IMPROVING AIDS BEHAVIORAL SURVEYS USING T-ACASI

A. SPECIFIC AIMS.[1]

A growing body of empirical data indicates that increasing the privacy of the interview context can dramatically increase the validity of survey measurements of AIDS-related and other sensitive behaviors (see Section B). For personal visit surveys, self-administration, using either paper-and-pencil forms or new computer-assisted self-interviewing systems (CASI), eliminates the need for respondents to reveal sensitive information to a human interviewer. These methods have been shown to substantially increase the likelihood that respondents will report engaging in sensitive, stigmatized, or illicit behaviors during a personal visit survey and to decrease the likelihood that they will report engaging in socially desirable or normative behaviors.

Until recently, telephone surveys could not offer a similarly private interview context. This has been an unfortunate limitation of telephone surveys, and it may have compromised our understanding of the prevalence and patterns of AIDS-related sexual and drug use behaviors, since the largest surveys of AIDS-related behaviors in North America, Europe, and Australia have used telephone survey methods.

A recent advance in telephone survey methods, telephone audio computer-assisted self interviewing (T-ACASI), now offers AIDS researchers the option of providing a fully private interview context for telephone survey measurements of sexual and other sensitive behaviors. In a T-ACASI survey, a human interviewer is used to screen and recruit eligible subjects. However, once the subject has been recruited and has completed nonsensitive portions of the survey, the phone call is transferred to the T-ACASI system and computer-controlled, pre-recorded questions are read to the subject. The subject provides responses by pressing keys on a touch-tone telephone. Evidence (see Section C) from large-scale, nonsensitive surveys using T-ACASI technology operated by the Bureau of Labor Statistics and from a pilot T-ACASI survey of sexual behavior conducted in Cook County, Illinois and Wake County, N.C. by the present investigators indicate that use of T-ACASI procedures is feasible. Furthermore, data from our pilot T-ACASI survey indicate that:

- respondents report feeling more comfortable providing sensitive information about their sexual histories to a T-ACASI system rather than to a human interviewer, and
- respondents are more likely to report engaging in sensitive, stigmatized, or illicit behaviors and less likely to report desirable, normative behaviors during a T-ACASI interview than during a standard telephone interview with a human interviewer.

The present application seeks support for a two-stage research project to asses the impact of T-ACASI methodology on survey measurement of AIDS-related and other sensitive behaviors. In Phase 1, we will assess the comparative validity and reliability of data provided during a T-ACASI interview to that obtained from a standard telephone interview using a human interviewer in a national probability of adults in the United States.[2]

The available empirical evidence leads us to suspect that T-ACASI should yield substantially higher rates of reporting of sensitive behaviors without any reduction in measurement reliability. In Phase 2, we will contrast the results obtained from T-ACASI interviewing to results obtained using the considerably more expensive, personal visit survey procedures that afford similar levels of privacy (i.e., an in-person Audio-CASI survey).

Specific aims of this research program are the assessment of:

- the extent, if any, to which new T-ACASI technology may increase the validity and reliability of telephone survey measurements of AIDS-related and other sensitive behaviors;
- the extent to which T-ACASI telephone survey methods may provide a reasonable substitute for more expensive, in-person survey measurement procedures; and
- the costs and barriers to adoption of this technology (assuming that the technology is found to yield improved measurements).

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[1] In this proposal, we have drawn freely upon the PI and Co-PIs contributions to jointly authored works, including Turner, Forsyth, et al., 1993; Miller, Turner, and Moses, 1990: Ch. 6; Turner, Miller, and Catania, 1995; Turner, Danella, and Rogers, 1995; Catania et al., 1994. With the exception of this note, we do not note the numerous places in which we have paraphrased or excerpted from these works. In addition, some descriptions of standard RTI facilities and operational procedures have been drawn from RTI institutional documents.

[2] In September 1995, our research team applied for a competitive supplement to MH54320 (J. Catania, PI) in order to add T-ACASI data collection to the upcoming Gay Urban Mens Survey. That application was favorably reviewed by the NIH study section, receiving a priority score in the 6th percentile. The Stage 1 experiment proposed in the present application parallels the GUMS supplement. However, the GUMS is designed to survey a probability sample restricted to ***gay men residing in urban areas.*** In its review of our GUMS supplement application, the study section noted that T-ACASI had significant potential to improve the validity of survey measurements of sensitive behaviors, but it also noted that the findings from the GUMS research would not necessarily generalize to women and heterosexual men. The Stage 1 experiment proposed in this application will fill this important gap.

In addition to these basic scientific aims, the proposed research will have an additional, immediate and important scientific application. The survey instrument we will use in this research will replicate that used in the major NIH-funded program of telephone surveys of AIDS-related behaviors: the National AIDS Behavioral Survey (NABS) research program (Catania et al., 1992a; 1994).[3] By replicating measurements from these surveys, we will also be able to make direct assessments of the extent of the biases that may affect these widely used sources of information on AIDS related behaviors in the United States.

B. BACKGROUND AND SIGNIFICANCE.

B.1. Central Role of Telephone Surveys in Shaping our Present Understanding of HIV-Risk Behaviors.

The need to survey the sexual and drug use behaviors of large, representative samples of the U.S. population has long been recognized as a prerequisite for understanding and retarding the spread of HIV. Presently, for HIV, the most deadly of sexually transmitted pathogens, all means of controlling its spread rely upon our ability to influence human behaviors. Moreover, even if a fully effective vaccine or therapy were to become available, the history of other STDs (Brandt, 1987) suggests that the need for protective behavior change will persist, as will the need for a reliable technology for monitoring the persistence of risk taking and behavioral changes.

These arguments for survey research on sexual behavior research have been echoed by groups reviewing AIDS research needs, including numerous committees of the National Academy of Sciences and the Institute of Medicine (1986, 1988; Auerbach et al., 1994; Turner, Miller, and Moses, 1989; Miller, Turner, and Moses, 1990), Ronald Reagan's Presidential Commission on the HIV Epidemic (1988), the National Commission on AIDS (1993), the General Accounting Office (1988), and others. Among the major reasons cited to conduct such surveys are that:

- In order to understand the spread of the HIV epidemic (and its potential for spread) it is necessary to know some simple "facts" about sex, such as the size of key population groups, including men who have sex with men and heterosexuals with multiple partners; rates of new partner acquisition, and so forth.
- In order to induce behavior change, it is necessary to understand what motivates and constrains those behaviors that risk transmission of HIV and how behavior change can be made more attractive.
- In order to measure the effectiveness of prevention efforts, it is necessary to have baseline data and data at regular intervals to assess whether protective change is occurring in the patterns of sexual behavior in the population.

Population-based surveys of sexual behaviors provide estimates of the prevalence and patterns of behaviors that affect the velocity and breadth of spread of HIV and other STDs. These same surveys provide information about the factors that shape risk-taking behaviors and thus provide hypotheses that can later be experimentally tested as to the sorts of behavioral interventions that may be effective in motivating individuals to change risk behaviors. Furthermore, continued surveillance of behaviors is needed to monitor the persistence of change and risk taking as new people enter the at-risk population, the current at risk population ages, and secular events modify human response to this epidemic.

The National Institutes of Health has responded to the need for such data by funding a number of major research projects to survey AIDS-related sexual and drug use behaviors in the U.S. population. Because of the substantial costs associated with sending field interviewers to tens of thousands of households across the country, the largest surveys of the adult population of the United States have used telephone survey techniques. The National AIDS Behavioral Survey research program (Catania et al., 1992a) conducted telephone interviews

[3]The most recent phase of this research program adopted a new name: the Family of AIDS Behavioral Surveys (FABS). For simplicity, we will refer to the original NABS surveys and the new FABS surveys as the National AIDS Behavioral Survey (NABS) research program.

TABLE 1. Selected major surveys of AIDS-related behaviors conducted by telephone.

SURVEY	N	POPULATION	REFERENCE
1990 National AIDS Behavioral Survey (NABS)	10,630	U.S. adults, ages 18-75	Catania et al., 1992
1995 National AIDS Behavioral Survey (NABS)	6,400	U.S. adults, ages 18-49	Catania et al., 1994
Analyse des Comportements Sexuels en France (ACSF)	20,055	French adults, ages 18-69	ACSF Investigators, 1992
New Zealand Partner Relations Survey	2,361	New Zealand adults, 18-54	Davies et al., 1993
Los Angeles Men's Survey	1,610	Los Angeles males, 18+	Montgomery et al., 1991
Australian Survey of Sexuality and Menopause	2,001	Australian women, 45-55	Dennerstein et al., 1994
California AIDS Survey (Communications Technologies)	2,012	California adults, ages 18+	Miller et al., 1990

with 10,630 respondents in 1990, and it is presently conducting telephone interviews with a new probability sample of 6,400 American adults (Catania et al., 1994) . These NIH-funded telephone survey efforts are substantially larger than surveys of adults' AIDS-related behaviors that have been undertaken using personal visit survey methods.[4] Because of the great efficiency of telephone survey methods, the NABS research program has been able to survey samples that were sufficiently large to permit analyses of key subpopulations (e.g., persons with multiple new sexual partners in the past year). Samples of comparable size would be almost prohibitively expensive to conduct using personal visit surveys of national samples. Telephone surveys have also played a crucial role in shaping our understanding of AIDS-related behaviors in other nations. As Table 1 shows, major AIDS surveys in France, New Zealand, and Australia have also used telephone survey methods.

In the United States, the NABS research program has been one of the most important sources of data on the prevalence and patterns of AIDS-risk in the U.S. population (Catania et al., 1992a; Binson et al., 1993; Dolcini et al., 1993; Grinstead et al., 1993; Sabogal et al., 1993; Peterson et al., 1993; Berrios, 1993; Choi et al., 1994). Findings from the 1990 NABS have helped focus public health attention on the extent and variability of risk taking across subpopulations. Among the major conclusions drawn from these data are that:

- Risk is more prevalent in major metropolitan areas; 15 to 31 percent of heterosexuals nationally and 20-41 percent in high risk cities reported at least one HIV risk factor.
- Young, unmarried adults have the highest levels of HIV related risk behaviors (e.g., 72 percent reported multiple sexual partners in the past 5 years and 35 percent in the past year).
- Respondents reporting multiple sex partners are more likely to be male, African American or White, single, highly educated, and young.
- Nationally, condom use is low, even among subpopulations with elevated risks of encountering HIV. Only 17 percent of persons with multiple sex partners and 13 percent with risky sex partners reported using condoms all the time.
- Contrary to rational expectations, levels of condom use decrease as the number of sexual partners in the past year increases.
- Nationally, the use of HIV antibody testing is low among those at risk of HIV infection. Less than 40 percent of heterosexuals at risk for HIV had been tested.

Because the NABS and similar telephone surveys have played a central role in shaping our understanding AIDS-risk behaviors, there is considerable cause for concern over emerging evidence (see Section B.3 and B.4) suggesting that interview modes that require respondents to disclose sensitive, stigmatized, or illicit behaviors to a human interviewer are subject to nontrivial reporting biases. In particular, requiring respondents to disclose to a human interviewer that they have engaged in such behaviors appears to substantially diminish the willingness of respondents to report engaging in these behaviors.[5] If such biases were to affect telephone surveys, such as the NABS, it might substantially alter our estimates of the prevalence and patterns of HIV-risk behaviors in the U.S. population.

B.2. Scope of the Epidemic

[4] So, for example, Tanfer's (1993) National Survey of Men (NSM) conducted in-person interviews with 3,321 men and the University of Chicago team (Laumann et al., 1994) interviewed 3,432 adults in their National Health and Social Life Survey. The lone exception to the generalization that in-person surveys of adult sexual behavior have been conducted on much smaller sample sizes than comparable telephone surveys is the British National Survey of Sexual Attitudes and Lifestyles (Johnson et al., 1994) which conducted in-person interviews with 18,876 respondents.

[5] Here and elsewhere through our discussions, it should be kept in mind that an analogous phenomenon has been demonstrated to occur in the reverse direction for desirable or normative behaviors (e.g., reporting of consistent condom use). The available evidence suggests that more private methods reduce reporting of such normative behaviors.

Federally-funded efforts to monitor AIDS-risk behaviors in the total population seek to understand and anticipate the potential for spread of HIV beyond those subpopulations in which HIV was initially concentrated. The false notion that HIV would remain within these initial populations ignored the progressive aspects of transmission that are associated with: (1) untreatable viral infections for which there are no vaccines, and (2) transmission probabilities that are strongly influenced by the long periods of time in which infected individuals may remain in the population as a source of contagion, in many cases asymptomatic and unaware of their infectivity.

