

**THE EFFECTS OF INTERVIEW PAYMENTS AND PERIODICITY
ON SAMPLE SELECTION AND ATTRITION AND ON RESPONDENT MEMORY:
EVIDENCE FROM THE PILOT STUDY OF THE NEW IMMIGRANT SURVEY**

Guillermina Jasso
New York University

Mark R. Rosenzweig
University of Pennsylvania

James P. Smith
Rand Corporation

January 1999

Revision of paper presented at the Conference on Data Quality in Longitudinal Surveys, Institute for Social Research, University of Michigan, 28-29 October 1998. We are grateful to participants for useful comments and especially to Robert A. Moffitt. This research was supported by the National Institutes of Health under grant HD33843, with partial support from the Immigration and Naturalization Service and the National Science Foundation.

1. INTRODUCTION

This paper examines the effects of interview payments -- incentives to participate in a survey -- and interview periodicity -- the number and frequency of interviews -- on sample selectivity and attrition and on retrospective memory recall. Our strategy is to use data from the Pilot Study of the New Immigrant Survey, a survey with two special features which enable assessment of the effects of interview payments and periodicity. First, the sampling frame for the New Immigrant Survey consists of the administrative records of specific named individuals, and thus we have a wide array of information on the population that we are surveying, information that can be used to assess the sample outcomes. Second, the Pilot Study carried out experiments on both interview payments and periodicity; interview payment experiments are used to ascertain the interview reservation price, and periodicity experiments to ascertain the effects of number and frequency of interviews on respondent resistance and memory.

Our analyses yield five main results: First, we find minimal selectivity among interviewed members of the sample, with the exception of selectivity on income. Second, there is a strong positive effect of income on both refusal to participate in the survey and on the interview reservation price. Third, respondent resistance increases with the length of the interval between survey rounds. Fourth, for given time to contact, the effect of more interviews is to increase respondent resistance to subsequent interviews. Fifth, however, increasing the frequency of interviews appears to provide more accurate estimates of retrospectively-reported events.

2. OVERVIEW OF THE NEW IMMIGRANT SURVEY AND PILOT STUDY

The New Immigrant Survey. The objective of the New Immigrant Survey (NIS) is to improve the data base on immigrants to the United States in order to substantially advance understanding of the socioeconomic status of immigrants and their children and the effects of immigration in the United States. The plan of the NIS is to carry out, for the first time, a comprehensive, multi-cohort longitudinal survey of new legal immigrants to the United States

based on representative samples of the administrative records, compiled by the U.S. Immigration and Naturalization Service (INS), pertaining to immigrants newly admitted to permanent residence. To monitor changes across cohorts, new samples will be drawn periodically. To monitor adaptation over time, each sample will be interviewed at regular intervals over the life cycle. To assess the immigrants' legacy, information will also be obtained about and from their children, both the immigrant children they brought with them and the U.S. citizen children born to them in the United States.

NIS Sample Design. The sampling frame for the New Immigrant Survey consists of all persons admitted to legal permanent residence during a given time interval. The administrative records which constitute the sampling frame include the address to which the new immigrant has requested that the green card be mailed. This is without doubt the best possible address at which to locate sampled immigrants.¹

The NIS Pilot Study. The NIS Pilot Study (NIS-P) is the first step in the NIS program of multi-cohort longitudinal research. The NIS Pilot Study has three aims: (i) to assess the cost-effectiveness of alternative methods for locating and maximizing the initial response rates of sampled immigrants; (ii) to explore the costs, feasibility, and effectiveness of alternative methods of tracking over time sampled immigrants after their initial contacts that will permit a longitudinal survey of a highly mobile population with minimal attrition; and (iii) to obtain immediately useful information from the NIS pilot that would both aid in the design of survey instruments for the full survey and provide new and important information on recently-admitted legal immigrants. The NIS-P follows the design of the NIS except that, for budgetary reasons, it is a telephone survey. Finding the telephone numbers of sampled immigrants was thus the first challenge, a point to which we return below.

¹ At admission to legal permanent residence, the new immigrant has all the rights of legal permanent residents. Evidence of legal permanent status initially consists of a stamp on the individual's passport; a special identification card, known popularly as a "green card," is then manufactured and mailed to the new immigrant, usually within six weeks to three months after admission to permanent residence.

NIS-P Sampling. The sampling frame for the NIS Pilot Survey consists of all persons who were admitted to legal permanent residence during the months of July and August of 1996. This is a sampling frame whose sampling units are specific named individuals. The total number of immigrants admitted during this period was 148,987. Because children are quite numerous among immigrants and because employment-based immigrants, in whom there is great interest, are a relatively small category, we drew a stratified random sample, undersampling children and oversampling the employment-based. The strata were defined as follows: (i) all children (defined as being under age 18 at admission to permanent residence) went into one stratum, with a probability of selection equal to .003715; (ii) all adults (defined as age 18 or over) with employment-category visas (including the spouses and children aged 18+ of employment-based principal immigrants) went into the second stratum, and were assigned the highest probability of selection (.047201); and (iii) all other immigrants went into the third and last stratum, assigned a probability of selection of .013486. Accordingly, employment-based adult immigrants were 3.5 times as likely to be drawn as other adult immigrants. The initial sample thus drawn numbered 2,001 persons. Of these, 20 declared as their place of intended residence a locale outside the 50 states (such as Puerto Rico, Guam, or the Virgin Islands); of these twenty, three adults were retained in the sample in order to test methods for contacting individuals outside the 50 states (two persons from Puerto Rico and one from the Virgin Islands), leaving an effective sample of 1984 persons. Of these, 1,839 were adult immigrants (including the three resident in Puerto Rico and the Virgin Islands).

