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Overall Participation and Subgroup Participation in an RDD  
Telephone Survey**

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# **The Effects of Introductory Scripts and Respondent Incentives on Overall Participation and Subgroup Participation in an RDD Telephone Survey**

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## **Abstract**

Introductory scripts have the potential to significantly impact cooperation in RDD surveys. The recent, steady decline in response rates for RDD surveys has increased the importance of the introductory statements conveyed to sampled households at initial contact. A second factor that has been shown to significantly affect response to telephone surveys is the offer of respondent incentives to sampled households. Incentives can generate immediate interest among household members. This paper analyzes results of an experiment conducted as part of an RDD survey, with introductory scripts and respondent incentives as the two experimental factors. The goal of this experiment was to determine how alternative introductory scripts and incentive offers affected participation in a statewide survey on tobacco use, especially among current smokers. This experiment was part of a methodological investigation into an observed decline in smoking prevalence estimates in the New York Adult Tobacco Survey (NY ATS), as compared to the steady prevalence rates observed for the same time period in the New York Behavioral Risk Factors Surveillance System (BRFSS) survey. The two alternative versions of the introductory scripts were the original NY ATS script that highlighted the survey focus on tobacco use, and an alternative script that closely followed the BRFSS protocol and indicated a broader focus on health behaviors. The two incentive conditions tested were no incentive initially offered and an initial offer of \$20. These two sets of alternative protocols provided four conditions to which sampled telephone numbers were randomly assigned prior to data collection. The results indicated that the initial offer of an incentive had a significant positive impact on overall response rates and on smokers' participation rate. Smokers' participation was also slightly higher in both conditions employing the alternative script compared to the non-incentive condition with the original script, but these differences were not statistically significant.

## Background and Overview

The difficulty in screening households and completing interviews in telephone surveys has increased significantly over the past several years, especially for surveys that use methods like random-digit dialing (RDD) that involve “cold calling” households. More widespread use of technology that allows households to avoid answering the telephone (such as answering devices, caller ID, and other call management systems) and apparently greater reluctance among most populations to participate in surveys by telephone appear to be key factors in the increasing effort and declining response rates observed in RDD surveys. For example, Curtin, Presser, and Singer (2005) note that the response rate for the Survey of Consumer Attitudes declined by 12% between 1979 and 1996, or about three-quarters of a percentage point each year. Not surprisingly, these authors also reported that the average number of calls required to complete interviews and the number of cases requiring refusal conversion on this survey both *doubled* over this same time period (Curtin, Presser, and Singer, 2000).

Successfully completing most general population surveys by phone is sufficiently challenging in the current telecommunications environment. The challenge of completing telephone interviews can be even greater when key subgroups in the population may have specific reasons to be reluctant to participate. For example, cocaine users who are sampled for a survey on drug use, or high school drop outs who are asked to complete a survey about education, may decline the survey requests because they are reluctant to report their relevant experiences. In these and similar survey contexts, the potential cost may be nonresponse bias by excluding members of the target population who have relevant differences in experiences and views from those who do participate in the survey. This concern reflects a key principle of survey nonresponse: nonresponse error is often less a function of the overall nonresponse rate, and more dependent on how nonrespondents differ from respondents in ways that introduce error (Groves and Couper, 1998)

This study examines the impact of two survey design features – introductory scripts and respondent incentives – on subgroup and overall nonresponse to a quarterly RDD survey on tobacco use, the New York Adult Tobacco Survey (NY ATS). A series of experiments were implemented across the four quarters of this survey in 2006 to determine how alternative introductory scripts and incentive offers affected participation and especially participation among current smokers. These experiments were motivated by an investigation into an observed decline in smoking prevalence estimates in the NY ATS, as compared to the steady prevalence rates observed for the same time period by the New York Behavioral Risk Factors Surveillance System (NY BRFSS) survey.<sup>1</sup> A comparison of the ATS and BRFSS protocols indicated that the introductory scripts and incentive payment procedures were the only two survey design features that both clearly differed between the two studies and also offered viable reasons as to why

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<sup>1</sup> Between the first quarter of 2004 and the fourth quarter of 2005, the smoking prevalence estimate from the NY ATS declined from 20.6% to 12.6%. The smoking prevalence rate in the NY BRFSS was 20.3% at the start of 2004 and was unchanged (20.5%) at the end of 2005.

smokers' participation might differ between the two surveys.<sup>2</sup> For this reason, these two design features were incorporated into a two by two factorial experiment to assess the role each might be playing in both smokers' and overall participation in the NY ATS.

The paper discusses the potential role of introductory scripts and respondent incentives in RDD surveys as background for the rationale behind the NY ATS experiments. The details of the experimental design and analysis plans are then described with respect to the expected outcomes. The results of the initial experiment, and subsequent one-factor experiments, are then presented and discussed. Finally, this paper considers the implications of these findings for the (1) the general use of introductory scripts and incentive payments in RDD surveys and (2) research on the interaction between key design features and sentiments among potential respondents. Our conclusions suggest that both overall and subgroup participation in telephone surveys must be considered in light of the challenges of communicating study goals to sample members and external conditions that may be relevant to the target population.

## **Introductory Scripts and RDD Survey Participation**

Recent research has identified a number of survey design features have been shown to affect participation in RDD telephone surveys. Topic, sponsorship, pre-notification procedures, introductory scripts, interview length, and use of incentives have all been shown to affect response to RDD surveys. (Cantor, 2006; Groves, Singer, Corning, and Bowers, 1999; Groves, Singer, and Corning, 2000; Groves, Presser, and Dipko, 2004; Link and Mokdad, 2005). Despite this research, few published studies have investigated the impact of specific modifications to introductory scripts on RDD survey participation.