Given the lag time between infection and appearance of symptoms, the full extent of HIV infection that already exists among heterosexuals in the United States may not be apparent for many years to come (Curran et al., 1985; Goedert et al., 1986; Anderson, 1993). Shifts over time in the distribution of AIDS cases across risk categories point to the need for a broader understanding of the antecedents and determinants of transmission in the general population. The percentage of U.S. AIDS cases related to heterosexual contact increased from 1 percent in 1983 to 4 percent in 1988 (Holmes et al., 1990), 6 percent in 1992 (CDC, 1992) and 10 percent in 1994 (CDC, 1995). Among the 58,428 cases of AIDS cases reported in women by the end of 1994, 38 percent were related to heterosexual contact (CDC, 1995).

While no group is theoretically exempt from HIV, existing data, albeit imperfect, indicate that the burden of infection is not uniformly distributed throughout the population. African American and Hispanic men and women continue to be more likely to be diagnosed with AIDS than white men and women (CDC, 1990; Bakeman et al., 1986a; Bakeman et al., 1986b; Bakeman et al., 1987; Holmes et al., 1990; CDC, 1995). 1994 was the first year in which CDC surveillance found the majority (53 percent) of AIDS cases in men were among Black and Hispanic males; and in this year, Black and Hispanic women were grossly over represented in AIDS surveillance statistics (57 percent and 20 percent of cases in women respectively) (CDC, 1995). Hispanics and African Americans also have a relatively high rate of other STDs including ulcerative STDs, which are cofactors for HIV disease (CDC, 1991).

The dynamic nature of public health problems posed by HIV and other STDs calls for ongoing surveillance efforts to track not only the infections but the behaviors and related factors associated with their transmission. Such data are critical to understanding the dimensions and distribution of risk as well as targeting, tailoring, and evaluating intervention efforts. Some data need to be updated; others need to be generated de novo. For example, while drug use, violence and STDs, including HIV, appear to overlap, less is known about the distributions of these problems in the national population, a gap which resulted in an Institute of Medicine recommendation for the Public Health Service to "monitor and respond to concurrent epidemics (such as drug use, violence, and infectious diseases) that will alter the course of the HIV epidemic" (Auerbach et al., 1994: p.6). Investigators also have called for additional data from the general population that address the range of behaviors associated with the transmission of HIV -- including substance use and sexual risk taking, but note both political and methodological impediments to gathering such data (Cottler et al., 1990).

In the following sections (B.3 - B.5), we describe evidence that strongly suggests that measurement techniques which require respondents to disclose sensitive, stigmatized, or illicit behaviors to a human interviewer introduce non-trivial negative biases in estimates of the prevalence of these behaviors. This evidence has disturbing implications for past and ongoing research programs using survey methods to assay the prevalence and patterns of AIDS-risk behaviors in the U.S. (and other) populations. After reviewing this evidence, we will describe a new measurement procedures for telephone surveys that hold promise of reducing these biases (Section C). We will subsequently propose a research program to evaluate the feasibility and impact of adoption of this technology on telephone survey measurements of AIDS-related and other sensitive behaviors (Section D).

B.3. Quality of AIDS-Related Survey Data: Issues and Concerns

As is the case with all surveys, the usefulness of AIDS-related surveys hinges upon data quality. However, participants in such surveys are asked to provide information about very intimate, sensitive, and sometimes illegal behaviors. AIDS researchers have found these behaviors compelling to understand but problematic to measure. Concerns persist regarding the quality of AIDS survey data. We know that adults typically underreport many sexual activities[6] and that there are important gender-related differences in the error structure of these data. In national probability surveys in the United States and Europe, for example, it has been found that men consistently report greater numbers of heterosexual partners than women (which is an algebraic impossibility) (Smith, 1992; Dolcini et al., 1993; Morris, 1993). Similarly, among individuals reporting risk factors for HIV and other STDs, men are more likely than women to report the use of condoms during heterosexual sex (Catania et al., 1992a). In both of these cases, the extent to which the gender difference reflects overreporting by men or underreporting by women is not known.

There is a complex array of factors that can distort survey measurements, but there are many fewer ways to independently corroborate the accuracy and reproducibility of these measurements of sexual behavior. To date, evidence of validity and reliability has largely been gleaned from reports by regular sexual partners, test-retest reliability studies,

[6] For reviews, see Miller, Turner, and Moses, 1990: Ch.6; Bradburn and Sudman, 1979; Catania et al., 1990; Turner, Danella, and Rogers, 1995. Also see Johnson et al., 1994; McQueen et al., 1989.

and independent replication of surveys on the same population.[7] A National Academy of Sciences' review of studies conducted prior to 1990 concluded that there is little question that surveys of sexual behavior can enlist the cooperation of research subjects and that resulting data can be consistent across surveys, across couples, and within subjects interviewed prospectively over time. Nevertheless, this review also noted that consistency does not guarantee accuracy, that there is ample evidence of error and bias in existing surveys of sexual behavior, and that such evidence should be of concern to investigators (Miller, Turner, and Moses., 1990: Ch.6).

A non-private interview procedure that requires subjects to report sensitive and illegal behaviors to a live interviewer invites concerns about response biases.[8] It is generally accepted by survey methodologists that biases in the reporting of illicit or stigmatized behaviors in general population surveys produces a net negative bias in estimates of the prevalence of these behaviors in the population. This occurs because the number of survey respondents who deny engaging in stigmatized or sensitive behaviors that they in fact have engaged in is expected to be larger than the number who falsely report behaviors that they have not engaged in (Catania et al., 1990; Miller et al., 1990; Turner, Lessler, and Gfroerer, 1992; Bradburn et al., 1979). Thus, increased rates of reporting of sensitive behaviors under more private survey conditions are interpreted to reflect a reduction in the reporting bias and thereby an increase in the accuracy of the measurements. For highly normative behaviors, the reverse is expected.

In the last two years, strongly suggestive evidence has become available concerning the magnitude of the biases that can afflict survey measurements of sexual behaviors that require disclosure to a live interviewer; we review that evidence below. It should be noted that we believe this evidence is properly termed "suggestive" rather than conclusive for our purposes because: (1) most of the evidence is derived from studies of sensitive behaviors other than sexual behaviors, and (2) the interpretation of the evidence must rely upon the assumption that increased reporting of a sensitive or stigmatized behavior is, in fact, less-biased reporting of this behavior.

B.4. Evidence of Bias in Measurement of AIDS-Related Behaviors Made in Personal Visit Surveys

A number of recent studies provide evidence of self-disclosure bias from in-person surveys of sensitive behaviors. These studies also demonstrate that survey measurements are dramatically affected by level of privacy afforded by the interviewing mode. Results from a large-scale, randomized experiment embedded in the National Household Survey of Drug Abuse indicate that measurements made with self-administered questionnaires (SAQs) yielded higher estimates of the prevalence of illicit drug use than those made using interviewer-administration of the same questionnaires (IAQs) (Turner, Lessler, and Devore, 1992). The relative advantage of SAQs in encouraging more complete reporting of drug use appears to be a direct function of the sensitivity of the behavior being reported. Thus, mode effect was greatest for reporting of cocaine use---particularly recent cocaine use. The SAQ yielded estimates of the prevalence of recent cocaine use that were 2.4 times higher than estimates obtained with IAQ. This advantage was less pronounced for reporting of marijuana use, and it was almost nonexistent for reporting of alcohol use by adults.[9] Recent analyses of a parallel experiment embedded in the National Longitudinal Survey of Youth also found SAQs to yield more frequent reports of illicit drug use than IAQs (Shober et al., 1992).

Dramatic mode effects have also been seen when women are offered a SAQ to report on abortion. Jones and Forrest (1992) have used data from abortion providers to estimate the extent of bias in abortion reporting in three major national survey programs.[10] They found substantial biases in abortion reporting in all of these surveys. Estimates derived from women's self reports in the 1988 National Survey of Family Growth (NSFG), for example, were found to include only 37 percent of the abortions reported annually by abortion providers during 1984-87. In the 1988 NSFG respondents were also given a SAQ to offer them a second, private, opportunity to report past abortions (London and Williams, 1990). Jones and Forrest report that use of this SAQ increased women's reporting of abortions from 39 to 71 percent of the level reported by abortion providers.

The aforementioned caveats and data do not warrant -- in any way -- dismissal of extant self-reported data. Rather we believe they should motivate a careful methodological assessment of the extent to which self-disclosure biases may afflict measures of sensitive behaviors and investigation of methods to reduce such bias.

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[7] Clark and Wallin, 1964; Levinger, 1966; Seage et al., 1989; Jacobson and Moore, 1981; Blumstein and Schwartz, 1983; Coates et al., 1988; Kahn et al, 1988; see reviews in: Miller, Turner and Moses, 1990, Ch. 6; Turner, Miller, and Moses, 1989, Ch. 2; Turner, Danella, and Rogers, 1995.

[8] Self-disclosure theory (Jourard, 1971; Catania et al., 1986; Dindia and Allen, 1986) posits that people will disclose more honestly and with more detail to those with whom they feel most comfortable. Respondents scoring higher on scales of self-disclosure in interview situations report higher levels of sexual behavior and lower rates of item nonresponses (Catania et al., 1986; Herrold and Way, 1988). It should also be noted that some subpopulations may find it more difficult to disclose information than others. As Mason and colleagues (1995) note, Spanish-speaking Latino men appear more likely than English-speaking Latinos or Whites to withhold information on HIV-positive serologic status as well as gay or bisexual orientation. Such differences in response bias across populations distort prevalence estimates for subpopulations and can engender spurious bivariate and multivariate associations due to correlated response errors.

[9] However, even for alcohol use, the SAQs appeared to encourage more frequent reporting of alcohol use by 12- to 17- year olds---a group for whom alcohol use is illicit. For this group, estimates of the prevalence of recent alcohol use were 1.4 times higher when self-administered questionnaires were used.

[10] NSFG, National Longitudinal Survey of Youth, and Kantner and Zelnik's national surveys of young women.

B.5. Evidence of Bias in Measurement of AIDS-Related Behaviors Made in Telephone Surveys

While economy argues for use of telephone survey methodology to collect AIDS behavioral data on the population, such surveys may be vulnerable to serious biases. Two major studies have reported comparisons of prevalence estimates of illicit drug use obtained from interviewer-administered telephone surveys versus those obtained with self-administered questionnaires (during in-person surveys). (Table 2 summarizes the relevant results.) Gfroerer and Hughes (1992) compared results from the 1988 National Household Survey of Drug Abuse (an in-person survey using self-administered questionnaires) to results from a 1988 national telephone survey conducted for the Food and Drug Administration (FDA). The FDA survey used questions modelled on the NHSDA; and Gfroerer and Hughes went to considerable lengths to match the composition of the two samples.[11] With relatively large samples (Ns=5,018 and 1,965), Gfroerer and Hughes found that the self-administered questionnaires yielded significantly and substantially higher estimates for the reporting of illicit drug use. The relative increase in estimated prevalences ranged from 33 percent for reporting any lifetime use of marijuana to 121 percent for reporting use of cocaine in the past 12 months.

Aquilino (1994) has recently reported similar results from an experiment in which respondents were randomly assigned to different survey modes. The experiment was embedded in a probability survey of households in the 37 largest metropolitan areas in the United States. To remove the impact of differences in recruitment by survey mode, the experiment began with an initial in-person household contact for the purpose of screening and recruitment of respondents. Respondents recruited in this manner were then randomly assigned to a survey mode.[12] Aquilino's results are generally consistent with those obtained by Gfroerer and Hughes, although the estimated prevalences of drug use are higher, and there is some variation in the observed mode differences. Overall, Aquilino found self-

[11] For example, households without telephones were excluded from the NHSDA database and re-editing and post-stratification weighting were used to make the data processing and samples composition for the surveys comparable.

[12] Households without a telephone were excluded from analyses.

Insert Table 2 here

administered surveys produced higher prevalence estimates for most measurements of drug use. So, for example, self-administered surveys produced a 63 percent relative increase in the reporting of marijuana use and a 32 percent relative increase in the reporting of any lifetime cocaine use. Aquilino also reported similar analyses for his oversample of 532 African-American respondents. While the small sample sizes in this subanalysis make the estimates rather unstable, there is highly suggestive evidence that African-American respondents were particularly sensitive to the survey mode and much more likely to report drug use in a self-administered survey versus a telephone survey.

