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Multiplicity Study of Marriages and Births in Israel

A description is given of an experimental study, the aims of which were to test the feasibility of conducting a multiplicity survey for marriages and births in Israel and to evaluate different counting rules. The study included an evaluation survey from which estimates of the components of mean square error were obtained. The results indicate that an overall reduction of mean square error was obtained by use of a multiplicity rule instead of a conventional rule. The substantial reduction in sampling error attained by the use of multiplicity was not offset by the slight increases in response bias and variance.

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FOREWORD

The National Center for Health Statistics contracted with the Israel Central Bureau of Statistics to undertake methodological studies in Israel and in the Administered Areas to evaluate the multiplicity design of household sample surveys for compiling vital statistics. This is a report of the evaluation project that was conducted in Israel.

This project involved a close and effective working relationship between the staffs of the Central Bureau of Statistics and the National Center for Health Statistics. Representing the Center, I served as the Project Officer, and Dr. Sidney Goldstein, Brown University, served as a demographic consultant to the project. Dr. U. O. Schmelz and Dr. Gad Nathan served as the Project Directors for the Central Bureau of Statistics.

Acknowledgments are due to the United States-Israel Binational Science Foundation for awarding a grant to the project. The Foundation's support made it possible to carry out the full study design despite the adverse effects of a rapid inflation and several devaluations of the Israel currency during the course of the project.

Monroe G. Sirken, Ph.D.
Associate Director for
Mathematical Statistics

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SYMBOLS

Data not available	- - -
Category not applicable	. . .
Quantity zero	-
Quantity more than 0 but less than 0.05	0.0
Figure does not meet standards of reliability or precision	*

MULTIPLICITY STUDY OF MARRIAGES AND BIRTHS IN ISRAEL

Gad Nathan, Usiel O. Schmelz, and Jay Kenvin, *Israel Central Bureau of Statistics*

Chapter 1. INTRODUCTION

Some years ago, Dr. Monroe G. Sirken of the U.S. National Center for Health Statistics developed a statistical model for a new type of household sample survey design—the design of household surveys with multiplicity. This type of design appears to be particularly well suited for improved estimation by means of a single-time household sample survey of the number of vital events that occurred in a population. Improved estimation as compared to a conventional household survey is to be obtained through reduction of sampling error and of certain biases.

In the conventional household survey, only events occurring to members of the particular household interviewed are studied. The multiplicity survey also covers events occurring in other households. The additional households to be covered are defined by the specific rules adopted for the survey. For instance, the survey may cover events to others living elsewhere in the survey population who are relatives (parents, siblings, children, etc.) of members of the household actually interviewed. Consequently, the same event may be reported by more than one household.

Increasing the number of households which can report an event obviously increases the number of events reported by a given sample of households, as compared to a conventional survey. This does not automatically decrease sampling variance in the same way as is achieved by increasing the number of households in a conventional survey; in practice, however, sampling variance can be reduced in many cases. Furthermore, although some types of response errors may increase in a multiplicity survey due to the reporting of events by relatives, certain types of coverage bias encountered in a conventional survey—those due to there being no household in the survey population to report an event—may be reduced. For instance, it has been surmised that the use of multiplicity may increase the chances of locating deaths which occurred in single-person households.

In order to put this design into operation, it is necessary that each household sampled report, in addition to the events that occurred to its own residents:

(a) Events that occurred to a network of specified persons, such as relatives, living elsewhere.

(b) The number of households containing persons eligible to report an event according to the specific rules adopted for the survey. This number is called the "multiplicity" of the event.

To estimate the total number of events of any type studied in the population, each event reported in the survey is weighted by the reciprocal of its multiplicity. This weighting is needed to adjust for the multiple number of households eligible to report the same event. Thus events which could have been reported by a larger number of households make a smaller contribution to the estimate than those which could have been reported by a smaller number of house-

holds. While it is the number of different households in the network that determines the multiplicity of each event, operationally it will often be necessary to list all the persons in the network who are eligible to report and then ascertain in how many different households they reside, as several relatives may be living together.

Like the conventional survey, the multiplicity survey collects information on various characteristics of each event covered and of the persons concerned, whether they live in the household interviewed or elsewhere. Further information on the methodology and results of surveys with multiplicity may be found in papers by Sirken (1970) and Sirken and Royston (1973). (See bibliographical list, appendix I.)

After several practical tests of the new method had been undertaken in the United States, negotiations took place in 1970 between the U.S. National Center for Health Statistics (NCHS) and the Israel Central Bureau of Statistics concerning the feasibility of conducting a multiplicity survey for some types of vital events in Israel and in the Administered Areas. Israel seemed to offer several advantages for testing the new design:

1. Israel has a very heterogeneous population with regard to type of culture, family structure, etc., including Jews of different origin (from Europe, from Asia-Africa) and of different duration of stay in Israel and non-Jews of various religions. In addition, Arabs in the Administered Areas differ from those in Israel in some respects.

2. Household surveys—the Labour Force Surveys—comprising a representative sample of the population are undertaken every week and are regularly tabulated for each quarter of the year.

3. There exist reliable and detailed demographic statistics for Israel, which can be used for assessing the results of the multiplicity survey.

The Israel Central Bureau of Statistics gladly accepted the idea of undertaking a trial of the new multiplicity design in Israel and the Administered Areas, with the twofold intention of collaborating generally in methodological progress and of acquiring a technique which might be of assistance in its own work. In particular, death reporting through routine notification is known to be very deficient in the Administered Areas, and a successful application of the new technique might remedy this lacuna.

Since the purpose of the multiplicity study was to test a new survey design and the consequent innovations in fieldwork procedures, the project consisted of two stages each for Israel and for the Administered Areas: the basic survey and an evaluation study. The necessity of a special evaluation study, involving an additional round of interviews, influenced some features of the basic survey (e.g., the need to collect detailed addresses of relatives so as to make possible the evaluation interview); it also influenced the timetable and the budget of the entire project.

The topics of inquiry for the multiplicity surveys in Israel and in the Administered Areas were to be specific types of vital events (births, marriages, and deaths). The events that occurred during 1 calendar year were to be reported.

Linking the fieldwork for the basic multiplicity survey in Israel to the continually conducted Labour Force Survey (LFS) had great practical advantages but also imposed certain limitations, which will be set out later in this report. In the Administered Areas the same sampling frame was used as for the current LFS there, but the actual enumeration interviews were eventually separated.

After the funds initially thought necessary could be secured by NCHS, an agreement was concluded in summer 1971 between NCHS and the Israel Central Bureau of Statistics. It provided for the conduct of a multiplicity survey on vital events in Israel and the Administered Areas, including special evaluation studies.

The actual implementation of this project was beset with numerous difficulties and delays, particularly for the following reasons:

1. By the time the U.S. funds became available, somewhat belatedly, and the contract could be signed, the Israel Central Bureau of Statistics already had had to make active preparations for taking a na-

tional census of population and housing, held eventually during May-June 1972. The census taxed to the utmost the Bureau's professional and organizational resources. The two collaborating institutions therefore agreed that commencement of work on the multiplicity survey should be postponed until some time after the census enumeration so that the planning stage for the survey would not start before fall 1972. This postponed date of commencement was adhered to.

2. In recent years Israel has experienced rapid inflation and several devaluations of the local currency. The budgetary estimates for the multiplicity survey were drawn up in 1970 and formulated in Israeli currency. As time went on, these amounts became clearly insufficient. As a result of financial stringency, repeated cuts had to be made in the survey program. Eventually it was found that the survey could be concluded only with the help of an additional grant from the United States-Israel Binational Science Foundation, which itself became available later than anticipated, entailing further postponements of operations.

3. The war of October 1973 and its protracted aftermath seriously affected the multiplicity surveys. The major hostilities occurred during the time between the pretests and the full-scale implementation of the multiplicity survey in Israel. As a consequence of the considerable number of deaths caused by the war to the Jewish population and the recent date of these losses, one of the planned topics of inquiry—deaths of relatives—had to be hastily exchanged in the questionnaires for interviewing in Israel by another topic unaffected by such sad emotions. Furthermore, the massive and prolonged callups strongly affected the available staff at headquarters, in the regional offices, and among the rank and file of the enumerators. All this resulted not only in delays but especially in regrettable losses of quality. With a staff of enumerators consisting largely of new and inexperienced workers, many of whom soon dropped out and had to be replaced by other fresh recruits, and with a depleted staff at the regional level for training and supervision of the new interviewers, the Israel Labour Force Survey, to which the basic survey of the multiplicity study was linked, failed to achieve the high completion rates customary before the war. On the other hand, it may be a cause for some gratification that the survey could be conducted at all, with substantial results, despite the disruptions produced by the sudden outbreak of war.

It had originally been intended to study births and deaths among Jews in Israel; births among non-Jews in Israel (deaths among them are too few to be studied through a small sample of the population); and births, child deaths, and other deaths (the two latter with appropriately different multiplicity rules) among Arabs in the Administered Areas. After the pretests the survey of non-Jews in Israel had to be dropped for reasons of economy that will be set out more fully in a later chapter of this report. And, as indicated above, questions about deaths to Jews in Israel were dropped as a consequence of the 1973 war. Marriages were substituted as a second topic of inquiry, along with births.

The years for which respondents were asked to report events were eventually 1973 for Israel, where the fieldwork for both basic survey and evaluation study was carried out in the first half of 1974, and 1974 for the Administered Areas, where the fieldwork took place at the beginning of 1975.

Several evaluation strategies were utilized in the surveys. Both in Israel and the Administered Areas, a round of special evaluation interviews was held with relatives of some of the persons enumerated in the basic survey (the latter having provided the names and addresses of these relatives). This made it possible to compare reports on the same event from two households belonging to the same multiplicity network. For both births and marriages in Israel and for births in the Administered Areas, aggregative comparisons

could also be made from the available vital statistics. In the pretest stage in Israel only, vital events reported by routine notification were reinvestigated through household interviews. This approach, which checks the accuracy of subsequent reporting in a household interview against the current notification but is not directly aimed at evaluating the multiplicity technique, was eventually discarded.

Though undertaken by the same organization, the Central Bureau of Statistics, within the framework of the same contract, the basic surveys and evaluation studies for Israel, on the one hand, and for the Administered Areas, on the other, are to be viewed as distinct operations. The populations are demographically and culturally very different (as reflected in differences of contents and layout of the respective multiplicity questionnaires), the scope and quality of the already available vital statistics documentation is different, and all fieldwork for the two territories was conducted separately. Also, the results of applying the multiplicity technique were markedly different. Hence, each operation requires a separate report.

The following chapters of this report will deal with the multiplicity survey in Israel only, excluding the Administered Areas. The design and execution of the basic survey and of the evaluation study are described in detail in chapters 2 and 3, respectively. The integrated results of both these stages are presented and discussed in chapter 4 in terms of testing the application of the multiplicity technique under Israeli conditions. Also in chapter 4 are data on co-residence of relatives, a byproduct of the analysis of the multiplicity network of relatives and the distribution of relatives according to households. In chapter 5 selected points from the experience gathered in Israel are considered, and some remarks are proffered on matters that may deserve thought and exploration in future applications of the multiplicity technique. The report terminates with a summary (chapter 6) and a series of appendixes containing a bibliographical list, the questionnaires used, and other documentation.

In one respect, the Israeli multiplicity study has proved the obvious: the superiority of a complete current registration of vital events over subsequent household surveys, in which the respondents are not always the persons most directly concerned and have to rely on memory, facing particular difficulties in the dating of events which occurred at the turn of two calendar years. But this is almost beside the point.

The real purpose of the Israeli multiplicity survey and its evaluation program has been to compare the efficiency of this new technique with a conventional household survey. As applied in the Israeli survey, the multiplicity technique did indeed greatly reduce the total mean square error, primarily through reduction of the sampling variance. The findings with regard to biases were less clear-cut. This problem, as well as that of response errors and of the interesting divergencies that emerge when different multiplicity rules are put into operation, will be set out and discussed in later chapters of the report.

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Chapter 2. THE BASIC SURVEY: DESIGN AND EXECUTION

2.1 Purpose and General Design

The major purpose of the study of multiplicity techniques was methodological rather than substantive. This was particularly the case for Israel, where adequate data on vital events are obtained on a current basis from virtually complete registration, so there is no reason to expect that data from a sample survey, however sophisticated, would afford an improvement. The existence of reliable data from a well developed registration system does, however, provide a standard against which the results of the multiplicity study may be checked.

The purposes of the basic survey in Israel were primarily to test the feasibility of using multiplicity techniques in a population which is in general relatively advanced but which is also very heterogeneous with respect to cultural background, and secondly to serve as a basis for evaluating the multiplicity technique in the specified population. Thus it was necessary to test the ability and willingness of members of various groups to report events which occurred to relatives, to inform enumerators of a few details concerning the events, and to report the addresses of relatives of the person to whom the event occurred (the event person). This last point served both as the means for determining the number of households in which the relatives of the event person lived—that is, the multiplicity of the event—and as the means for selecting the sample for the evaluation study, to be discussed later. It should be noted, however, that the multiplicity could have been determined by direct questions without obtaining addresses of relatives.

In addition, it was necessary to assess the difficulties involved in technical aspects of a survey with multiplicity, such as questionnaire formulation, training and organization of field staff, and coding and processing.

Finally, the basic survey was designed to serve as a basis for evaluating the efficiency of different multiplicity "counting rules." The "counting rule" of a multiplicity survey is defined as the rule which links each event to the households of relatives who should report it, and it is determined by the network of relatives covered. The efficiency of different multiplicity counting rules relative to the efficiency of a conventional survey was to be measured by the total mean square error and its components—overreporting and underreporting biases, sampling variance, and response variance. The detailed basic survey estimates were to be compared with the current demographic estimates based on vital registration. Reports of events in the basic survey were also to serve as a framework for the evaluation survey sample and for the comparisons needed to evaluate the error components.

In order to achieve these goals, the survey was originally designed to cover as much of the population of Israel as possible, both to simplify comparisons with current demographic estimates and to represent the heterogeneity of the population. Since the basic unit of inquiry was to be the household (with one respondent answering for all members), the population of kibbutzim and institutions was excluded from the survey population. These populations are generally included in sociodemographic sample surveys in Israel (e.g., the Labour Force Survey). However, information is not obtained directly from the person but from records available in the office of the institution or the kibbutz. Information on vital events occurring to a person's relatives is not usually available from such basic personal records. The high cost of special individual interviews for the multiplicity survey weighed in favor of excluding these subpopulations,[a] especially since for Jews they include only about 5 percent of the population. The Bedouin tribes in the south were excluded for similar reasons.

According to the original plans, the survey population included the total resident *de jure* population, according to the Bureau's regular definitions, excluding residents of kibbutzim and institutions and the Bedouin population of the south. As explained below, a decision to in-

[a]An alternative method that includes events to persons in institutions is proposed in section 5.2.

clude only the Jewish population in the survey was subsequently reached.

An important design decision related to topics to be included in the multiplicity survey. Since a comparison with reliable, current demographic estimates was required, it was decided from the start to limit the field of possibilities to basic vital events of relatively high frequency—births, deaths, and marriages. The initial design, as tried in the pretest, included births for Jews and for non-Jews and deaths for Jews only. Subsequently, the basic survey covered births and marriages. The recall period was fixed as 1 calendar year to attain sufficient frequencies, on the one hand, and to reduce recall error associated with long recall periods, on the other.

A basic design decision for multiplicity surveys is on the extent of the reporting network connected to each event—the counting rule. Wider counting rules, while usually reducing sampling variance, may increase reporting bias and place an added burden on the fieldwork. A hierarchical series of counting rules was required to study the relative efficiency of each rule. Another requirement was that the counting rules elicit sufficient frequencies of events. This can be attained by considering the relative position of an event in the life cycle and ensuring that relatives of appropriate different generations are included in the network (e.g., both siblings and parents of married persons, both siblings and sons and daughters of deceased persons).

It was decided to limit multiplicity reporting on births to the female line, primarily to ensure a feasible burden on the field staff. Therefore reports on births to women in the household and to daughters and sisters of women in the household were requested. However, for deaths (the frequency of which is much lower), reports were requested in the pretest on spouses, parents, siblings, and children of all persons in the household. Similarly, in the final survey, the counting rule for marriages was all persons in the household and children and siblings of all persons in the household.

The feasibility of carrying out a large-scale multiplicity study in Israel was enhanced by the fact that a quarterly Labour Force Survey (LFS) is carried out on a current basis. A concise description of the survey can be found in the publication of the Central Bureau of Statistics (1975). The survey, besides its main purpose of providing data on the labor force and its characteristics, serves as an omnibus for collecting additional data on the labor force and other topics. The LFS is a quarterly survey with a self-representing probability sample on a rotating panel basis, with four panels each quarter. The total sample size per quarter since January 1974 has been about 12,000 households. (Previously it was half this size.) At an early stage of planning, the LFS was found to be a suitable vehicle for carrying out the basic multiplicity survey.

The general design of the basic survey was to add to the LFS questionnaire a multiplicity questionnaire, which included questions on events to the specified relatives and the addresses of relatives who could have reported these events, in order to determine the multiplicity of each event. Before fixing the final detailed basic design, described in section 2.3, a pretest was carried out, as described in the following section.

2.2 The Pretest

The objective of the pretest for the basic survey was fundamentally to test the feasibility of the general design, described above, on a small scale. Specifically the pretest was designed to test the attitudes and reactions, both of household members and of field staff, to questions relating to events, especially to those which occurred to relatives outside the households, and to questions on the existence and whereabouts of relatives. Additional objectives were to test out the proposed procedures and questionnaires, to get rough measures of durations of interviews, and to serve as a basis for a pretest of the evaluation survey. The pretest of the evaluation survey, which comprised an evaluation of reports from the basic survey pretest and an evaluation of reporting on events registered in vital records, is described in chapter 3.

The topics covered in the pretest were those originally planned for the basic survey—births for Jews and non-Jews and deaths for Jews only. The multiplicity counting rules, outlined above, were limited to the female line for births; i.e., including reports on births to daughters and sis-

ters of females in the household as well as births to women in the household. This was done because the frequency of births is sufficiently high that even limited multiplicity counting rules were expected to provide high yields, and it was not possible to burden enumerators with too many reported events. For deaths, however (and subsequently for marriages), a wider multiplicity counting rule was required, both in order to attain sufficient yields for analysis and to increase the probability of there being an eligible surviving relative. Thus reports were requested on deaths of spouses, parents, sons, daughters, brothers, and sisters of all persons in the household irrespective of sex.