Our strategy is to conduct interviews with sample members from the second and third strata (these are age 18 and older) and with the parents or caregivers of the sample members in the first stratum. In this paper we focus on the second and third strata, examining data on adult sampled immigrants.

NIS-P Survey Cycle and Periodicity Experiment. The NIS Pilot Study consists of a baseline survey, a three-month follow-up of half (randomly chosen) of the baseline-interviewed sample, a six-month follow-up of all the baseline-interviewed sample, and a one-year follow-up,

also of the baseline-interviewed sample. The purpose of the three-month interview is to assess the effects of interview number and frequency on sample attrition and recall bias.

Locating Sampled Immigrants. Because the NIS-P is a telephone survey, the first step was to obtain telephone numbers for the sampled immigrants. Methods used to obtain telephone numbers included: (i) using telematching services based on telephone company electronic and white page databases (matching on name and address, electronic directory assistance, and matches given on address only); (ii) calling directory assistance; and (iii) sending out advance notification letters -- to the address to which the sampled immigrants had requested that their green cards be mailed -- informing prospective respondents that they had been selected to participate in the survey. In these letters, respondents for whom we did not have telephone numbers were asked to send back an address and telephone update card or to call a toll-free number.

Through these methods we obtained telephone numbers for slightly over half of the sampled immigrants aged 18 and over (53.3 percent -- 980 out of 1839). This group was ready for contact.

Several factors contributed to not obtaining all telephone numbers using these methods on the sample of new immigrants. The principal constraining factor was a quite high rate of unlisted and unpublished numbers. Unlisted and unpublished numbers are a growing trend in this country and there are particularly high rates in those geographic areas in which immigrants concentrate. For example, unpublished and unlisted telephone numbers run as high as 69% in some urban areas which coincide with high immigrant concentration such as Los Angeles. These problems, which exist for all populations, were compounded by very common last names for some ethnic groups (e.g., Koreans). Finally, immigrant women and minors typically do not have telephone numbers listed in their names (in some origin-country groups wives have surnames which are different from their husbands' surnames).

Finding the telephone number for the remainder (46.7 percent) required expensive fieldwork. A staff of field trackers were sent out to locate respondents and obtain telephone

numbers in areas where sampled immigrants were concentrated: Los Angeles area, Northern California, and parts of the following states -- New York, New Jersey, Connecticut, Florida, Texas, Washington, Michigan, Virginia, Maryland, Massachusetts -- as well as Washington, DC. For budgetary reasons, we eliminated from fieldwork those cases whose addresses indicated were living outside the most populous states or in widely scattered areas; the number thus eliminated was 226 (12.3 percent of the sampled immigrants aged 18+). Not all cases which received fieldwork led to contact with the prospective respondent, as fieldwork indicated that some of the sampled immigrants had no telephone number or had moved.

Interview Payments. A variety of incentives for participation in the survey was provided. At the baseline, all prospective respondents were made an initial offer of \$5 for participation. Among those who refused, the offer was raised to \$100. At the six-month interview, a randomly chosen half of the prospective respondents were offered \$5, and the other half \$15. Among those who refused, a randomly chosen half were offered \$25, and the other half \$100.

Interview Language. Interviews were conducted in 18 languages. The interview instruments were translated into six languages -- Spanish, Chinese, Russian, Polish, Korean, and Vietnamese. In addition, bilingual interviewers fluent in eleven other languages were hired to interview respondents in those languages.

Information from Administrative Record. As noted above, a useful feature of the NIS design is that, given that the sampling frame consists of the administrative records of all persons admitted to legal permanent residence in a particular time interval, we have information on the population to be surveyed. In the analyses reported below we use this information to assess the selectivity of response rates and attrition rates.

The INS immigrant record provides information on age, sex, marital status, as well as on several migration-relevant variables. These include country of origin, detailed information on the immigrant visa class of admission, the month and year of admission to legal permanent residence and, for the subset of immigrants who are adjusting from a nonimmigrant status, year

of admission to that nonimmigrant status.²

We use the information on class of admission to construct indicator variables for six major visa classes. These are: spouse of U.S. citizen, parent of (adult) U.S. citizen, employment-based, sibling of U.S. citizen, refugee/asylee, and diversity immigrants. The last four visa classes -- employment, sibling, refugee/asylee, and diversity -- include the principal immigrant's spouse and minor children.

It is also possible from information on the immigrant record to construct a measure of immediately prior residence in illegal status. First, the immigrant visa type includes some categories designated for illegals who qualify for permanent residence; examples include suspension-of-deportation cases and registry-provision legalization.³ Second, the nonimmigrant class codes include a category for persons who entered without inspection and are adjusting to legal permanent residence without first leaving the country, under the section 245(i) provisions of the Immigration and Nationality Act in effect in 1996. Accordingly, we construct a measure that classifies as having prior illegal experience those sampled immigrants whose immigrant visa signals prior illegal experience plus those whose nonimmigrant visa indicates prior illegal experience.⁴

Occupation is the only indicator of skill in the INS immigrant record. It is coded by a 29-category variable devised by INS based on the 1980 Census; the 29 categories represent 25 occupation groups and four non-occupation groups (such as "housewife" and "student"). We construct an earnings measure by coding the 25 occupation groups by the average earnings of all

² However, long residence with a succession of nonimmigrant visas is ignored except for the final nonimmigrant status.