B.6. Significance of the Proposed Research

T-ACASI technology offers a unique and important opportunity to reduce the biases that afflict telephone survey measurements of AIDS-related and other sensitive behaviors. In the past, telephone surveys required respondents to disclose sensitive information to a human interviewer. T-ACASI technology affords complete privacy to respondents, which, we hypothesize, should reduce the reporting biases described in the preceding section. Data from a recently completed pilot test lends substantial support to this hypothesis (see Section C below.)

In addition to privacy, T-ACASI offers the traditional advantages of computer-assisted survey technologies, including computer-controlled branching through a complex questionnaire, automated consistency and range checking, and automatic production of data files. Furthermore, these technologies provide a completely standardized measurement system; every respondent hears the same questions asked in exactly the same way. Because questions and response categories are presented aurally to participants, traditional survey constraints concerning literacy and English language skills are eliminated.

The research proposed in this application can have important implications for survey and other research on AIDS-related behaviors. High quality data are needed to provide meaningful prevalence estimates of both risky and protective actions being taken by adults in the U.S. as well as the antecedents, determinants, and correlates of risky and protective behaviors. It is abundantly evident that these behaviors are not static and that changes in their distributions point to the need for continued surveillance. The findings from this research will be relevant not only to the conduct of cross sectional surveys, but they may have special applications in longitudinal data collection. T-ACASI measurement technology offers potentially important advantages in following cohorts and collecting data at different time points because it can support "call-in" data collection, offering respondents the possibility to participate in an interview at a time most convenient for them, thus decreasing the burden of participating in these efforts and perhaps decreasing attrition.

Findings from the proposed research may also of interest not only to individuals working in survey and measurement methodology, but also to researchers engaged in program evaluation and those interested in testing and refining behavioral theories related to the transmission of HIV and other STDs. Because the measurements included in this study address the antecedents, determinants, and dependent variables of several AIDS-related theoretical models, it will also be possible to assess the impact of measurement biases on the parameters of key behavioral models, such as harm reduction, social diffusion, reasoned action, and AIDS risk reduction models as well as social learning theory (Bandura, 1992; Catania et al., 1989a, 1990b; Fishbein, 1990). Given the likely importance of such data in refining AIDS programs, theories, and policies that redirect ever-shrinking resources, improving the quality of the underlying data can improve our understanding of the evolving needs of the general population as well as those of gender, racial, and ethnic minorities who have borne a major share of the burden of the HIV/AIDS epidemic.

C. PRELIMINARY WORK: DEVELOPMENT OF NEW SURVEY TECHNOLOGIES.

C.1. In-Person Audio-CASI.

During the past four years, researchers at RTI developed an audio computer-assisted self interview (Audio-CASI) technology to administer complex questionnaires in personal interview surveys (Turner, 1993; O'Reilly and Turner, 1992; O'Reilly et al., 1994; Cooley et al., in press). Using portable laptop computers, respondents listen to questions through headphones and enter answers by pressing labelled keys. The recorded audio component provides a good quality sound; it does not rely on synthesized voices and presents no significant delays in the playback of audio-delivered questions.[13] This private interview mode can be used with any respondent who can hear and speak; it does not require literacy in any language.[14]

[13] While this new technology bears a superficial resemblance to attempts to use Sony Walkmen to read survey questions (Camburn et al., 1991), it is, in fact, fundamentally different. In particular, A-CASI is computer-controlled and thus is capable of executing skip patterns, checking for out of range responses and inconsistencies across similar questions, and generating data files.

[14] Field experiments have found that it is possible to use Audio-CASI to interview subjects who speak only Korean or Spanish using field interviewers who speak only English (Turner, Rogers, et al., 1995; Hendershot et al., 1995). English speaking field interviewers carry cellular telephones when recruiting and interviewing Korean or Spanish speaking subjects. If field interviewers encounter problems rostering the household, recruiting subjects, or conducting the interview, they can call the office where Korean and Spanish speaking interviewers are available to assist in the process. This provides an efficient and cost-saving way of including subjects that would otherwise be excluded from national samples.

The in-person Audio-CASI interviewing system as developed by RTI is robust, and it can be used by the average field interviewer in a broad range of environments using light-weight, economical laptop systems. The audio software is integrated with a standard computer-assisted interviewing (CAI) software system. A number of support capabilities are provided through the PC function keys: the screen display can be blanked out or left on; the audio can be turned on or off; questions can be repeated; the respondent can back up through the questionnaire or can elect to refuse to answer a particular question.

Findings from our early Audio-CASI pilot tests (O'Reilly et al., 1994) indicated that:

- Audio-CASI technology is stable and can be used with a minimum of disruption to typical survey and research routines.
- Virtually without exception, respondents had no trouble using this new technology. This was true for both educated persons and those with substantial literacy problems, for the young and the old, for English speaking respondents and for those who spoke only Spanish or Korean.
- Even literate respondents reported preferring the new technology to paper-and-pencil SAQ.
- Although the pilot test samples were small, statistically significant increases were found in the reporting of some sensitive behaviors, such as abortion.

During 1995, RTI's in-person audio-CASI technology was adopted for use in two major national surveys: the NCHS-NIH National Survey of Family Growth (Cycle V; N = 10,000 females, ages 15-44) and the NIH-funded National Survey of Adolescent Males (New Cohort; N=1,741 males, ages 15-19). Preliminary data from these large-scale surveys indicate that Audio-CASI technology was well accepted by field interviewers and survey respondents in personal visit surveys, and it produced substantial increases in the reporting of sensitive behaviors. In the NSFG, for example, one-fifth of women increased the number of abortions or sexual partners when reinterviewed using Audio-CASI (Kinsey et al., 1995). Similarly, preliminary data from a randomized experiment embedded in the NSAM, indicated a significant, four-fold increase (from 1.1 to 4.7 percent) in the number of adolescent males who reported having male-male sexual contacts (Turner, Ku, et al., 1995) in Audio-CASI than in paper and pencil SAQ.

C.2. Telephone Audio-CASI (T-ACASI) Technology.

Early experimentation with T-ACASI was begun at the Bureau of Labor Statistics (BLS) during the late 1980s under the rubric of Touch-Tone Data Entry (TTDE) surveys (Werking et al., 1988a,1988b; Werking and Clayton, 1990; Clayton and Harrell, 1989; Clayton, 1991). In the early 1990s, RTI adopted an analogous touch-tone data entry procedure for call-in randomization of subjects for clinical trials. Such early systems are presently being used by BLS in monthly collection of data from the vast majority of the 350,000 establishments that respond to the Current Employment Survey. Methodological research indicates that error rates using TTDE technology are low (Phipps and Tupek, 1990), and TTDE has considerable advantages over other methods in terms of cost and timeliness of data reporting (Werking and Clayton, 1990). These initial T-ACASI applications have been limited to quite simple data collection tasks, typically involving only 5 to 10 questions asked without skip patterns or other tailoring of the survey instrument (Weeks, 1992). The Current Employment Survey, for example, requires only 1 minute and 45 seconds for the average respondent to complete. In addition, both the subject populations and the subject matter for these early T-ACASI efforts were limited to routine, nonsensitive reporting of commercial or technical data by trained, and highly literate respondents.

In the Winter of 1994-1995, scientists at RTI extended their in-person Audio-CASI system to allow it to conduct complex, call-in or call-out[15] T-ACASI surveys. This new system is an outgrowth --both in its motivation and its architecture -- of the technology RTI developed for in-person Audio-CASI surveys of sexual and contraceptive behaviors. These efforts resulted in the development of a software platform that fully integrated Audio-CASI and T-ACASI capabilities and that can be implemented on a wide array of hardware. In the T-ACASI system, PCs equipped with a hardware interface can handle incoming and outgoing calls. Under this system, the administration of a survey can be painlessly migrated from one environment to another by merely cloning the relevant software and digitized voice files.

C.3. Pilot Test of T-ACASI.

RTI's T-ACASI system became fully operational in February, 1995. Encouraging preliminary results are available from a pilot study which implements the questionnaire from the National AIDS Behavioral Survey under T-ACASI (NABS) (Catania et al., 1992a). Beginning on April 11, 1995, RTI scientists initiated a full-scale pilot study of T-ACASI technology. This study used a within-subjects experimental design to test respondents' reactions to this new technology.

[15] In a call-in survey, the respondent initiates the interview by calling a number that is answered by the T-ACASI interviewing system. In a call-out survey, a human telephone interviewer calls the respondent and subsequently transfers the call to the T-ACASI system.

Each respondent[16] in this experiment received a standard set of introductory NABS questions eliciting nonsensitive personal characteristics and attitudes by the live telephone interviewer. Then subjects received one-half of their sensitive NABS questions using standard telephone interview methods and the second half with T-ACASI technology. (The order of presentation is balanced across the experiment.) The NABS survey includes a wide variety of questions on sensitive issues including: heterosexual and same-gender sexual experiences, HIV serostatus, and drug use. At the conclusion of the interview, respondents were asked a series of questions by a human telephone interviewer evaluating their experiences with each mode of interviewing. (Attachment 1 presents a fuller description of the study and our results.)

In addition to evaluating the feasibility of implementing the new T-ACASI technology, this preliminary study was intended to test two hypotheses:

H_1 Respondents will feel more comfortable reporting sensitive sexual behaviors to a computer in a T-ACASI interview than to a human interviewer in a standard telephone interview.

H_2 Respondents will be more likely to report engaging in stigmatized or sensitive behaviors (e.g., anal intercourse) and less likely to report normative behaviors (e.g., always using condoms) in the more private T-ACASI interview mode than in a standard telephone interview with a human interviewer.

The results of this pilot study encourage us to believe that a T-ACASI implementation of the NABS is both feasible and will improve the quality of the resultant data. As noted in Attachment A, touchtone telephones were almost universally available in the urban households included in our pilot study (Cook County, Illinois). In addition, we found that the T-ACASI system proved stable, and interviewers had relatively little difficulty using it.[17]

Our substantive hypotheses were generally confirmed by our pilot study -- albeit with rather weak power given the small sample size (N = 263). Our pilot study results indicate that:

- Our T-ACASI technology is stable and well tolerated by respondents.[18]
- Among those who judged one method superior, T-ACASI was thought by a 5:1 ratio to provide better protection of respondents' privacy.
- Among respondents who had an opinion, T-ACASI was thought by a 7:2 margin to be more likely to elicit honest reporting of sexual and drug use behaviors.
- Among respondents who preferred one method, respondents reported by a 3:1 ratio that T-ACASI was a better way to collect information on sensitive behaviors.
- Among respondents who reported a preference, T-ACASI was thought by a 2:1 ratio to provide a more comfortable environment for answering sensitive questions.

Given the small sample sizes available for the analyses presented here (maximum Ns for each condition were: 122 and 141), we feared that there would be inadequate statistical power to detect differences across interview modes. On the contrary, our preliminary analyses indicate that there were significant (or borderline) differences in the responses given to many of the most sensitive questions asked in this survey. Anal intercourse among heterosexuals is probably the most sensitive behavior that is reported by a sizable proportion of the American population.[19] It is thus the behavior that should be most likely to evidence the effects, if any, of the increased privacy afforded by T-ACASI. In the pilot study there was a 10 percentage point difference in the percentage of respondents who reported engaging in anal sex during their lifetime (26.7% for interviewer-administered questioning versus 37.1% for T-ACASI). T-ACASI also substantially increased the likelihood that respondents would admit:

- never using a condom [16% vs. 8%];

[16] The sample for this experiment was restricted to persons 18 to 45 years of age. Persons under age 18 were excluded due to the difficulties of obtaining written parental consent in a telephone survey. Persons over age 45 were excluded since they have a low incidence of STDs and risky sexual practices (Catania et al., 1992a, p. 1103; Laumann et al., 1994, Tables 5.4a and 11.2.) The experiment used a composite sample with two strata. The first and largest strata (N = 210) was recruited from a probability sample of households with listed telephones in Cook County, Illinois (which includes the city of Chicago and surrounding areas). The second strata (N = 53) included patients recruited from the Wake County STD Clinic in Raleigh, N.C. This second strata was included in the experiment to provide information on the impact of T-ACASI in a population that had a history of HIV-risk behaviors.

[17] During the course of the study and during post-study review, we discovered, for example, that respondents with two-line phones or call-waiting would occasionally discontinue the interview to take another call. Our T-ACASI system initially treated such lengthy "silences" on the line as a terminated interview. We have altered the operations of our system so that after waiting several minutes for the respondent to come back on the line, our T-ACASI program will notify the telephone interviewer of this occurrence so that the telephone interviewer can recontact the respondent to complete the interview.