For both types of events multiplicity reporting was hierarchical within the interviewed household. This means that events reported by a relative of higher order (e.g., by the mother of the event person) were not to be reported again by a relative of lower order residing in the same household (e.g., by a sister of the event person). The order adopted for births was: to women in the household, to daughters living elsewhere, and to sisters living elsewhere (if not previously reported as daughters). That for deaths was: to spouse, to parents, to children, and to siblings, again each category excluding deaths already reported. For the pretest an attempt was made to include reports also on births which occurred to women belonging to the household during the reference period (the year 1972) who had meanwhile died.

The questionnaire administered in the pretest comprised three basic parts. The first was the regular LFS questionnaire (see Central Bureau of Statistics (1975)), which included a listing of all persons in the household, with basic demographic characteristics, and a detailed labor force questionnaire for each person aged 14 and over. The second was the multiplicity screening questionnaire booklet, on which was recorded, separately for births and for deaths, whether events occurred to specified relatives during the reference year. The last part comprised event reports, which were used only in the case that an event was reported on the screening questionnaire. A separate sheet was completed for each event reported. It contained demographic data on the event (month of occurrence, sex of the child born or of the deceased, age of mother or of deceased, and residence of deceased at the time of the event). It also included a list of all relatives who could have reported the event, together with a complete address for each.

Originally, two different procedures were considered for the completion of the event reports. According to the first alternative, the enumerator was to complete all three parts of the questionnaire—including the event report, in the case that an event was reported—during a single visit, with callbacks as necessary. Since there were apprehensions that household members might not be able, at the time of the enumerator's visit, to report fully and accurately on the details of events which occurred to relatives outside the household, and especially on addresses of relatives, an alternative of mail-back for event reports was considered. Under this procedure the enumerator was to complete the LFS questionnaire and the screening questionnaire on his initial visit. If an event was reported, an event report was to be left with the household for self-completion and mailed back by the household respondent. This was to be complemented by enumerator followup for nonresponse. As planned, the first alternative was tested first (with mail-back, however, for households not contacted after three visits). Since this alternative provided relatively good results (whereas very few questionnaires were received by mail from households not contacted), it was decided to use this procedure without testing the mail-back procedure.

For technical reasons, it was not feasible to carry out the pretest of the basic survey as an addition to the regular LFS. Furthermore, a less expensive (i.e., highly clustered) sample was required with only a limited number of enumerators. Thus the pretest was conducted on the basis of a judgment sample of six 1972 Census of Population and Housing enumeration districts (ED's) for Jews and six for non-Jews. For the Jewish population two ED's were selected from Jerusalem (one from a high and one from a low socioeconomic area), one each from Haifa and from Tel Aviv, and two from an urban locality in the Tel Aviv conurbation. For the non-Jewish population two ED's were selected from each of three localities—Nazareth, a Moslem village, and

a Druze village. For each ED about 10 addresses of households with at least 3 persons each which did not participate in the census 20 percent sample stage were selected.

The pretest of the basic survey was carried out in May and June 1973 by eight enumerators. They had had previous experience in the LFS and were given a supplementary 4-hour training period, including practice interviews, after studying written instructions and completing home exercises.

In general, the pretest indicated that the design proposed was feasible and could be used with only minor modifications. Completion rates were high—questionnaires were completed for 60 out of 66 Jewish addresses and for 60 out of 61 non-Jewish addresses. Noncompletion was primarily due to not locating addresses or households (four cases). It should be noted that the addresses had been recorded about 1 year earlier in the 1972 census. Only one absence (after three callbacks) and one refusal were recorded for Jewish households.

The average duration of interview was 8.0 minutes for Jewish households and 11.4 minutes for non-Jewish households. Households with events required, on the average, only about 5 minutes more interviewing time than households without events (11.4 versus 5.4 minutes for Jews and 14.1 versus 9.6 minutes for non-Jews). Interview time varied considerably by locality, primarily due to variation in household size. Although exact numerical comparisons were not possible, it seems that interview duration for households without events was hardly greater than that for the regular LFS interview.

Finally, the pretest clearly indicated that respondents were, in general, both willing and able to report on events occurring to relatives. This was in contrast to strong prior apprehensions on this point on the part of the field staff. However, some difficulties were encountered in reporting on deaths and, in a few cases where no adult female was available for interview, on births. Moreover, respondents found difficulty in reporting exact street addresses of relatives. Even though they often knew the location exactly and how to reach it, they could not provide the exact name or numerical identification of the address. In fact, less than half the addresses reported were complete. This point was further borne out in the evaluation survey pretest, in which about 20 percent of complete addresses reported by relatives were not located.

The major design modifications resulting from the pretest related to the format and the formulation of the questionnaire, to the coverage of events to be reported, and to the reporting of addresses of relatives. The questionnaire format was changed to that of a booklet in which both the screening questionnaire and the event reports were bound together. Slight changes in the wording of the questions and in the questionnaire flow were also made on the basis of the pretest experience. Event-reporting coverage was limited to reports of events to members of the household at the time of the interview and to their relatives, irrespective of their situation at the time the event occurred. Thus, for instance, births which occurred abroad in 1973 (the reference period for the final survey) to new immigrants were included, while births in Israel to women who had emigrated before the interview were not included. Finally, attempts were made to increase the usefulness of addresses reported for relatives by adding husband's first name for married women, by recording telephone numbers (at home and place of work), and by placing more emphasis on address reporting in enumerator training. The final design, incorporating these and other modifications, is described more fully in the following section.

2.3 The Final Design

Two major changes in the final survey design were made between the termination of the pretest in June 1973 and the execution of the basic survey in the first quarter of 1974. The first was a change in topics—the replacement of deaths by marriages. This was primarily a result of the Yom Kippur war in October 1973 and its aftermath. The subject of death was considered too sensitive for inclusion as a topic so shortly after the war with its tragic losses. The topic of marriages was chosen to replace it because multiplicity counting rules similar to those for births could be used without further pretesting, and

the event has sufficient frequency to be useful for analysis by the sample size envisaged.

The second major change concerned the survey population—the limitation of the basic survey in Israel to the Jewish population only. This was done primarily out of budgetary considerations. Differences between the Jewish and the non-Jewish population would have necessitated separate analyses for the two subpopulations and therefore a large relative sample size for non-Jews. In addition, the regular LFS sample includes only about 800 non-Jewish households per quarter. While this is sufficient to estimate prevalent labor force characteristics, it is small for estimation of relatively rare events such as birth and deaths and certainly would not provide a sufficient number of cases for evaluation. The use of an additional quarter to increase the number of events would result in too much variation of the time period between the interview and the reference year (1973). Furthermore, the parallel study in the Administered Areas would provide some insight on the applicability of multiplicity methods to a non-Jewish population, albeit less used to contact with modern conditions than the non-Jewish population of Israel.

The final design thus followed the general outline in section 2.1 with the changes and the modifications resulting from the pretest conclusions described in the previous section. The population of potential event-reporters (members of the event household and their specified relatives) was finally defined as the regular resident *de jure* Jewish population (strictly speaking, members of households whose head was Jewish) at the time of the survey (February-March 1974), excluding residents of kibbutzim and institutions, according to the standard definitions of the LFS.

The events on which reports were requested were all births and marriages which occurred to members of the above population in 1973. Reports were requested on births even if the baby had since died and on marriages even if they had since terminated. Persons currently belonging to the survey population were included even if at the time of the event they did not belong to the survey population (e.g., if they then resided abroad or in a kibbutz). However, events which occurred to persons who were in the survey population at the time of the event but not at the time of the interview (e.g., births to women who had moved abroad or to a kibbutz since the birth) were not included. The same rules applied, obviously, to the network of relatives on whom reports were received. Although this does not conform exactly to the definition used for regular demographic estimates of vital events, the differences were thought to be small and counterbalancing. While exact proof of this was not available from the survey, the results did give an indication that these differences were indeed very small.

The reporting relatives were finally defined as mothers and sisters of women who gave birth and as parents and siblings of persons who married (in that hierarchical order). This facilitated analysis of the survey by the following alternative counting rules.

1. *Conventional rule*—based only on reports of events to members of interviewed household (each with multiplicity of 1).

2. *Restricted multiplicity rule*—based on reports of events to members of the interviewed household, to their children (for marriages), or to daughters of women in the household (for births). The multiplicity for this rule is the number of different households in which the event person and the person's parents (or mother) reside.

3. *Full multiplicity rule*—based on reports of events in the interviewed household, marriages occurring to children and siblings, or births occurring to daughters and sisters of persons in the household. The multiplicity for this rule is the total number of different households recorded in the multiplicity network.

It should be noted that the definitions of relationship included only biological relations with at least one common parent (half-siblings) and legal adoptions, but not stepchildren or step-siblings (with no common parent). Further-

more, it should be noted that for marriages the event is defined as "a person marrying," so a single marriage is counted as two events occurring to two partners (with different multiplicity networks).

The sample design of the basic survey, as mentioned above, was on the basis of a selected part of the regular LFS in the first quarter of 1974. The LFS is a self-representing probability sample selected as a systematic random sample from apartment tax lists in all large urban localities (the certainty stratum) and in a stratified P.P.S. sample of smaller localities. The basic features of the sample design can be found in Central Bureau of Statistics (1975), although it should be noted that the sample size has meanwhile been doubled, with the result that the certainty stratum's proportion has been increased considerably. The sample is subdivided into four panels and into 13 enumeration weeks, each part being a self-representing probability sample. For the multiplicity study sample eight enumeration weeks (the 5th through the 12th) for three out of the four panels were used, providing a gross sample size of about 5,000 households. This was about the same size as originally planned, via all 13 weeks and all four panels of a quarter, when the LFS sample was half its present size. Since institutions and kibbutzim, where clustering effects may be high, were not included, the sample could be regarded for all practical purposes as a simple random sample without replacement.

The LFS questionnaire comprises a listing of all household members and their basic demographic characteristics, which include year and month of birth, year of present marriage, and if the present marriage is the first one (appendix III). This listing was used as the basis for screening in the multiplicity survey, and the data on characteristics was an aid in this procedure. The supplementary multiplicity section included, separately for births and for marriages, a screening questionnaire and a set of event reports, all bound in a single booklet (appendix II). The regular LFS enumerators interviewed for the multiplicity study as an additional topic after completion of the regular LFS questionnaire. The respondent for the multiplicity study, as for the LFS, could be any available household member aged 14 and over. This was usually the household head or his wife. More stringent respondent rules could be employed, but in this case the use of a rule different from that of the LFS would have increased costs considerably and might not even have been possible within the existing interviewing arrangements.

In the screening questionnaire all ever-married women under 50 in the household were listed (Q. 1) and it was ascertained if any of them gave birth during the year 1973 (Q. 2). Similarly, all household members who got married in 1973 were identified from the LFS listing (Q. 13). Then, all ever-married women aged 34 and over were listed (Q. 4) and asked if they had married daughters living elsewhere in Israel (Q. 5) and, if so, if any of them gave birth in 1973 (Q. 6). Similarly, all ever-married persons aged 34 and over were listed (Q. 14) and asked if they had sons or daughters living elsewhere in Israel who married in 1973 (Q. 15). Finally, all women and girls in the household whose mother did not reside there were listed (Q. 8) and asked if they had sisters living elsewhere in Israel (Q. 9). For each sister listed they were asked if she gave birth in 1973 (Q. 10). All persons who did not have a parent residing in the household were listed (Q. 17) and asked if they had siblings living elsewhere in Israel who married in 1973 (Q. 18). The screening procedure is shown (for births) in diagrammatic flow form in figure 1.

Only if an event occurred to a member of the survey household or to a specified relative was it necessary to complete an event report. This included basic demographic data on the event (sex of event person, month of occurrence, age of mother or of married person, and locality of wedding) and a listing of the multiplicity network (the woman who gave birth and her mother and sisters, or the person who got married and his or her parents and siblings). The address of each listed relative was recorded, and from this the total number of separate households in which the members of the network lived (i.e., the multiplicity) was determined. Supplementary information (telephone numbers at home and at work, first name of husband, etc.) was requested, both in order to determine unequivocally which persons lived in the same household and, especially, for location of relatives in the evaluation survey.

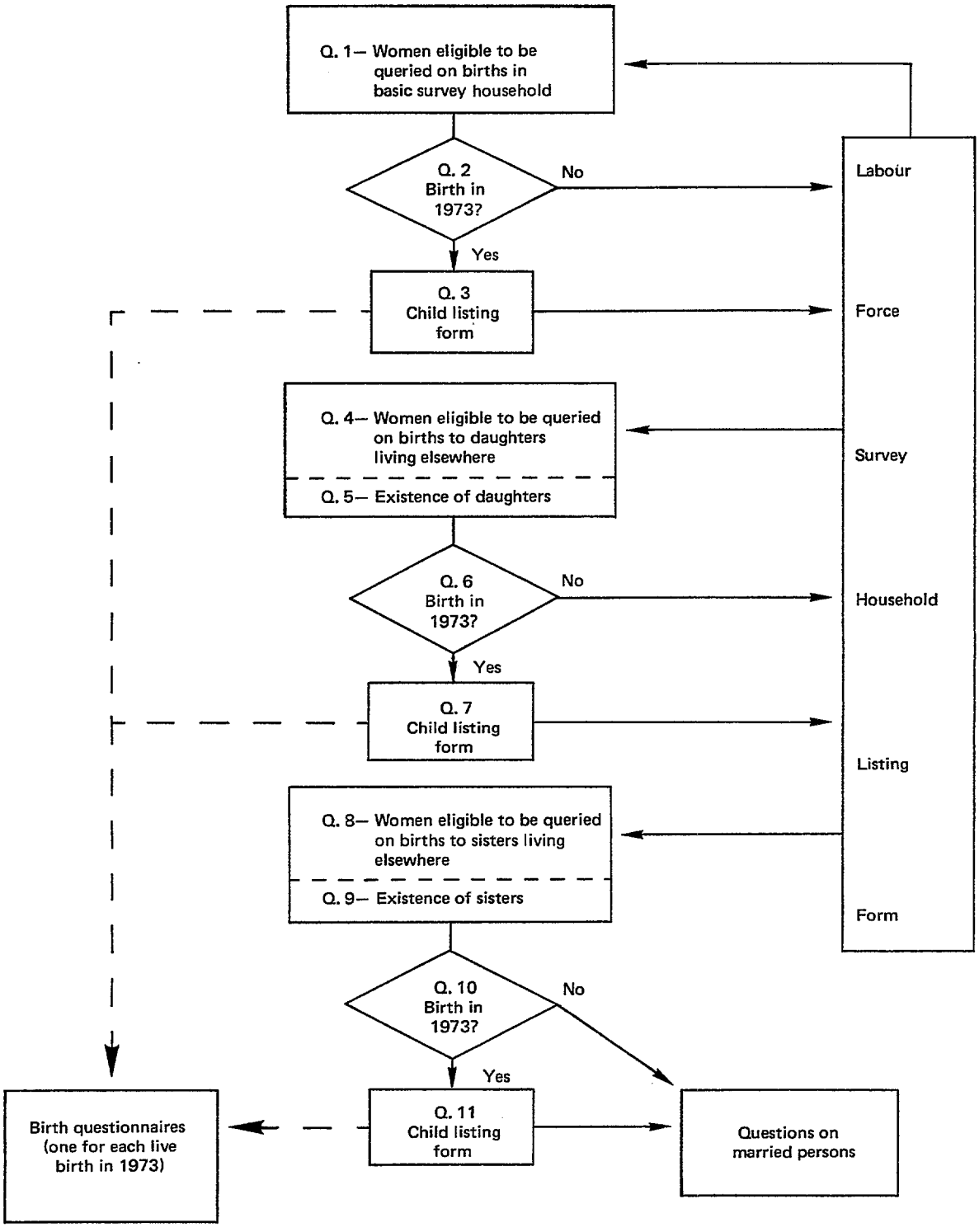

Figure 1. Questionnaire flow diagram—births.

2.4 Execution of the Basic Survey

Although the execution of the basic survey followed, fundamentally, the planned design described above, field operations encountered severe difficulties due to the special conditions of the first quarter of 1974. This was primarily due to the fact that, although the Yom Kippur war had officially terminated 2 months earlier, there was still a war of attrition and a high degree of mobilization of reserves. This severely affected the availability of experienced field staff at all levels, both directly, for men, and indirectly, for married women who found it difficult to work because of family problems. The problem was aggravated by the fact that, as scheduled, the LFS sample was doubled in the first quarter of 1974, while the training and recruitment of the necessary additional field staff, scheduled for the last quarter of 1973, was held up due to the war. All this resulted in a high turnover rate of field staff and a high proportion of new and inexperienced enumerators. In addition, the high rate of mobilization caused dislocations in the survey population since many families whose head was mobilized left their regular residence temporarily to stay with parents or other relatives.

The fieldwork was carried out by about 30 LFS enumerators, who received a supplementary full day's training session for the multiplicity study, with practice interviews, after reading the special enumerators' manual and completing exercises at home. Supervision was carried out in the Bureau's three regional field offices by the regular LFS field supervisors, who also checked and hand edited the multiplicity questionnaires. Due to the field-staffing problems mentioned above, the field timetable by enumeration weeks could not always be strictly adhered to and some interviews had to be postponed to the first part of the second quarter. In particular, the interviews of some 60 households in Jerusalem were postponed and carried out together with the evaluation survey. Overall, enumeration for the basic survey was carried out from January 27 through April 10.

The primary factor affecting completion rates in the LFS was difficult field conditions. The following completion rate data, which show this, are synthesized from reports from various sources, with some minor adjustments and imputations:

(1)	Total addresses sent to field (Jewish localities, excluding kibbutzim and institutions):	5,185
(2)	Total LFS questionnaires received:	4,197
(3)	Less non-Jewish households in Jewish localities:	92
(4)	Jewish households for multiplicity survey—(2) - (3):	4,105
(5)	Multiplicity questionnaires completed:	4,084
(6)	LFS completion rate—(2)/(1):	0.81
(7)	Multiplicity survey completion rate—(5)/(4):	0.99
(8)	Estimated overall completion rate—(6)×(7):	0.81

It should be noted that the LFS completion rate of 0.81 (line 6) expresses losses due to both "zero cases" (vacant dwelling units, commercial units, etc.) and to genuine nonresponse (addresses not located, residents not at home, refusals, etc.). Unfortunately, the work pressure on the field staff did not enable a detailed report on the breakdown of noncompletion by cause. However, prewar LFS overall completion rates for 1972 through 1973, which are not strictly comparable to those for the specific multiplicity study population, were about 0.86-0.88, and noncompletion was approximately equally divided between zero cases and nonresponse. Since the same sample frameworks were used, the increase in noncompletion must be assigned primarily to increased nonresponse. The specific multiplicity survey completion rate (line 7) was practically maximal and, as anticipated by the results of the pretest, it was possible to complete multiplicity questionnaires for virtually all households in which the LFS interview could be completed. Overall completion rates varied considerably between field districts, from 0.68 in the Jerusalem district to 0.86 in the Haifa district. This was primarily a function of differential difficulties in recruitment of experienced field staff and in supervision in different areas.