³ The registry provisions of U.S. law grant permanent residence to individuals who have resided illegally in the United States for many years. Currently, as fixed by the Immigration Reform and Control Act of 1986, the requisite period of residence must have begun on or before January 1, 1972. Prior to this legislation, the registry cutoff date was 30 June 1948.

⁴ This measure is largely a measure of prior "entry without inspection" experience and thus understates the true immediately prior illegal experience; as well, it understates the total prior illegal experience.

native-born men aged 21-65 working at least 35 hours in a "typical" week and at least 50 weeks in 1979, based on the public use 1:1000 sample from the 1980 U.S. Census. The resulting occupational earnings measure may be thought of as a broad measure of skills. It captures shifts in earnings due to shifts in occupation but not earnings differences within the broad occupation category.⁵

3. RESPONSE AND ATTRITION RATES IN THE NIS-P AND OTHER SOCIAL SCIENCE SURVEYS

Table 1, panel A, reports the number of sampled immigrants by seven response outcomes at baseline -- completion of the interview, refusal by the prospective respondent, refusal by someone in the prospective respondent's household, and four types of noncontact, namely, no fieldwork, no telephone number, moved, and other no contact (including illness and death). As shown, 61.4 percent of the sampled immigrants completed the baseline interview and 9.2 refused to be interviewed, with an additional 1.2 percent refusing via a household member. The remaining 28 percent are cases where no contact was ever made with the sampled immigrant or a member of his/her household (Table 1, panel B).⁶

The overall response rate of 61.4 percent in the baseline round masks the high response rates when contact was actually made with prospective respondents – 85.5 percent (Table 1, panel C). The high completion rates among contacted immigrants suggest that if the NIS-P had been a face-to-face survey rather than a telephone survey, with fieldwork conducted with all sampled

⁵ Caution is required in interpreting occupation, as follows. For immigrants who acquire employment-based visas, occupation refers to the occupation which enabled visa acquisition. For all others, occupation refers to the self-reported occupational title. Among immigrants who adjust status, occupation probably refers to actual or intended occupation in the United States. However, for newly-arriving, non-employment-based immigrants, it is not known whether the occupation they report refers to occupation in the home country or in the United States.

⁶ With respect to refusals by a member of the sampled immigrant's household, it is not known whose decision the refusal was. For example, an elderly sampled immigrant may have been discouraged from participating in the survey by grandchildren who wanted to use the telephone, etc., etc.

immigrants rather than with only a subset, overall response rates would have been substantially higher. The full New Immigrant Survey will be a face-to-face survey, eliminating most of the noncontacted component of the nonresponse.

Table 1 also reports the completions and refusals at the 6-month round. Here, the total number of prospective interviewees consisted of those who had completed the baseline interview (1130 persons aged 18+). Of these, 91.5 percent completed the 6-month interview, and 8.5 percent refused to be interviewed.

All surveys encounter difficulties in finding respondents; some prospective respondents, if found, will refuse to participate in spite of the best efforts of survey organizations. Table 2 lists some relevant data for some of the better social science surveys. In sharp contrast to the NIS-P, all surveys listed relied on face-to-face contact in the baseline round. The baseline completion rates for HRS and AHEAD were around 80% while that for the 1968 PSID was 76%. Reasons for non-completion are relevant for comparisons to the NIS-P. For example, there are very few cases of noncontact in HRS, whereas in the NIS-P a large fraction of our non-completes is attributable to noncontact, due to reliance on the telephone. If these no-contact cases are deleted, the baseline experience of the NIS-P stands up well using these surveys as a standard.

There is one type of nonresponse where the NIS-P has a distinct advantage. In many household surveys, interviews are not conducted and it will never be known whether or not the household was eligible for the survey. This problem is most apparent in the PSID-Immigrant sample. Because immigrants who arrived since 1968 were not part of the PSID sample frame, the PSID recently added a supplemental sample of such households to preserve cross-sectional representativeness. A household screen was used to find a very selective sample -- immigrants who were not in the United States in 1968. As Table 2 indicates, in many cases no contact was made with the household or there was unknown eligibility in a household that was contacted. If completion rates are defined over all such households, it would only be 19%. In the NIS-P, in contrast, there is no ambiguity about eligibility, and this is one of the unique advantages of using INS administrative records. Even if the noncontacted households are excluded, the completion

rate of the PSID immigrant survey is 72%. Since PSID did face-to-face contact and the NIS-P only partially relied on field work, the overall completion rate in the NIS-P stands up quite well for an immigrant population.

Panel B of Table 2 lists attrition data for these gold standard surveys. The periodicity is two years in HRS and AHEAD and one year in the PSID. Attrition rates in HRS and AHEAD run about 7% or 8%. While its initial attrition rate was 11%, PSID has now stabilized at a much lower attrition rate of about 3% per year. All these surveys rely on face-to-face tracking of difficult cases so that noncontact is a minor reason for attrition. This option of face-to-face tracking was not available for budgetary reasons in the NIS-P. Since face-to-face tracking would be available in a full New Immigrant Survey, sample attrition would not be a more serious problem in a New Immigrant Survey than in the better social science surveys, despite the special character of the target immigrant population.