A number of reasons could account for the dearth of research on introductory scripts, but the primary reason may be that it is often difficult to manipulate and assess differences in introductions in RDD surveys. Some telephone surveys often have a "preferred" introductory script that interviewers are expected to follow fairly closely when presenting the study to each potential respondent. Significant variations from this preferred opening script require approval from relevant stakeholders, such as study sponsors, human subjects committees, and others. Other telephone surveys allow interviewers considerable freedom to personalize and tailor the introduction. In these studies, the lack of standardization of the introduction presented to each sample unit makes it impossible to determine how different elements of the script affect participation.

Furthermore, initial interactions between interviewers and potential respondents are typically brief in RDD surveys. This factor makes it difficult both to observe significantly different introductory scripts and to collect sufficient data to evaluate the reactions of sample members to

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<sup>2</sup> The review of the NY ATS and NY BRFSS procedures found few differences in sample design and selection, data collection procedures, or post-survey data adjustments that could reasonably be expected to contribute to the observed difference in smoking prevalence estimates between the two surveys.

the initial survey request, including those who hang up and are never successfully contacted again. Survey researchers can, and occasionally do, ask survey participants what features about the survey request positively affected their participation, but this same question can typically not be asked of nonrespondents.

Theoretical perspectives on survey participation provide some general guidance for how survey introductions might affect response. Groves, Cialdini, & Couper (1992) argue that compliance with specific survey requests is likely to be related to six psychological principles: reciprocity, consistency, social validation, authority, scarcity, and liking. These principles provide some relevant (and testable) hypotheses on how different elements of a survey request might affect participation. For example, sample members who are offered an incentive payment as part of a survey request may feel a greater obligation to reciprocate by completing the interview. Likewise, respondents should be more likely to cooperate with a survey request if they perceive the request is coming from an appropriate, legitimate authority. An important corollary to these hypotheses is that these factors can only affect decisions to participate when they are (1) successfully communicated to potential respondents and (2) salient to the person's view of the study (Groves, et al, 2000; Groves, et al, 2004). Survey researchers routinely consider exactly what kinds of factors should be communicated and which are most likely to be salient to sample members when they attempt to devise effective survey introductions. Of course, researchers often have limited information about which factors are most likely to resonate with a specific target population or subgroup for a specific survey implementation.

Some researchers have attempted to conduct experimental assessments of alternative introductory scripts for telephone surveys. De Leeuw and Hox (2004) recently conducted a total of 29 experiments in various telephone surveys primarily comparing the absence or presence of a non-solicitation statement (i.e., "I am not selling anything"). These experiments indicated an overall positive impact of including non-solicitation statements across a variety of telephone surveys. Unfortunately, these authors did not experiment with additional variations to the introductory script, such as varying the purpose of the survey or its sponsor.

Perhaps the most rigorous experimentation with multiple features of introductory statements was completed by Vaden-Kiernan, Cantor, Cunningham, Dipko, Malloy, and Warren (1997) for the *National Survey of American Families (NSAF)*. This research involved stepwise comparisons between alternative survey introductions that manipulated several key elements, including:

- details on the sponsorship of the study,
- statements emphasizing the importance of the study,
- alternating the first question asked in the introduction,
- statements of non-solicitation, and
- offering a monetary incentive.

Although these comparisons generally involved relatively small sample sizes from CATI pretests, the researchers did find significant patterns in the cooperation rates obtained with alternative introductions. The most successful introduction across this series of comparisons emphasized the impact of the survey for each specific state, included a non-solicitation statement, highlighted the brief nature of the initial screening, and excluded the offer of a \$5 incentive payment.

Cowling, Johnson, Holbrook, Warnecke, and Tang (2003) provide an empirical illustration of the potential impact of alternative introductory scripts on survey participation that is directly relevant to the NY ATS survey context. These researchers conducted an experiment as part of the California Adult Tobacco Survey (CATS) where sample members were randomly assigned to one of two different introductory scripts. The goal of the experiment was to determine if smokers' participation in the survey was influenced by how the survey purpose was presented. The first script was the original CATS script and emphasized that the survey was about tobacco use. An alternative introductory script presented the focus of the survey as a more general health interview. Overall, comparisons between the groups with the original versus the alternative introduction revealed no significant differences in smoking prevalence estimates between the groups. Further analysis revealed that the type of introduction interacted significantly with respondents' sex: women who received the original introduction about tobacco use were less likely to report smoking compared to those who received the alternative introduction on general health. The authors speculated that increasing anti-smoking sentiments in the state of California may have influenced women's reluctance to report smoking behavior. Such sentiments could have produced social stigma among women and led them to underreport smoking. Interestingly, the authors obtained no such observations and conclusions among male CATS participants.

Cowling, et al's (2003) research illustrates an important principle of survey response: respondent characteristics and external conditions that are beyond survey design features can affect participation and can also interact with specific design features. Groves, et al (2004) found that some sample members (political contributors) were more likely to participate in surveys on a wide variety of topics, other sample members (new parents) were less likely to participate in surveys on all topics, and still others (teachers) were more likely to participate only when the survey topic was of interest to them (education and schools). Harris-Kojetin and Tucker (1999) examined Current Population Study (CPS) data from 1960 to 1988 and found that periods of higher presidential approval were associated with lower refusal rates to this federal government survey. To the extent that sample members' characteristics and/or external contexts become salient to individuals' decisions to participate fully in a survey, design features can interact with factors such as topic and sponsorship in ways that directly impact survey nonresponse and/or measurement. Cowling, et al (2003) concluded that women were less likely to report smoking behavior, but it may also be the case that female sample members who smoked were less likely to participate in the survey when they recognized that the survey was

about tobacco use. Either factor, and the two factors combined, could explain the lower smoking prevalence estimate observed among women when the original introduction on tobacco use was used.