[18] Some instances of "dropped connections" were encountered during the first days of interviewing, but they were not so frequent as to disrupt our operations. To deal with these occasional glitches, our hardware interface has been "trained" to better adapt to the telephone switching system environment in which it is working (i.e., the electronic characteristics of our telephone lines when they "hang up", generate flash tones, busy signals, etc.)

[19] Laumann et al. (1994) report finding that 25.6 percent of men and 20.4 percent of women (ages 18 to 59) report having had anal intercourse.

- infrequent discussions[20] of their sex lives with their partner [55% vs. 28%];
- unstable sexual relationships (i.e., their most recent sexual relationship lasted less than 6 months [16% vs. 5%]);
- very limited sexual experience (that is, having no sexual partners in adulthood [7.1% T-ACASI vs. 3.3% with human interviewer]; having had no sexual intercourse in the past 6 months [4.8% vs. 1.7%]; and, having had intercourse 10 or fewer times in the past 6 months [46% vs. 22%]).

Conclusions from Pilot Test. While results of this small pilot test must be viewed cautiously, they do support our hypotheses that (1) subjects would prefer a T-ACASI interviewing mode when answering sensitive questions, and (2) that T-ACASI increases the likelihood of subjects reporting sensitive behaviors and decreases overreporting of normative behaviors. These results indicate that T-ACASI does hold promise of both improving the quality of data to be collected in telephone surveys and providing valuable information for use in recalibration of estimates of sensitive behaviors made in major AIDS telephone surveys, such as the NABS.

D. RESEARCH DESIGN.

D.1 Overview.

Our proposed research program consists of two stages that seek answers to interrelated questions about the potential impact of Telephone Audio-CASI technology upon future surveys of AIDS-related behaviors. Section D.1 provides an overview of both Stages 1 and 2 with detailed descriptions in sections D.5 and D.5, respectively.

Stage 1. In Stage 1 of our research, we will embed a fully randomized experiment in a large-scale telephone survey (N = 3,294) of AIDS-related behaviors. This survey will replicate the ongoing National AIDS Behavior Surveys (NABS), but it will randomly assign one-half of respondents[21] to receive the sensitive sections of the NABS questionnaire using the new T-ACASI technology. The second half of the sample will be interviewed using standard interviewer-administered telephone survey procedures replicating the administration used in the NABS. This research design will permit us to address a number of specific questions about the measurement characteristics of T-ACASI surveys, the most important of which is whether T-ACASI technology will, in fact, reduce the reporting bias inherent in interviewer-administration of sensitive questions in telephone surveys.

In the T-ACASI condition of our Stage 1 experiment, we will duplicate the conditions that would prevail if T-ACASI were adopted as the routine mode of administration for telephone surveys of sensitive topics. In the T-ACASI treatment, the survey interviewer will obtain the cooperation of the respondent, conduct the initial household roster, administer nonsensitive questions from the survey, instruct the respondent in the use of T-ACASI, and then transfer the call to the T-ACASI system. During the course of the interview, the respondent will be able to return to a human interviewer by pressing a designated key (0 , for operator) on their phone. Other functions are available by pushing a two-key mnemonic code: *B (Backup), *R (Repeat question), *N (No Answer), etc. Pressing the asterisk (*) key alone will play a "help" message describing these key sequences and how to return to a human operator. At the conclusion of the T-ACASI section of the interview, calls are transferred back to a human interviewer to close out the interview. (Our experience with a pilot T-ACASI administration of the NABS [see Section C and Attachment A] indicates that respondents have little difficulty using these procedures.)

In the Standard Interviewer Administered Telephone Survey Condition (T-IA) of our Stage 1 experiment, we will duplicate the telephone survey procedures used by the NABS research program, which are similar to those of other high quality telephone surveys. Briefly, interviews were conducted using computer assisted telephone interviewing (CATI). After gaining cooperation and recruiting an eligible respondent, an interviewer conducted an interview by reading verbatim introductory text and questions as they were presented on the computer screen and entering responses using the keyboard. Interviewers could explain terms and provide other types of help to respondents using information and guidelines supplied in training.[22] All questionnaire "skip patterns" were automatically executed by the CATI system.

[20] Discussions less than once a month or never with most recent partner.

[21] This random assignment will occur for households with a touchtone telephone. As described below, we estimate that 5 to 15 percent of households will not own a touchtone telephone. These households will be excluded from the experiment, but data will be collected from them in a standard telephone interview. These data will provide valuable information on the demographic characteristics and AIDS-risk behaviors of respondents who cannot currently be interviewed using T-ACASI.

[22] A analogous "help" facility can be provided under T-ACASI (e.g., pre-programmed definitions of term, probes, etc.). Our experience during the NABS T-ACASI pilot study (see below) did not indicate a strong need for including such a facility. However, the research team intends to revisit this question during Stage 1 pretesting.

Analysis of data collected from this Stage 1 experiment will permit:

- A rigorous assessment of the extent to which T-ACASI may reduce the bias introduced into standard telephone survey measurements by the requirement that respondents disclose sensitive sexual, drug use, and other characteristics to human interviewers;
- A better understanding of the types of AIDS-related telephone survey measurements that are subject to such biases;
- Investigation of the impact, if any, of T-ACASI technology on the internal consistency of AIDS-related survey measurements;
- Recalibration of the univariate and multivariate response distributions from previous NABS surveys to take account of any significant measurement biases.

Stage 2. In Stage 2 of our research, a small national probability sample of 524 respondents will be recruited at the door and asked to participate in a repeated measures experiment. Each respondents in this experiment will be interviewed using both T-ACASI and a personal visit survey procedures. (The interviews will be conducted two weeks apart, and the order of use of the two modes will be balanced across the experiment.) This Stage 2 repeated measures experiment will permit a relatively economical assessment of the extent to which T-ACASI telephone surveys might provide a reasonable substitute for personal visit surveys using similarly private but much less expensive interviewing procedures.[23]

The T-ACASI interview condition of our Stage 2 experiment will use the same procedures used for Stage 1. In *the personal visit interview condition of our Stage 2 experiment*, the sensitive portions of the survey instrument will be administered using a laptop computer that executes the same software used in the T-ACASI condition and plays the same digitized voice files. In this personal visit Audio-CASI interview, however, the respondent hears the questions through headphones attached to the laptop computer and enters responses on the laptop computer's keypad. In this condition, the survey interviewer will proceed in a manner parallel to the telephone interviewer in the T-ACASI condition. Thus the interviewer would obtain the cooperation of the respondent, conduct the initial household screening, administer the nonsensitive portions of the questionnaire, and instruct the respondent in the use of the Audio-CASI equipment. The interviewer would then retire a discreet distance to allow the respondent to complete the Audio-CASI interview without fear that their responses were being observed. The interviewer would be available to answer questions about the operation of the machinery and to intervene should the respondent have insurmountable problems with the equipment or the questionnaire.[24]

Analysis of data collected from this Stage 2 experiment will permit:

- assessment of the relative bias in reporting of AIDS-related behaviors in T-ACASI surveys versus more expensive personal visit surveys using procedures that provide equivalent privacy (T-ACASI versus in-person Audio-CASI) and samples that are identical in each mode;
- assessment of the relative costs of T-ACASI and an equivalent personal mode of survey measurement;[25]
- preliminary assessment of the impact of differential nonresponse on the sample composition and AIDS-risk profiles of survey samples recruited for T-ACASI versus in-person interviewing.[26]

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[23] In our proposed Stage 2 experiment, we will contrast T-ACASI telephone survey technology to personal visit Audio-CASI interviews. Although paper-and-pencil self-administered questionnaires might provide an equally private interview mode, we have chosen not to employ them because: (1) comparisons of T-ACASI and private personal visit interviews would confound mode of interview with use of the computer; and (2) there is evidence suggesting that in-person Audio-CASI produces substantially lower levels of bias in reporting of male-male sex than paper SAQs (Turner, Ku, Sonenstein, and Pleck, 1995).

[24] Our experience using these procedures in the 1995 National Survey of Family Growth (N = 10,000) and the 1995 National Survey of Adolescent Males (N=1,741) leads us to expect that such interventions will rarely be required.

[25] These analysis will use cost data from both Stage 1 T-ACASI and those Stage 2 interviews selected for initial in-person Audio-CASI interviewing.

[26] This will be derived from analyses of data from Stage 1 on touchtone phone ownership and comparison of AIDS-risk behaviors obtained during Stage 1 (T-ACASI condition) and Stage 2 (T-ACASI condition). Stage 1 will be recruited by telephone and Stage 2 by in-person contacts.

D.2 Survey Instrument.

Most of the AIDS-related questions and introductory material to be included in the instrument for the proposed survey will be taken from the National AIDS Behavioral Survey (NABS) program (see Appendix 4). We have selected this instrument for several reasons. The instrument has been pretested and used in a telephone survey of a national probability sample of adults in the United States with an oversample of African Americans and Hispanics. Thus, it provides information on the acceptability of the questions in our targeted sample. Clearly, holding the wording of questions constant across surveys facilitates comparison of the data, and the NABS instrument includes a full battery of items on sexual risk taking, drug use, and protective behaviors that are critical to understanding the spread of HIV infection. Because these questions touch upon very sensitive and even illegal behaviors, we expect that the mode experiment embedded in the proposed Stage 1 study will provide very valuable information about response bias. Moreover, the NABS instrument -- by design -- goes beyond an inventory of individual behaviors and includes items on the social context of sexual risk taking and drug use. As we have stated elsewhere, information on the dynamics of sexual interaction is critical to the understanding of risk taking, including impediments to condom use and delay in initiation of intercourse (Miller et al., 1990). It is within the dyad that individuals negotiate safer sex and preference for sexual activities. It is also within the dyad that sexual coercion and violence can occur. Thus, to provide a more complete picture of the social context in which AIDS-related risk taking occurs, we will also include items on interpersonal violence from national surveys. In focusing on these items as well as on the quality of the resulting data, we hope to improve estimates of risk taking in the national population as well as by key racial, ethnic, age, and gender groups and to refine descriptions of subgroups most in need of intervention.

NABS telephone interviews were completed in 35 to 40 minutes on average. This is well within the limits of standard, successful telephone interviews. The instrument for the proposed study will be approximately the same length and will include items on the following topics.

Demographic Measures: These measures include age, race, ethnicity, gender, marital status, and sexual relationship status.

AIDS-Risk Behaviors: Introductory screening items precede all questions on sexual behavior, and the Audio-CASI system will automatically present questions that are sexual orientation- and gender-appropriate for each respondent based on the screening information provided. Questions concerning numbers of sexual partners are clearly needed to assess potential exposure, but information on specific sexual practices is also needed, since probability of transmission varies greatly. In addition, partner-specific information will be gathered on the time between first date and initiation of sex (defined as mutual masturbation, vaginal, oral, and anal sex) as well as on the frequency of specific sexual practices. In addition to partner-specific information on frequency of condom use, a 12 item NABS scale will be used to assess condom attitudes, including embarrassment, enjoyment during sex, and ease of use. The section on alcohol and other drug use also includes questions on needle use behaviors.

Social Context: Questions concerning the nature of the relationship as well as sexual negotiation will be asked of each person the respondent had sex with in the past year, up to 10 sexual partners and starting with the most recent partner.[27] Demographic information on partners will also be collected.

Interpersonal Violence. There are two important national surveys from which questions concerning victimization and perpetration of violence will be selected. The first, the National Crime Survey (NCS), which is sponsored by the U.S. Department of Justice, arguably includes the best developed battery of victimization questions. The second, the National Family Violence Surveys, which conducts telephone interviews of the national U.S. population, has assayed levels of noncriminal violence within families. (See for example, Wolfner and Gelles, 1993.) While the number of questions concerning violence in the proposed study will be limited, the information generated is needed to look at the confluence of AIDS-related risk behaviors. In addition, it could provide valuable information to the criminal justice research community on population-based estimates of violence and mode effects on self reported data.

The existing NABS instruments are available in both English and Spanish, and we will conduct interviews in both languages. All new questions and introductory materials for the present study will be developed in English and translated

[27]Tailoring T-ACASI and Audio-CASI procedures to accommodate rostering of these partnerships will require some developmental work. (Exploratory work on this problem was done for the 1995 NSFG.) Our T-ACASI and personal visit Audio-CASI equipment does permit the interactive recording of spoken mnemonics to identify each partner. These mnemonics can then be integrated as a programmed variable in the spoken questions, e.g., "Did you ever use a condom when having sexual intercourse with [MNEMONIC-1]?"

into Spanish. The validity of the Spanish translation of the survey questionnaire will be validated through an independent back translation. Discrepancies identified by this procedure will be resolved in consultation with the translation team that developed the Hispanic NABS instruments.