The completed questionnaires were hand edited in the district field offices (with serious edit-failures returned to the field) and finally at the center in Jerusalem. The most frequent error found was noncompletion of questions 12 and 20 (totals of events reported). This item could

be completed in the office, but noncompletion limited the possibility of consistency checks.

Negative answers to questions 2, 6, 10, 15, and 18 (reports on events) were also occasionally omitted. A few imputations were made at the central editing stage, mostly on the basis of comparisons with the household listing form, for events which should have been reported by the interviewed household. In addition, five reports on events to persons not in the survey population at the time of interview were withdrawn from processing at the central editing stage, in accordance with the definition of the event-reporting population in section 2.3. On the other hand, of the 1,553 events finally processed (887 births and 666 marriages), 11 events (all marriages) were to persons in the survey population at the time of interview but not at the time of the event.

Questionnaires of households with events were coded directly for punching, one record for each event, with common fields for data on households with more than one event. The record format is given in appendix IV. The punched records underwent automated logical checks and editing and in some cases missing data and edit-failures were corrected or imputed. Basic computations were made by computer to determine, separately for births and for marriages, multiplicity values for the three counting rules, the total weighted contribution of each household according to the three rules, and the co-residence codes. The tabulations on which the results presented in chapter 4 are based were then completed, primarily by the standard SPSS programing package.

Chapter 3. THE EVALUATION SURVEY: DESIGN AND EXECUTION

3.1 Purpose, General Design, and Pretest

A major component in the study of the usefulness of the multiplicity technique is knowledge of the efficiency of multiplicity estimates for different counting rules as compared with conventional estimates. The efficiency depends on components of sampling error, response variance, and response bias. Basic parameters necessary for this purpose are measures of the extent of overreporting and underreporting of events which occurred during the reference year and the extent to which households report correctly on demographic details of the basic survey. These can be determined by means of an independent set of reports on the same events.

Thus it was decided at the earliest planning stage to include a substantial evaluation survey as an integral part of the multiplicity study. The purposes of the evaluation study were to provide estimates for rates of overreporting and underreporting of events by different degrees of relatives, to estimate the reliability of multiplicity reporting, and to provide some measures of response errors in reports on the demographic characteristics of the events reported. The parameters thus measured were to be synthesized, by means of a theoretical model explained in brief in appendix VI. This makes possible comparison of the efficiency of estimates for different counting rules as measured by the total mean square error and its components—sampling variance, response variance, and response bias.

Initially, two different evaluation techniques were considered. The first, which itself consists of two alternatives, relied on the virtual completeness in recording of vital events by the register in Israel and was to be based on a comparison between replies of respondents in a multiplicity survey and the data in the vital records. The evaluation could be done by starting out with reports on events from the basic survey and searching the appropriate vital record for comparison. This, however, would obviously not allow for any measure of underreporting in the basic survey. Furthermore, the efficiency of a measure of overreporting would strongly depend on the possibility of complete matching, which is extremely difficult due to changes of address and incomplete address reporting. This alternative was therefore dropped from consideration before the pretest stage. The other alternative was to use vital records as a source of events and then to send interviewers with the multiplicity questionnaire to event households in order to ascertain if the event was correctly reported. While this approach to register evaluation could only measure underreporting by members of event households (together with a measure of response errors in their reports on demographic characteristics), it was tried out in the pretest. It was not included in the final design for reasons specified below.

The second evaluation technique considered, and finally adopted, was a field evaluation based on reports of events in the basic survey. For a sample of events reported in the basic survey, interviews in households of relatives (as reported in the basic survey) were to be carried out by the same procedure and with the same questionnaire as used in the basic survey. This was intended to be, as far as possible, an independent replication rather than an improved procedure. A comparison of responses on the two questionnaires thus obtained would enable measures of differences in reporting of the same event in multiplicity reporting and in demographic data on the event. In order to determine which report was correct in the case of a difference, one of the households was to be the event household so that its LFS household listing could be used for adjudication. It should be mentioned at this stage that reports on events with multiplicity one (i.e., no other household of relatives was qualified to report the event) could not be evaluated by this alternative.

To test the feasibility of these alternative evaluating techniques and their integration with the basic survey procedure, a pretest was carried out in conjunction with the basic survey pretest described in section 2.2.

For the register evaluation, addresses of events from live-birth notifications and from

death notifications were selected for specific months in 1972. There were 24 addresses of Jewish households and 35 addresses of non-Jewish households. For each event, the address was given to the enumerator and a photocopy of the notification retained for comparison. Since the register evaluation pretest was carried out together with the basic survey pretest during May and June 1973, the interviewers could not know which addresses were for the basic survey and which for the evaluation survey.

Interviews were completed for 19 out of the 24 Jewish addresses (1 household not located and 4 absences or refusals) and for 26 out of the 35 non-Jewish addresses (6 households not located and 3 absences). All the non-Jewish households and all but three of the Jewish households reported the event. A comparison of demographic characteristics indicated high discrepancies for age (of mother or of deceased), lower discrepancies for month of event, and no discrepancies for sex.

For the field evaluation pretest, addresses of relatives were selected from network listings for event reports of the basic survey households and of the register evaluation. Interviewing was carried out by the same enumerators and according to the same procedures as those of the basic survey and register evaluation pretests, as a continuing integrated process, in order to minimize the knowledge of the interviewers that a report on an event was expected. Out of 52 Jewish households, only 39 were interviewed (10 not located and 3 absences or refusals), and out of 50 non-Jewish households, 44 were interviewed (2 not located and 4 other reasons for noncompletion). The low completion rate for Jews was primarily a result of poor address reporting, even for addresses judged to be complete at the field evaluation sample selection stage. On the basis of certain decision rules (to be specified fully in the following section) cases of overreporting and underreporting could be identified, and comparisons of demographic data on the events could be made. The numerical results are not significant because of the small size, but the field evaluation pretest did indicate that this approach could serve its purpose if the problem of incomplete address reporting (as described in section 2.2) could be reduced.

Thus, results of the evaluation pretest indicated that both the field evaluation and the register evaluation techniques were feasible for measuring errors. However, the limitations of the register evaluation—its lack of direct connection with the basic survey and the possibility of discovering only underreporting by event households—made it imperative to consider it only as a possible supplement to a field evaluation. Primarily for budgetary considerations, the register evaluation was finally dropped and all effort concentrated on the field evaluation.

3.2 Final Design of the Evaluation Survey

The final design of the evaluation survey was basically that tried out in the field evaluation pretest. It was based on a sample of addresses of relatives reported in the basic survey. Although the interviewing was to be carried out by the same LFS enumerators employed for the basic survey, this design involved a widely scattered sample not derived directly from the LFS sample, as was the basic survey. Taking into consideration the budgetary limitations and especially high costs that this design involved, a maximal sample size of only about 500 households could be considered. Originally it was planned to use half for the evaluation of reports on births and half for the evaluation of reports on marriages. However, practical limitations, primarily due to the small number of marriages with usable addresses, made it necessary to change the distribution to about 200 marriages and about 300 births.

The design ensured that for each pair of reports on the event being compared—one from the basic survey and one from the evaluation survey—one of the reports would be for the event household and one for the household of a relative. This was done to enable use of the information in the LFS household listing of the event household for reconciliation in the case of a discrepancy between the two reports. In order to represent both degrees of relationship (parents and siblings) for each type of event, it was necessary to select four subsamples of addresses for each type of event, as follows:

Report in basic survey by:	*Evaluation by household of:*
1. Event household	Mother's mother/parent of married person
2. Event household	Mother's sister/sibling of married person
3. Mother's mother/parent of married person	Event household
4. Mother's sister/sibling of married person	Event household

The intention was to sample approximately equal numbers of addresses from each of the above groups. This had to be modified somewhat in the execution stage (see table A), because of differential frequencies of addresses available and the necessity to complete the evaluation survey within a short time after the end of the basic survey.

Addresses which were incomplete or in outlying localities were excluded. With these limitations, sampling was basically on a quota basis in the sequence in which questionnaires reached the central office. However, since LFS enumeration weeks are self-representing random samples, the evaluation samples can be considered as approximations of simple random samples within each group. It should be pointed out again that only events reported with multiplicity of more than one were represented in the evaluation survey and that no attempt was made to evaluate

Table A. Execution of the evaluation survey

Event evaluated, where event initially reported, and where event evaluated	Addresses sent to survey division (1)	Addresses sent to field (2)	Questionnaires completed (3)	Completion rate, (3) ÷ (2) (4)
Births				
Total	324	283	234	0.83
Reported in event household; evaluated in household of mother's mother	95	71	60	0.85
Reported in event household; evaluated in household of mother's sister	65	55	45	0.82
Reported in household of mother's mother; evaluated in event household	96	93	78	0.84
Reported in household of mother's sister; evaluated in event household	68	64	51	0.80
Marriages				
Total	208	182	140	0.77
Reported in event household; evaluated in parent's household	57	45	42	0.93
Reported in event household; evaluated in sibling's household	46	34	27	0.79
Reported in parent's household; evaluated in event household	48	48	34	0.71
Reported in sibling's household; evaluated in event household	57	55	37	0.67

nonreports or reports with multiplicity one. Some evaluation of these events could have been done based on a comparison with the LFS listing. However, under certain assumptions (specified in appendix VI) the possibility of estimating the error components without bias is not affected by excluding these events, so they were not evaluated.

The addresses selected were to be transcribed on LFS listing forms and given to LFS enumerators who had participated in the basic survey. They were to complete the LFS listing form and the multiplicity questionnaires (but not the LFS questionnaires) according to the same procedure as in the basic survey.

The evaluation survey questionnaires were to be matched with the relevant basic survey questionnaires and the two reports in each pair—one from an event household and one from the household of a relative—compared. Where necessary, the following criteria for assessing overreporting and underreporting were used in conjunction with the LFS listing form:

1. Events reported in both the basic survey and evaluation survey were to be considered correctly reported true events.
2. The status of events not reported in the evaluation survey was to be adjudicated on the basis of the LFS listing form of the event household (mother's household for births; married person's household for marriages).
   (a) If the event was not listed in the event household or was listed for a different year, it was to be treated as a nonevent, overreported by the basic survey household.
   (b) If the event was listed in the event household as occurring in the correct year (even if marked as deleted at a later visit, due to death or divorce), it was to be treated as a true event, underreported by the evaluation survey household.

This adjudication rule implicitly assumes independence between event reporting and the LFS listing, which is not always obtained in practice. The possibility of dependence and its effects on the evaluation results are discussed in appendix VII.

Comparisons of the two reports on the multiplicity network and on demographic data for the same event could be made at the same time.

On the basis of a theoretical model, under certain simplifying assumptions, basic evaluation parameters (such as overreporting and underreporting rates for different relatives and multiplicity reporting response variance) could be estimated. The parameters could be synthesized—together with measures from the basic survey—to provide, under the assumptions of the model, estimates of the total mean square error and its components (e.g., sampling variance, response variance, and reporting bias) for each of the three counting rules considered. Full details on the model used and the methods of estimation are given in Nathan (1976), and a nontechnical description of the model is given in appendix VI.

3.3 Execution of the Evaluation Survey

Selection of the sample events for the evaluation survey began during the first week of the basic survey fieldwork, at the end of January 1974. Multiplicity questionnaires from the basic survey arrived at the center in Jerusalem together with their respective LFS questionnaires. Both questionnaires had already undergone an initial edit in one of the three regional field offices. The multiplicity questionnaires were scanned, and all events reported were listed on a special form intended to serve as the framework for the selection of events for the evaluation survey sample. Separate forms were used for births and marriages. In addition to the questionnaire serial number, the form provided for the listing of the number of usable addresses (complete and not in outlying localities) for each type of report (i.e., report by the event household, by the household of a parent, or by the household of a sibling).

Since most LFS questionnaires reached the center within 6 or 8 days of the date of interview, it was possible to obtain almost a full set of the completed questionnaires taken during a given enumeration week within the week there-

after. The listing procedure described above was followed for 5 of the 8 weeks during which the survey was in the field. The listed events were then scanned and the evaluation survey sample selected from them according to the design set out in section 3.2, separately for births and for marriages for each of the four report groups. The actual distribution of addresses by type of event and report group is shown in column (1) of table A.

The addresses selected from the special listing form were sent to the Survey Division of the Bureau for processing. There were pressures imposed by the need for speed, the general conditions still pertaining with respect to field staff (section 2.4), and the requirement that the evaluation survey be carried out by the regular LFS enumerators together with their regular LFS work. Consequently, only 87 percent of the selected addresses could be sent to the field. For the 465 addresses finally sent to the field, 374 completed questionnaires were received (80 percent), slightly more for births, and slightly less for marriages (see columns (3) and (4) of table A).

As before, the relatively low completion rates are primarily attributable to the Yom Kippur war. Evaluation survey interviewing was carried out during April and May 1974, when the effects of the war and its aftermath were still felt. Even though many men had been demobilized by that time, some families still had not returned to their own homes or had left the addresses at which they had been listed in the basic survey. Although in many instances the interviewers were able to locate them, this movement tended to lower the efficiency of the interviewing process. In addition, the turnover of enumerators was still acute, particularly in Jerusalem, where none of the evaluation survey enumerators had had experience with the basic survey.

The design of the evaluation survey imposed an extra burden on the enumerators. In the basic survey, the survey household was reached via the regular sampling procedure of the LFS, in which enumeration assignments are clustered geographically. However, in the evaluation survey, the enumerator was given a set of scattered addresses in addition to his regular LFS assignment. While this problem was recognized by the supervisors, shortage of personnel limited the amount they could adjust the workloads.

From table A it is seen that for births there is little variation in completion rates for different types of reports (all close to the average level of 0.83). However, for marriages considerable variation exists, although it must be pointed out that small numbers are involved. Completion rates were lowest (0.67-0.71) when the initial report was from the household of a parent or sibling of a newly married person and the evaluation was in the household of the newly married couple. This is probably because newly married couples tend to be highly mobile and their addresses may not yet be well known to relatives. In addition, newly married men are often young and were likely to have been kept in the army a long time, so their wives might still have been living with relatives at the time of the evaluation survey. On the other hand, the higher completion rate when evaluation was done in the household of parents of newly married persons (0.93), is probably due to the fact that members of the older generation tend to be less mobile and their addresses are perhaps better known than the addresses of siblings are. It should, however, be kept in mind with respect to the completion rates that the household members listed in the basic survey as reported persons (i.e., persons about whom questions were asked on events to themselves or to their relatives) were not necessarily the actual respondents (i.e., persons who reported on the addresses of network members). This is so since the usual LFS respondent rules were followed, by which the respondent could be any household member aged 14 and over.

In table B frequencies are presented of the reasons given by enumerators for noncompletion. For both births and marriages, almost two-thirds of the households for which questionnaires were not filled out were not located (lines 1 and 2 of table B). More than half of the total noncompletion was due to inability to find an address because of inadequate information furnished in basic survey households. Temporary absence was the reason for about a quarter of the total noncompletion, indicating the high mobility of households already described.

As indicated earlier, the evaluation survey was intended to be an independent replication

Table B. Reasons for noncompletion in the evaluation survey, by type of event

Reason for noncompletion	Total	Births	Marriages
All reasons-	91	49	42
1. Address not found-------	49	30	19
2. Household has moved---	8	3	5
3. Absent temporarily-	23	11	12
4. Refused------	9	4	5
5. Interviewer error-------	2	1	1

of the basic survey in the sense that independent reports on the same event were to be obtained. However, some of the enumerators may have been aware that an event had been reported in the basic survey for the network to which an evaluation household belonged. Possibly, some of them assumed that an event had in fact occurred, and probed or settled doubtful cases with that in mind. Although supervisors attempted to limit this effect during the fieldwork, it is not clear to what extent estimates of response errors were affected.

After preliminary editing in the field offices, the evaluation survey questionnaires were returned to the center, where the four basic documents for each event—two multiplicity questionnaires and two LFS household listing forms—were brought together. Data from these sources were transferred to a single Evaluation Survey Processing Form (see appendix V) to enable easy comparison of the data from the two surveys. On the basis of the decision rules, detailed in the previous section, the event reporting status was coded: correctly reported in both surveys; or overreported or underreported in one of the surveys (and correctly reported in the other). It should be pointed out that most of the overreporting discovered was due to reports on events which occurred outside the reference year (1973). Further editing and a few corrections and imputations were carried out at this stage. Final processing of the evaluation survey was manual, by means of the Evaluation Survey Processing Form, on the basis of the model and methods described by Nathan (1976) and, in nontechnical form, in appendix VI.