In the NY ATS, the observation that smokers were more likely to refuse than non-smokers suggested that Cowling, et al's (2003) approach and findings could be relevant to this problem. Smokers in New York (of either gender) may be more reluctant to participate and/or admit to smoking when they are cued to the fact that the study is about tobacco use. Current smoking policies in the state of New York provide some potential reasons for smokers to feel that their behavior is increasingly stigmatized. This may lead smokers to be more reluctant to talk about their tobacco use, especially to an interviewer who is calling on behalf of the primary institution for shaping public smoking policy, the New York State Department of Health. New York passed a statewide ban on all smoking in indoor public places, including restaurants and bars, that went into effect on July 24, 2003. Furthermore, the New York state legislature has been considering banning smoking in other public spaces, including outdoor areas such as parks, beaches, and entrances to buildings. Cigarette excise taxes in the state of New York are currently the fifteenth highest in the country, and these taxes are doubled by the local tax added in New York City. As a result, smokers in New York may perceive that their smoking is increasingly restricted and their behavior increasingly marginalized. Furthermore, smokers may feel that the New York State Department of Health bears significant responsibility for inhibiting their smoking in the state and may therefore be reluctant to participate in a survey on tobacco use sponsored by this department. These considerations suggest that a survey introduction emphasizing the topic of tobacco use and sponsorship by the New York State Department of Health may suppress smokers' participation in the study and/or their willingness to identify themselves as smokers. Alternatively, if the introductory script de-emphasized the focus of the survey on tobacco use, smokers may be more receptive to the survey request.

## **Respondent Incentives and Participation in RDD Surveys**

Offering an incentive payment can make the initial request in a survey introduction more attractive to sampled households and keep potential respondents on the phone longer as the interviewer explains purpose and procedures. Singer, Van Hoewyk, Gebler, Raghunathan, and McGonagle (1999) found respondent incentives generally improve response rates in telephone surveys. Two other effects of incentives on survey participation in telephone surveys that Singer, et al (1999) found were (1) a greater difference in response rates between the incentive and no incentive conditions for high burden surveys and (2) a greater impact of incentives on response to surveys that would have relatively low participation without incentives. The first finding indicates that longer and/or more demanding surveys benefit more from incentives than shorter and/or less demanding surveys. This finding also suggests that respondents who perceive survey burden to be high, either because of the length or nature of the interview protocol, will be more likely to participate when an incentive is offered (Groves, et al, 2000). The

second finding indicates that incentive payments are quite effective when other motives to participate (such as interest in the topic or support for the sponsor) are absent, as Singer (2002) reiterates. As a consequence, we can expect potential respondents who are indifferent or disinclined to participate in a particular survey to be more motivated by the offer of an incentive. This conclusion provides one basis for adopting a protocol where only sample members who initially decline the survey request are offered incentives to participate.

Cantor, O'Hare, and O'Connor (2007) recently completed an extensive review of the literature on the impact of using incentives specifically in RDD telephone surveys. Their analyses of both experimental and non-experimental studies produced two conclusions relevant to the NY ATS incentive protocol. First, promised incentives ranging from \$5 to \$35 did not increase RDD response rates at the screening stage, but the evidence suggested that promised incentives of at least \$15 are effective in increasing response to the interview stage (Cantor, et al, 2007, p. 36). This conclusion highlights the important distinction between the screening and interviewing stages of RDD surveys, and reinforces the notion that screening households is challenging even when significant incentives are promised. This conclusion also draws our attention to other factors that may be salient to potential respondents at the screening stage and beyond. RDD respondents who have completed the screening protocol should be more informed of the survey sponsor, topic, and procedures than they were at the initial introduction to the study. As a result, respondent at this point are in a position to weigh the incentive offer (or lack thereof) against these other factors in deciding whether to participate (Groves, et al, 2000).

A second relevant conclusion was that little evidence supports the concern that providing incentives significantly affects survey results or data quality in RDD surveys (Cantor, et al, 2007, p. 36). This is an important consideration, as respondents who participate primarily because of an incentive offer may be less motivated to answer survey questions accurately and completely if they are not particularly interested in the topic or simply want to reduce the survey burden. Cantor, et al's (2007) review indicates that offering an incentive to the NY ATS should not increase the proportion of respondents who are unwilling to provide accurate and complete answers, regardless of their interest in the survey topic or other survey features.

From 2003 through 2005, the respondent incentive plan used in the NY ATS followed a differential incentive approach based on refusal status. NY ATS sample members (and other contacts in households) were only offered an incentive when they initially refused to participate. Following a refusal, the sample member was offered \$20 to complete the interview.<sup>3</sup> Analysis of NY ATS data has shown that smokers tended to be over-represented among respondents who are paid the refusal conversion incentive (Currivan, 2005). The effectiveness of incentives for improving overall response rates in both routine and more burdensome surveys suggests that paying incentives to initial refusers would generally produce a more representative sample. All else being equal, the lowered nonresponse rate is likely to reduce the potential (or degree) of

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<sup>3</sup> Initial refusers who then complete the survey after being offered the incentive payment typically range from 20% to 25% of the final set of completed interviews for each quarter of the NY ATS.

nonresponse bias. Of course, this conclusion only holds if the increased response decreases differences between respondents and nonrespondents. In theory, increasing participation is likely to reduce nonresponse bias, but for some samples greater response may have no effect on sample composition or actually produce a less representative sample (Groves and Couper, 1998; Groves, et al, 2004). For example, if respondents who initially refused have similar characteristics and give similar answers to those who did not refuse, adding these sample members to the final sample will not have a significant impact on data representativeness.

The important point about offering an incentive payment to initial refusers is that this practice generally increases the participation of sample members who would otherwise be difficult to include in the survey (Singer, et al, 1999). For example, as suggested in the preceding section, smokers may be more reluctant than non-smokers to participate in a survey about smoking sponsored by their state health department or a federal health agency. In this scenario, refusal conversion payments would seem likely to produce a more representative sample than would otherwise be the case by including more smokers in the final sample. This outcome would represent a major intended consequence of the refusal incentive protocol on sample composition: to improve the representativeness of the sample by including those who are less inclined to participate in the survey.