D.3 Measurement of Experimental Outcomes

The proposed experiments will use a variety of measures of data quality to evaluate the effects of interview mode of administration on data quality and reporting biases. Our primary focus will be upon measuring the impact of interview condition upon relative reporting bias for reporting of sensitive behaviors. We focus upon relative bias because direct evidence of absolute reporting bias is usually impossible to obtain. (There is, for example, no unambiguous way to determine the "true" number of sexual partners a respondent has had in the past 12 months.) It is thus unlikely that we will be able to compare unambiguous estimates of the *absolute* bias in estimates obtained using our alternative survey procedures. Plausible inferences about the **relative** magnitude of reporting biases will still be possible, however. Investigators studying the reporting of sensitive sexual behaviors (e.g., Bradburn et al., 1979; Turner et al., 1995) alcohol and drug use (e.g., Waterton and Duffy, 1984; Aquillino, 1994; Turner et al., 1992) typically assume that the net reporting bias for sensitive, stigmatized, and illicit behaviors is negative. That is, they assume more persons conceal behaviors in which they have engaged than report behaviors in which they have not engaged.[28] These investigators then attempt to identify survey procedures, that would increase reporting of these behaviors (in the aggregate). We propose to take a similar approach, while also investigating the availability of other validational evidence. It should also be possible to devise other crude indicators of impact of alternate methodologies on reporting biases. For example, the aggregate distributions of the number of opposite-sex partners reported by the male and female segments of a population must complement each other (i.e., the **mean** number of female partners reported by males must equal the **mean** number of male partners reported by the females) if all mating occurs within a closed population and it is accurately reported. While this assumption will not be met, there is good reason to suspect that survey methodologies which reduce the substantial observed discrepancies between the mean number of partners reported by males and females (Smith, 1991) do so by reducing the relative reporting biases.

Other measures of data quality we will use include: interview completion rates, item response rates, "don't know" and "refusal" response rates, and measures of response consistency across related items. We have found that these measures of data quality to be sensitive indicators in our post methodological studies of alternative survey procedures. These measures of data quality have been used effectively to assess alternative procedures for defining complex survey concepts (Hubbard, 1992; Hubbard et al., 1992;), anchoring survey item reference periods (Cox et al., 1992; Hubbard, 1992; Hubbard et al., 1992; Turner, et al., 1992), and formatting self-administered questionnaires (Cox et al., 1992; Lessler and Holt, 1987; O'Reilly et al., 1992;Turner, et al., 1992). These measures of data quality have also been found useful in other methodological studies of measurement reliability and validity (e.g., Burkheimer et al., 1991; Cox et al, 1992).

To allow a comparative evaluation of the cost and other operational aspects of the new survey technology, a variety of quantitative and qualitative information will be captured from the survey operations. These will include: professional and interviewer labor and expenses for each survey mode, the response rates achieved in each condition, and the data transmission and processing costs. In addition, an interview protocol will be developed to elicit interviewers reactions to this the new technology, and we anticipate will conduct structured group discussions to elicit the interviewers' suggestions for future improvements in the T-ACASI technology.

D.4 Alternative Experimental Designs.

In selecting an appropriate research design, our research team considered and rejected a number of alternatives. We gave considerable thought to several alternative designs that appeared on first consideration to be less costly to execute. Several of these designs would have limited the scope of the proposed samples to a geographically restricted population, such as a metropolitan area or a region. Although this would seem to proffer possible cost savings, the savings, in fact, were found to be minimal and they would come at the price of limiting the generalizability of our research findings. For telephone surveys, both interviewer-administered and T-ACASI, labor costs are equivalent whether the survey sample is restricted to a small geographic area or covers the nation. Savings in long-distance telephone costs by restricting the scope of the sample would amount to only a small fraction of the total survey budget. Against such minor cost savings must be balanced both a loss of generalizability of our research findings and the forfeiture of the data required to understand and adjust for measurement errors that may afflict estimates derived from past rounds of the NABS survey program. It was the unanimous judgment of our research team that this would not be a wise research strategy.

Given the decision to use a national sample in Stage 1, there were strong disincentives to surveying a different population in Stage 2. Direct comparisons between Stage 1 results and Stage 2 require either that the samples be drawn from the same or substantially equivalent populations or that the effects of differences in sample composition on the

[28] As noted elsewhere, highly normative behaviors are assumed to have a reverse bias.

reported prevalence of AIDS-related behaviors be sufficiently well understood that these effects can be adjusted for without introducing uncertainty into our estimates. (It was the judgment of our research team that this is well beyond the current state of our knowledge.)

While we have chosen to use a national sample in our proposed Stage 2 experiment, we did adopt two strategies that will substantially reduce costs. First, we have chosen to use a repeated measures experiment that will provide more than adequate power for our inferences with a sample that is one-sixth the size of that needed for the two-group simple randomized experiment proposed for Stage 1. Secondly, we propose using a highly clustered sample design in Stage 2, which will permit 12 field interviewers to conduct the experiment within 96 segments located in 12 PSUs. These design choices radically reduce the interviewer labor and travel costs of the Stage 2 experiment.

In addition to our consideration of alternative ways of reducing cost, our research team did consider several alternative designs that would have provided more straightforward inferences about the relative effects of the three interview modes and that also did not require the somewhat artificial requirement that Stage 2 respondents submit to the same interview twice. The simplest of these designs expands upon an experiment conducted by Aquillino (1994). In this research design, rather than conducting two stages of research, we would have drawn a single a national sample and randomly assigned respondents to be interviewed using one of three (or more) alternative strategies, e.g., in-person interviewing using Audio-CASI, telephone interviewing, or Telephone Audio-CASI. Following Aquillino, sample composition could be held constant by recruiting all respondents during an initial in-person interview.

While most members of our research team found the inferential simplicity of this design appealing, the costs of executing such a design with the requisite sample of 4 to 5 thousand respondents would be quite substantial. It was our collective judgment that there are still many important things we must learn about the characteristics of T-ACASI measurements of AIDS-related behaviors before it will be prudent to attempt this more ambitious (and expensive) design.

D.5. Details of Stage 1 Experiment.

Stage 1 Sample Design. The target population for this study will be all U.S. residences with telephones. Since the T-ACASI treatment requires access to a touchtone telephone, we will restrict the experimental portion of the sample to touchtone telephone residences in order to obtain strict comparability for the embedded experiment on mode of interview. Residences with touchtone telephones will be randomly assigned to either the T-ACASI or the standard interviewer-administered telephone (T-IA) mode of interviewing. Residences without touchtone telephones will be excluded from the experiment. However, interview data will be collected form this subpopulation (using standard telephone survey procedures) in order to permit a characterization of their demographic composition and AIDS-risk profiles. (Empirical data suggests that more than 90 percent of respondents will have a touchtone telephone.)[29] The proposed sample design will oversample Hispanic and Black households, yielding a projected total sample of 3,294 including roughly 455 Hispanic and 797 Black (non-Hispanic) respondents.

Our sample design uses a list-assisted, probability sampling technique that includes an initial pre-screening of sampled numbers using an autodialer in order to identify nonworking numbers and thereby to reduce the costs of subsequent interviewer screening to identify residential telephone numbers with an eligible respondent. Using the most recent Bell Communications Research tape of current area code+exchange prefix combinations (Lepkowski, 1988), we will generate a probability sample of all "possible" working telephone numbers. We will use commercially available information on the number of listed numbers in working "100-blocks" to stratify and allocate the sample to increase the yield of residential numbers. (100-blocks of telephone numbers have the same initial 8 digits, e.g., 301-230-46XX.) In general, we have found that when using stratification and sample allocation in conjunction with autodialer pre-screening, it is sufficient to pre-classify 100-blocks into two sampling strata: (1) those with no listed numbers, and (2) those with 1 or more listed numbers. (The sample design sequence for the selection of residences is summarized in Appendix Table 3-1.) In sampled residences with more than one person in the 18-49 age group, we will roster all household members aged 18 to 49 and randomly select one respondent per residence.

Since autodialer pre-screening can be conducted on a very low per unit cost basis,[30] our proposed approach is preferable to clustered sampling techniques such as the Mitofsky-Waksberg procedure, because it does not risk the losses

[29] Our T-ACASI survey in Cook County, Illinois (Chicago and surrounding areas) encountered very few households lacking a touch-tone phone among households with a respondent aged 18 to 45 (1 percent). In a national survey conducted in 1992 by the CBS News-New York Times poll, 521 of 567 respondents (92 percent) in the age range 18 to 49 reported that they were speaking from a touchtone telephone. (These results are derived from our analysis of a public use dataset supplied by the Roper Center for Public Opinion Research at the University of Connecticut at Storrs.)

[30] 16 cents per number screened.

of sample precision arising from positive intra-cluster response correlations in the Mitofsky-Waksberg procedure.[31] In addition to cost efficiency, the proposed procedure allows for simpler and quicker call scheduling since there is no need to wait until primary numbers are screened before attempting additional numbers in the same 100-block (as is required in the Mitofsky-Waksberg procedure).[32]

Sampling Weights for Stage 1 Experiment. Appropriate weights will be developed for estimates based on each mode for the touchtone telephone population. A separate, appropriately weighted estimate will be developed to allow for inference about the total population based only on combined data from the T-IA experimental mode sample and the T-IA data for the non-touchtone residences.

Stage 1 Interview Procedures. Interviews will be conducted in a manner that replicates, as far as possible, the interview procedures used in the NABS, including availability of interviewing in both English and Spanish language. Some procedural alterations will be required to accommodate the use of T-ACASI technology, but these changes will be kept to the minimum required. All interviews will be conducted under circumstances where the respondent feels comfortable with answering personal questions regarding sensitive behaviors, such as sexual behaviors. The interviewer will ask respondents if they have sufficient privacy and, if not, will arrange another time or will contact at another phone number to attempt to increase privacy. The strictest confidentiality of respondents' data and possible identifiers will be maintained. Respondents will be told that their answers are completely confidential, their names will not be associated with responses provided, and that they are free to refuse answering particular questions and are free to decline participation at any time. A trained telephone interviewer will contact and screen a household for an eligible respondent and attempt to recruit an eligible subject. To enhance credibility of the study and to ease any concerns the respondent might have, the following procedures will be implemented:

(1) Advance letters will be sent to all identifiable[33] households notifying them of the impending call from a telephone interviewer.

(2) A call-back telephone number will be provided to each respondent; this connects the potential respondent to the survey director, who will verify the bona fides of the organization conducting the survey.

(3) The interviewer will provide detailed information to respondents on who is conducting this research and why it is being conducted.

Once an eligible sample member is identified, the interviewer will obtain informed consent and will attempt to complete the interview at the time of screening. If this is not possible, an appointment will be scheduled. Interviewers will make a minimum of eight callbacks in attempting to complete each case, and they will arrange callbacks on different days and at different times of the day. In general, the evenings and weekends will be the most productive times. In the

[31]All of the following sample size discussions assumed a survey design effect of 1.0. Since we will be using an unclustered sample with only moderately unequal weighting, we expect a very low survey design effect. Stratification of the sample and the use of covariates in the analyses should further reduce the error associated with the test statistics for testing the types of hypotheses discussed above. Given this overall sample design and estimation approach, we do not feel it should be necessary to select a larger sample than that suggested by a design effect close to 1.0.

[32]We will employ sampling stratification of 100-blocks as a means of improving the screening rate as discussed above. We will also explore the use of county identifiers on the sampling frame as a means of identifying areas with heavy concentrations of Hispanics or Blacks to improve our ability to oversample these populations. Since such techniques can lead to highly unequal weights and the consequent increase in the design effect, we have proposed to oversample Blacks and Hispanics primarily by screening and subsampling methods. Other residences (and perhaps some Black residences) will be subsampled through a staged replicate approach to releasing the sample. As the required sample size for the "other" group is satisfied, subsequent replicates will screen only for the oversampled groups (Blacks and/or Hispanics) as needed. These final replicates may be limited to geographic strata with nontrivial concentrations of these special target populations. The final sampling weights applied to the data will reflect the differential sampling rates inherent in these procedures.

[33] For the Stage 1 telephone survey, letters will be sent to all sampled numbers with listed telephones. Addresses will be obtained using an automated system that matches addresses to listed phone numbers. This service is performed by a commercial vendor.

event of a refusal to complete the questionnaire, multiple conversion attempts will be made. When an eligible sample member refuses to participate, the interviewer will complete a non-interview report. This includes information about the expressed reasons why the respondent refused, the interviewer's impressions of what may have influenced this decision, and the interviewer's suggestions regarding how to obtain an interview. An interviewer specially trained in refusal conversions will review this information prior to recontacting the potential respondents.[34] To maximize completion rates, at least two attempts will be made to convert refusals, and all break-offs will be recontacted.[35] In cases where it is not possible to convert a refusal, an abbreviated interview will be conducted to elicit demographic information to provide information concerning volunteer bias.