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Chapter 4. RESULTS

4.1 Basic Results on Yields and Network Sizes

The basic survey results are presented in tables C and D. Table C gives the distributions of raw yields (number of events reported per household) and of weighted yields (sum of event reports weighted by the reciprocal of their multiplicity) for each of the three counting rules considered. The counting rules are: the conventional rule (events occurring to members of in-

Table C. Raw yields (number of events per household) and weighted yields (number of event reports weighted by reciprocal of multiplicity), by type of event and counting rule

Type of yield	Counting rule					
	Conventional[1] (1)	Restricted multiplicity[2] (2)	Full multiplicity[3] (3)	Conventional[1] (1)	Restricted multiplicity[2] (2)	Full multiplicity[3] (3)
Raw yield	Births			Marriages		
Total	4,084	4,084	4,084	4,084	4,084	4,084
0	3,758	3,521	3,318	3,963	3,785	3,581
1	324	534	657	4	177	364
2	2	27	97	116	119	117
3	-	2	12	-	2	20
4	-	-	-	1	1	2
Weighted yield						
Total	4,084	4,084	4,084	4,084	4,084	4,084
0	3,758	3,521	3,318	3,963	3,785	3,581
0.01-0.19	-	-	52	-	-	78
0.20-0.29	-	-	164	-	-	103
0.30-0.39	-	-	193	-	11	100
0.40-0.49	-	-	19	-	-	6
0.50-0.59	-	459	250	-	159	107
0.60-0.69	-	-	29	-	-	12
0.70-0.79	-	-	8	-	-	13
0.80-0.89	-	-	1	-	2	22
0.90-0.99	-	-	-	-	-	4
1.00-1.49	324	93	48	4	60	39
1.50-1.99	-	8	1	-	55	14
2.00-2.99	2	3	1	116	11	5
3.00+	-	-	-	1	1	-

[1]Events occurring to members of interviewed households only.
[2]Events occurring to members of interviewed households and to their offspring.
[3]Events occurring to members of interviewed households, to their offspring, and to their siblings.

terviewed households only, each event having a weight of 1); the restricted multiplicity rule (events occurring to members of interviewed households and to their offspring); and the full multiplicity rule (events occurring to members of interviewed households, to their offspring, and to their siblings). Multiple reports of births by event households are seen to be negligible, whereas, of course, practically all reports by event households on persons marrying are double. The increase in report yields for wider counting rules is primarily in single reports, both for births and for marriages. Besides the obvious concentration at zero, weighted yields are concentrated at values of 1.0 and 0.5 for the restricted multiplicity rule, corresponding to event persons with no parent (or mother) outside the household and to event persons with parents (or mother) in another household, respectively. The weighted yields are dispersed over a wide range of intermediate values for the full multiplicity rule, with concentrations at values 1/5, 1/3, 1/2, 2/3, and 1. Weighted yields greater than 1 are obtained only if more than one event of the same type is reported by the same household. For instance, a birth to a woman in the household without mother or sisters in the survey population (multiplicity 1) and a birth to her

Table P. Events by number of households in multiplicity network, harmonic mean of multiplicities, and number of reporting households, by multiplicity counting rule, type of event, and type of report

Multiplicity counting rule, type of event, and type of report	Number of households in network											Harmonic mean of multiplicities	Number of reporting households
	Total	1	2	3	4	5	6	7	8	9	10		
FULL MULTIPLICITY													
Births	Percent												
Total	100.0	5.0	26.8	27.6	21.4	9.5	5.3	3.3	1.0	-	0.1	2.75	887
By event household	100.0	12.5	32.3	28.4	15.9	7.0	2.7	0.9	0.3	-	-	2.27	328
By mother	100.0	(1.1)	43.6	25.6	17.3	6.0	4.9	0.4	0.8	-	0.4	2.63	266
By sisters	100.0	-	5.5	28.7	31.4	15.4	8.5	8.5	2.0	-	-	3.83	293
Marriages													
Total	100.0	6.2	26.3	24.5	15.3	9.9	5.6	5.9	3.2	1.4	2.0	2.79	666
By event household	100.0	15.4	33.7	22.5	12.5	7.1	3.7	2.1	2.1	0.8	-	2.19	240
By parent	100.0	(2.2)	43.2	27.6	12.4	3.8	4.3	2.2	2.2	-	2.2	2.61	185
By siblings	100.0	-	5.8	24.1	20.3	17.4	8.3	12.4	5.0	2.9	3.7	4.17	241
RESTRICTED MULTIPLICITY													
Births													
Total	100.0	14.6	85.4	-	...	...	...	...	...	...	...	1.75	594
By event household	100.0	23.8	76.2	-	...	...	...	...	...	...	...	1.62	328
By mother	100.0	(3.4)	96.6	-	...	...	...	...	...	...	...	1.93	266
Marriages													
Total	100.0	20.2	76.5	3.3	...	...	...	...	...	...	...	1.68	425
By event household	100.0	33.3	65.4	1.2	...	...	...	...	...	...	...	1.51	240
By parent	100.0	(3.2)	90.8	5.9	...	...	...	...	...	...	...	1.98	185

NOTE: Numbers enclosed in parentheses are errors. According to counting rules, cell should be empty.

daughter who lives elsewhere but does not have sisters living elsewhere (multiplicity 2) would give a total weighted yield of 1.5. The larger decrease in sampling variance due to multiplicity for births than for marriages can already be inferred from the differences between the distributions of weighted yields (primarily because of the concentration of weighted yields for the conventional rule at 2 for marriages as against the concentration at 1 for births).

Table D gives the distribution of network sizes by type of report for the two multiplicity rules and the harmonic mean of the multiplicities. The harmonic mean is the average contribution of a reporting household to the sample total of weighted events. Obviously the network sizes for reports from households of parents and siblings are higher than those reported directly by the interview households, since their network sizes should be at least 2. However, eliminating the first two categories, the conditional distributions for network sizes of 3 or more are not too different.

4.2 Components of Error and Comparisons With Demographic Data

The fundamental results of the study, estimates of total events and their error components compared according to counting rules, are presented in table E. The current demographic data—line (1)—are based on the virtually complete vital registration for the year 1973, with approximate adjustments for the survey population (i.e., excluding the official figures for kibbutzim and institutions[b]). The basic survey estimates—line (3)—were obtained by summing the weighted yields of event reports (the distribution of which is given in table C) and multiplying by a uniform sample inflation factor. This inflation factor is the ratio of the estimated survey population (from current demographic data) to the net sample size. Differential sample inflation by characteristics as carried out in the LFS could not be applied.

The other results of table E were estimated according to the model described fully in Nathan (1976) and in nontechnical form in appendix VI, on the basis of both the basic survey and the evaluation survey. Thus the estimates of net bias—line (4)—were obtained from the evaluation survey as the difference between estimates of the undercoverage and overcoverage biases, based on estimates of the underreporting and overreporting rates for reports by the various types of relatives. They are not based on the difference between survey estimates and current demographic data. The revised survey estimate—line (2)—was then obtained from the basic survey estimate—line (3)—corrected by subtracting from it the estimated net bias—line (4). The estimates of the total mean square error and its components—the squared bias, the sampling variance, and the response variance—were obtained, according to the model, from results of the basic and the evaluation surveys. Next, relative standard errors are given as percentages of the revised survey estimate. Current demographic data for vital rates and basic survey estimates of vital rates (95-percent confidence intervals) are given per 1,000 population. Also shown are the percent differences between the survey estimates and the demographic data (relative to the latter).

In studying the results of table E, it must be emphasized that they are based on a model with various simplifying assumptions and that the results are estimated on small samples with substantial noncompletion rates. In addition to noncompletion in the basic survey (about 20 percent), it must be remembered that the evaluation survey sample was selected only from households of relatives whose addresses were well reported and that outlying areas were excluded. Furthermore, a completion rate of less than 80 percent was obtained for the evaluation survey. However the methods of estimation (basically ratio estimation) take nonresponse into account, so at least the methodological purpose of the survey—comparisions between counting rules—should not be affected considerably by these limitations.

A first glance at table E shows considerable differences between the current demographic

[b]In fact, only events in institutional localities (i.e., rural institutions which are separate localities) could be excluded. However, the number of events in other institutions is thought to be rather small.

Table E. Estimates of components of error, by counting rule, and current demographic data

Current demographic data and estimate	Counting rule					
	Conventional	Restricted multiplicity	Full multiplicity	Conventional	Restricted multiplicity	Full multiplicity
	Births			Marriages (persons marrying)		
(1) Current demographic data	64,200	64,200	64,200	49,700	49,700	49,700
(2) Revised survey estimate	59,970	60,526	58,016	45,130	42,781	42,518
(3) Basic survey estimate	60,018	62,305	58,952	43,916	46,325	43,586
(4) Estimate of net bias	+48	+1,780	+935	-1,214	+3,544	+1,068
Undercoverage bias	-1,223	-2,857	-4,766	-1,214	-1,437	-3,618
Overcoverage bias	+1,271	+4,637	+5,701	0	+4,980	+4,687
Components of mean square error ($\times 10^{-3}$)						
Total mean square error	10,131	10,106	6,274	8,997	18,365	5,485
Squared bias	2	3,168	874	1,474	12,557	1,141
Total variance	10,129	6,938	5,400	7,523	5,808	4,344
Sampling variance	9,825	5,900	3,815	7,307	4,969	3,154
Response variance	303	1,038	1,584	216	839	1,190
Relative standard error (percent)						
Total root mean square error	5.31	5.25	4.32	6.65	10.02	5.51
Bias (absolute value)	0.08	2.94	1.61	2.69	8.28	2.51
Sampling standard error	5.23	4.01	3.37	5.99	5.21	4.18
Response standard error	0.92	1.68	2.17	1.03	2.14	2.57
Vital rate (per 1,000 population)	Births			Marriages (couples)		
Current demographic data	23.9	23.9	23.9	9.2	9.2	9.2
Basic survey estimate (95-percent confidence interval)	22.4±2.3	23.2±2.3	22.0±1.8	8.2±1.1	8.6±1.6	8.1±0.9
Percent difference of basic survey estimate from current demographic data[1]	-6.5	-2.9	-8.1	-11.6	-6.7	-12.2

[1]Relates to rates and to absolute numbers, since the same denominator was used for both the current demographic rates and the survey rates.

data and the survey estimates. Both the basic survey estimate and the revised survey estimate are systematically far below the demographic data for all three counting rules, both for births and for marriages. This is not surprising in view of the fact that the demographic data are based on virtually complete vital registration, whereas the survey estimates are based on a retrospective sample survey relying on recall. However, the differences between the revised survey estimates and the demographic data are covered by 2 standard errors for the conventional and restricted multiplicity rules, and they only barely exceed 2 standard errors for the full multiplicity rule.

It should be borne in mind that the estimates for the different counting rules are not independent. Thus the differences between the demographic data and survey estimates may be due to a systematic sampling bias because of noncompletion both in the basic survey and in the evaluation survey. They may be due to a systematic counting rule bias, for instance, one caused by events for which all possible reporters have died, emigrated, or are otherwise outside the survey population. Another possibility is that they were caused by biases in the estimation of coverage errors due to lack of independence between the LFS listing and event reporting. (See details in appendix VII.) None of these biases were taken into account in the model. However, comparisons by demographic characteristics of the survey distributions with those of the national demographic data (see section 4.3) do not indicate any striking differences, and it is unlikely that either of the two types of bias could be considerable. Furthermore, the fact that the differences are similar for the different counting rules (though slightly larger for the full multiplicity rule) indicates that the biases not considered in the model should not affect the comparisons of overall errors between counting rules very much.

An examination of the total mean square errors shows that the overall errors for the full mutliplicity rule are considerably lower than those for the conventional rule, both for births and for marriages (for the sample size of the survey—4,084 households). However, for the restricted multiplicity rule, a considerably larger mean square error than for the conventional rule is obtained for marriages (due to an extremely high bias, over 8 percent, for this counting rule), while for births it is close to that of the conventional rule. The relative root mean square errors for births (4.3-5.3 percent) are lower than the errors for marriages (5.5-10.0 percent), primarily because of lower biases.

The difference between the results for births and for marriages may be due to differences in questionnaire design for the two topics. The report for marriages in the household (Q. 13) was made on the basis of the LFS listing form. Therefore, respondents' overreporting and underreporting of marriages in event households could not be measured by applying the adjudication rules, and only the component due to enumerator error could be evaluated. Overreporting would have been found if an enumerator had recorded in Q. 13 a marriage not listed (or listed for another year) in the LFS form, no cases of which were found. Underreporting was found when the enumerator failed to record in Q. 13 a marriage which was listed in the LFS form. Another reason for the differences between the results for births and for marriages might be differential nonresponse for event households, since recently married couples without children are less likely to be at home than households with a recent birth.

The composition of the mean square error differs considerably between counting rules, especially with respect to the bias and variance components. The variance (sampling and response) accounts for practically all the mean square error for births with the conventional rule, but accounts for only about 30 percent for marriages with the restricted multiplicity rule. Hence, each of the components must be considered separately. In addition, this implies that the above-mentioned relationships between the overall errors for the different counting rules hold only for the sample size of this survey and may differ considerably for other sample sizes, since sample size affects only the variance component.

As could be expected, the net biases are generally larger for the multiplicity counting rules than for the conventional rule. For births it is close to zero for the conventional rule and goes up to +2.9 percent for the restricted multiplicity

rule. For marriages it ranges from -2.7 percent for the conventional rule up to +8.3 percent for the restricted multiplicity rule. However, in absolute value the net bias for the full multiplicity rule for marriages is somewhat less than that for the conventional rule. The differences are explained by the analysis of the biases into their components shown in table F.

The first three lines of this table give the estimates of overcoverage, undercoverage, and net relative biases separately for each degree of reporting relative, as obtained from the evaluation survey. It should be emphasized again that, since reporting was hierarchical, the biases relate to event reports by the specified relatives only if they did not reside in households of closer relatives (e.g., by sisters not living with their mother or with the baby's mother). The averages of these biases (weighted by the contribution of reports for each type of relative to the total revised estimate) give, for each counting rule, the total event-reporting biases in the lower part of the table. For the conventional rule these are simply the biases of reports by event households; for the restricted multiplicity rule they are weighted averages of the biases of reports by event house-

Table F. Analysis of relative biases by household of reporting relative and counting rule

Reporting relative and counting rule	Births			Marriages		
	Undercoverage bias	Overcoverage bias	Net bias	Undercoverage bias	Overcoverage bias	Net bias
REPORTING RELATIVE	Percent					
Event household	-2.04	+2.12	+0.08	-2.69	0	-2.69
Household of parent	-10.34	+16.20	+5.86	-9.76	+36.96	+27.20
Household of sibling	-20.51	+15.73	-4.78	-21.74	+7.41	-14.33
COUNTING RULE						
Conventional						
Total bias	-2.04	+2.12	+0.08	-2.69	0	-2.69
Restricted multiplicity						
Total bias	-4.72	+7.66	+2.94	-3.36	+11.64	+8.28
Total event reporting bias	-5.28	+7.62	+2.34	-4.88	+11.46	+6.58
Contribution of multiplicity reporting variance	+0.56	+0.04	+0.60	+1.52	+0.18	+1.70
Full multiplicity						
Total bias	-8.22	+9.83	+1.61	-8.51	+11.02	+2.51
Total event reporting bias	-9.14	+9.73	+0.59	-9.81	+10.86	+1.05
Contribution of multiplicity reporting variance	+0.92	+0.10	+1.02	+1.30	+0.16	+1.46

holds and by households of parents; while for the full multiplicity rule, biases of reports from all three types of relatives are averaged. Finally, for the two multiplicity counting rules, estimates of the contributions to the biases from the response variance of multiplicity reporting have to be added to obtain the total bias. Note that this variance is a contribution to the bias due to the fact that the estimate is based on averages of ratios, and it is not to be confused with the response variance of the estimate itself.

The analysis of table F shows that undercoverage biases increase consistently with distance of relationship, similarly for births and marriages, from 2.0-2.7 percent for event households, to 9.8-10.3 percent for parents' households, to 20.5-21.7 percent for siblings' households. This explains the consistent increase in total event-reporting undercoverage for wider counting rules: 2.0-2.7 percent for conventional, 4.9-5.3 percent for the restricted multiplicity rule, and 9.1-9.8 percent for the full multiplicity rule. However, the contribution of the multiplicity reporting variance, which is positive for multiplicity counting rules, reduces these differences somewhat. Thus the total undercoverage bias is reduced for the full multiplicity rule to 8.2-8.5 percent and for the restricted multiplicity rule to 4.7 percent for births and 3.4 percent for marriages.

The overcoverage biases also vary considerably by reporting relative. Whereas overreporting is minimal for event households (2.1 percent for births and zero for marriages), it is very high for relatives' households and higher for those of parents than those of siblings. Overreporting by parents' households reaches an extremely high 37.0 percent for marriages (mostly marriages occurring in 1972 and 1974) and 16.2 percent for births. The resulting total overcoverage biases, which are only slightly increased by the contribution of the multiplicity reporting variance, are thus high for both the multiplicity rules (7.7-11.6 percent), and they offset the undercoverage biases to give total positive net biases. The overcoverage bias for the conventional rule is similar to the undercoverage bias for births (giving a net bias close to zero) and is zero for marriages (giving a negative net bias).

The increase in bias resulting from the use of multiplicity counting rules is offset by the decrease in variance. (See table E.) The decrease from the variance of the conventional estimate is greater for births (about 50 percent for the full multiplicity rule and about 30 percent for the restricted rule) than for marriages (about 40 percent and 20 percent, respectively).

This decrease is due to the decrease in sampling variance, which is the major component in the variance. The relative sampling standard errors for the conventional rule—5.2 percent for births and 6.0 percent for marriages—are reduced to 4.0 percent and 5.2 percent, respectively, for the restricted multiplicity rule, and to 3.4 percent and 4.2 percent, respectively, for the full multiplicity rule.

The response variance, however, increases with wider counting rules, from a relative response standard error of 0.9-1.0 percent for the conventional estimate, to 1.7-2.1 percent for the restricted rule, and to 2.2-2.6 percent for the full multiplicity rule. The increase in response variance is also reflected in the increase in the gross bias (the absolute sums of the overcoverage and undercoverage biases). However, this may be due to the fact that the variance of the estimates of bias increases as a function of the response variance.

Because of the different relative weights of the bias and variance components for different counting rules, the relative efficiency of the multiplicity estimates (relative to the conventional estimate) decreases with increasing sample size. The increases in efficiency of multiplicity estimates as sample size decreases imply improved multiplicity estimates for small subgroups. Certain indications of this can be seen in the comparisons of distributions by characteristics in the next section. Some calculations of the relative efficiency as a function of sample size are shown in table G. They indicate that for marriages the full multiplicity rule estimates are always more efficient than the conventional rule estimates because both bias and variance are smaller for the full multiplicity rule. For births the full multiplicity rule estimates are more efficient for samples up to a population size of about 19,000 (relative error of 2.5 percent). Because of its higher variance and bias, the restricted rule is less efficient than the full multiplicity rule for

Table G. Relative efficiencies (compared to conventional survey), by sample size and counting rule

Sample size	Counting rule			
	Restricted multiplicity	Full multiplicity	Restricted multiplicity	Full multiplicity
	Births		Marriages	
500	119	131	97	124
1,000	116	130	89	123
2,500	108	126	75	122
5,000	98	121	63	120
7,500	90	117	57	119
10,000	84	112	52	118
15,000	75	105	47	116
20,000	68	98	44	115

all sample sizes. Due to the high bias of the restricted multiplicity rule, it is also less efficient than the conventional rule for marriages for all practical sample sizes and for births for all sample sizes greater than approximately 4,400 (relative error of 5.1 percent).

Thus the limitations of the model and the estimates notwithstanding, there are clear indications that in a situation like this survey situation in the ranges of sample sizes generally considered, the full multiplicity rule offers definite superiority for the same size sample over the conventional rule. Cost considerations may reduce this superiority somewhat. The restricted multiplicity rule (conventional plus parents) is, however, counterindicated, primarily because of high overcoverage bias due to parents (especially for marriages). It might be worthwhile to consider a different restricted counting rule based on conventional plus siblings (without parents); the structure of the present survey did not, however, allow this counting rule to be evaluated fully. Further remarks on this are given in section 5.1.