4. COMPARING SAMPLED AND INTERVIEWED IMMIGRANTS

We first exploit the availability of information on the characteristics of all the sampled individuals to assess the representativeness or selectivity of the baseline respondents. Completion of the baseline interview of the NIS-P is the result of two processes: (i) locating the sampled immigrant, and (ii) conditional on locating the sampled immigrant, completing an interview. Both processes are potentially affected by the characteristics and behavior of both the survey team and the prospective respondent. Devices available for locating and attracting prospective respondents differ in cost; and their use as well as the subsequent outcome may not be unrelated to the prospective respondents' characteristics. Accordingly, a prime challenge is to assess the cost-effectiveness and the outcomes of the survey tools.

Based on the information from the administrative records, we compare the characteristics of the interviewed immigrants with those of the sampled immigrants. As shown in Table 3, the two groups are remarkably similar. Interviewed immigrants are younger by only about one year, on average, than the sampled immigrants, and their percent married is lower by less than two

percentage points.

Information on class of immigrant admission indicates that persons who become immigrants as the spouses of U.S. citizens, with employment-based visas, or by adjusting from a refugee/asylee status are slightly overrepresented among the interviewed, while parents and siblings of U.S. citizens, as well as diversity (lottery) immigrants are somewhat underrepresented. The largest discrepancies involve parents and siblings, but the largest of these -- for siblings -- does not reach two percentage points (a discrepancy of 1.9 percentage points).

The sampled and interviewed immigrants also do not differ appreciably on time in the United States prior to admission to legal permanent residence; nor do they differ with respect to prior illegal experience.

Immigrants from Europe are slightly overrepresented (20.2 percent of the interviewed versus 17.4 percent of the sampled), and immigrants from Africa and Asia slightly underrepresented (a discrepancy of one percentage point in the case of Africa and almost three percentage points in the case of Asia).

Table 3 also reports percentages for three origin countries, Mexico, the Philippines, and countries of the former Soviet Union, selected because they account for the largest shares of immigrants (e.g., 13.1 percent of the sampled immigrants were born in Mexico and 7.1 percent in the Philippines). As shown, the interviewed underrepresent immigrants born in Mexico (by one percentage point) and the Philippines (by less than half of one percentage point) and overrepresent immigrants born in the former Soviet Union (1.7 percentage points).

Immigrant Characteristics and Mechanisms of Nonresponse. Table 3 does not distinguish between types of nonresponse, blending together the two distinct processes of noncontact and refusal. To begin to assess whether the observed albeit minimal over- or underrepresentativeness of immigrants of particular characteristics among the interviewed is attributable to noncontact or to refusal, we report in Table 4 differences in characteristics between immigrants who completed the interview and immigrants in each of the six non-completion categories presented in Table 1. The first column repeats the information from Table 3, and the subsequent columns provide both

the difference and the t -ratio for testing the statistical significance of the difference. For example, the row for prior illegal experience indicates that while immigrants with prior illegal experience constitute 11 percent of the interviewed, they constitute 16 percent of those with no telephone but only 6 percent of those in the other no contact category (mostly ill or dead); however, neither of these differences is statistically significant (the only statistically significant difference is with the group that did not receive fieldwork).

The results in Table 4 indicate that sampled immigrants with whom it is more difficult to establish contact include the elderly (visible in the statistically significant differences in age and in percent immigrating as parents of U.S. citizens), siblings, and diversity immigrants, as well as immigrants who are already in the United States when they become legal permanent residents. The mechanisms, however, appear to differ. While siblings are more likely to have moved before contact was made and diversity immigrants less likely to have a telephone, the elderly are more likely to have become ill or died.

Refusal by the prospective respondent is more likely to occur among older immigrants, immigrants who are married, and immigrants with sibling or employment visas. Refusal by a household member is more likely among immigrants from Asia.

Combining the results of Tables 3 and 4 suggests that the reason that the interviewed are younger and less likely to be married is that age and being married are associated with refusal by the prospective respondent, with, additionally, older immigrants more prone to noncontact due to illness or death. The underrepresentativeness of parents, siblings, and diversity immigrants also appears to be related to distinct mechanisms, namely, noncontact for parents (due to illness or death), no telephone for diversity immigrants, and both noncontact (due to residential mobility) and refusal for siblings.

Similarly, the overrepresentativeness of European immigrants appears due to their living in fieldwork areas combined with the lowest observed refusal rates. Relatedly, the result that refugee/asylee immigrants are overrepresented among the interviewed reflects the facts that they live in areas where fieldwork was conducted and that they move less. As for the

underrepresentativeness of immigrants from Asia, it appears due to the very high refusal rates (they constitute 36 percent of the interviewed but 47 percent of the refusals by respondent and 68 percent of the refusals by household members).

Viewed jointly, the results of Tables 3 and 4 also provide evidence of countervailing effects. Consider, for example, sampled immigrants with employment visas. They are slightly overrepresented among the interviewed, but this masks two important facts. First, they apparently live in areas where fieldwork was carried out, and thus the likelihood of locating them is substantially higher. Second, however, they have a substantially high refusal rate. Thus, if they lived in areas where fieldwork was not carried out and if residential and refusal propensities are unrelated, they would have been seriously underrepresented among the interviewed.