Interview questions will be presented in order of increasing sensitivity (Bradburn and Sudman, 1979). For example, questions concerning beliefs about various sexual behaviors will be asked in the latter part of the interview with questions about actual sexual behavior near the end. Great care will be taken in introducing sensitive interview topics. Screening material and introductions to questions have been used in the NABS and were found to work very well. Prior to obtaining consent to participate, respondents will be given a brief summary of the purpose of the study. This provides respondents with a context in which to understand why questions about sexual and other sensitive behaviors are being asked.

After the respondent consents to participate, the interviewer introduces the study and collects basic demographic data from the respondent, and asks the nonsensitive sections of the survey. The sensitive portions of the interview will then begin in one of two modes. In the standard telephone survey mode, the interviewer, using a computer, asks questions of the respondent and enters answers provided by the respondent using the keyboard. Like the Audio-CASI system, the computer in the standard telephone survey mode automatically provides skip patterns as the interview progresses, conducts range checks on responses, and produces a data set. However, unlike the Audio-CASI system, the interviewer verbally poses questions to the subject who must state responses aloud. In the T-CASI mode, the call is transferred to RTI's automated data center where the subject hears prerecorded questions and response categories and answers by pushing touch-tone phone buttons. When the participant has completed the recorded portion of the interview, the call is automatically sent back to a live interviewer, who closes out the interview in the same manner for both modes.

Stage 1 Interviewer Selection and Supervision. We propose to use 40 telephone interviewers to complete 3,294 telephone and T-ACASI interviews in Stage 1. In selecting interviewers for this study we will consider: their experience in general survey research procedures; their ability to ask sensitive questions about sexual behavior; and their proficiency in the use of the computer-assisted interviewing techniques. Telephone survey interviewers will be recruited from among staff with experience in similarly sensitive survey projects.

The quality of the data these field interviewers collect and the overall success of the survey will depend upon the quality of the training, supervisory direction and support provided to these field interviewers. For both personal visit (Stage 2 only) and telephone surveys (Stages 1 and 2), an experienced Survey Director and Assistant Survey Director Collection Manager will recruit and select field interviewers, make training site arrangements, assist with interviewer training, and manage all data collection activities. The Survey Director will be the lead trainer and will be available to resolve problems throughout the data collection periods.

Stage 1. Interviewer Training. Survey-specific interviewer manuals will be developed, and they will include a discussion of the use of T-ACASI technology and problems commonly encountered in conducting interviews dealing with sexual, drug use, and other AIDS-related behaviors. A 3-day training session will be held for all experienced telephone interviewers working on this study. Any newly hired interviewers will receive additional training in telephone survey procedures. Interviewer training will include a review of the background and objectives of the study; a detailed examination of the data collection procedures and forms, including control cards, screening forms, and a question-by-question review of the questionnaire. Demonstrations and practice exercises to obtain informed consent, schedule interviews, complete screenings, and administer the questionnaire will be a major focus of the training.

[34] The Data Collection Manager may send a refusal conversion letter requesting that the sample member reconsider, if an address is available.

[35] In our recent pilot test, there were few breakoffs. During the developmental stages of T-ACASI, several breakoffs resulted from call waiting functions. That is, during the interview, the respondent received a signal that he or she was receiving another phone call. In attempting to put the T-ACASI system on hold to take the other call, the connection was broken.

Additional, interviewer training[36] will emphasize two different sets of skills: working effectively, comfortably, and securely with a sensitive subject matter, and becoming proficient at instructing the respondent to use T-ACASI. The sensitivity of the subject matter will be discussed at an informal session designed to make the interviewers feel more comfortable in asking questions that contain sexually explicit language and descriptions and to learn how to lessen the tensions and anxieties these questions may induce in respondents. A clinician or counselor experienced in discussing sexual behavior will be part of this informal session. During training, the interviewers will first become acquainted with the basic hardware and software features of the CASI equipment. "Hands-on" experience in conducting several mock interviews will be the basic teaching method. The interviewers will be trained by the Survey Director, Assistant Survey Director, with assistance from the Telephone Survey Unit Manager (telephone interviewer training) or the Field Data Collection Manager and the field supervisor (field interviewer training for Stage 2).

Stage 1 Pretest. The Stage 1 pretest will occur in the third quarter of Year 1 and will involve a dress rehearsal of the Stage 1 T-ACASI survey. Prior to this pretest, the T-ACASI NABS questionnaire and other operational procedures will be revised based upon our experiences in the pilot test described in Section C. We therefore propose, barring any unforeseen problems, to conduct only one round of pretesting prior to conducting the Stage 1 field experiment. The pretest will be conducted with a probability sample of 200 respondents sampled in a manner identical to the procedures to be used in the full survey. The pretest sample will be designed to yield a minimum of 50 Hispanic respondents and 50 non-Hispanic Black respondents. During the pretest screening and interviewing, all procedures and software that we expect to use for the main survey will be fully implemented and tested. The screening and interviewing will be closely monitored by survey director and Telephone Survey Unit supervisors. Any problems observed with the instrumentation, software, or procedures will be documented; interviewers will complete a Problem Report Form whenever any kind of difficulty is encountered. Problem resolutions implemented during the pretest interviewing will also be recorded. At the conclusion of the pretest, project staff will debrief the interviewers and supervisors to obtain their reactions to the procedures and instrumentation. Results from this pretest, problem documentation and interviewer debriefings will be reviewed by the investigators and survey staff. Final modifications to the instruments and field work procedures will be made as necessary.

Quality Control. As done in the NABS, the quality of telephone survey operations will be maintained over time by an interviewer monitoring system designed to reinforce the lessons taught during interviewer training. Interviewer monitoring serves four objectives: (1) improving interviewer performance by reinforcing good interviewing behavior and discouraging poor behavior; (2) collecting information about the quality of the interview process; (3) providing a tool to insure that the measurement process is fully standardized; and (4) detecting and preventing deliberate breaches of survey procedure, such as data falsification. RTI's Telephone Survey Unit (TSU) uses a system of call monitoring that provides both quantitative and qualitative evaluation of interviewing at the question-by-interviewer level (or any higher level).[37] Experienced monitors listen to a sample of 20 questions in sequence during an ongoing interview and they code five aspects of the interview-respondent interchange for each question. These are: question delivery (question asked as worded, changed in a minor way, changed in a major way, or skipped), probing (satisfactory, non-neutral, or incomplete), and feedback (satisfactory, or inappropriate), and data entry (correct or incorrect). Call-monitoring data is entered into the computer by monitors while the interview is in progress, using an integrated system of CATI and computer-assisted monitoring (CAM). In the CATI/CAM system, the monitor's computer screen is split into two regions. Approximately 80 percent of the screen is a slave screen, which displays the interview and the entries made by the interviewer who is being monitored. The lower 20 percent of the screen is reserved for call monitoring data entry. Qualitative feedback to an interviewer will be provided within one hour of a monitor's observing 20 sequential questions. Quantitative feedback (for example, the rate of wording changes and incorrect probes) will be given at an aggregate level during regularly scheduled interviewer meetings.

Statistical Power of Proposed Stage 1 Experiment. For comparing two modes of data collection (T-ACASI and interviewer-administered telephone, T-IA) when estimating a prevalence rate, P , we define a mode effect E_P as the ratio

[36] In developing training and other operational procedures, we will benefit from similar experiences in preparing for the NSFG, the NSAM, and the sexual behavior component of the National Household Seroprevalence Survey pretest, (all of which were conducted by RTI staff and from the excellent preparatory work reported by Laumann et al. (1989a,b,c,d) for the their national survey of adult sexual behaviors. Where appropriate, we have adopted their suggestions in developing the procedures for the present proposal.

[37] This system is a modification of a monitoring system developed by Couper, Holland, and Groves (1992), which provides for question-by-question coding of interviewer and respondent behaviors as well as the interaction between the interviewer and the respondent.

$$E_P = \frac{P_{T-ACASI}}{P_{T-IA}}$$

where $P_{T-ACASI}$ is the expected rate of reported prevalence under the T-ACASI mode and P_{T-IA} is the expected rate o reported prevalence under the T-IA mode. Our null hypothesis is that $H_0: E_p = 1$. We selected a sample size that would provide adequate power to reject the hypothesis of no effect when the T-ACASI condition could, in fact, be expected to produce a 25 percent *relative* increase (i.e., $E_p \geq 1.25$) in the reporting of key sensitive, stigmatized, or illicit behaviors. (We sought similar power for normative behaviors, for which we predict decreases in reporting under T-ACASI.) One of the most important prevalence estimated from prior rounds of the NABS is the proportion of U.S. adults who are presently at elevated risk of HIV infection, based on their reports of sexual and other HIV-risk behaviors. From the 1990 NABS data, Catania et al. (1992) estimated this prevalence to be 12.6 percent[38] (Catania et al., 1992). Our proposed sample sizes of 1,400 per interview mode would have moderate but adequate power (0.76) to detect a 25 percent relative increase from .13 to 16 percent in this estimate. For other HIV-related behaviors with higher estimated prevalence, the proposed sample sizes provide excellent power. For reporting of a history of anal sex, the proposed design has power of 0.97 to detect a 25 percent relative increase (from 24 to 30 percent) across interviewing conditions. For reporting 2+ sexual partners during the past five years, the proposed design has a power of .99 for detecting an equivalent mode effect (i.e., an increase from 43 to 53 percent).

Our sample size determination was also driven by our need to have sufficient power to detect noteworthy differences across race/ethnic groups in their reaction to the shift in interviewing modes. Our design will sample Hispanics and non-Hispanic Blacks at twice the rate of the rest of the population. Our power analysis assumes that we will ultimately wish make some comparisons between pairs of race/ethnic groups to test the null hypothesis,

$$H_0: \frac{E_{P1}}{E_{P2}} = 1.00$$

where E_{P1} and E_{P2} are the mode effects for two race/ethnic groups arbitrarily labeled as 1 and 2. We have chosen a minimum race/ethnic group sample size of 200 (within each experimental condition) that would permit moderate power to detect differential mode effects where the above ratio of mode effects was equal to or greater than 1.75. This power analysis requires assumptions about prevalence rates and average mode effects, since the same differentials in mode effects can arise from quite different values of the prevalence rates by group and mode. (Appendix Table 3-2 illustrates the estimation problem for a two-group comparison of mode effects. Appendix Table 3-3 presents two examples in which the average mode effect is 1.2 and the same average prevalence rates hold by mode [0.36 for T-ACASI and 0.30 for T-IA], but where under the alternate hypothesis, quite different mode effects exist for the two race/ethnic groups.) For our proposed minimum race/ethnic group sample size (within interview condition), we will have moderate power of at least 0.77 to detect variations across race/ethnic groups in which the ratio of mode effects equals or exceeds 1.75 -- under either set of assumptions we explored.

Schedule for Stage 1. During the first year of the project period, the instrument will be finalized and translated into Spanish, and recorded in both English and Spanish for T-ACASI; the CATI and T-ACASI systems[39] will be programmed and debugged; and a pretest will be conducted. While the T-ACASI system is now fully operational, the pretest will provide information on remaining operational problems that warrant attention prior to commencing data collection for the main Stage 1 study. In addition, nontechnical operational issues will be addressed during the pilot study. For example, training materials for interviewers and operation materials will be prepared. The pretest, which is a dress rehearsal of the Stage 1 study, will be conducted on a probability sample of 200 people selected from the Stage 1 frame. After reviewing pretest results and interviewer debriefings, final modifications to the instruments and field work procedures will be made. Data collection for the Stage 1 main study will begin late in Year 1; we expect that data collection will be completed during the first quarter of Year 2.

[38]Not including the declining contribution of HIV infection acquired through pre-1984 transfusions.

[39]The same CAI (computer-assisted interviewing) system that supports our T-ACASI applications will function as a stand-alone CATI system for the interviewer-administered telephone survey condition. This will assure that functional differences in system software are not confounded with differences in interviewing mode.