4.3 Distribution by Demographic Characteristics

As mentioned in the introduction, the multiplicity technique permits not only estimation of the total number of events but also estimation of their distribution by characteristics on which data have been collected for each event report. The data obtained for each event, both in the basic survey and in the evaluation survey, were age (of baby's mother or of married person), sex (of child or of married person), month of occurrence, and district in which the event occurred.

Estimated distributions by these characteristics of events reported in the basic survey are presented for each of the three counting rules and compared with official statistics in tables H-L. In table H age-specific rates are also compared. The sources of the official statistics are shown at the foot of each table. In table H, the columns headed "Vital records" show special computations of age-specific rates of events which occurred in a population closely resembling the survey population; events in kibbutzim and rural institutions have been removed, and these populations have also been removed from the base figures on which the rates are computed. However, it was not possible to exclude urban institutions. In tables J-L, the rates shown in the vital records columns refer to the total Jewish population in Israel.

The limitations relating to the total estimates, discussed in section 4.2, are obviously still stronger in relation to distributions. Thus all distributions are given as percentages only, and the rough estimates of relative standard errors are, in these instances, only the regular sample

Table H. Percent distribution of events, specific rates per 1,000 population, and estimated relative standard errors, by counting rule, type of event, and age of event person

Type of event and age of event person	Vital records[1]	Counting rule			Vital records[1]	Counting rule			Counting rule		
		Conventional	Restricted multiplicity	Full multiplicity		Conventional	Restricted multiplicity	Full multiplicity	Conventional	Restricted multiplicity	Full multiplicity
BIRTHS	Percent distribution				Specific rate per 1,000 population				Estimated relative standard error		
All ages---	100.0	100.0	100.0	100.0	98.1	91.7	94.4	88.1	0.05	0.05	0.04
15-19 years------	5.9	6.7	7.1	5.9	30.5	32.7	36.4	30.3	0.21	0.15	0.13
20-24 years------	37.6	31.7	36.3	37.4	184.5	145.7	171.6	163.9	0.10	0.07	0.07
25-29 years------	31.9	34.8	33.4	34.4	190.6	194.3	191.8	183.4	0.09	0.07	0.07
30-34 years------	14.9	15.2	12.8	13.5	133.6	128.2	110.3	108.5	0.15	0.13	0.11
35 years and over----------	9.8	11.6	10.4	8.9	28.3	31.3	28.9	22.8	0.16	0.15	0.13
MARRIAGES											
Male											
All ages---	100.0	100.0	100.0	100.0	39.0	33.3	35.1	31.8	0.09	0.07	0.06
15-19 years------	3.3	6.0	6.9	7.1	6.5	10.0	12.5	10.0	0.38	0.27	0.26
20-24 years------	52.2	58.1	55.4	55.0	98.2	93.4	93.6	85.2	0.12	0.10	0.09
25-29 years------	29.2	26.5	29.7	29.4	66.2	51.3	60.4	55.1	0.18	0.13	0.12
30 years and over----------	15.3	9.4	8.0	8.5	14.1	7.4	6.6	6.5	0.30	0.26	0.23
Female											
All ages---	100.0	100.0	100.0	100.0	38.5	33.6	35.8	34.4	0.09	0.07	0.06
15-19 years------	26.3	26.9	29.5	27.7	53.8	49.0	57.3	50.9	0.18	0.13	0.12
20-24 years------	53.3	52.9	55.5	57.0	102.7	88.3	98.5	98.2	0.13	0.10	0.09
25-29 years------	12.2	15.1	12.6	11.8	28.5	30.7	27.3	24.9	0.24	0.21	0.18
30 years and over----------	8.2	5.0	2.4	3.5	7.1	3.7	1.9	2.7	0.41	0.41	0.27

[1]SOURCE: For births—Central Bureau of Statistics, Vital Statistics, 1973-1974 (in press). For marriages—unpublished tabulations of the Central Bureau of Statistics. Survey population for both is total Jewish population less kibbutzim and institutional localities.

Table J. Percent distribution of events and estimated relative standard errors, by type of event, counting rule, and sex of event person.

Sex of event person	Births				Marriages			
	Vital records[1]	Counting rule			Vital records[1]	Counting rule		
		Conventional	Restricted multiplicity	Full multiplicity		Conventional	Restricted multiplicity	Full multiplicity
	Percent distribution of events							
Male----------	51.2	54.6	54.0	54.6	50.0	49.6	49.3	47.8
Female----------	48.8	45.4	46.0	45.4	50.0	50.4	50.7	52.2
	Estimated relative standard error							
Male----------	...	0.08	0.06	0.05	...	0.09	0.07	0.06
Female----------	...	0.08	0.06	0.06	...	0.09	0.07	0.06

[1]SOURCE: Central Bureau of Statistics, Statistical Abstract of Israel, 1974, pp. 75 and 59. Based on total Jewish population in Israel in 1973.

estimates of total variance (between the weighted contributions of households, including zeroes), divided by the total sample size. They are not comparable to the estimates of relative standard errors given in table E, which are based on the more sophisticated model. Because of the heterogeneous nature of the population and the small numbers involved, the empirical material cannot fully explain the differences found nor could reasons be given for the differences.

From table H it can be seen that most of the percentages by age and age-specific rates for all three counting rules do not differ from the official statistics by more than 2 standard errors. For births, significant differences are found in ages 20-24 for the conventional rule. This is, of course, the modal age group for births, and it is curious that the discrepancies should be concentrated here rather than at the extremes. For marriages, however, the significant differences are

Table K. Percent distribution of events and estimated relative standard errors, by type of event, counting rule, and month of event

Month of event	Births				Marriages			
	Vital records[1]	Counting rule			Vital records[1]	Counting rule		
		Conventional	Restricted multiplicity	Full multiplicity		Conventional	Restricted multiplicity	Full multiplicity
	Percent distribution of events							
January	7.8	5.8	6.2	6.5	6.8	8.4	7.8	6.6
February	6.7	4.6	4.4	5.9	6.6	6.6	5.6	6.4
March	7.8	10.4	9.6	9.5	9.7	7.5	8.2	8.8
April	7.6	7.6	7.4	7.0	7.2	11.5	9.4	8.3
May	8.0	11.3	10.1	9.2	5.0	7.5	6.8	6.0
June	8.2	9.2	7.4	7.2	9.9	9.7	10.1	7.5
July	8.6	8.6	8.9	8.7	9.4	5.3	7.0	8.2
August	9.6	10.1	10.2	9.5	14.0	15.0	12.0	13.1
September	9.2	6.7	7.4	8.5	12.0	10.6	12.9	13.4
October	9.2	10.4	10.7	10.4	4.8	5.3	6.0	6.9
November	8.6	6.7	7.9	7.4	6.8	6.6	8.1	7.0
December	8.9	8.6	9.8	10.3	7.8	5.8	6.0	7.7
	Estimated relative standard error							
January	...	0.23	0.17	0.15	...	0.32	0.24	0.21
February	...	0.26	0.19	0.15	...	0.36	0.25	0.22
March	...	0.17	0.14	0.12	...	0.34	0.23	0.18
April	...	0.20	0.16	0.14	...	0.28	0.22	0.21
May	...	0.17	0.13	0.13	...	0.34	0.27	0.26
June	...	0.18	0.15	0.14	...	0.33	0.24	0.19
July	...	0.19	0.15	0.13	...	0.41	0.20	0.15
August	...	0.17	0.14	0.12	...	0.24	0.20	0.16
September	...	0.21	0.16	0.13	...	0.28	0.18	0.15
October	...	0.17	0.14	0.12	...	0.41	0.26	0.21
November	...	0.22	0.17	0.16	...	0.36	0.22	0.19
December	...	0.19	0.14	0.12	...	0.38	0.26	0.17

[1] SOURCE: Central Bureau of Statistics, Monthly Bulletin of Statistics, March 1975, pp. 6 and 7. Based on total Jewish population in Israel.

found precisely in the age groups of lowest frequency (15-19 for males and 30 and over for females). Thus, notwithstanding the large gross errors in estimates of totals, overall the net differences are small. It should be noted that for births the distribution of the full multiplicity rule is closest to that of the official statistics; for marriages the situation is less clear.

From table J it is seen that the distributions of event persons by sex according to all counting rules are close to those of the official statistics. From table K it appears that the results of the survey are fairly close to the official statistics with respect to month of occurrence for all counting rules. However, it should be noted that the sharp decline in the number of newly married persons in October is not fully reflected in the survey distributions. But again, the differences are not large enough to cast doubt on the validity of the survey. The full counting rule provides a closer approximation than the other two rules, both for births and for marriages.

From table L it appears that all reporting rules show a smaller proportion of events in Jerusalem and the south than do the official statistics. Although it must be kept in mind that the district for which the event is reported is not necessarily the district in which the basic survey interview was taken, it seems likely that the low proportion reported for Jerusalem and the south reflects the special difficulties which existed in the fieldwork of the Jerusalem regional office in early 1974. (See section 2.4.) The possibility of appropriate weighting to take into account differential noncompletion (as carried out in the LFS) may have to be considered in the future in the light of these results.

In table M an attempt is made to examine

Table L. Percent distribution of events and estimated relative standard errors, by type of event, counting rule, and district of occurrence

District of occurrence	Births				Marriages			
	Vital records[1]	Counting rule			Vital records[2]	Counting rule		
		Conventional	Restricted multiplicity	Full multiplicity		Conventional	Restricted multiplicity	Full multiplicity
	Percent distribution of events							
Jerusalem and south	26.0	22.3	20.6	20.1	19.3	12.8	15.8	14.9
Haifa and north	22.8	24.4	24.5	24.6	22.9	31.0	24.6	24.8
Tel Aviv and center	51.2	53.4	54.9	55.3	57.8	56.2	59.6	60.3
	Estimated relative standard error							
Jerusalem and south	...	0.12	0.10	0.09	...	0.26	0.17	0.13
Haifa and north	...	0.11	0.09	0.08	...	0.17	0.13	0.12
Tel Aviv and center	...	0.08	0.06	0.05	...	0.13	0.09	0.07

[1]SOURCE: Central Bureau of Statistics, Statistical Abstract of Israel, 1974, p. 62. Based on total Jewish population in Israel.

[2]SOURCE: Unpublished tabulations of the Central Bureau of Statistics. Based on total Jewish population in Israel.

Table M. Underreporting and overreporting by relative's household, by type of event and characteristics of event and of head of event household

Event and characteristics of event and of head of event household	Underreporting by household of:						Overreporting by household of:					
	Mother's mother/ parent			Mother's sister/ sibling			Mother's mother/ parent			Mother's sister/ sibling		
	Total	Under-reported	Rate	Total	Under-reported	Rate	Total	Over-reported	Rate	Total	Over-reported	Rate
BIRTHS												
Total[1]	58	6	0.10	39	8	0.21	76	14	0.18	52	9	0.17
Month of event												
January-February, November-December	16	2	0.12	13	2	0.15	21	10	0.48	15	4	0.27
March-October	42	4	0.10	26	6	0.23	54	3	0.06	34	4	0.11
Age of mother												
15-29 years	47	6	0.13	27	6	0.22	53	11	0.17	29	6	0.21
30 years and over	11	-	-	12	2	0.17	12	2	0.17	22	3	0.13
Type of locality												
3 large cities	12	-	-	10	3	0.30	19	5	0.26	13	3	0.23
Other	46	6	0.13	29	5	0.17	57	9	0.16	39	6	0.15
District												
Tel Aviv and Central	33	5	0.15	23	3	0.13	48	7	0.15	36	5	0.14
Other	25	1	0.04	16	5	0.31	28	7	0.25	16	4	0.25
Years of school of household head												
0-8 years	18	3	0.17	16	5	0.31	46	8	0.17	23	4	0.17
9-11 years	21	1	0.05	11	1	0.09	12	2	0.17	13	2	0.15
12 years or more	18	1	0.06	12	2	0.17	15	4	0.26	15	2	0.13
Continent of birth of household head[2]												
Asia-Africa	34	5	0.15	25	7	0.28	49	8	0.16	33	9	0.27
Europe-America[3]	23	1	0.04	14	1	0.07	27	6	0.22	19	-	-
MARRIAGES												
Total[1]	41	4	0.10	24	6	0.25	32	9	0.28	37	4	0.11
Month of event												
January-February, November-December	12	2	0.17	6	2	0.33	12	3	0.25	10	3	0.30
March-October	29	2	0.07	18	4	0.22	20	6	0.30	27	1	0.04
Age of married person												
15-19 years	4	2	0.50	1	-	-	5	2	0.40	4	1	0.25
20 years and over	37	2	0.05	23	6	0.26	27	7	0.26	33	3	0.09
Type of locality												
3 large cities	30	2	0.06	12	4	0.33	20	5	0.25	15	2	0.13
Other	11	2	0.18	12	2	0.17	12	4	0.33	21	2	0.10
District												
Tel Aviv and Central	23	4	0.17	14	5	0.36	23	5	0.22	23	3	0.13
Other	18	-	-	10	1	0.10	9	4	0.44	14	1	0.07
Years of school of household head												
0-8 years	9	4	0.44	6	2	0.33	16	5	0.31	14	2	0.14
9-11 years	8	-	-	7	1	0.14	9	2	0.22	10	2	0.20
12 years or more	19	-	-	11	3	0.27	7	2	0.29	12	-	-
Continent of birth of household head[2]												
Asia-Africa	14	2	0.14	8	2	0.25	10	4	0.40	24	4	0.17
Europe-America[3]	25	2	0.08	14	4	0.29	22	5	0.23	13	-	-

[1]Total includes some persons not included in individual breakdowns due to lack of data.
[2]If household head was born in Israel, continent of origin of household head's father is shown.
[3]Including Israel.

NOTE: Total = total events reported by specified relative.
Rate = events misreported as proportion of total events reported by specified relative.

differential overreporting and underreporting by relatives according to characteristics of the event or the household. Biases in reporting by event households were too small to be analyzed in this way. The data are the raw numbers of total events, underreported events, and overreported events from the evaluation survey for each type of relative. The rates computed should be regarded as only very rough indications of differential misreporting because of the very small numbers involved, and they are not comparable to the overall overreporting and underreporting biases of tables E and F. Furthermore, it should be noted that the household characteristics are those of the household head and not necessarily those of the respondent.

The results indicate that for the extreme months (January-February and November-December) there is more overreporting of births by households of both mothers' mothers and mothers' sisters and more overreporting of marriages by siblings' households than there is for the middle months (March-October). However, underreporting does not seem to be connected specifically with marginal months, except possibly for marriages reported in parents' households.

There are some weak indications of more overreporting and underreporting of births to younger mothers and of more overreporting of younger persons marrying by households of both parents and siblings. Possibly there was also underreporting of younger marriages by parents' households. The results by type of locality and by district are conflicting, and no clear conclusions can be reached.

For both births and marriages, underreporting is consistently highest for the lowest educational level. (This characteristic relates to the head of the event household.) However, the highest overreporting rates for births are for households whose head had completed at least secondary education.

The breakdown by continent of birth (again for head of event household) indicates more underreporting of births for households of Asian or African background (in which fertility is higher). This does not necessarily hold for marriages. Overreporting by siblings' households of both births and marriages is also high for this group, but differences in overreporting by parents' households are insignificant.

Finally, in table N the discrepancies in reporting characteristics of the same event by the event household and by the household of a relative are given. In this case no decision was made on which report was correct. The results show that in all cases consistency between the event household report and that of the parents' household was greater than the consistency of reporting by siblings' households, especially for marriages. For households of both mothers and sisters, less than half of the reports on age of mother were consistent; for over 20 percent the inconsistency was more than a single year. Higher consistency was reached for reports by parents' households on age at marriage, but there was similar low consistency for reports by siblings' households. Reports were more consistent for month of marriage than for month of birth. Only about 15 percent inconsistencies in reports of multiplicity (number of households in network) were found for births, by households of both mothers and sisters and, for marriages, by parents' households. However, more than a quarter of the reports from siblings' households on multiplicity of marriages were inconsistent. Virtually all inconsistencies in multiplicity reporting were of only a single unit.

4.4 Co-residence

The information obtained in the basic survey on relatives of the event persons (baby's mother or married person) and on which of them reside in the same household was collected in order to construct the multiplicity networks. However, as a byproduct, this information can be used to provide data on residence patterns of event persons and their relatives. The partial data obtained from the basic survey on co-residence (i.e., specified relatives living together in the same household) are presented in tables O to Q.

It should be pointed out first that these data obviously relate only to networks of relatives of event persons, since no information on relatives was obtained from households which did not report an event. Secondly, the data relate to the classification of events primarily by the co-resi-

Table N. Reporting discrepancies between event household and household of relative

[Includes only events reported by both households]

Characteristic and discrepancy	Births—comparison with reports from household of:		Marriages—comparison with reports from household of:		Births—comparison with reports from household of:		Marriages—comparison with reports from household of:	
	Mother	Sister	Parent	Sibling	Mother	Sister	Parent	Sibling
	Absolute number				Percent distribution			
Total	114	74	60	51	100	100	100	100
Month of birth or marriage								
None	75	43	47	35	66	58	78	69
1 month	22	14	7	9	19	19	12	18
2 months or more	17	13	6	7	15	18	10	14
Not applicable	-	4	-	-	-	5	-	-
Age of baby's mother or married person								
None	49	28	45	21	43	38	75	41
1 year	42	24	14	24	37	32	23	47
2 years or more	23	19	1	6	20	26	2	12
Not applicable	-	3	-	-	-	4	-	-
Multiplicity								
None	98	62	52	37	86	84	87	73
1 household	13	11	8	13	11	15	13	25
2 households or more	3	1	-	1	3	1	-	2

dence status of the parents of the event person (including co-residence of parents with other siblings of the event person outside the event household), in accordance with the hierarchical nature of the multiplicity rules. Thus no data have been processed on the co-residence of siblings with event persons. Neither were data processed on some further aspects of co-residence such as breakdowns by co-residence status within the event household or by characteristics of event persons.