Of course, the results just presented are mainly descriptive and examine immigrant characteristics one at a time. It is important thus to conduct a multivariate analysis that will permit assessment of the *ceteris paribus* effects of immigrant characteristics on nonresponse.

Determinants of Nonresponse. We now address the question of the effects of immigrant characteristics on immigrant behaviors culminating in either completion of the baseline interview or nonresponse. Table 5 presents multinomial logit estimates of a model that contrasts the interviewed with five types of nonrespondents – immigrants in the two refusal categories and the three no-contact categories of no telephone, moved, and other no contact -- investigating the effects of personal and visa characteristics. Of course, sampled immigrants who received fieldwork received a different treatment than sampled immigrants who did not, including sampled immigrants who did not need fieldwork because they had already been found; if immigrant characteristics are correlated with residential propensities, then it is important to control for residing in a no-fieldwork area. Accordingly, we include a binary variable indicating whether the immigrant lives in an area that was eliminated from fieldwork.

We estimated models with origin-continent and origin-country dummies; statistical tests reject country effects, and thus we omit country and continent regressors. The continent and country differences observed in the descriptive comparisons appear to reflect differences in visa

characteristics and in the survey staff's fieldwork decisions. Accordingly, the estimates presented in Table 5 describe the effects of age, marital status, and sex, the six visa groupings plus prior illegal status and time in the United States, and the no-fieldwork-area dummy.

The estimates indicate that diversity immigrants and immigrants with prior illegal status are significantly more likely to not have a telephone and that parent, sibling, and diversity immigrants are more likely to have moved. Sibling and employment immigrants are significantly more likely to refuse to be interviewed. These results sharpen our understanding of nonresponse mechanisms and pave the way for efficient use of resources in locating sampled immigrants and attracting them into an interview.⁷

Thus far, we have focused on immigrant characteristics that are available in the immigrant record for all the sampled immigrants. As noted above, the immigrant record also contains, for a subset of the immigrants, their occupation; and, as described above, we constructed a measure of occupational income defined as the average earnings in 1979 of workers in that occupation employed full-time year-round. To assess the effect of income level, we estimate a second multinomial logit model that includes occupational income as well as a binary variable for whether the income measure is missing.

The results, reported in Table 6, indicate that, at least among men, income is the most important predictor of nonresponse at baseline. Occupational income is tightly linked with refusal by the respondent, being statistically significantly associated only with refusal by the respondent. Refusal by the respondent, in turn, is statistically significantly shaped only by occupational income and the indicator variable for missing occupational income; no other variable has a statistically significant effect on respondent refusal. Quantitatively, a \$10,000 increase in income at the mean leads to a 22-26 percent increase in the probability of refusal. To interpret the effect of the missing-income dummy, we note that among male interviewed

⁷ The results also indicate that residing in areas excluded from fieldwork is associated with refusal by the prospective respondent. This is an intriguing result that we hope to study further. But note that in the full NIS fieldwork will be conducted with everyone.

immigrants who did not provide an occupational title to INS, almost half (44 percent) reported in the NIS-P baseline that they were employed and their earnings are on average \$19,000 higher than the earnings of male immigrants who did provide an occupation to the INS. Thus, higher earnings are significantly related to a higher probability of refusal, suggesting that the effect of employment visa observed earlier merely reflects the operation of income. Additionally, immigrants who do not provide an occupational title -- higher-earnings immigrants -- are more likely to move and thus not be found.

The only visa characteristic effect that survives analysis once income is introduced, among men, is the higher probability of diversity immigrants to not have a telephone. At least among men, income wipes out all other effects.

In both the models for the full sample and for men only we also tested for differences between refusal by the prospective respondent and refusal by a household member, and found no significant differences. Accordingly, in the next section, where we estimate the interview reservation prices of contacted immigrants, we include in the sample individuals who completed the interview and individuals who refused to participate, inclusive of sampled immigrants whose refusal was made by a household member.

We conclude from these analyses that among legal immigrants participation in a survey is not selective by origin country and that the observed selectivity by visa class of admission reflects, at least among men, a strong income effect. The higher the immigrant's income, the higher the probability of refusal.

5. DETERMINANTS OF THE INTERVIEW RESERVATION PRICE

The estimates in Tables 5 and 6 suggest that unwillingness to provide a survey interview is not randomly distributed in the contacted sample of immigrants in the population. Estimates of who is more reluctant to participate, however, do not provide much guidance as to how to reduce interview nonparticipation. As part of the NIS-P, experiments were undertaken to assess the responsiveness of individuals to interview payments. Two kinds of experiments were carried out:

First, for the baseline and 6-month interviews, all contacted individuals were initially offered a payment if they successfully completed an interview. Those resistant to the interview were then offered a higher payment, of \$100 at the baseline interview and either \$25 or \$100 (randomly determined) for the 6-month round. The second experiment, conducted at the 6-month round, varied the initial payment randomly, offering approximately half the sample of those completing the baseline interview either \$5 or \$15 (all individuals were offered \$5 initially for the baseline interview) if they completed the 6-month interview.