The refusal incentive plan originally followed in the NY ATS may have contributed to the observed differences in smoking prevalence estimates between the NY ATS and NY BRFSS data. The NY BRFSS does not offer any incentives, even to those who initially refuse. The difference in how the two surveys are presented to potential respondents may have exacerbated the impact of the incentive plan on smokers' participation. If smokers are more willing to participate in a survey about tobacco use when offered a monetary incentive to do so, then the NY ATS incentive procedures may have been disproportionately relying on refusal conversion efforts to produce a representative proportion of smokers. Even if the refusal conversion efforts were otherwise quite similar across the two studies, the NY ATS effort depended upon successful communication of the incentive payment to smokers in order to overcome initial reluctance among this group. This created the possibility that the NY ATS may have been including fewer smokers overall because the initial offer without an incentive was less attractive to smokers than the refusal conversion offer. Because only a small percentage (about 8 to 10%) of initial refusers have been successfully "converted" each quarter, significant risk of excluding smokers in the NY ATS was produced.

This reasoning suggests that the NY ATS incentive plan could have been a contributing factor to the observed difference in smoking prevalence estimates compared to the NY BRFSS, but not likely a primary cause. That is, the impact of the incentive plan was contingent on sample members' initial reactions to factors like the topic and the sponsorship of the survey. Still, the potential importance of how respondent incentives could affect overall participation and participation among smokers argued for examining alternative incentive procedures among NY ATS sample members.

## Methods

### *Design of the Experiment*

The NY ATS is a quarterly statewide household survey of New York residents aged 18 and older that began in the third quarter of 2003. The 25 to 30 minute survey measures the prevalence of current smoking, smoking and smoking cessation behaviors, and attitudes and beliefs about tobacco use and tobacco products. The quarterly surveys are designed to provide timely surveillance and evaluation data to inform the New York State Tobacco Control Program. Beginning in the second quarter of 2004, the NY ATS telephone interviews have been completed by Macro International.

To assess the impact of the introductory scripts and incentive plan on smokers' and overall participation, we designed a two by two factorial experiment to contrast the original NY ATS survey procedures with alternative procedures. The primary aim of this experiment was to allow evaluation of factors that might be contributing to the difference in smoking prevalence estimates observed between the NY ATS and NY BRFSS studies. Conveniently, the experiment also allowed us to conduct a broader assessment of how introductory scripts and respondent incentives affect overall participation the survey.

The experiment was designed and implemented in the first quarter of data collection in 2006. When telephone numbers were sampled for this quarter of data collection, each case was randomly assigned to one of four experimental groups. **Figure 1** displays the four experimental conditions, which divided sample members among two versions of the introductory script and two alternative incentive plans.

The two introductory scripts tested in the experiment were the original NY ATS script and an alternative script designed to describe the purpose of the survey as a general health survey. The original introduction to the New York ATS read:

**Figure 1: Experimental Design of the Initial Introduction and Incentive Study**

		Incentive Offer	
Introductory Script	Original / \$0	Original / \$20	
	Alternative / \$0	Alternative / \$20	

“Hello, this is \_\_\_\_\_ calling on behalf of the New York State Department of Health. We are conducting an important research study about adults’ beliefs and experiences with tobacco use. I would like to speak to a member of the household who is age 18 or older. Would that be you?”

In contrast, the alternative introduction de-emphasized the focus of the survey on beliefs and experiences related to tobacco use. This alternative script was designed to present the NY ATS as a more general health survey, like the NY BRFSS introductory script. The BRFSS survey is introduced as an interview covering a variety of health risk factors, and does not single out specific risk behaviors such as smoking. The alternative introductory script read:

“Hello, this is \_\_\_\_\_ calling on behalf of the New York State Department of Health. We are conducting an important research study about factors that affect adults’ health status and their beliefs about health conditions. I would like to speak to a member of the household who is age 18 or older. Would that be you?”

In addition, because many households associated with sampled numbers first learn about the NY ATS through other modes of contact, we also created alternative versions of the advance letter, the answering machine message, and the privacy manager message.<sup>4</sup> The text for each of these alternative contact messages was altered in the same way as the alternative introductory script, by substituting the script that explained the purpose of the study with the more general statement about health issues. As applicable, all sampled cases received an advance letter, introductory script, answering machine message, and privacy manager message consistent with their assignment to either the original script or alternative script condition. Because the alternative introductory statement did not emphasize tobacco use like the original script, we expected that households with smokers would initially be more receptive to the alternative statement and more willing to participate in the survey.

The second element of the experiment was the incentive offer. Under the original NY ATS incentive plan, potential respondents were not offered an incentive unless they initially refused to participate. For all cases coded as initial refusals, the refusal conversion protocol offered these sample members a \$20 check to complete the interview. The alternative incentive condition was to offer sample members \$20 as part of the initial survey request. Because the incentive offer was added to the introductory script, this created four different protocols to match the four experimental conditions. For the two conditions in which the \$20 incentive was offered, the original and alternative introductions added the following statement:

“Each eligible participant who completes an interview will receive \$20 as a token of our appreciation.”

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<sup>4</sup> Under the current sampling plan, about 68 percent of sampled telephone numbers are matched to a valid address and are therefore included in the pre-notification mailing. Of course, the proportion of sample members who actually receive and read the advance letters is likely significantly lower.

Like the introductions, the incentive offer was also added to the advance letter, the answering machine message, and the privacy manager message.<sup>5</sup>

The alternative incentive protocol was mainly designed (1) to allow assessment of whether the immediate offer of \$20 would reduce resistance among smokers to participate in a tobacco survey and (2) to contrast this outcome with smoker's response to a survey portrayed as a more general health interview. Because 50% of the sample was assigned to the incentive condition, this protocol also allowed us to assess survey participation among all sample members compared to the no-incentive condition. We expected that offering the incentive would both produce higher participation among smokers and higher participation among all sample members compared to the no incentive condition.