The co-residence classification first differentiates between parents in the survey population and those not in the survey population (deceased, abroad, or residing in kibbutzim or institutions; no further breakdown is available for this category). Parents in the survey population are then categorized according to whether or not they reside in the event household (with the event person). Finally, parents in the survey population who reside outside the event household are classified according to their co-residence with other children (siblings of the event person) and, for marriages, also according to co-residence with spouse. For births the classification obviously relates only to mothers' mothers and their co-residence with the baby's mother and, if residing outside the event household, their co-residence with other daughters. For marriages, events are classified separately for the

Table O. Residence of mother's mother (for births)

Residence of mother's mother	Total	In survey population	Not in event household
	Percent		
Total	100.0	...	...
Not in survey population[1]	17.5	...	...
In survey population	82.5	100.0	...
In event household	7.2	8.7	...
Not in event household	75.3	91.3	100.0
With daughters	31.1	37.8	41.3
Without daughters	44.2	53.5	58.7

[1]Comprises deceased, living abroad, or living in a kibbutz or institution.

Table P. Percent distribution of parents of married persons by residence of parents, according to sex

Residence of parent	Sex of married person					
	Both sexes		Male		Female	
	Father	Mother	Father	Mother	Father	Mother
	Percent distribution					
Total	100.0	100.0	100.0	100.0	100.0	100.0
Not in survey population[1]	23.0	[2]33.7	28.1	[2]35.7	17.6	[2]31.2
In survey population:						
In event household	16.5		13.7		19.1	
Not in event household	60.5	66.3	58.2	64.3	63.3	68.8
Without spouse	6.0	11.8	7.1	13.2	5.0	10.5
Without children	4.2	6.4	5.5	7.5	3.1	5.3
With children	1.8	5.4	1.6	5.7	1.9	5.2
With spouse	54.5	54.5	51.1	51.1	58.3	58.3
Without children	18.5	18.5	16.6	16.6	20.1	20.1
With children	36.0	36.0	34.5	34.5	38.2	38.2
Percentage in event household out of total in survey population	21.4	---	19.1	---	23.2	---

[1]Includes deceased, living abroad, or living in a kibbutz or institution.
[2]Breakdown not available for mothers.

married person's father and mother and also by the sex of the married person.

Table O shows that an estimated 17.5 percent of births in 1973 occurred to women whose mother was deceased or living abroad, in a kibbutz, or in an institution. Of those mothers' mothers in the survey population, only 8.7 percent resided in the same household as the baby's mother. Of mothers in the survey population who resided outside the event household, 41.3 percent resided with other daughters (sisters of the baby's mother).

It is seen from table P that a higher proportion of fathers of newly married persons (than of mothers' mothers) were deceased or living abroad, in a kibbutz, or in an institution (23.0 percent). This proportion was higher for fathers of grooms (28.1 percent) than for fathers of brides (17.6 percent), probably because of the difference in age of brides and grooms. Of the married persons' fathers in the survey population, however, a higher proportion (than of mothers' mothers) resided together with the married person (21.4 percent). Of course, marriage usually precedes births and relates to an earlier stage of the separation of marrying children from their parents. The proportion living with the event person was lower for fathers of grooms (19.1 percent) than for fathers of brides (23.2 percent), i.e., there was more tendency for the newlyweds to live with the bride's father than with the groom's father. Unfortunately parallel data for mothers of married persons are not available. However, the total of married persons' mothers who were either not in the survey population or were in event households (33.7 percent) is less than the corresponding figure for married persons' fathers (39.5 percent), and is less both for grooms and for brides. It is conjectured that this is primarily due to a lower proportion of mothers outside the survey population (e.g., deceased) rather than to a lower proportion living in event households.

Table Q gives the percentage distributions of married persons' parents living outside the event household by their co-residence with spouse and with other children (siblings of the married persons). The data show that about 90 percent of married persons' fathers not living with the married person had a wife in the household, while only about 82 percent of married persons' mothers had a husband in the same household,

Table Q. Percent distributions of married persons' parents residing outside event household, by co-residence with spouse and with children

Co-residence of parent with children	Sex of married person and co-residence of parent with spouse								
	Both sexes			Male			Female		
	Total	With spouse	Without spouse	Total	With spouse	Without spouse	Total	With spouse	Without spouse
	Father								
Total	100.0	90.1	9.9	100.0	87.8	12.2	100.0	92.1	7.9
With children	62.5	59.5	3.0	62.0	59.3	2.7	63.3	60.3	3.0
Without children	37.5	30.6	6.9	38.0	28.5	9.5	36.7	31.8	4.9
	Mother								
Total	100.0	82.2	17.8	100.0	79.5	20.5	100.0	84.7	15.3
With children	62.4	54.3	8.1	62.6	53.7	8.9	63.1	55.5	7.6
Without children	37.6	27.9	9.7	37.4	25.8	11.6	36.9	29.2	7.7

probably due to the higher widowhood of women. The percentages are higher for parents of the bride than for parents of the groom, presumably again because of the differences in average age of the marriage partners. The percentages of parents living with other children is similar for mothers and for fathers, both for parents of the groom and for parents of the bride (62-63 percent). While most of the parents living with their spouses had other children in the household, the situation was reversed for parents without a spouse. However, relatively more mothers without a husband had children in the household (about half) than fathers without a wife (about a third). Brides' parents without spouses were more likely to have children living with them than were grooms' parents without spouses.

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Chapter 5. DISCUSSION OF SPECIFIC POINTS

5.1 Choice of Multiplicity Counting Rules

As pointed out in section 4.2, the restricted multiplicity rule is counterindicated, primarily because of high overcoverage bias due to parents (especially for marriages). This bias component also contributes to the bias for the full multiplicity rule, since reports by parents are included under this counting rule. A conjecture which will be considered briefly here is that a "modified restricted" counting rule based on conventional plus siblings (without parents) might be more efficient than the full multiplicity rule (with parents). The probable increase in sampling variance due to a more restricted counting rule might well be offset by a reduction in response variance and particularly a reduction in bias.

While the processing carried out did not directly permit a complete quantitative evaluation of this alternative restricted counting rule, a partial reprocessing of the evaluation survey provided separate estimates of overcoverage and undercoverage biases for households of parents with siblings and for households of parents without siblings. The estimates were made according to the methods described in section 3.2 and are presented in table R, a modification of table F. The relative biases in table R could be reweighted by the contributions conjectured for reports by event households, for reports by households of siblings with parents, and for reports by households of siblings without parents according to the co-residence data presented in section 4.4. Some indications of the biases to be expected under certain simplifying assumptions for a restricted rule of conventional plus siblings could then be obtained.

Households of parents are divided into two groups, according to whether or not they included siblings of the event person, in table R. Results indicate that, under Israeli conditions, undercoverage biases were significantly higher for households of parents with siblings than for households of parents without siblings, both for births (15 percent with siblings versus 8.9 percent without siblings) and for marriages (11.1 percent with siblings versus 7.1 percent without siblings). This may be due to the fact that, in Israel, households with siblings have a greater than average proportion of large families of lower socioeconomic status and educational background. (As indicated in table M, underreporting was significantly higher for parents' households whose head was from Asia or Africa or from a lower educational background, both for births and marriages.) On the other hand, the phenomenon of underreporting might be more general and due to an inherent tendency of parents with more children to underreport events of their children just because there are more of them. It should be noted, furthermore, that, even for parents' households with siblings, undercoverage biases were much lower than for households of siblings without parents.

Overcoverage biases for both types of parents' households were very similar for births, but for marriages were again considerably higher for parents' households with siblings than for those without siblings. This could be due to the same explanations conjectured above for undercoverage biases. However, from table M it is seen that while overreporting rates for marriages were higher for households of parents whose head was from Asia or Africa, there were no significant differences according to educational level of household head. Table R shows that for births overcoverage biases for both types of parents' households were similar to those of siblings' households without parents, but for marriages even parents' households without siblings were associated with considerably larger overcoverage biases than those of siblings' households.

Again it should be emphasized that the above estimates of bias were obtained under the respondent rules of the survey, which were those of the LFS (any available person aged 14 and over). The respondent rule obviously may affect the bias. Especially for a modified restricted rule, a more stringent respondent rule might be indicated, such as limiting respondents to reporting relatives themselves (e.g., siblings, even in households with parents). Further experimentation would be necessary in order to determine

Table R. Analysis of relative biases, by type of event, reporting relative, and counting rule

Type of event, reporting relative, and counting rule	Weight			Relative bias		
	Households of parents[1]	Full multiplicity rule	Modified restricted rule	Undercoverage bias	Overcoverage bias	Net bias
BIRTHS						
Reporting relative			Percent			
Event household	-	45.1	54.7	[2]-2.04	[2]+2.12	[2]+0.08
Household of parent	100.0	29.8	-	[2]-10.34	[2]+16.20	[2]+5.86
Without siblings	58.7	17.5	-	-8.89	+16.42	+7.53
With siblings	41.3	12.3	14.9	[2]-15.00	[2]+15.12	+0.12
Household of sibling (without parents)	-	25.1	30.4	[2]-20.51	[2]+15.73	[2]-4.78
Counting rule						
Modified restricted multiplicity (event plus siblings):						
Total bias	...	...	...	-8.67	+8.29	-0.38
Total event reporting bias	...	...	...	-9.95	+8.19	-1.40
Contribution of multiplicity reporting variance	...	...	...	+0.92	+0.10	+1.02
Full multiplicity (event plus parents plus siblings):						
Total bias	...	...	...	[2]-8.22	[2]+9.83	[2]+1.61
Regular restricted multiplicity (event plus parents):						
Total bias	...	...	...	[2]-4.72	[2]+7.66	[2]+2.94
Conventional (event only):						
Total bias	...	...	...	[2]-2.04	[2]+2.12	[2]+0.08
MARRIAGES						
Reporting relative						
Event household	-	47.7	52.4	[2]-2.69	[2]0	[2]-2.69
Household of parent	100.0	23.7	-	[2]-9.76	[2]+36.96	[2]+27.20
Without siblings	37.5	8.9	-	-7.14	+13.93	+6.79
With siblings	62.5	14.8	16.2	-11.11	+54.58	+43.47
Household of sibling (without parents)	-	28.6	31.4	[2]-21.74	[2]+7.41	[2]-14.33
Counting rule						
Modified restricted multiplicity (event plus siblings):						
Total bias	...	...	...	-8.74	+11.33	+2.59
Total event reporting bias	...	...	...	-10.04	+11.17	+1.13
Contribution of multiplicity reporting variance	...	...	...	+1.30	+0.16	+1.46
Full multiplicity (event plus parents plus siblings):						
Total bias	...	...	...	[2]-8.51	[2]+11.02	[2]+2.51
Regular restricted multiplicity (event plus parents):						
Total bias	...	...	...	[2]-3.36	[2]+11.64	[2]+8.28
Conventional (event only):						
Total bias	...	...	...	[2]-2.69	[2]0	[2]-2.69

[1] From table O for births; from table Q for marriages.
[2] From table F.

the effect of different respondent rules on response biases.

The weights in table R were estimated from those obtained for the full multiplicity rule, with the weight of parents' households allocated to households with and without siblings according to co-residence data (tables O and Q). The estimated biases of event households, of households of parents with siblings, and of households of siblings without parents were then weighted according to these weights to provide estimates of the event-reporting biases for the modified restricted counting rule. The contributions of multiplicity reporting variances estimated for the full multiplicity rule were then added to obtain estimates of total bias for the modified restricted rule. It was not possible to estimate this contribution separately for the modified rule, but it is, in any case, small and probably close to that of the full multiplicity rule.

The results are shown in the sections of table R labeled "Counting rule," and are compared with the previously defined counting rules. They show that the biases for the modified restricted rule are similar to those of the full multiplicity rule, although the decrease in overcoverage bias for births does decrease the net bias somewhat. For marriages there would certainly be no gain

from the use of a modified restricted rule (as compared with the full multiplicity rule); for births it is extremely doubtful that the decrease in bias would offset the increase in variance. However, the net bias of the modified restricted rule was considerably smaller than that for the regular restricted rule, for both births and marriages. This was because undercoverage biases were more similar to overcoverage biases for the modified restricted rule than for the regular restricted rule. Since the variance of the modified restricted rule is probably not very different from that of the regular restricted rule, the modified rule would be preferable to the regular restricted one.

If the higher biases for parents' households with siblings are indeed a general phenomenon not due to specific Israeli conditions, a different modified counting rule which excludes these households might be considered. The modified rule would include reports by event households, by households of siblings without parents, and by households of parents only if no siblings are in the household. Calculations similar to those above for this "modified full" multiplicity rule give the following estimates of total bias:

	Births	*Marriages*
Net bias	+1.19	-4.15
Undercoverage bias	-7.77	-8.25
Overcoverage bias	+8.97	+4.10

While this modification reduces the overcoverage bias for marriages considerably, the net bias of the modified rule is higher than that for the regular full multiplicity rule for marriages and only slightly lower for births. Modifying the regular restricted rule by limiting it to event households and to parents' households without siblings was not considered. This would be very close to a conventional rule because of the small relative weight of parents' households without siblings.

Another modification of the multiplicity counting rule which could be considered would be to restrict reporting to relatives residing within a limited geographical area (e.g., in the same city or district) or to a well-specified subpopulation. The main purpose of the restriction of the counting rules, geographically or by subpopulation, would be to attain smaller response errors, under the assumption that reporting might be more reliable on events which occurred to relatives in the same geographical area or subpopulation. However, operational problems in the application of the geographical or other classifications might offset this advantage to some extent. The restriction of the counting rule would also, in general, increase sampling variance. A quantitative evaluation of the efficiency of such modifications, while feasible on the basis of a reprocessing of material from the present study, would be seriously limited by the small sample size. Furthermore, this modification would probably only prove worthwhile for a country far larger than Israel.

5.2 Operational Problems

During the execution of the basic and evaluation surveys a number of operational problems arose which are specific to surveys of this type. Some of these will be set forth in this section, and possible solutions will be considered.

One point of ambiguity that arose as a consequence of the nature of the multiplicity study and its linkage to the LFS concerns the distinction between household and dwelling unit. The LFS definition of household is a group of persons who live in the same dwelling unit and prepare most of their meals together. The LFS sampling unit, however, is the physically distinct dwelling unit, usually an apartment. If several households share one dwelling unit, the procedure is to fill out a separate household listing form and multiplicity questionnaire for each household and to count each household as a separate unit of the sample. Since the decision as to whether the inhabitants of a dwelling unit constitute one or several households is made on the spot by the enumerator, according to the standard LFS definition, there may be some inaccuracies.

A more complicated problem arose in connection with the differentiation of households in the listing of relatives. This problem was aggravated by instances of partial or complete failure by the enumerator to list the full set of names

and addresses of relatives of the event person. Some of these questionnaires were returned to the field, but even so not all addresses could be obtained. It sometimes happened that incomplete or unclear addresses were given for some of the named relatives, and it was not clear to the coder if they lived together or apart. It will be noted that an instruction at the top of the address listing form of the questionnaire (see appendix II) directs the enumerator to write "Like (No.)" for all but the first person listed as living at the same address. But this could mean not only persons living in the same household but also persons living in different households in the same dwelling unit or in different dwelling units in the same building (or even in the same "block" of buildings in certain new areas). Futhermore, it occasionally happened that several relatives were indicated only as living in the same locality, with no further details provided. Although multihousehold dwelling units are rare in Israel, a direct determination of the number of different households in the network rather than its determination by addresses should be considered for future studies. This is especially so since the ascertainment and recording of full addresses, which are cumbersome and time-consuming, are not imperative for the determination of multiplicity and could be limited to an evaluation subsample (if done at all).

An important aspect of this problem is the fact that the enumerators sometimes thought of the list of addresses as being needed only for the purpose of sample selection for the evaluation survey. Thus it might not seem critical if some of the addresses were not complete. Further attention should be given to the problem of getting the point across to enumerators that the number of households in the multiplicity network is a datum no less important than the reporting of the event itself. To overcome this problem, it may be helpful to use a procedure whereby the enumerator first obtains the names of all persons in the survey population who have a specified relationship to the event person and then ascertains systematically, by a special question, which of these persons live in the same household. Despite this occasional problem, the results of the evaluation survey indicate that reporting of multiplicity networks was generally satisfactory once an event was reported.

Mention has already been made of the problems faced by the enumerators due to time pressures. In particular, in a multiplicity survey in which many events are reported, the time spent filling out event questionnarires becomes a burden. If more than a single birth or marriage is reported for the same type of relative in a given network, several address listings must be obtained that are essentially identical but arranged in different order. For instance, if two sisters outside the interview household gave birth in the same year, the address listings for both events would include the same names, but the positions of the two sisters would be interchanged. If it appears at the planning stage that this will not be a rare situation (as in the case of the multiplicity study carried out among the Arab population in the Administered Areas), the questionnaire should be arranged in such a way that one listing of the network suffices for all events which occurred in the same network during the reference period.

Mention has been made in section 2.1 of the decision to exclude from the survey population persons living in kibbutzim or institutions. The exclusion of these groups from the survey population caused some difficulties both in processing and in comparing survey estimates with current demographic data. It might, however, be feasible to include events occurring to persons in subpopulations in which direct interviews are expensive without contacting the persons involved individually if sufficient information is available from office records. Essentially this could be done by applying the conventional rule with respect to events of persons in the subpopulation on the basis of information in the office records, and no further information on relatives or on events to relatives would be required. However, this would mean that reports on relatives in the subpopulation and events occurring to them would have to be excluded from the questionnaires of the rest of the population on the basis of appropriate information to be obtained (special question, comprehensive listing of addresses, etc.).

It must also be kept in mind that the passage of time may affect any multiplicity network be-

cause of actual moves and changes in household composition. The events which are the subject of the survey occurred during 1973. The basic survey was conducted between February and April 1974, and the evaluation survey was conducted between April and June 1974. It was decided to accept the distribution of addresses in a network as of the date of each enumeration, even though this artificially increased the discrepancies between the basic and evaluation survey reports. An alternative design for a similar study might be to ask, as part of the evaluation survey, if any of the persons listed had moved recently and, if so, to ascertain relevant details so as to be able to reconcile successive reports. While clumsy, this procedure becomes more advisable as the interval between the two surveys increases. In order to reduce nonresponse in the evaluation survey due to address changes, it might also be possible to recontact basic survey households for current addresses of relatives who have since moved.

Finally, it should be noted that many Jews in Israel are cognizant of the dates of vital events to members of their family and to other relatives in terms of the Jewish calendar. This practice is followed by many nonreligious Jews, not only by the orthodox population. In cases in which a respondent could give the date of an event only by the Hebrew calendar and the corresponding civil date was not clear to the enumerator, enumerators were instructed to record the Hebrew date in the questionnaire. A final determination of the date of the event was made at the editing stage.