D.6 Details of Stage 2 Experiment

Stage 2 Sample Design. The target population for the Stage 2 experiment is adults aged 18-49 who live in telephone households with a touchtone telephone. The exclusion of non-telephone households and telephone households with rotary dial telephones is necessary in order that all sample households are capable of participating in both the in-person Audio-CASI interview as well as the T-ACASI interview. We will employ a national area probability household sample design to obtain 524 survey responses. Because of the expense, we do not plan to oversample any population component for the Stage 2 field survey.[40] We will select 12 primary sampling units consisting of counties or contiguous clusters of counties. Prior to selecting the sample we will stratify the county area frame by Census region (4 regions) and metropolitan status. This should ensure broad representation across the country geographically and across urban and rural settings. Within each sample PSU, we will select a sample of 8 area segments. RTI's Sampling Department will provide maps for the selected area segments, and interviewers will follow standard procedures for counting and listing residences within their assigned segments. Approximately 17 households will be randomly selected within each area segment, and this should yield an average of 5 to 6 eligible respondents per segment. The resultant full-probability sample will be self-weighting, with the exception of the random sampling of individuals within households that have two or more eligible respondents. Sampling weights will be computed to reflect this probability sampling of individuals within such households.

Assuming a combined response rate (i.e., proportion of eligible sample persons completing both interviews) of 70 percent, a household eligibility rate (i.e., proportion of households with touchtone telephones and a person between 18 and 49 years) of 48 percent, we propose an initial sample size of 1,638 households for a yield of 524 households completing both interviews. A sample size of 524 households completing both the Audio-CASI and T-ACASI interviewers in a repeated measures design will provide sufficient power to detect *relative* differences as small as 10 percent for the key analysis variables (see discussion of power below). One-half of sampled cases will receive the personal visit Audio-CASI interview first and the remaining half will receive the T-ACASI interview first. This will allow the effect of the order of administration on response to be estimated and eliminated from the mode comparisons (see Appendix 2 and Winer [1971]).

Stage 2 Interview Procedures. Face to face recruitment of households for the dual interview experiment will be used regardless of which mode (Audio-CASI or T-ACASI) is administered first. This means that for T-ACASI- first households, the interviewer must return to the household following its successful recruitment and interview by T-ACASI in order to conduct the Audio-CASI reinterview. There are two important reasons for specifying this seemingly inefficient field design. First, to avoid confounding the comparison of mode measurement biases with differential mode nonresponse bias, the respondent population agreeing to both interviews should be made as equal as possible regardless of the order of administration. This would argue for the same recruitment procedures for Audio-CASI-first and T-ACASI-first households. Secondly, recruiting all households by telephone, though possibly less costly than face to face recruitment, suffers from much lower contact and cooperation rates than in-person recruitment (Groves and Lyberg, 1988), and thus generalizations to the target population would be problematic.

Advance letters will be sent to all households[41] notifying them of the impending visit from a field interviewer. Upon arriving at the household, interviewers will screen the household to determine whether they have a touchtone telephone and whether the household includes an adult between the ages of 18 and 49. If more than one age-eligible adult lives in the household, the interviewer will create a roster of eligible adults and implement a within-household random selection to determine which one will be interviewed. Once an eligible sample member is identified, the interviewer will obtain informed consent and, if the household has been assigned to the in-person Audio-CASI-first condition, the interviewer will attempt to complete the interview at the time of screening. If the respondent has been assigned to the T-ACASI-first condition, the interviewer will schedule a time for the respondent to receive a call from the T-ACASI interviewer. At the time of recruitment, interviewers will be informed of the two incentive payments that will be paid for participation in the study. A payment of $5 will be paid upon completion of the first interview and $20 upon completion of the second interview. (The higher payment for the second interview is intended to increase retention of respondents in the study.) In both conditions, appointments for the second interview will be scheduled upon completion of the first. Where possible, appointments will be scheduled for 9 days after the first interview, and strenuous efforts will be made to insure that second interviews are completed within the time period of 9 to 19 days after the first interview.

If the selected resident is not available at the time of household screening, the interviewer will return at a later time to obtain the selected sample person's cooperation and, if appropriate, conduct the Audio-CASI interview. Field interviewers will make a minimum of eight in-person callbacks in attempting to complete each case, and they will arrange

[40] The multistage design for this area probability sample is summarized in Appendix Table 3-4.

[41] This will be possible for all sampled households since addresses will be available from counting and listing of the area segment.

callbacks on different days and at different times of the day. In general, the evenings and weekends will be the most productive times. When possible, additional telephone recontacts will be attempted until such time as the interview is obtained or the respondent refuses the request for an interview. In the event of a refusal to complete the questionnaire, multiple conversion attempts will be made.[42] T-ACASI interviewers will use similar procedures to cope with missed appointments for interviews or reinterviews. If pretesting suggests it might be appropriate (see below), consideration will be given to increasing the monetary incentives for completion of the second interview.

Stage 2 Interviewer Selection. Phase 2 will use a staff of 6 TSU telephone interviewers (working part time) to staff the T-ACASI component of the Stage 2 experiment. These interviewers will be selected from among those TSU interviewers who participated in Stage 1. 12 field interviewers will be recruited and trained to conduct the field interviewing. Interviewers will be recruited from the PSUs. To the maximum extent possible, they will be drawn from the ranks of professional interviewers who have worked on other RTI surveys of sensitive behaviors (e.g., the National Household Survey of Drug Abuse), and particularly those that have used Audio-CASI technology running on a laptop computer (e.g., the National Survey of Family Growth and the National Survey of Adolescent Males)

Stage 2. Interviewer Training. A 3-day training session will be held for field interviewers in Research Triangle Park, N.C. General interview training procedures for the T-ACASI condition will be equivalent to those used in Stage 1. Additional topics to be covered in Stage 2 include: (1) counting and listing procedures required for area probability sampling, (2) communications with the sampling department for arbitration of ambiguous listings; (3) approaches to residences and procedures for dealing with commonly encountered problems (e.g., apartment building with controlled access); (4) use of laptop computers for CAPI and Audio-CASI portions of the survey; (5) use of telecommunications for communication of data and E-Mail to the survey staff at RTI central site. The TSU interviewers will receive parallel training in sessions that will overlap in time with those for the field interviewing staff. Training sessions for the two groups of interviewers will be coordinated to allow joint training on operational and logistical aspects of the study.

Stage 2 Pretesting. Two rounds of Stage 2 pretesting will be conducted, focusing upon testing and refining the operational and logistical arrangements for the relatively complicated fieldwork that will be undertaken in Stage 2. (The survey instruments and the T-ACASI interviewing procedures will have been refined and finalized during Stage 1.) For each pretest, field interviewers will interview samples of 25 eligible respondents from three purposively-chosen area segments. Different PSUs will be selected for each pretest, and area segments will be chosen to assure that at least one of the selected segments in each pretest includes high concentrations of monolingual Spanish-speaking adults. Experience gained in mounting these pretests will, we anticipate, lead to refinements in operational procedures for explaining to respondents the reasons for conducting re-interviews, for scheduling appointments, for coordinating the work of field and T-ACASI interviewers, and for increasing the motivation of respondents to take part in the study and complete the second interview. (If the first pretest indicates it would be appropriate, we will test alternatives to the proposed $5 and $20 sequence of incentive payments in the second pretest.)

Stage 2 Quality Control. The quality of interviewer performance during the interviewer-administered portion of the T-ACASI interviews will be monitored in the same manner as in Stage 1. For the Stage 2 field work, the following procedures will be implemented to insure the quality of the field interviewers work: (1) after completing four initial recruitments or refusals, the interviewer will hold a telephone conference with the Assistant Survey Director to review the results of this work, (2) problems identified will be reviewed with the interviewer, and if necessary, additional training will be provided; (3) interviewers will be required to report weekly to their supervisor on the progress of their field work; (4) problems will be identified and discussed, and possible solutions will be reviewed; (5) for respondents assigned to receive the in-person Audio-CASI interview first, the T-ACASI interviewer will verify the conduct of the initial field interview; (6) for other fieldwork, fifteen percent of each interviewer's completed interviews and five percent of their completed screenings will be selected for verification calls to determine if the screening or interview was conducted and if proper procedures were followed.

Statistical Power of Stage 2 Experiment. To determine the sample size required for our Stage 2 repeated measures experiment, we consider a single questionnaire item and a test of the hypothesis of equivalent mode effects for the two interviewing conditions. Let M_A and M_T denote the mode effect for Audio-CASI and T-ACASI, respectively and consider tests of H_0: $M_A = M_T$ versus H_a: $M_A \neq M_T$. The test statistic is $p_A - p_T$ divided by an estimate of the standard error of the difference given by $s.e.(p_A - p_T) = \sqrt{(1-R)2\pi(1-\pi)/n}$ (see Bureau of the Census, 1985), where R is the r
the item (assumed to be the same for both modes) and π is the true prevalence rate for the item. For the present power analysis, if H_0 is not rejected, we would like to conclude that the two mode biases are sufficiently small that we may treat the two modes as "equally effective" in reducing reporting biases for sensitive behaviors. We specify the threshold for

[42]The Data Collection Manager will send a refusal conversion letter requesting that the sample member reconsider. The letter will be followed by an in-person visit by the original interviewer, another interviewer, or the field supervisor. In cases where telephone contact is possible, the field supervisor may attempt to convert the refusal over the telephone.

reaching this conclusion as a 10 percent relative difference in the estimated values of π. Thus, we would like the power of the test when $M_A - M_T = .10\pi$ to be high, e.g., 0.80 or, preferably, 0.90. Since we are interested in determining whether T-ACASI is "as good" as the more expensive in-person Audio-CASI for collecting this information, we will test H_0 against the one-sided alternative, H_a: $M_A > M_T$ and reject H_0 if the test statistic is greater than the 95th percentile of the standard normal distribution. Thus, if H_0 is rejected, we may conclude that $0 > M_A > M_T$ and T-ACASI has a negative bias that is larger in magnitude than the Audio-CASI mode bias. Conversely, for items which are known to be *overreported*, we will test H_0 against H_a: $M_A < M_T$ in a similar fashion. If H_0 is not rejected, we can say with probability equal to the power of the test for .10π, that the relative difference between modes does not exceed 10 percent.[43]

Our power analyses using these specifications indicate that for hypothesis tests with $\alpha = .05$ for items having very good reliability ($R = .90$), our proposed Stage 2 repeated measures experiment will have excellent power to detect a 10 percent relative difference between modes (>0.90) for prevalence rates exceeding .25. For items having lower reliability ($R = .80$), power exceeds 90 percent for prevalence rates of .40 and higher. (Appendix Table 3-5 presents power estimates for our Stage 2 design for a wide range of item reliabilities and estimated prevalances.)

Schedule for Stage 2. Preparations for the Stage 2 Experiment will early in the first quarter of Year 3. These will include preparation of preliminary operational design for field work and drafting of training materials. Two rounds of pretesting will be conducted during the first half of Year 3. Barring unforeseen complications, we would propose to begin field work for Stage 2 in the third quarter of Year 3. The fieldwork period will last approximately 12 weeks and it will be completed during the third quarter of Year 3. Statistical analysis and publication of findings from both phases of the research will continue through Year 4.

D.7. Data Analyses.

The experimental manipulation and sample design provide the framework for the most important analyses in both Stages 1 and 2. The Stage 1 experiment has been designed to test the hypothesis that T-ACASI interviewing will reduce the relative magnitude of the underreporting bias that commonly affects survey measurements of highly sensitive and stigmatized behaviors as well as overreporting of highly normative behaviors. We also wish to learn whether any such gain is accompanied by a degradation in other aspects of survey quality and whether T-ACASI technology may be particularly appropriate to any one segment of the population or for particular types of questions. Thus, data analysis for Stage 1 will address the following tasks: (1) estimating the prevalence of the different behaviors as well as their antecedents and correlates by mode of interview; (2) estimating the relative distribution of these behaviors and characteristics across different gender, age, racial, and ethnic groups; (3) identifying the items that are most/least affected by interview mode; and (4) assessing the internal consistency of sensitive measurements by mode of survey administration.

Since many variables of major interest will be polytomous or binary, hierarchical log-linear and logistic regression modeling will be the tools of choice for many analyses (Goodman, 1970, 1971, 1979; Bishop et al., 1975; Haberman, 1978). For example, in items screening respondents for compliance with "safer sex" guidelines, use of condoms might be categorized into the following four categories: (a) consistent use with both casual and steady partners, (b) consistent use with casual partners but no use or inconsistent use with steady partner, (c) no use or inconsistent use with both casual and steady partners, and (d) other patterns of condom use. Furthermore, we will want to test whether the experimental mode manipulation affected the reporting of such condom use. All analyses will be conducted on weighted data. If weighting effects introduce a nontrivial design effect on our estimates we will use the computer algorithms for fitting log-linear and logistic regression models to data from complex samples that have become widely available in the decade(Fay, 1987; Shah et al., 1990). If needed, standard errors and confidence intervals can be adjusted to reflect the effects of sample weighting using RTI software (Shah, 1994).