These compensation experiments provide a means of characterizing the distribution of interview resistance in the population. One convenient way to summarize the distribution of interview resistance is by the interview reservation price (IRP). Presumably, contacted members of the target population compare their well-being from participating in the survey to that from nonparticipation when asked to participate. The reservation price of participation is the pecuniary payment that would make the contacted person indifferent between participation and nonparticipation. Knowledge of the IRPs of the targeted population would permit a more efficient use of interview payments, as those predicted to have low reservation wages could be offered lower interview payments, thus saving money, while reserving the higher payments for those predicted to have high IRPs. The targeting of interview payments would not only increase overall survey participation, for a given payment budget, but would reduce the selectivity of the interviewed sample by increasing the participation of high-IRP individuals and may reduce the interval between first contact and a completed interview by avoiding the process of “bidding” up the interview payment, thus conserving additional survey resources.

Table 7 provides the nonparametric distribution of IRPs (ρ 's), for those individuals contacted in the baseline and 6-month rounds. As can be seen, the limited experiment at the baseline, with only two payment levels, and the fact that 82% of those contacted participated at the \$5 payment, does not provide a great deal of information about the distribution of the IRP. It is apparent, however, that a payment as high as \$5 was probably not necessary for the bulk of the sample, while a \$100 payment was insufficient to obtain the participation of 10% of the sample.

The experiments at the 6-month round provide more sample intervals. The 6-month interval distribution also suggests, consistent with that for the baseline, that most of the sample that participated in the first-round interview would have participated for less than \$5 in the 6-month round. Thus, providing an initial payment of \$15 rather than \$5 was not successful in increasing the participation rate. However, the results suggest that over 8.5% of those participating in the baseline interview had an IRP at the 6-month round exceeding \$25, and almost 3% had an IRP exceeding \$100.

Given that for the full NIS, the INS-record information will be available to the survey staff, information on how the characteristics of the target population provided in those records are related to IRPs will be useful in identifying to whom to offer high interview payments and low interview payments in order to minimize sample attrition at least cost. We can use the information from the NIS-P on the payments offered, on interview participation, and on characteristics in the INS records for the sample to estimate the relationship between the characteristics of the sampled population and the IRPs, with an assumption about the population distribution of IRPs. Table 7 suggests that the IRP distribution may be highly skewed. If we assume that the IRPs have a lognormal distribution, then we can use the technique of (normal-distribution) "interval" or "grouped data" regression to identify the relationships between the characteristics of the sample and the IRPs by applying a log transformation to the payments offered.

We can also test using these data one of the conclusions of the survey literature, that interview resistance increases with the length of time between contacts. We can assess to what extent the IRPs rise with contact interval length because there is wide variation in the sample in three intervals: (i) between admission to legal permanent residence (contact with the INS) and first contact for the NIS-P baseline survey; (ii) between the date of interview completion at the baseline and the first contact for the next round; and (iii) between the date of interview completion for those in the 3-month sample and first contact for the 6-month round. Table 8 provides the mean number of days that elapsed for each of these first-contact intervals and the

standard deviation of days for each interval for all contacted respondents. Note that the variation in time to first contact is not correlated directly with the behavior of the immigrant, unlike the time to completion of an interview, which depends on compliance behavior. However, there was no plan at the survey organization to randomize contact intervals, so that it is possible that such intervals are correlated with unmeasured factors related to immigrant interview resistance. Caution must thus be applied in interpreting the relationship between contact interval length and the IRPs. This is especially so for the post-baseline contact attempts, when the survey field staff had compliance experience with the individual immigrants. We examine below to what extent these first-contact intervals are correlated with observables correlated with IRPs.

The first column in Table 9 reports the estimates of the determinants of the log-IRPs for all initially contacted immigrants, excepting those immigrants residing in zipcode areas in which the survey organization did not carry out field visits to immigrants who did not provide telephone numbers by mail or for whom a telephone number could not otherwise be obtained.⁸ The covariates include the time from immigration to first contact and its square, as well as those INS-record characteristics used to examine nonresponse at baseline in Tables 4 and 5. The results indicate that the distribution of IRPs is not wholly random. Married immigrants and employment-visa immigrants have IRPs approximately four times those of all other immigrants excepting sibling-visa immigrants, who have IRPs over 5 times higher.⁹ The results also suggest that the reservation price for interview participation does rise, at a decreasing rate, with time to first contact. At the sample mean, a one month (30-day) delay in time to first contact from the date of immigration almost doubles the reservation price. However, not surprisingly given the baseline nonparametric distribution displayed in Table 7, the average IRP (ρ) based on the estimates is less than one cent. Thus, for most immigrants, the amount of the interview payment

⁸ This is to obtain a sample that is homogeneous with respect to contact treatment.

⁹ Again, the origin countries of the immigrants do not appear to be significantly related to IRPs. The set of major country dummy variables when included in the specification reported in Table 8 for the total immigrant population and for male immigrants was not jointly statistically significant at the .4 level.

is not an important factor for participation, although the threshold for participation differs significantly across different visa classes.