### ***Sample Selection and Data Collection***

The NY ATS sample follows a stratified dual frame design, with sampled telephone numbers drawn equally from (1) a random-digit-dial (RDD) frame and (2) a residential directory-listed frame. This design provides a representative sample while increasing the "hit rate" of current residential units to improve data collection efficiency. Both sample frames are stratified across eight county groups that encompass the entire state, producing a total of 6 strata. The sample design permits the calculation of precise statewide and regional estimates. The only addition to the standard sampling procedures for the first quarter of 2006 was to randomly assign all sampled numbers to one of the four experimental conditions. This procedure eliminated any possibility of the sampling procedures introducing error to the experimental design.

Within each household associated with the sampled numbers, one adult age 18 or older is selected randomly using a modified Trolldahl-Carter selection procedure. This selection procedure is designed to maintain gender balance among respondents by sampling males and females equally in households with at least one eligible man and at least one eligible woman. This is the same procedure followed in most BRFSS surveys, including the New York BRFSS.

When households were reached in quarter 1 of 2006, interviewers delivered one of the four alternative introductory scripts as they appeared in the CATI protocol for each case. Interviewers were regularly monitored by supervisory staff to ensure that the introductions were being read as presented on the screen. At the same time, interviewers continued to respond to any further questions about the goals and procedures of the NY ATS in the same way as they had in previous quarters. The experiment was not intended to deceive sample members nor to withhold information from them about sponsorship of the study and the kinds of questions asked. The intent was to assess sample members' initial reactions to two sets of introductory statements with somewhat different descriptions of the overall survey goals.

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<sup>5</sup> The incentive payment information was also added to the informed consent statement to all respondents in the two \$20 incentive conditions.

The first quarter NY ATS interviews were completed between March 16 and May 4, 2006.<sup>6</sup> Interviewing was conducted seven days a week, with the exception of major holidays. Standard quality control procedures for telephone interviewing, such as regular use of silent audio and visual monitoring to evaluate interviewers' performance, were maintained. Interviews were offered in either English or Spanish, based on respondent preference.

Interviewers followed a standard set of telephone data collection procedures adopted for both ATS and BRFSS surveys. These procedures included the stipulation that cases not otherwise finalized be attempted at least 15 times across multiple calling periods (e.g., days of the week and times of day) before being finalized as non-interviews. The refusal conversion protocol allowed up to three refusals from non-selected respondents in households, but only two refusals from selected respondents. Of course, respondents who specifically requested that their household not be called again, those who expressed agitation and/or used abusive language, and those who threatened legal action were finalized at the initial refusal and no longer pursued.

A total of 1,788 interviews were completed in the first quarter of 2006 under the experimental conditions. The AAPOR response rate 4 for the entire sample was 19.7%. The average interview length was 26.5 minutes, although interviews with smokers averaged 35.3 minutes versus 24.8 minutes for non-smokers. Overall, the data collection results for the quarter 1, 2006 sample looked consistent with preceding quarters in terms of the distribution of completed cases, out of scope cases, refusals, and other non-interviews (such as no contacts, unresolved numbers, and unavailable respondents). **Table 1** provides the distribution of completed interviews across the four experimental conditions and key demographics within each group and for all completed interviews. As this table indicates, none of the small differences in demographic characteristics across the four groups differed significantly. These results confirm that the randomization to experimental conditions appeared to be successful.

### ***Measures and Analysis***

All of the measures used in this analysis were collected through the CATI interview system used for the 2006 NY ATS. The interview included standard demographic items such as respondent's age, gender, ethnicity, race, education, income, employment status, and household composition. The primary substantive distinction among respondents, smoking status, was measured by the combination of two items. These two items are standard in statewide ATS and BRFSS surveys. Additional questions on beliefs about the health risks of smoking, views on current New York state smoking policies, and current health status and conditions were also included in the interviews. Among these survey items, the analysis focuses on current smoking status. The Appendix provides each of the specific survey items and the coding of each variable in the analysis.

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<sup>6</sup> Although the standard data collection period for quarter 1 of the NY ATS is January through March, administrative delays resulted in the later field period.

The analysis was conducted in two parts. First, we examined the results of the initial two by two factorial experiment varying the introductory scripts and incentive protocols. This analysis used cross-tabulation and corresponding tests of significance to ascertain differences across the experimental conditions with respect to both the overall participation of sample members and participation of smokers. Another important indicator was the percentage of completed interviews that were refusal conversions. Looking at refusal conversion results provides a sense of initial cooperation among sample members in each experimental group. We also calculated response rates for each of the four experimental conditions so that we could

**Table 1: Demographic Characteristics across the Four Experimental Groups**

<i>Demographic Characteristics</i>	<i>Experimental Groups</i>				<b>All Completed Interviews</b>
	<b>1 - Original Intro / No Incentive</b>	<b>2 - Original Intro / \$20 Incentive</b>	<b>3- Alternative Intro / No Incentive</b>	<b>4- Alternative Intro / \$20 Incentive</b>	
<b>Age</b>					
18 to 24	4.8	5.0	2.9	4.1	4.2
25 to 44	28.5	26.2	28.5	28.5	27.9
45 to 64	41.5	43.3	39.9	39.5	41.1
65 plus	25.3	25.6	28.8	27.9	26.8
<b>Gender</b>					
Male	39.2	42.0	35.7	36.6	38.5
Female	60.8	58.0	64.3	63.4	61.5
<b>Ethnicity</b>					
Hispanic/Latino	94.2	93.3	94.3	92.9	93.6
Not Hispanic/Latino	5.9	6.7	5.7	7.1	6.4
<b>Race</b>					
White	82.3	82.5	85.2	82.9	83.2
Black/African-American	7.9	9.0	6.8	10.0	8.5
Asian	2.0	1.8	2.3	1.2	1.8
American Indian/Alaska Native	1.0	0.8	0.3	0.2	0.6
Other/Multiple	6.9	5.9	5.5	5.7	6.0
<b>Education</b>					
Less than high school	5.9	7.1	4.9	6.9	6.3
High school graduate or GED/equivalency	22.6	26.9	24.1	27.9	25.6
Some college or 2-year Graduate	24.6	24.9	25.9	25.5	25.2
4-year college graduate or higher degree	46.9	41.1	45.1	39.7	42.9

