the conclusion of Groeneveld, Hannan, and Tuma that the NIT effect is destabilizing. We cannot determine whether the NIT program is more stabilizing in Table 6 because of

Table 6

Effects of Treatments on Estimated Proportions of Time Unmarried, Allowing for Reconciliations and Remarriages: Ratio of Treatment Group's Proportions to Control Group's Proportions[a]

Experimental Group	Reconciliations Only: Proportion of Time Not Married to Original Husband[b]		Reconciliation and Remarriage: Proportion of Time Not Married[b]	
	(1)	(2)	(3)	(4)
	Proportion	Ratio of Treatments to Controls	Proportion	Ratio of Treatments to Controls
Control	.282	...	.233	...
NIT	.267	.95	.246	1.06
TR/NIT	.394	1.40	.294	1.26
TR	.344	1.22	.310	1.33

[a]The entries in this table are derived from the log-linear rate models used for Table 5 for the rate of ending spells of marriages and the rate of ending spells of being unmarried. In the "reconciliations only" case, the spell of marriage is defined as the time in the first spell of marriage to the original husband plus the time in the first spell of reconciliation. In the "reconciliation and remarriage" case, the spell of marriage is defined as the first spell of marriage to the original husband plus the time in the first spell of either a reconciliation or a remarriage. A marriage spell is considered to be truncated in the "reconciliations only" case if the woman is with her original husband at the end of the experiment. In the case of reconciliation or remarriage, the marriage spell is considered to be truncated if the woman is married at the end of the experiment. Also from Table 5 are the estimates of the rate of ending a first spell of dissolution by wives who experienced a dissolution. A dissolution spell is considered to be truncated if the woman is not reconciled (or is not remarried) at the end of the experiment. Expected values for the rates are obtained by evaluating the log-linear rate model at the sample means of the personal and family control variables.

[b]The proportion is $\frac{E(m)}{E(m) + E(u)}$, where the expected length of marriage, E(m), is the reciprocal of r_{mu}, the estimated constant rate of transition from being married to being unmarried. See note a for the two definitions of a marriage spell. The expected length of time not married, E(u), is the reciprocal of r_{um}, the estimated constant rate of transition from being unmarried (after a first dissolution occurs) to being either reconciled only or to being reconciled or remarried.

reporting differences in SIME-DIME, or whether the mix of separations and reconciliations is truly a difference in the response to NIT compared to AFDC. Either explanation is defensible on a priori grounds.

CONCLUSIONS

Our principal conclusion is that the data from SIME-DIME do not demonstrate that an NIT program would increase marital breakups among married couples with children. There are four reasons why we reach this diametrically opposite conclusion to that of Groeneveld, Hannan, and Tuma. First, we separate the "pure" NIT program from the NIT treatment that combined training, and we found a small effect of the "pure" NIT on marital breakups—an increase of 13 percent—when we used the full sample of couples for the full duration of the experiment. Second, our adjustments for presumed attrition bias, which were similar in magnitude to the adjustments recommended by Groeneveld, Hannan, and Tuma, reduce the 13 percent effect to 4 percent. Third, we argue that the rate of marital breakups should decline with time and decline differentially between treatment and control groups. When we use a model that allows for such time dependence on the rate of marital breakups, we find that neither the "pure" NIT program nor the experimental program that combined an NIT and training (TR/NIT) has an effect on marital breakups. TR/NIT had shown a sizable destabilizing effect in the statistical model that assumed a constant rate of marital breakups over time.

Last, we examine reconciliations and remarriages, following earlier work of Groeneveld, Hannan, and Tuma. We present evidence that the estimated pro-

portion of time that NIT wives were married to their original husbands was as high or higher than the proportion of time control wives were married. Taking account of reconciliations and remarriages showed about the same destabilizing effect of TR/NIT as when the first incidence of a marital breakup was assumed.

We have no hesitation in rejecting the strongly worded conclusion of Groeneveld, Hannan, and Tuma that the NIT plans in SIME-DIME "dramatically increased the rate at which marriages dissolved among white and black couples."[32] We are equally quick to credit Groeneveld, Hannan, and Tuma with methodological advances, with thoroughness in compiling the data, and with their diligent analytic and empirical responses to criticisms and suggestions during the several years of their research on this project. The fact that our results are so different is testimony to the complexities of the design of SIME-DIME, to the difficulties of inferring behavior with long-run consequences on the basis of a short-term experiment, to the uncertainty in estimating the correct time patterns when the form of time dependence is not known and the experiment is short, and to the complications that arise when reconciliations, remarriages, and resplitting need to be accounted for.