Another conclusion of the survey literature is that the higher-income population is likely to be underrepresented in surveys. We can use our measure of occupational income for the immigrants to assess to what extent income, or occupational skill, is correlated with the interview reservation price. The second column of Table 9 reports the estimates of the determinants of the IRP for male immigrants. For this population, occupational income and the non-reporting of an occupation are the only significant determinants of the IRP besides time to first contact, just as occupational income and the non-reporting of an occupation were the only significant determinants of refusal to participate in the survey. The estimates suggest that the interview reservation price for those immigrants with an occupational income of \$38,700 (the top 5% of the occupational income distribution in the sample) is 5 times that of immigrants with an occupational income at the sample mean of \$18,700 for those reporting an occupation. Higher-skill immigrants thus do appear to be more resistant to being interviewed. Moreover, the reservation price for those immigrants not reporting an occupation to the INS is over 9 times higher than that of other male immigrants.¹⁰ The INS records thus provide a very good signal for gauging which immigrants have high interview resistance. Again, however, the average population IRP for the baseline interview is low for this population, at 2.4 cents.

6. IDENTIFYING THE EFFECTS OF CONTACT DELAY AND FREQUENCY OF INTERVIEW ON INTERVIEW RESISTANCE

A second survey experiment carried out as part of the NIS-P was to ascertain the effects on sample attrition and on reporting accuracy of increasing the frequency of interviews. Half of

¹⁰ 44% of those male immigrants who were interviewed at baseline and did not report an occupation to the INS provided both an occupation and earnings information to the NIS-P. Those reporting earnings at baseline who did not report an occupation on the INS form had earnings that were \$19,000 higher than those of the male immigrants who did report an occupation. This suggests that part of the reason that those who did not report an occupation to the INS have a higher interview reservation price is that they have higher earnings.

the sample of those completing the baseline interview were randomly selected to receive both a three-month and a six-month interview, the other half only receiving a six-month interview.¹¹ In the next section we explore the effects of having more frequent interviews on recall bias. Here we look at the effects of having more frequent interviews on the interview reservation price and thus on sample attrition. In particular, we estimate how completing a three-month interview affected the interview reservation price at the six-month interview.

There are two hypothesized effects on interview resistance from increasing the frequency of interviews. First, the shorter contact periods should, as our estimates in Table 9 for the baseline round suggest, lower the IRP at each round. However, asking for additional interviews may increase respondent “fatigue” and hence increase resistance in subsequent survey rounds. We can identify the latter effect, given that those receiving the three-month interview were randomly selected, by controlling for the length of time between the first six-month-round contact and the prior completed interview date, regardless of whether that interval was nonrandomly determined by the survey field staff. Including the interval length in the specification controls for the effect of having an extra interview at three months on the interval between contacts at the six-month round, thus isolating the pure effect on respondent resistance of having an extra interview. As long as there is some variation in time from last contact at the six-month interview that is not determined by having a three-month interview, the extra- interview effect is identified. However, to infer from the relationship between the contact interval and the IRP the pure effect of interval length, given the number of interviews, on the IRP requires the additional assumption that time to first contact at the six-month round between the completion of the three-month interview or the completion of the baseline interview was also essentially random.

Table 10 provides the estimates of determinants of the six-month round IRPs for the

¹¹ An exception to this is that 168 persons eligible for the three-month interview were only given the six-month interview, as explained in footnote 12 below. Inclusion of an indicator variable for this group of excluded three-month-eligible respondents in the analyses reported below indicated that they were not statistically significantly different from those both eligible for the three-month interview and contacted for a three-month interview.

immigrants who completed the baseline interview. The first specification, estimates from which are reported in column one, includes as determinants (i) the INS-record characteristics of the immigrants used in our prior estimates, (ii) the number of days that elapsed between the completion of the last interview by the immigrant and the first contact with the immigrant made by the survey field staff for the purpose of obtaining a six-month interview, (iii) that interval squared, (iv) the total elapsed time between the date of immigration and the six-month first contact, and (v) whether or not the immigrant had been interviewed in the three-month round.

The estimates indicate that having the extra interview at three months, controlling for time to last contact, significantly increases the interview reservation price at the subsequent round, by almost fourfold. The three-month/six-month experiment thus suggests that there is an important effect of adding extra interviews, controlling for frequency of contact, on respondent resistance to subsequent interviews. The estimates also indicate that the average IRP at the six-month survey round is \$3.37, higher than that estimated for the baseline, also consistent with the effect of increasing respondent fatigue across rounds (but still lower than the lowest initial payment offer of \$5).¹²

The coefficient estimates suggest that on average the randomly-selected three-month interviewees would require a payment of \$13.50 at the next round while those not asked to complete the additional prior interview would only need \$3.37, if the contact interval were the same. On average, however, the contact interval is three months shorter at the six-month round for those receiving the three-month “treatment” and the estimate of the interval effect on the interview reservation price is positive, as in the Table 8 estimates. The net effect on the IRP at the six-month round of adding the intermediate round interview (the combined respondent fatigue effect and the shorter interval effect) at the mean interval length for those not receiving a three-month interview is to lower the IRP by \$1.42 -- the fatigue effect is dominated by the contact interval effect. This suggests that among those completing the 6-month round interview, those

¹² Note that the average IRP at the baseline round for those completing the baseline and thus included in the six-month sample is lower than the total population IRP reported in Table 9.

who also received the three-month interview are less selective but inherently more resistant (have higher IRPs) while among those who completed the six-month survey with the same contact interval those who also had a three-month interview are more selective and have lower IRPs.