5.3 Length and Position of Recall Period

As pointed out in section 2.3, the reference period for which reports on events were requested was fixed as the year 1973. Since most of the fieldwork was carried out during February-June 1974, this necessitated recall periods of from 1 month up to 18 months. It is well known that response errors are, in general, affected by the length of the recall period (see Sirken and Sabagh (1968) and Nathan (1973)). These response errors consist primarily of underreporting biases due to recall lapses which usually increase as the recall period lengthens (see Som (1959)). However, overreporting is also affected by the length of the recall period, primarily due to the "telescoping effect," i.e., reporting events which occurred outside the reference period as having occurred within the period (see Neter and Waksberg (1964)).

Although the effect of the length of the recall period was not initially defined as a research area for the study, there is some evidence of this effect in the material available. As mentioned in section 3.3, practically all the overreporting discovered was of events which in fact occurred, but not during the reference period. Similarly, as mentioned in section 4.3, there are indications that overreporting by certain types of relatives is connected with the marginal months at the beginning and end of the reference period (see table M). On the other hand, the smaller proportions of births reported for January and February relative to the vital records (see table K) may be due to underreporting.

The reference period of 1 year was chosen to diminish recall effects, on the one hand, and to ensure a sufficient number of events, for both the basic and evaluation surveys, on the other hand. The use of a full calendar year was also decided upon with the idea that it might reduce response errors, and interviewing was carried out as shortly after the reference period as possible for this reason. However, these decisions were not based on quantitative data, and it seems that the length of the reference period and the timing of the survey may well be important design parameters for multiplicity surveys which should be given further attention.

The full reprocessing of the basic and evaluation surveys according to varying reference periods, in order to evaluate the effect of the length and position of the reference period with respect to all components of error, would have been prohibitively expensive. However, rough measures of biases and standard errors for all possible consecutive runs of months were prepared for the conventional and full multiplicity rules, and selected results are presented in table S.

The approximate measure of bias used is the percent difference for each period between the cumulative survey estimates and the cumulative

Table S. Relative differences (between survey estimates and vital record estimates) and relative standard errors for selected reference periods, by type of event and counting rule

Type of event, counting rule, and last month of reference period	Length of reference period							
	3 months		6 months		9 months		12 months	
	Relative difference	Relative standard error	Relative difference	Relative standard error	Relative difference	Relative standard error	Relative difference	Relative standard error
BIRTHS								
Conventional								
June	+0.101	0.104	-0.009	0.078	...	...	...	...
October	-0.135	0.109	-0.025	0.074	-0.056	0.063	...	...
December	-0.101	0.109	-0.118	0.076	-0.051	0.061	-0.068	0.053
Full multiplicity								
June	-0.124	0.079	-0.122	0.055	...	...	...	...
October	-0.125	0.072	-0.125	0.052	-0.123	0.042	...	...
December	-0.056	0.075	-0.091	0.051	-0.101	0.042	-0.106	0.036
MARRIAGES								
Conventional								
June	+0.146	0.179	+0.002	0.132	...	...	...	...
October	-0.228	0.167	-0.084	0.122	-0.099	0.103	...	...
December	-0.199	0.220	-0.218	0.133	-0.113	0.106	-0.119	0.093
Full multiplicity								
June	-0.138	0.124	-0.156	0.084	...	...	...	...
October	-0.140	0.089	-0.139	0.072	-0.149	0.060	...	...
December	-0.026	0.110	-0.099	0.068	-0.110	0.060	-0.125	0.052

current demographic data (adjusted to the totals of table E), both in absolute numbers. The approximate measures of standard errors are computed, as described in section 4.3, from the regular sample estimate of total variance (between the weighted contributions of all households, including zeros), divided by the total sample size.

The results are to a large extent inconclusive, primarily due to the lack of any systematic pattern in the monthly relative differences and to the large standard errors involved. However, some indications of tendencies may be pointed out.

As seen previously in table K, the months of January, February, and November have high negative differences between the survey percentage distributions for births (both for the conventional and for the multiplicity rule) and data from vital records. However, for the conventional rule it can be seen from table S that of the reference periods shown only the 6-month reference periods ending June and October have a

considerably smaller relative difference than the full year. This reduction is not offset by the increase in variance due to the shorter period. No such effect is found for the full multiplicity rule.

For marriages the highest negative differences between the percentage distributions shown in table K were found for July and December by the conventional rule and for June and July for the multiplicity rule. These are offset by high positive differences for April by the conventional rule and for October by the multiplicity rule. Table S shows that the first 6-month period (January-June) has the smallest relative difference for the conventional rule, while for the full multiplicity rule all periods of 6 months or more have relative differences of about the same or higher order of magnitude as for the full year. It should be pointed out that the monthly intensity of marriages in Israel varies considerably (table K). This is partly because of religious limitations on marriages at certain times (e.g., a period in the Hebrew calendar which usually falls around May). This variation was further increased in 1973 by the war in October. However, even in normal times the measurement of marriages for a reference period of less than a full year would cause difficulties in the estimation of a yearly rate.

Further inspection of table S indicates that, as expected, relative differences usually decrease with increasing length of reference periods with the same last month. Nevertheless, it is noteworthy that the bias of the full multiplicity estimate based on the reference period ending in December increases monotonically with the increase in the length of the reference period for both births and marriages. However, the estimates of error obtained above are based on small sample sizes and are thus subject to serious limitations. Furthermore, the reprocessing is only an approximate simulation. For instance, response to a direct question on a 6-month period may differ from that obtained by cutting off reports for 6 months from a question on events for a full year. However, the findings are applicable to a strategy whereby data are collected for a reference period of a year but are processed only for a cutoff, partial period.

5.4 Possibility of Application to Other Topics

As pointed out in the introduction, in view of the virtually complete vital registration, a retrospective field survey, either conventional or multiplicity, for measuring vital events in Israel is not justified. The purpose of the study was thus primarily methodological, and an important aim was to gain experience with the multiplicity technique for its possible application to other topics for which administrative data do not exist and conventional surveys might be prohibitively expensive. Since only births and marriages were included as topics in the present study, any discussion of the applicability of multiplicity methods to other topics must of necessity be conjectural.

First, it should be pointed out that the two topics selected finally, under the impact of the 1973 war, for study in Israel are rather similar from the demographic point of view. Both births and marriages occur primarily to young persons concentrated in a narrow age range. For both births and marriages, parents of event persons tend to be found in similar age groups. The same is true of siblings of event persons. The original design, which included deaths instead of marriages, would have provided more information on the feasibility of other applications since a completely different age group would have been involved.

However, in general, the results of the study do indicate the feasibility of inquiry on events which occurred to relatives outside the event household. Also, the quality of reporting, both on events and on the multiplicity network of relatives (with their addresses), and the prospective gains in efficiency are sufficiently high relative to the conventional method to warrant further investigation into the applicability of multiplicity methods to other topics.

There are a number of general principles which must be taken into account when considering whether it might be profitable to investigate a given topic by means of multiplicity techniques. Some of the more important of these are presented here:

1. The event or characteristic to be studied must be salient and clearly definable so that

it can be reported by relatives not living in the event household. Births, marriages, and deaths satisfy this criterion, while the situation with respect to internal migrations, for example, is not so straightforward.

2. If data on the event or characteristic in question are already available from reliable statistics, it is generally not worthwhile to use survey techniques. But survey techniques, including multiplicity, may be considered when other sources are known to be deficient or nonexistent.

3. There are situations where the event person or event household is not in the survey population. The use of multiplicity may be warranted in the study of persons living abroad, or in the study of members of an earlier generation who may be dead but have surviving relatives in the survey population who can report on them. This situation may arise, for instance, in the study of intergenerational mobility.

4. A common problem in survey research involves the study of rare events or small groups within the population. Multiplicity could be useful in surveys of religious sects or other groups not distinguished in regular statistical sources, as well as the incidence or prevalence of health conditions, such as persons with incapacities of specified types (if sufficiently definable for interviews with lay respondents). However, as the relative frequency of the event or characteristic declines, it becomes necessary to make use of special sampling frameworks in addition to any use of multiplicity. For example, in Israel even the use of a widespread counting rule would not make practicable the use of the whole LFS as a framework for studying persons whose families come from a certain town or small region abroad. On the other hand, if the specific population of persons whose families come from the foreign country in which this town or region is located can be found (e.g., via a population census or register) and an appropriate sample created, the use of multiplicity in locating those persons who come from the town or small region in question may increase the efficiency of the survey.

In reviewing the possibilities of applying multiplicity techniques to topics other than births and marriages, it has been assumed that there is available a well-developed statistical reporting system. In this situation, the use of multiplicity is seen as providing supplementary data or the reporting system is seen as providing the sampling framework within which the multiplicity technique is to be applied. It is outside the scope of this report to consider the use of multiplicity in situations where well-developed and reliable statistical reporting systems do not exist. However, the presentation in a future report of the results of the multiplicity study carried out in the Administered Areas will provide the opportunity for an examination of this topic.

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Chapter 6. SUMMARY AND CONCLUSIONS

It has been demonstrated that the study of vital events in the United States by means of a conventional retrospective household sample survey can be improved, with respect to sampling error, by the use of multiplicity techniques, whereby events are reported not only by the households in which they occurred, but also by households of relatives linked to them according to specified counting rules (see Sirken and Royston (1973)). The aims of the present study were to test the feasibility of conducting a multiplicity survey for certain types of vital events in Israel and to evaluate different counting rules with regard to the total mean square error and its components—response bias, response variance, and sampling variance.

The study was conducted for births and marriages by means of (a) a basic survey carried out as an addition to a part of the Central Bureau of Statistics' regular Labour Force Survey and (b) an evaluation survey in which reports for a subsample of events reported in the basic survey were checked by interviewing households of relatives. Estimation of the components of error was based on a theoretical model, with some simplifying assumptions, which permitted the evaluation of the relative efficiency of various multiplicity counting rules as compared with a conventional survey.

The feasibility of carrying out a multiplicity survey for the relatively heterogeneous population of Israel was well demonstrated, some operational problems notwithstanding. Furthermore, at least for certain vital events, this can be done as a marginal addition to a regular household sample survey. Certain modifications in the questionnaire and interviewing procedure used in the survey reported on here in order to overcome operational problems in future multiplicity surveys have been proposed in this report.

The virtually complete vital records system in Israel makes retrospective household sample surveys for estimation of vital event data superfluous. Indeed, the aim of this study was primarily methodological, and the comparisons of vital record data with the survey estimates indicated, as expected, that a retrospective sample survey, whether conventional or multiplicity, does not provide complete coverage. However, the differences were generally not statistically significant. Estimates of total mean square error for the different counting rules definitely indicated the superiority of a wider multiplicity rule over the conventional survey and over restricted multiplicity rules. Although the multiplicity survey has higher response bias and variance than the conventional survey, this is more than offset by the reduction in sampling variance.

The multiplicity survey also permits the breakdown of estimates of total events by various demographic characteristics and the computation of specific rates. These too compare rather well, within the limits of error, with current demographic data. Furthermore, the multiplicity survey can provide, as a byproduct, data on the co-residence of relatives with event persons (new mothers and newly married persons), which are not available from a conventional survey unless special questions are asked.

The efficiency of a multiplicity survey compared to a conventional survey may possibly be enhanced by certain modifications in the survey design. Modifications in the multiplicity counting rules, in the length and position of the recall period, in the weighting method for estimation, in the respondent rule applied, and in other aspects of the survey design may both reduce further the mean square error and overcome some of the operational problems. However, further research and experimentation have to be carried out in order to test such modifications.

The results and experience gained in this study will be of importance in developing possibilities of applying multiplicity methods to topics for which administrative data do not exist and for which a conventional survey might be prohibitively expensive. These could be in a wide range of areas, such as health, migration, or intergenerational mobility.

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APPENDIX I

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APPENDIX II

MULTIPLICITY SURVEY QUESTIONNAIRE

STATE OF ISRAEL
Central Bureau
of Statistics

SURVEY OF BIRTHS AND MARRIAGES: 1973
HOUSEHOLD QUESTIONNAIRE

QUESTIONNAIRE NO. _____

To the enumerator: Include babies born alive who died afterward.
— If you list a baby who died without ever being named, write "None" in the space for listing given name.

A. BIRTHS IN 1973

1. Enumerator: List at right the names of all ever-married women in the household up to age 50 (born in 1924 or after) If none - go to Question 4.	1._____	2._____	3._____
2. Did_____(name) have a baby in 1973?	1. Yes (to Q. 3) 2. No (to Q. 4 if no more women)	1. Yes (to Q. 3) 2. No. (to Q. 4 if no more women)	1. Yes (to Q. 3) 2. No. (to Q. 4)

3. What are the names of the babies born to_____(name)?

(Return to Q. 1 if there is another woman listed there).

Serial No.	Given name	Family name
11		
12		
13		
14		

4. Enumerator: List at right the names of all ever-married women in the household aged 34+(born in 1940 or before). If none, go to Question 8.	1._____	2._____	3._____
5. Does_____(name) have any ever-married daughter or daughters who live elsewhere in Israel?	1. Yes. How many?____ (To Q. 6) 2. No (to Q. 8 if no more women)	1. Yes. How many?____ (To Q. 6) 2. No (to Q. 8 if no more women)	1. Yes. How many?_____ (To Q. 6) 2. No (to Q. 8).
6. Did any of these_____(number) daughters of_____ (name) have a baby in 1973?	1. Yes (to Q. 7) 2. No (to Q. 8 if no more women)	1. Yes (to Q. 7) 2. No (to Q. 8 if no more women)	1. Yes (to Q. 7) 2. No (to Q. 8)

7. What are the names of the babies born to the daughter(s) of_____(name)?

(Return to Q. 4 if there is another woman listed there).

Serial No.	First name	Family name
21		
22		
23		
24		

8. Enumerator: List at right the names of all women and girls in the household whose mothers do not also live here. If none, go to Question 12.	1. _____	2. _____	3. _____
9. Does _____ (name) have an ever-married sister or sisters who live elsewhere in Israel?	1. Yes. How many? _____ (To Q. 10) 2. No (to Q. 12 if no more women).	1. Yes. How many? _____ (To Q. 10) 2. No (to Q. 12 if no more women).	1. Yes. How many? _____ (To Q. 10) 2. No (to Q. 12).
10. Did any of these _____ (number) sisters of _____ (name) have a baby in 1973?	1. Yes (To Q. 11). 2. No (To Q. 12 if no more women).	1. Yes (To Q. 11). 2. No (To Q. 12 if no more women).	1. Yes (To Q. 11). 2. No (To Q. 12).

11. What are the names of the babies born to the sister(s) of _____ (name)?

(Return to Q. 8 if there is another woman listed there).

Serial No.	Given name	Family name
31		
32		
33		
34		

12. Enumerator:

A. List at right the total number of babies born in 1973 and recorded in Questions 3, 7, 11. [ ]

B. If any babies were born in 1973, fill out a "Birth Questionnaire" for each baby.

C. If no babies were born in 1973, write "0" in the space provided and go to page 5, Question 13.

BIRTH QUESTIONNAIRE

Serial number of the baby.

The baby's name: _____
Given Family

A. In which month was the baby born? (If not known, write xx and write below those details which are given to you, such as: born a week after Chanukkah, born during the winter).

_____

B. Sex of the baby.

1. Male
2. Female

C. What was the age of the mother when the baby was born? (If not known, write xx).

D. Questions on the baby's mother and her relatives (her mother and sister).

Enumerator: If an address is already listed, write, "Like (No.)". ADDRESS

	Family name	Given name	Husband's given name	Serial No.	Locality	Street and No.	Phone	Notes*
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. Mother of the baby				1				
2. Does the mother of the baby's mother live in Israel now?				2				
1. Yes →								
2. No (to Q. 3).								
3. Does the baby's mother have sisters living in Israel now?								
1. Yes →	A			3				
2. No (end of birth questionnaire).	B			4				
	C			5				
	D			6				
	E			7				
	F			8				

*If the address is not complete, and there is no home phone, ask how can the family be reached (for example: *via* a phone at work or at a neighbor) and indicate under "notes".

B. MARRIAGES IN 1973

Enumerator: Include also among the married persons those who married in 1973 and whose marriages have since been ended.

13. Enumerator: List at right the names of household members who married in 1973.

(If both spouses live here, list each of them).

41. _____ 43. _____

42. _____ 44. _____

14. Enumerator: List at right the names of all ever-married persons in the household aged 34+ (born in 1940 or before)

If none, go to Question 17.

Couples		Individuals		
1a _____	2a _____	3 _____	4 _____	5 _____
1b _____	2b _____			

15. Does _____ (name(s)) have sons or daughters who married in 1973 and who live elsewhere in Israel?

1. Yes. (To 16) 2. No. (To 17	1. Yes. (To 16) 2. No. (To 17	1. Yes. (To 16) 2. No. (To 17	1. Yes. (To 16) 2. No. (To 17	1. Yes. (To 16) 2. No. (To 17)

If no one else is listed in Q 14)

16. What are the names of the sons or daughters married in 1973 and not living in this apartment?

(Return to Q. 14 if there is another person listed there).

Serial No.	Given name	Family name
51		
52		
53		

17. Enumerator: List at right the names of all adults and children in the household who have *no* parent living here.

If none, go to Question 20.

1. _____	2. _____	3. _____	4. _____	5. _____

18. Does _____ (name) have brothers or sisters who married in 1973 and who live elsewhere in Israel?

1. Yes (To 19) 2. No. (To 20	1. Yes. (To 19) 2. No. (To 20	1. Yes. (To 19) 2. No. (To 20	1. Yes. (To 19) 2. No. (To 20	1. Yes. (To 19) 2. No. (To 20)

If no one else is listed in Q. 17).

19. What are the names of the brothers and sisters who married in 1973 and who live elsewhere in Israel?

(Return to Q. 17 if there is another person listed there).

Serial No.	Given name	Family name
61		
62		
63		

20. Enumerator:

A. List at right the total number of persons married in 1973 and listed in Questions 13, 16, 19. [ ]

B. If anyone was married in 1973, fill out a "Marriage Questionnaire" for each person listed here. (If two spouses are listed in Question 13, fill out a marriage questionnaire for each of them).

C. If no marriages occurred in 1973, write "0" in the space provided and terminate the interview.

MARRIAGE QUESTIONNAIRE

Serial number of the person married in 1973.

Name: _____
Given Family

A. In which month did he/she marry? (If not known, write xx and write below those details which are given to you, such as: married a week after Chanukkah, married during the winter).

_____

B. Sex of the newly married person (who is listed above).

1. Male.
2. Female.

C. How old was he/she at the time of the marriage?

D. In which locality did the marriage take place?

_____

(If the marriage took place abroad, write "Abroad").