**Note:** Percentages in the table are based on unweighted data. None of the differences in percentages across the four experimental groups was statistically significant at  $p < .05$ .

assess the impact of the alternative protocols on overall data collection results. Because smoking prevalence is such a critical estimate for the NY ATS, we also generated the weighted smoking rate for each experimental group to better understand the how each condition may have affected this estimate.

The second part of the analysis was based on further experiments with the incentive protocol across quarter 2, 3 and 4 of 2006 of the NY ATS to evaluate the impact of the \$20 incentive offer on overall participation and smokers' participation. These analysis allowed us to determine the impact of alternative incentive conditions with the introductory script held constant across all cases. In this way, these data could also be used to inform our findings from the initial experiment in quarter 1.

When comparing two groups (e.g., experimental conditions), we ran t-tests in order to determine whether the differences in means/proportions across the two groups were significant. When comparing outcomes for more than two groups, we ran chi-squared tests of significance that test for independence between the various groups. For all tests of significance, we adhered to the traditional cut-off where p-values less than 0.05 were considered statistically significant.

### Results 1: The Initial 2 x 2 Experiment

Table 2 provides key data collection results for all Q1 2006 NY ATS respondents across the four experimental groups, including the proportion of completed interviews and refusal conversions. Contrary to our expectation, no differences were observed between the two

**Table 2: Survey Outcomes across the Experimental Groups - All Respondents**

<i>Survey Outcomes</i>	<i>Experimental Groups</i>				<i>All Respondents</i>
	<i>1 - Original Intro / No Incentive</i>	<i>2 - Original Intro / \$20 Incentive</i>	<i>3- Alternative Intro / No Incentive</i>	<i>4- Alternative Intro / \$20 Incentive</i>	
<b>Completed Interviews % (n)</b>	23.0 (411)	27.6 (493)	21.8 (389)	27.7 (495)	100 (1,788)
<b>Refusal Conversions % (n)</b>	24.1 (99)	21.5 (106)	24.2 (94)	22.8 (113)	23.0 (412)
<b>Response Rates (AAPOR 4)</b>	18.3	21.7	17.1	21.7	19.7

**Note:** For simplification, percentages and counts represent the completed cases for each of the four experimental groups and for all respondents. Tests of significance included all cases assigned to each of the four experimental groups, not just completed interviews. All percentages and counts and are based on unweighted data.

original introduction and alternative introduction conditions with respect to either completed interviews or refusal conversions. Varying the purpose of the survey did not appear to have any impact on sample members' participation.

Under both the original and alternative introductory scripts, the proportion of completed interviews was somewhat greater for the incentive than the no incentive conditions. For each version of the introduction, the incentive condition resulted in five to six percent more interviews compared to the no incentive condition. Despite this pattern, the differences between incentive conditions were not statistically significant at conventional levels ( $p < .05$ ). For refusal conversions, the results indicate that a slightly greater percentage of refusal conversion interviews were completed under the no incentive condition versus the incentive condition. These differences were small and also not statistically significant. Consistent with the patterns for completed interviews and refusal conversions, the response rates (APPOR RR4) were three to four percent higher for the two incentive conditions.

Turning to the survey results for current smokers only in Table 3, no differences were observed between the two introductory scripts for either completed interviews or refusal conversions. One significant difference was found based across incentive conditions. A greater proportion of current smokers participated under the incentive condition when the original

**Table 3: Survey Outcomes across the Experimental Groups – Currently Smoking Respondents**

<i>Survey Outcomes</i>	<i>Experimental Groups</i>				<i>All Currently Smoking Respondents</i>
	<i>1 - Original Intro / No Incentive</i>	<i>2 - Original Intro / \$20 Incentive</i>	<i>3- Alternative Intro / No Incentive</i>	<i>4- Alternative Intro / \$20 Incentive</i>	
<b>Completed Interviews</b> % (n)	12.5 <sup>2</sup> (51)	18.3 <sup>1</sup> (90)	15.2 (59)	15.9 (78)	15.7 (278)
<b>Refusal Conversions</b> % (n)	27.5 (14)	18.9 (17)	28.8 (17)	25.6 (20)	24.5 (68)
<b>Weighted Smoking Rates</b>	17.0	19.0	19.8	17.6	18.4

**Note:** For simplification, percentages and counts represent the completed cases for each of the four experimental groups and for all smoking respondents. Tests of significance included all cases assigned to each of the four experimental groups, not just completed interviews. Unless otherwise indicated, percentages and counts and are based on unweighted data.

<sup>1,2</sup> The difference between the result for the experimental group and the other experimental group indicated by number was significant at  $p < .05$ .

introduction was presented. Participation among smokers was about six percent greater in the incentive condition. No such difference was observed with the alternative introduction.

Differences in refusal conversions were also observed across incentive conditions. Under the original introductory script, the proportion of interviews completed via refusal conversion efforts was about nine percent greater in the no incentive condition. Under the alternative introduction, the proportion of interviews completed via refusal conversion efforts was only about three percent greater in the no incentive condition. Neither difference was statistically significant. The lack of significance between the original introduction conditions was likely the result of the small sample sizes involved. Despite some differences in completed interviews and refusal conversions, the weighted smoking prevalence estimates did not vary significantly across experimental groups.