Because we hypothesize that the privacy afforded by T-ACASI should increase reporting of noncompliance with safer sex guidelines, the first model fitting exercise would focus on rejecting the null hypothesis that reporting of condom use [C] was independent of (i.e., unaffected by) the mode [M] of survey administration. However, we recognize that this

[43]Since the sample we select for Stage 2 will be highly clustered, we need to consider the sampling design effect in our power calculations. An advantage of the repeated measures design is that person characteristics are perfectly matched between the two modes and thus differences between Audio-CASI and T-ACASI should not interact appreciably with geographic location. We expect that the sampling design effect will be small. Nevertheless, we can account for the presence of a location effect in the analysis by incorporating a design variable in the model for each PSU. Thus, in computing the power of the repeated measures design, we have assumed that the design effect due to clustering is either negligible or can be removed using analysis of covariance techniques.

model may be overly simplistic if, for example, some subgroups are more affected by mode than others. Thus, we will also construct three-way tables, such as race [R] by condom use [C] by mode [M], to see if the interior cells could be adequately fit by a model constrained to fit the marginals [CM] and [R]. If mode appears to vary by race, then a formal test will be made for the higher order interaction [CMR].

Basic analyses such as these will be repeated for prevalence rates of other sensitive behaviors. In addition, other indicators will be used to assess the quality of responses. For example, we will compare rates of item nonresponse and inconsistent reporting by mode. For T-ACASI to be a truly promising candidate for broad use in surveys of sensitive behaviors, we would require that these indicators of data quality not deteriorate substantially from the levels observed in the standard telephone condition.

The analyses will permit both testing for relative measurement bias in the prevalence estimates produced by the standard telephone interview mode and also the construction and statistical estimation of models appropriate for the adjustment of estimates generated from items from recent NABS telephone surveys, taking into account any biases identified.[44] We will not simply substitute the T-ACASI estimates for those generated in other, similar telephone surveys. Rather, the bias in these measurements will be modelled, and estimates of model parameters will be used to make appropriate adjustment to prevalence rates, noting, where appropriate, such adjustment could effect key parameters of several key AIDS-related theoretical models of risk and health behaviors, such as harm reduction, social diffusion, reasoned action, social learning, as well as the integrated AIDS Risk Reduction Model (ARRM) (Bandura, 1989; Catania et al., 1989; Fishbein and Middlestadt, 1989; Weinstein, 1989).

In our Stage 2 analyses, we will test the null hypothesis that the quality of data collected by T-ACASI is equivalent to that collected by more expensive, in-person Audio-CASI interviews. Traditional within-subjects analysis of variance modelling provides a basic framework for analysis of metric variables in a repeated measures experiment, and polytomous variables may be analyzed within an appropriate logistic regression framework. Appendix 2 provides further details of the statistical motivation, power, and analysis of such repeated measures experiments.

Some analyses be undertaken to provide a crude assessment of differences in sample composition and AIDS-risk profiles that arise due to use of telephone versus in-person recruitment of respondents. Analyses will compare the characteristics of respondents recruited for the T-ACASI condition in the Stage 1 experiment and the T-ACASI-first condition in the Stage 2 experiment. Results of these analyses will be (somewhat) helpful in understanding the impact on AIDS estimates of the seemingly inevitable difference in response rates for in-person versus telephone surveys (Groves, and Lyberg, 1988). Generalizations from our results will, however, be subject to uncertainties introduced by the relatively small size of the Stage 2 sample and the relatively unusual survey for which recruitment was done in Stage 2 (i.e., participation in two identical surveys for which a $25 incentive was paid).

Finally, in addition to analyses assessing the effects of T-ACASI technology on measurement bias and error, we will also attempt to quantify the impact of T-ACASI on survey costs. Measures will include the following: total time required for each interview; response rates achieved in each mode; and operational and data processing costs for each mode. In addition, we will gather qualitative information to summarize reactions of field interviewers to different modes as well as their observations on respondent acceptance to this new technology.

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[44] Adapting procedures from the National Survey of Adolescent Males (see Turner, Ku, Sonenstein, and Pleck, 1995) estimates of the magnitude of the impact of T-ACASI on NABS measurements derived from the Stage 1 experiment would be used to recalibrate past NABS estimates that were derived from standard telephone interviews. Since most of the responses of interest are binary responses (e.g., had anal intercourse/did not), we would use logistic regression to compute adjustments using a model such as:

$$\ln \frac{p_i}{1-p_i} = \beta_o + \beta_{T-ACASI} X_{i,T-ACASI} + \sum_{j=2}^{J} \beta_j X_{i,j}$$

where p_i is the probability that the behavior is reported by respondent $X_{i,T-ACASI}$ is a dummy variable whose value is set t 1 if respondent i is assigned the T-ACASI treatment and to 0 otherwise. The variables, $X_{i,j}$ $(j \geq 2)$ represent other potential explanatory variables which may not be equally represented in the T-ACASI and Standard NABS treatment groups (e.g., age, race, ethnicity, etc.) The β 's are the coefficients of the logistic regression model. In particular $\beta_{T-ACASI}$ can be interpreted as the log of the odds ratio for reporting of the sensitive behavior under T-ACASI relative to the odds of reporting the behavior in a standard telephone interview. This model would be applied to the NABS data that are being collected using standard telephone survey techniques in order to estimate the distribution of responses that would have been obtained if the interviews had been conducted using T-ACASI.

E. HUMAN SUBJECTS REVIEW.

E.1. Characteristics of Research Population.

The subject population includes adults from the general population aged 18 to 49 residing in households with telephone service. The Stage 1 sample will also include an oversample of African-Americans and Hispanics in order to support subgroup analyses. Since a major focus of this research is sexual behaviors and other behaviors that risk transmission of HIV, we have chosen not to interview persons ages 50 and older. Past research on sexual behaviors (see, Catania et al., 1992a), drug use (see Turner, Lessler and Devore, 1992, Fig. 7-4), and the age profile of new AIDS cases (see CDC, 1995) indicate that older segments of the U.S. population are less likely to engage in behaviors that place them at risk of HIV infection.

E.2. Sources of Research Material.

Data for this research will be obtained from three different modes of interviews using survey instruments that replicate measurements made in a major survey of sexual and other AIDS-related behaviors (NABS) and in selected surveys of interpersonal violence. The three modes are: (1) standard telephone interview; (2) telephone Audio-CASI; and (3) personal visit Audio-CASI. Because the new interviewing modes (2 and 3) afford more privacy to respondents than traditional interview modes and because the questions included in our instrument are taking from previous surveys that have demonstrated high levels of acceptability to respondents, we do not anticipate that either the mode or the instrument present new problems.

E.3. Recruitment of Subjects and Consent Procedures.

Subjects in both the Stage 1 and Stage 2 surveys will be recruited as probability samples of the U.S. adult population aged 18 to 49 residing in households with telephone service. All research materials and human subject protection procedures for this T-ACASI research program are being submitted to the IRB at RTI.

The recruitment of subjects and consent procedures are the same as those used in similar studies conducted by this research team. Briefly, all participants will be informed as to the nature and purposes of the study, including content of the interview and how data confidentiality will be protected. Neither the sponsoring agency nor any persons wishing to use the public use tape for secondary analysis will have access to telephone numbers of respondents. To further insure confidentiality, only authorized personnel will be permitted at interviewer stations when interviews are being conducted. In Stage 1, when a respondent is contacted by telephone and agrees to participate in the study, an implied, verbal consent procedure will be used in lieu of written consent. Only those who state that they are willing to participate will be interviewed. The practicalities of conducting a written consent procedure (where the individual signs a consent form) is a greater risk to confidentiality than using this implied consent procedure. Written consent will be obtained from all Stage 2 participants.

E.4. Potential Risks to Subjects.

The risks to the rights and welfare of the individual are confined to whatever risks may be involved in completing a telephone interview or Audio-CASI interview that will not last longer than 40 minutes. We believe that the T-ACASI and Audio-CASI experimental modes afford more privacy than the standard telephone interviewing techniques and thus may be more acceptable to respondents. Some participants may experience slight embarrassment due to raising of sensitive issues, although we believe this should be diminished under the T-ACASI and Audio-CASI conditions.

There is a possible, but highly improbable, potential for breach in confidentiality; procedures to protect against such breaches are described below. All subjects will be reassured that they are under no obligation to respond to the survey and may terminate their participation at any time. Respondents who may be concerned about the authenticity of the request for a research interview will be provided with telephone numbers at Research Triangle Institute to verify the bona fides of the interviewer or to contact a representative of the IRB.

E.5. Procedures for Protection Against Risk.

We recognize that every subject has the right to privacy, to refuse participation, to accurate representation of study goals, and to be treated with dignity. Because disclosure of sensitive research data could have serious consequences for respondents, we handle information about individuals in ways that prevent unauthorized access at any point during the study. Specifically, we prohibit the release of personally identifying formation about participants to anyone outside the project team. Usually, only the staff member who collects the data is aware of the respondent's name. While some of the information to be collected is sensitive or potentially embarrassing to respondents, we believe that the T-ACASI technology being tested in this field experiment minimizes and may, perhaps, eliminate the potential discomfort.

Maintaining Confidentiality of Data. Maintaining the confidentiality of individually-identifiable data requires explicit procedures. All RTI research units employ standard confidentiality procedures protect such data; these include the following.

- Every staff member involved in any phase of handling sensitive or personally-identifiable research information is required to sign a legally binding confidentiality agreement. (Both existing staff and newly hired personnel must sign this agreement.)
- Access to data is restricted to those staff members who have signed confidentiality agreements.
- Special permission is required to photocopy data or to remove it from the work area. When copies are made, their locations are tracked at every point until they are finally destroyed.
- Staff members are not allowed to interview or process data for subjects whom they know personally.
- The disclosure of personal identifiers to outside sources requires explicit consent by the subject.
- The project director provides all staff with detailed instruction on confidentiality requirements and procedures.
- The project director is empowered to implement any additional procedures required to ensure confidentiality of all research data.

E.6. Why Risks Are Reasonable.

This research will provide sorely needed information about how best to ask questions about a variety of behaviors having public health and policy implications for the general population in the U.S. Benefits for this study include:

- improved estimates of AIDS risk behaviors, primary and secondary prevention measures of the general population in the U.S.;
- improved understanding of the distribution of factors related to HIV prevention and risk;
- better recommendations to guide intervention programs; and
- a careful assessment of a new technology to gather better data while improving the conditions under which measures of sensitive behaviors are made.

We believe that the risks of discomfort or embarrassment to subjects are largely outweighed by the social benefits of the information obtained. Furthermore, respondents are informed that their participation is voluntary and that they may refrain from answering any question.

F. VERTEBRATE ANIMALS. Not applicable

G. CONSULTANTS AND COLLABORATORS.

As part of our research team, we will include two co-PIs from outside the applicant organization: Joseph Catania from the Center for AIDS Prevention Studies at the University of California at San Francisco and Robert Boruch from the University of Pennsylvania. To simplify administrative arrangements, reimbursement for these two Co-PIs will be arranged through a simple subcontract between RTI and the Co-PIs universities.

The proposed research program will be directed by researchers from the Research Triangle Institute, which will provide the Principal Investigator (Turner), two Co-PIs expert in AIDS behavioral research (Miller) and survey measurement research (Biemer), a senior statistician expert in survey sample design (Chromy), a computer scientist expert in computer-assisted interviewing systems (Cooley), an experienced survey director (Zelon) plus staff to supervise survey operations and to provide other support to the research team. This team will be augmented with one Co-PI (Catania) expert in telephone surveys of sexual and other AIDS-related behaviors and one Co-PI (Boruch) expert in survey measurement and research on violence.

Letters of support from these Co-PIs at UCSF and UPENN are included with this application from RTI. In addition, budget requests, biographical sketches, and statements of "other support" are included for these collaborators. The PI and most Co-PIs have collaborated with each other in the past (e.g., Turner, Miller, and Moses, 1989; Coyle, Boruch, and Turner, 1991; Turner, Miller, and Catania, 1995). The three organizations (RTI, UCSF, and UPENN) operate numerous Federal research grants and contracts, and we expect a successful collaborative relationship between researchers and organizations on this project.

H. CONSORTIUM AND CONTRACTUAL ARRANGEMENT.

As noted in Section G, support for Drs. Catania and Boruch will be arranged by a subcontract between the applicant organization (RTI) and the co-PIs universities.

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