E. Questions on the newly-married person and his/her relatives (parents, brothers, and sisters).

Enumerator: If an address is already listed, write "Like (No.)". ADDRESS

	Family name	Given name	Husband's given name	Serial No.	Locality	Street and No.	Phone	Notes*
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. Present address of the newly married person				1				
2. Does_____(name)'s father live in Israel now? 1. Yes → 2. No (to Q. 3).				2				
3. Does_____(name)'s mother live in Israel now? 1. Yes → 2. No (to Q. 4).				3				
4. Does_____(name) have brothers and sisters who live in Israel? 1. Yes. How many?	A			4				
2. No. (Terminate the questionnaire).	B			5				
	C			6				
	D			7				
	E			8				
	F			9				

*If the address is not complete, and there is no home phone, ask how the family can be reached (for example: *via* a phone at work or at a neighbor) and indicate under "Notes".

Scheduled week | 01 | 02 | 03 | 04 | 05 | 06 | 07 | 08 | 09 | 10 | 11 | 12 | 13 |

LABOUR FORCE SURVEY

Region _____

Settlement | Yr | Qtr | Apt. No. | H.H. No. | Change

QUESTIONNAIRE NO.

Name _____

Address _____

Comments and corrections _____

Name of settlement _____

A. LIST BELOW ALL HOUSEHOLD MEMBERS WHO GENERALLY RESIDE IN THIS APARTMENT

Serial Number	Family name	First name	Relation to head of household	Sex		Birth date		Marital status		Married only		Country of birth	Israel born Father's country	Year of immigration	Years of study
				Male	Female	Year	Month	Panel A	Changes	Year of present marriage	Is this first marriage?				
1.															
2.															
3.															
4.															
5.															
6.															
7.															
8.															

B. RELIGION OF HOUSEHOLD HEAD

1. Jewish 2. Christian 3. Moslem 4. Druze 5. Other; indicate _____

Panel	Is there a special questionnaire?	Date	Name of interviewer
A	Y/N		
B	Y/N		
C	Y/N		
D	Y/N		

Causes of non-interview	Panel			
	A	B	C	D
1. Insufficient address				
2. Apt. not located at address				
3. Apartment empty				
4. Not a dwelling unit, explain				
5. A new family lives here now				
6. The family has left temporarily				
7. Other				

APPENDIX IV. RECORD FORMAT:

BASIC SURVEY PROCESSING

Serial No.	*Name of field*	*Codes*
1.	*Household Data*	
1.1	Serial no. of household	L.F.S. code
1.2	Size of household	1-8 Number of persons 9 9 or more persons 0 N.A.
1.3	*Characteristics of Household Head*	
1.3.1	Sex	1. Male; 2. Female; 0. N.A.
1.3.2	Year of birth	Last two digits; xx. N.A.
1.3.3	Continent of birth	1. Israel-born - father Israel-born 2. Israel-born - father born Asia-Africa 3. Israel-born - father born Europe-America 4. Israel-born - continent of father unknown 5. Born Asia-Africa 6. Born Europe-America 0. N.A.
1.3.4	Year of immigration	Last two digits; xx. Israel-born and N.A.
1.3.5	Years of school	No. of years; xx. N.A.
1.4	*Evaluation Survey participation*	0. Not in Evaluation Survey 1. In Eval. Survey via births only 2. In Eval. Survey via marriages only 3. In Eval. Survey via both births and marriages
1.5	*Number of Events*	
1.5.1	Births	Number of events
1.5.2	Marriages	Number of events
2.	*Event Data*	
2.1	*Serial No. of event:*	
2.1.1	Type of event	1-3. Birth; 4-6. Marriages 1, 4. Event in core HH 2, 5. Event in offspring 3, 6. Event to sibling
2.1.2	Serial within detailed type	1-9.
2.2	Month of event	01-12. Month; xx. N.A.
2.3	Sex (child/married person)	1. Male; 2. Female; 0. N.A.
2.4	Age (mother/married person)	00-99. Age; xx. N.A.
2.5	Type of locality (mother/marriage)	First two digits of C.B.S. type of settlement code:

Serial No.	Name of field		Codes
2.5	Con.		xx. N.A. 75 Qibbuz 99. Marriage abroad
2.6	Sub-district (mother/marriage)		C.B.S. standard code; xx. N.A. 99. Marriage abroad
2.7	Number of persons in network		N.A. xx.
2.8	Number of households in network		N.A. xx.
2.9	*Household Configuration in Network*		
2.9.1	Event person		r_i = 1, . . . *i*. Minimal line number of persons listed in same HH.
	Births	*Marriages*	
2.9.2	Mother	Father	r_i = 0. Person not listed precedes listed person.
2.9.3	1st sister	Mother	
2.9.4	2nd sister	1st sibling	r_i = B. Person not listed - subsequent to last listed person.
	•	•	
	•	•	
	•	•	
2.9.9	7th sister	6th sibling	r_i = X. N.A.
2.9.10	8th sister	7th sibling	r_i = 01, . . . , *i*. Minimal line number of persons listed in same HH.
•	•	•	
•	•	•	
•	•	•	r_i = 00. Person not listed - precedes listed person.
•	•	•	
•	•		
2.9.16	14th sister	13th sibling	r_i = BB. Person not listed - subsequent to last listed person.
			r_i = XX. N.A.

Serial No. of network _____

Basic survey	Evaluation study

Type of event, Type of report

	Basic Survey	Eval. study
Name of event person		
Month of event		
Sex of event person		
Age of event person		
Type of settlement		

Basic Survey	Evaluation study	
		Total persons in household
		No. of persons in household aged 14+
		Sex of H.H.H.
		Year of birth of household head
		Marital status of H.H.H.
		Continent of origin of H.H.H.
		Year of immigration of H.H.H.
		Education of household head

Was the event reported in the evaluation study? _____

Is the event person listed on the envelope of the event household? _____

	Evaluation study	Basic Survey
Number of persons in network		
Event person		
Father (marriages)		
Mother		
Siblings		
Total		

Number of households in multiplicity network

Basic Survey	Evaluation study	Event household	Event H.H. + parents	All

Event Reporting by

	Basic Survey	Evaluation study
Over-report	+	+
Correct	=	=
Under-report	-	-

APPENDIX VI.

THE MODEL FOR ESTIMATION OF ERROR COMPONENTS

As pointed out in section 3.2 (chapter 3), estimation of the error components was carried out on the basis of a theoretical model, full details of which are specified in Nathan's article (1976). In this appendix the model is explained, as far as possible, in nontechnical terms. In addition, the major underlying assumptions of the model are specified, and their relative importance and the possible effect of departures from the assumptions are discussed.

The model is divided into two parts, dealt with separately. In the first part expressions are developed for the various components of the mean square error of multiplicity estimates as functions of certain model parameters. In the second part a method is specified for estimation of the error components, on the basis of an evaluation survey, by estimating the basic model parameters or combinations of them. For reference purposes, the assumptions are numbered consecutively within each part, e.g., (1.1).

1. Decomposition of the Mean Square Error

The mean square error of the multiplicity estimate of the total number of events (or of any subtotal) is decomposed by the basic response error model of Hansen, Hurwitz, and Bershad (1961). The model disaggregates the mean square error into the sum of the sampling variance, the sampling bias, the response variance, and the response bias. It relies on a basic assumption:

(1.1) The values of variables measured in the survey are a single realization of the variables at one trial, out of a conceptual infinite series of trials, and may differ from their theoretical "true values."

For the multiplicity survey estimators, this assumption is made with respect to (a) the indicator variables specifying whether or not a report of an event is made by a specified type of relative within a given household (e.g., for births by the event household, by the mother's mother, or by the household of the mother's sister), and (b) the multiplicity of the event reported by that household.

All potential reporting of events (i.e., reported at a given trial by at least one household) relates either to "true events" (which occurred in the survey population during the reference period), or to "nonevents." Both true events and nonevents are either reported or not reported at any given trial. The following assumption is implied:

(1.2) There is no "counting rule bias" as defined by Sirken (1973); i.e., all true events (and all nonevents) are connected by the counting rule to one or more households and have a positive probability of being reported by at least one relative, over all possible trials, if any of their network households is in the survey.

As indicated in chapter 4, section 4.2, this assumption may be considered closely approximated for all births and marriages of Jews in Israel during the reference period. According to the actual definition of the population, specified in chapter 2, section 2.3, the assumption holds exactly.

With respect to reporting of the indicator variable, it is assumed that:

(1.3) The probability of (correctly) reporting a true event and the probability of (erroneously) reporting a nonevent, depend only on the degree of relationship of the reporting household and not on the specific event or reporting household or on the multiplicity. These probabilities are assumed to be positive.

This apparently strong assumption essentially implies the use of an average error-reporting probability over large subgroups of reports. The model is also applicable if average probabilities for much smaller subgroups (within which the probabilities may be assumed to be close) are used. However, a much larger evaluation survey would be required to estimate these probabilities.

With respect to the reports on multiplicity (the number of households in the network), it is assumed that:

(1.4) For each of the potential reporting households, the average of reports on multiplicity of the event, over all trials, is equal to the true multiplicity of the event.

(1.5) The relative response variance of reports on multiplicity over all trials is constant for all events and for all households reporting them.

These two assumptions, which are made to simplify the model, can be shown to have only a very small effect on the final results.

With respect to the sample design, it is assumed that:

(1.6) Sampling is simple random without replacement, independently of the trials.

As pointed out in chapter 2, section 2.3, sampling for the Israeli survey was random-systematic in urban areas and clustered in rural areas (about 6 percent of the survey population). Therefore, assumption (1.6) can be considered to hold approximately for the Israeli survey. It implies zero sampling bias and zero correlation between sampling and response deviations.

From assumptions (1.1)-(1.6) it can be shown that the response bias (the difference between the expected value of the estimate, over all trials and samples, and the true value) can be expressed, approximately, as a function of:

(a) The reporting probabilities defined by assumption (1.3).

(b) The relative response variance (over all trials) of reports on multiplicity, as defined by assumption (1.5).

(c) The contributions of reports by each type of relative's households, to the total numbers of true events and of nonevents.

The response bias can be broken down into overcoverage bias, undercoverage bias, and the contribution of multiplicity reporting variance, (b), as estimated in table F.

In order to express the response and sampling variances, a further assumption is necessary:

(1.7) Response deviations of contributions of resports are uncorrelated for different households and for different events.

This assumption implies that the correlated response variance vanishes. In fact its contribution to the response variance is believed to be small in the Israeli survey, since response deviations seem to be primarily due to pure respondent effects rather than to enumerator effects (which in general are the major contributor to the correlated component).

With assumption (1.7), both the response variance and the sampling variance are shown to be functions of parameters (a) to (c), above, and of certain moments of the reciprocals of multiplicities for each type of relative's households, for true events and for nonevents.

2. Estimation of the Error Components

In order to estimate the error components, estimates of each of the parameters that appear in the expressions for the error components must be obtained. Certain combinations of these parameters can be estimated directly from the basic survey by sample moments of the reciprocals of reported multiplicities.

For other parameters, however, the evaluation survey has to be used. To do this, it is assumed that:

(2.1) A simple random sample of events reported by a given type of relative's households is selected out of all such reports in the basic survey.

As noted in chapter 3, section 3.2, sampling for the evaluation survey was basically by a

quota sample selected from events reported in the basic survey in the sequence in which questionnaires reached the central office. However, since the enumeration weeks in the LFS are randomly assigned, the evaluation survey sample was approximately random. The exclusion of events with a reported multiplicity of 1 (only about 5-6 percent) should have only a small effect, especially since by assumption (1.3) the estimated error probabilities of events do not depend on the multiplicity.

It should be emphasized that the fact that the evaluation sample is selected only from *reported* events and that no evaluation is carried out with respect to households who did not report an event does not affect in any way the possibility of unbiased estimation of error probabilities or other model parameters. This is so since assumptions (1.2), (1.6), and (2.1) ensure that each true event and each nonevent has a positive *a priori* probability of being included in the evaluation survey. This inclusion probability is a function of the true multiplicity, of the reporting-error probability, and of whether it is in fact a true event or a nonevent.

The selection of households in which the evaluation survey is carried out is assumed to be as follows:

(2.2) For each reported event selected for evaluation, one of the households of a specified type of relative in its reported multiplicity network is randomly selected.

Departures from this assumption, which sometimes also affected assumption (2.1), arose from the fact that incomplete addresses and some addresses in outlying localities could not be used. (See details in chapter 3, section 3.3.)

The results of the comparison between reports in the basic survey and in the evaluation survey and the adjudication of differences are assumed to ensure that:

(2.3) For each event reported in the basic survey and selected for evaluation, a correct determination can be made as to whether it is a true event or a nonevent.

For events reported by both surveys (and therefore determined as correctly reported true events), this assumes that the probability of a nonevent being reported by two different households is negligible. Since for events not reported by the evaluation survey, the LFS listing of the putative event household was used to decide which survey was correct, assumption (2.3) may be affected by the possibility of dependence between event reporting and the LFS listing. The possibility of such dependence and its possible effect on the evaluation results is discussed in appendix VII.

With respect to multiplicity reporting, it is assumed that:

(2.4) The reports on multiplicity (number of households in the network) for the same event by the basic survey household and by the evaluation survey household are independent.

Under the above assumptions, all the terms of the overcoverage bias and of the undercoverage bias can be estimated. The estimates are based on ratios of weighted counts of correctly and incorrectly reported true events and nonevents for each type of relative's households (the weights being functions of the multiplicities and of the selection probabilities) and on the average of squared differences in reported multiplicity. In addition, all but minor terms of the sampling variance and of the response variance can be estimated in the same way.

APPENDIX VII.

EFFECT OF DEPENDENCE ON EVALUATION RESULTS

The estimation of overcoverage and undercoverage biases was made on the basis of comparisons between basic survey reports and evaluation survey reports, as specified in section 3.2. The adjudication of the "true" status of events reported in the basic survey but not reported in the evaluation survey was made on the basis of the LFS listing form of the event household. This decision criterion relied on an inherent assumption of independence between event reporting for the basic or evaluation survey and the completion of the LFS listing form. These operations were not always independent. In fact, they were dependent if the LFS listing was first recorded in the same interview as the basic or evaluation survey and, for marriages, in event households, since they were recorded in the multiplicity questionnaire on the basis of the LFS listing. In this appendix the possible effects of such dependence on the estimation of overcoverage and undercoverage biases are evaluated and the possibility of dependence considered for the various cases.

The analysis is summarized in table I, which relates to the evaluation of events reported in the basic survey but not in the evaluation survey.

Analysis is separate according to whether the

Table I. Possible effects of dependence on evaluation decisions for events not reported in the evaluation survey but reported in the basic survey

Analysis and type of household	Reporting household and LFS listing in event household			
	Event household in basic survey, relative's household in evaluation survey		Relative's household in basic survey, event household in evaluation survey	
	Event recorded in LFS listing (a)	Event not recorded in LFS listing (b)	Event recorded in LFS listing (c)	Event not recorded in LFS listing (d)
Evaluation decision:				
(1) Event household	Correctly reported	Overreported	Underreported	Correctly reported
(2) Relative's household	Underreported	Correctly reported	Correctly reported	Overreported
Possible effect of dependence:				
(3) Event household	Overcoverage underestimated	Overcoverage overestimated	Undercoverage overestimated	Undercoverage underestimated
(4) Relative's household	Undercoverage overestimated	Undercoverage underestimated	Overcoverage underestimated	Overcoverage overestimated
(5) Possibility of dependence[1]	Possibly dependent	Probably independent	Probably independent	Probably dependent

[1] See discussion below.

event household was in the basic survey and the relative's household in the evaluation survey—columns (a) and (b)—or the relative's household was in the basic survey—columns (c) and (d). Analysis is also according to whether the event was recorded in the LFS listing form—columns (a) and (c)—or not recorded—columns (b) and (d).

The resulting evaluation decisions are noted in lines (1) and (2) as they relate to the event household and to the relative's household, respectively. It should be noted that the decision on underreporting always relates to the evaluation survey household, i.e., to the relative's household for column (a) and to the event household for column (c). The decision on overreporting always relates to the basic survey household, i.e., to the relative's household for column (d) and to the event household for column (b).

The possible effect of dependence between event reporting by the event household and the completion of the LFS listing form as it relates to the estimation of overcoverage and undercoverage biases is given in lines (3) and (4) for event households and for relatives' households, respectively.

The effect of dependence would be to reverse the evaluation decision. Thus, for column (a), if the agreement of the LFS listing with the event report is due to dependence, the decision that it is a true event (underreported by the relative's household) might be erroneous (i.e., it may be a nonevent overreported by the event household). This implies that dependence in this case could result in overestimation of the undercoverage bias of relatives' households and in underestimation of the overcoverage bias of event households, while other biases are not affected. Similar considerations lead to the evaluation of possible effects for the remainder of the cases.

Next, the possibility and degree of actual dependence between the LFS listing and the completion of the multiplicity survey questionnaire in the event household is considered in line (5). For cases (b) and (c), the fact that the LFS listing *disagrees* with the reporting of the event household in the multiplicity survey must be considered as *prima facie* evidence of independence (or perhaps negative dependence, which obviously is not relevant).

On the other hand, if the LFS listing agrees with the event household's report, a degree of dependence is possible. If the event household was in the evaluation survey—column (d)—dependence is almost certain, since the LFS listing was then carried out at the same interview. If the event household was in the basic survey—column (a)—there is dependence for marriages, since the enumerators were instructed to record marriages to persons in the household from the LFS listing form (Q. 13). For births, dependence is almost certain for all households in the new panel (first enumerated together with the multiplicity survey) and for repeated panels if the previous interview occurred before the birth, since in both cases listing of the newborn baby in the LFS would be carried out in the same interview as the multiplicity survey. However, for households in repeated panels, if the previous interview occurred after the birth, the reports would probably be independent, since they were made at different interviews. The status of these cases—column (a), births reported by the event household in the basic survey and in the LFS listing but not by the relative's household in the evaluation survey—were checked according to the above criteria. Out of 14 such cases, 6 pairs of reports are probably dependent, 4 are probably independent, and for 4 no determination could be made.

Taking into account both the possibility of dependence and its possible effect, it can be seen that for the relevant cases—columns (a) and (d)—the overall effect is the possible underestimation of both types of bias for reports by event households and the overestimation of both types of bias for reports by households of relatives. This implies that, overall, biases for the multiplicity rule (to which reports by households of relatives contribute importantly) are overestimated, while biases for the conventional rule are underestimated. Thus the effect of the possibility of dependence on the comparison of counting rules would be to further enhance the superiority of the multiplicity rules to the conventional rule.

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