## **Results 2: Additional Incentive Experiments**

Although the initial experiment in Quarter 1, 2006 of the NY ATS indicated no significant differences in overall or smokers' participation based on the introductory script, the similarity of the alternative introduction to the BRFSS script resulted in this introduction being selected for subsequent quarters of the ATS. Differences based on the two incentive conditions argued for further investigation of how incentives might affect overall and smokers' participation in the ATS. For this reason, we conducted further experiments with the two incentive conditions in quarters 2, 3, and 4 of 2006. When phone numbers were sampled for these three quarters, 25 percent of sampled cases were randomly assigned to the \$20 incentive condition. The other 75 percent of cases were assigned to the standard no incentive condition. The survey introduction, pre-notification letter, and answering machine/privacy manager messages for all sample members used the alternative introduction protocol that described the survey as "an important research study about factors that affect adults' health status and their beliefs about health conditions." The only difference in the materials or scripts was that the \$20 incentive was offered to a quarter of sampled households.

Table 4 provides results for completed interviews and refusal conversions across quarters 2, 3 and 4 for all respondents and current smokers only. Among all respondents, the incentive condition produced a significant pattern of differences in both completed interviews and refusal conversions in at least two of the quarters. In both quarters 2 and 4, the distribution of completed interviews was about 70 percent in the no incentive condition and 30 percent in the incentive condition. Given that the sampling proportions were 75 percent to the no incentive condition and 25 percent to the incentive condition, the incentive essentially appeared to increase participation by about five percent over the no incentive condition. A similar pattern was observed for quarter 3, but this difference was closer to four percent.

Significant differences were also observed across incentive conditions for refusal conversions. Interviews completed via refusal conversion were about five percent greater in the

**Table 4: Key Survey Outcomes for Additional Incentive Experiments across Quarters – All Respondents and Currently Smoking Respondents**

Survey Outcomes	Survey Quarters					
	Quarter 2		Quarter 3		Quarter 4	
	No Incentive (75%)	\$20 Incentive (25%)	No Incentive (75%)	\$20 Incentive (25%)	No Incentive (75%)	\$20 Incentive (25%)
<b>ALL RESPONDENTS</b>						
Completed % Interviews (n)	69.6 (1,328)	30.4 (581)	71.4 (1,529)	28.6 (612)	70.5 (2,090)	29.5 (874)
Refusal % Conversions (n)	29.4* (390)	24.6* (143)	29.4 (450)	25.2 (154)	31.9* (667)	26.2* (229)
<b>SMOKING RESPONDENTS</b>						
Completed % Interviews (n)	14.0* (184)	18.9* (109)	14.4 (2190)	16.3 (99)	15.7 (326)	13.0 (113)
Refusal % Conversions (n)	34.2* (63)	19.3* (21)	36.1 (79)	25.3 (25)	32.2 (105)	25.7 (29)

**Note:** For simplification, percentages and counts represent the completed cases for each of the four experimental groups and for all respondents. Tests of significance included all cases assigned to each of the four experimental groups, not just completed interviews. All percentages and counts and are based on unweighted data.

\* Difference between the No Incentive and \$20 Incentive conditions was significant at  $p < .05$ .

no incentive condition for both quarters 2 and 4. Refusal conversions were likewise about four percent greater in the no incentive condition for quarter 3, but this difference was not statistically significant.

Among current smokers, participation was about five percent higher in the incentive condition compared to the no incentive condition in quarter 2. Refusal conversions were significantly greater in the no incentive condition versus the incentive condition in quarter 2. Neither of these results was replicated in quarters 3 and 4. Although differences in completed interviews were clearly insignificant in quarters 3 and 4, refusal conversions appeared to be greater in the no incentive condition for both quarter 3 (11 percent) and quarter 4 (seven percent). These differences were not statistically significant, likely due to the small sample sizes for refusal conversion cases among current smokers.

## **Discussion**

This research evaluated the impact of alternative introductory scripts and incentive offers on overall participation and smokers' participation in a statewide RDD survey. Contrary to our expectations, the alternative introductory script with a modified study purpose did not have any relationship to smokers' participation in the initial two by two factorial experiment. Despite moving the focus of the study purpose away from tobacco use, the introduction did not appear to have any impact on overall participation as well. These findings suggest that either (1) the alternative introduction was simply not more compelling to both smokers and non-smokers in the sample than the original script and/or (2) sample members did not really distinguish the purpose of the study as being ignorantly different across the two introductions. Given the generally brief interactions between interviewers and potential respondents in telephone surveys, sample members presented with either introduction simply did not grasp the unique focus of the study, regardless of whether they (or someone else in their household) ultimately participated. This conclusion highlights the challenge of effectively manipulating survey introductions in telephone surveys, and particularly in RDD surveys where sample members are less likely to be familiar with the study goals and procedures at initial contact.

Offering a \$20 incentive did produce some significant differences in overall participation and smokers participation. Both the original experiment and additional experiments varying the incentive condition showed that the incentive offer was associated with increased overall participation and reduced refusal conversion efforts relative to the no incentive condition. This finding suggests that, unlike the alternative purpose statements in the introductory scripts, sample members actually heard the \$20 incentive offer and generally reacted more positively to the study as a result.

The original quarter 1 experiment and the quarter 2 incentive experiment also indicated that smokers' participation was greater and refusal conversions among smokers were lower in the incentive condition. Surprisingly, these findings did not hold up in quarters 3 and 4. This

finding was the most puzzling result of the series of NY ATS experiments. Although the impact of the incentive offer on smokers' participation appeared to be modest, the differences were both consistent and significant across the first two quarters. We can think of no compelling explanation for the disappearance of this relationship in subsequent quarters. Despite the statistical significance observed in quarters 1 and 2, the relatively modest difference in smoker's participation suggests that the observed effects in the first two quarters could have simply been random variation. The results of the four quarterly experiments did not provide sustained support for the notion that smokers were more receptive to the survey request when the incentive offer was included. Further investigation of the impact of incentive offers on smokers' participation clearly seems warranted.