Our adjustments for attrition are necessarily speculative quantitatively, and qualitatively they are based on theorizing rather than on empirical evidence, which is unavailable. The time trends in marital breakups for each of the experimental groups are not reliably estimated, even though the point estimates of the time effects are large and theoretically justifiable. Efforts to use more

[32]See footnote 3.

complicated functions of time dependence were abandoned in the face of large standard errors for the coefficients of the time variables.[33] Similarly, in analyzing the proportion of time "together," our efforts to estimate the durations of second- and higher-order episodes of reconciliation, remarriage, and resplitting were unsuccessful because of the small sample sizes.

We conclude with comments on the policy significance of the research of Groeneveld, Hannan, and Tuma and of our reanalysis. Income maintenance programs on behalf of low-income husband-wife families are perennially controversial. The programs exist in various forms—food stamps, AFDC-UP (where UP refers to two-parent families in which an unemployed parent is the family's principal earner), and the earned-income tax credit are examples—and proposals to modify them are currently being debated. The negative income tax program, however, has receded from legislative consideration. The findings of Groeneveld, Hannan, and Tuma, although not the authors themselves, have played a role in opposing these programs. (See footnote 4.) Our reanalysis leads us to reject the interpretation of the original findings that income guarantees and payments to poor intact families will increase marital breakups.

[33] We found, for example, that adding a quadratic term in time did not significantly improve the fit of the model. We also estimated a model in which the last six-month period of the duration of a treatment plan was specified separately from the linear time trends for the first 30 (or 54 or 78) months of the experimental duration. This provided a check on whether the strongly negative interaction between time and treatment shown in Table 4 is simply attributable to a cessation of behavioral responses in the period before the experiment ended. As expected, the time trend was less strong for the treatment groups in the model in which the last six months were ignored, but the general results that are shown in Table 4 remain. These results are available to interested readers.

Finally, we call attention to the fact that however an income maintenance program affects the stability of existing marriages, this is only part, and perhaps a small part, of the problem of poor children in single-parent households. A full picture of this problem involves births to single women, the marriage (or remarriage) behavior of single women with children, the living arrangements of single women with children, and the decisions about first marriages and subsequent childbearing. How income maintenance programs affect marital stability and the length of time that children are with both parents is not yet well understood.

Table A.1

Individual Coefficients and Standard Errors (in Parentheses) of the Dummy Variables Used in Table 3 for the Treatments

Treatment Variable	Whites		Blacks		Hispanics	
	Coeffi-cient	Stand. Error	Coeffi-cient	Stand. Error	Coeffi-cient	Stand. Error
NIT - 3 yr.	.141	(.246)	.081	(.301)	-.283	(.362)
NIT - 5 yr.	.207	(.253)	.545	(.234)	.135	(.372)
NIT - 20 yr.	.124	(.383)	.120	(.420)	-.572	(.648)
TR/NIT - 3 yr.	.324	(.192)	.762	(.199)	.201	(.271)
TR/NIT - 5 yr.	.114	(.245)	.243	(.247)	-.144	(.305)
TR - 3 yr.	.208	(.236)	.354	(.242)	.005	(.369)
TR - 5 yr.	-.187	(.327)	-.060	(.300)	.320	(.407)
N	1100		784		485	

Note: See notes to Table 3 for more information on the analysis producing these results.

Table A.2

Individual Coefficients and Standard Errors (in Parentheses) of the Dummy Variables Used in Table 5 for the Treatments

Treatment Variables	Treatment Effects on the Rate of Ending: First Spell of Marriage plus the First Spell of Reconciliation (Row 1 of Table 5)		First Spell of Marriage plus the First Spell of Reconciliation or Remarriage (Row 3 of Table 5)		First Spell of Separation from Original Husband: Ended by Reconciliation[a] (Row 2a of Table 5)		First Spell of Separation from Original Husband: Ended by Reconciliation or Remarriage (Row 4 of Table 5)	
NIT - 3 yr.	.057	(.182)	.134	(.184)	.405	(.329)	.190	(.324)
NIT - 5 yr.	.245	(.174)	.224	(.180)	.293	(.276)	.150	(.260)
NIT - 20 yr.	.168	(.247)	.211	(.249)	-.363	(.558)	-.216	(.500)
TR/NIT - 3 yr.	.488	(.133)	.477	(.138)	-.096	(.257)	.067	(.234)
TR/NIT - 5 yr.	.043	(.170)	-.032	(.180)	-.209	(.278)	-.063	(.252)
TR - 3 yr.	.256	(.164)	.312	(.167)	-.159	(.342)	-.197	(.323)
TR - 5 yr.	-.124	(.208)	-.096	(.213)	-.055	(.346)	-.162	(.327)
N	2363		2363		553		553	

Note: See notes to Table 5 for more information on the analysis producing these results.

[a]Coefficients in this column refer to row 2a in Table 5. The coefficients underlying row 2b are similar and are not shown in this table.