Two factors could have clouded our findings on the impact of alternative introductory scripts and incentive offers on overall participation and smokers' participation. First, despite the four different scripts in the initial two by two experiment, interviewers continued to answer questions and provide information about the study that was the same across all conditions. That is, if sample members asked questions or requested information, interviewers continued to use standardized NY ATS materials. In cases where potential respondent did ask questions or make requests, interviewers responded in standard ways that were independent of the four specific protocols. As a result, inquisitive sample members may have received additional information before deciding on whether to participate that did not necessarily conform to their assigned protocol. The evidence suggests that this did not happen very often and therefore did not significantly influence the results of the experiments. Most initial interactions between interviewers and potential respondents were quite brief and quickly led to either completing the screener and interview or declining the survey request.

A second factor that limits the strength of our conclusions is that we cannot be certain the results for smokers indicate actual participation rates. Current smokers may have participated at similar rates across experimental conditions, but not identified themselves equally under each condition. This possibility implies that the alternative introductions may have indeed influenced smokers' reaction to the survey request, but not in the way we hypothesized. Cowling, et al (2003) concluded that younger adult females appeared to be under-reporting their current smoking behavior. Like our research, their study could not actually provide direct evidence that younger adult females who smoked were participating at the same rates across different survey conditions, but simply not identifying themselves as smokers equally under all conditions. Still, the possibility that the results for smokers may be at least partially determined by response error to the smoking status questions could not be ruled out in our experiments.

Our findings suggest several avenues to explore further. This paper has only provided basic results from the initial two by two experiment and the further incentive experiments. Additional analysis of non-smokers' versus smokers' participation under the different experimental conditions could shed some light on our findings. For example, examining the relationship of certain demographic characteristics with participation under different

experimental conditions by subgroups may indicate that particular subgroups did react differently to the introductory script or incentive offer. In a similar vein, we can also continue to compare NY BRFSS and NY ATS data to get a more detailed picture of how participation in each study is related to certain demographic characteristics, and especially those factors that are strongly related to smoking status. This analysis might not only clarify why and how smoking prevalence estimates vary across the two surveys, but also provide a broader sense of how both smokers and non-smokers may react differently to the ATS and BRFSS survey requests.

Given Cowling, et al's (2003) conclusions, we could also devise an experimental protocol to disentangle how smoking estimates are affected by lack of participation among smokers in the survey versus misrepresentation of smoking behavior among smokers when they do participate. The goal of such an experiment would be to determine whether the survey request is influencing unit nonresponse or response error among smokers, or some combination of the two. If successful, this experiment would provide valuable evidence on how survey design features, respondent characteristics, and external conditions can be related to participation and reporting among an important population subgroup.

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## Appendix: Survey Indicators and Coding for Variables Used in the Analyses

Demographic Characteristics	Survey Item	Variable Coding
Age	What is your age?  _____ (AGE IN YEARS)	1 = 18-24 2 = 25-44 3 = 45-64 4 = 65+
Gender	For survey purposes, I need to confirm if you are male or female? 1. Male 2. Female	0 = male 1 = female
Hispanic/Latino background	Are you Hispanic or Latino? 1. Yes 2. No	0 = no 1 = yes
Race	Which one or more of the following would you say is your race? 1. White 2. Black or African American 3. Asian 4. Native Hawaiian or Pacific Islander 5. American Indian, Alaska Native 6. Other (specify)	1 = White 2 = Black or African American 3 = Asian 4 = Native Hawaiian or Pacific Islander 5 = American Indian, Alaska Native 6 = Other/Multiple
Education	What is the highest level of school you completed or the highest degree you received? 1. Never attended school/only attended kindergarten 2. Grades 1 through 8 (Elementary) 3. Grades 9 through 12 (Some high school) 4. Grade 12 (High school graduate) 5. G.E.D 6. Some technical or vocational school 7. Some college, no degree 8. AA; technical or vocational school 9. AA; academic 10. BA, BS (College graduate) 11. At least some graduate or professional school 12. Graduate or professional degree	1 = Less than high school (1, 2, 3)  2= High school graduate or GED/ equivalency (4, 5)  3 = Some college or 2-year degree (6, 7, 8, 9)  4 = 4-year college graduate or higher degree (10, 11, 12)
Survey Outcomes	CATI Code or Survey Item	Variable Coding
Refusal Status	Refusal conversion: any prior refusal disposition code Non-refusal: never any refusal disposition code	0 = non-refusal 1 = refusal conversion
Response Rate	AAPOR RR3 calculation	$RR3 = I / [(I + P) + (R + NC + O) + e(UH + UO)]$

Smoking Status	<p>a. Have you smoked at least 100 cigarettes in your entire life?</p> <ol style="list-style-type: none"> <li>1. Yes</li> <li>2. No</li> </ol> <p>b. Do you now smoke cigarettes everyday, some days, or not at all?</p> <ol style="list-style-type: none"> <li>1. everyday</li> <li>2. some days</li> <li>3. not at all</li> </ol>	<p>0= Non-smoker where <math>a=2</math> <u>or</u> <math>(a=1 \text{ and } b=3)</math></p> <p>1 = Smoker where <math>(a=1 \text{ and } b=1)</math> <u>or</u> <math>(a=1 \text{ and } b=2)</math></p>
Smoking Prevalence	<i>(Same survey items as smoking status.)</i>	Weighted proportion where smoker if $(a=1 \text{ and } b=1)$ <u>or</u> $(a=1 \text{ and } b=2)$