As noted, to interpret the coefficients for interval length as providing estimates of causal effects requires that field-staff decisions concerning time to contact are not correlated with inherent respondent interview resistance. For example, the survey field staff may have adopted rules governing the sequencing of contacts for post-baseline interviews that took into account the difficulty of obtaining an interview at the baseline. One indicator, available to the field staff after the completion of a baseline interview was whether or not the respondent had to be offered the higher payment, an indication that the respondent has higher resistance likely correlated with the 6-month IRP. In the second column of Table 10 we report estimates of the determinants of the 6-month reservation price that includes in the specification whether or not the respondent at baseline required the higher \$100 payment. If this importantly affected the contact attempts, then its inclusion should significantly alter the coefficients on the interval variables. As can be seen, however, the estimates are hardly affected.

To more directly assess to what extent the time to first contact for the 6-month round from the baseline completion date was nonrandom, we regressed that interval length on whether or not the respondent had a three-month interview, the pre-baseline contact interval, the variable indicating whether or not the respondent required a \$100 payment to complete the baseline interview, and the set of INS-record characteristics of the respondent. These estimates thus characterize the “rules” used by the survey field staff to determine time to first contact. The estimates are reported in Table 11. The results indicate that (i) requiring a three-month interview shortened the last contact interval by three months, and (ii) the longer the time it took to contact a respondent the first time the shorter the post-baseline contact delay, possibly due to the impending end of the grant period, and (iii) neither the set of INS respondent characteristics nor the interview payment made were significant factors in determining time to first contact.

7. RETROSPECTIVE MEMORY RECALL

A design feature of the NIS-P permits tests of the effect of inter-round interval length on recall error. Baseline respondents were divided into two groups -- half were randomly assigned to have an interview at three months as well as at six months while the other half were given only a six-month interview.¹³ While the purpose of this experiment was to test the effect of interval length on attrition, it can be used to evaluate its impact on respondents' ability to recall. All respondents were asked to provide mobility, childbearing, training, and schooling histories back to the time of their previous interview. Those respondents interviewed in both the three- and six-month sample provided a six-month history in two parts, while those interviewed only at six months provide one six-month history. This experiment allows us to test whether survey interval length has any impact on the reporting of retrospective histories.

The substantive areas in the NIS-P which are amenable to this evaluation include geographical mobility, childbearing, and several aspects of training. For example, respondents were asked in both the three- and six-month instruments about the number of different addresses at which they had lived. In addition, they were also asked whether (since the last interview) they had visited a foreign country on a trip of more than two weeks duration. If the answer was yes, they were then queried about the number of times, total duration, and location of such trips. In addition to mobility, similar questions (with the same reference starting point of the date of the last survey) were included about whether they had a child, attended English language classes (and the number of hours attended), school attendance and vocational training.

If survey questions were clear and unambiguous, respondents understood their intent and always answered correctly by relying on their completely accurate recall of events, and the time interval to which questions refer were equivalent and identical, then we should obtain the same results by appropriately summing answers from the three-month and six-month interviews as

¹³ Some baseline cases were completed (and their three-month interval had arrived) before the three-month instruments were ready. These 175 cases were excluded from this experiment and data from these cases are not used in this section.

from the six-month interview alone (for those who did not receive a three-month interview). None of these three conditions will ever be met fully in any survey. Questions are never unambiguous either to the person asking or to the one being asked. Respondents were required to perform two interrelated but different cognitive tasks -- did an event happen or not and if so did it take place since the date of the last interview. Added on to any normal ambiguity associated with such questions, our situation may be compounded since many respondents have limited English language ability and interviews were conducted in multiple languages.

There are two cognitive tasks required of respondents -- remembering whether an event took place and placing it within a specific time frame. Event dating then involves not only remembering when an event occurred, but also its relation in time to the date of the last interview. If memory decay is a function of time since an event, it may be easier to recall when the time between surveys is shorter. This consideration implies that the sum of events from the 3-month sample would generally be greater than that obtained from the six-month sample alone. In addition, more meaningful or salient events tend to be forgotten more slowly so that reporting of salient events should be more similar in the three- and six-month samples.

Telescoping (mis-remembering of dates) is another particularly salient aspect of cognitive memory. Respondents may forward telescope into the survey interval events that took place prior to the last interview or backward telescope events that did take place within the survey specified time interval to a date preceding it. The evidence is mixed on what type of events are forward or backward telescoped. Some have argued that more vividly remembered events ("accessible") will be forward telescoped, but the evidence is inconclusive (see Smith and Thomas (1998)). The ability to place an event within the proper time interval may depend in part on how salient dates are that surround this event. In addition to the date of the last survey interview (not a particular salient date in itself), there are other dates a respondent may consider relevant in situating an event in time. Particularly memorable dates in a sample of legal immigrants would include the date of first arrival in the United States, the date of admission to legal permanent residence, and the date of green card receipt; respondents may have a good idea of whether or not an event

preceded or came after those dates.

In a recent paper, Smith and Thomas (1998) using the two Malaysian Family Life History Surveys (MFLS1 and MFLS2) investigated the ability of respondents to recall their lifetime migration histories. The two MFLSs, spaced twelve years apart, asked respondents to recall and give dates of all migration events (across district lines) since they were 15 years old which lasted at least three months. There are a number of findings from that work which may be relevant for our inquiry. In addition to the normal forgetting of events associated with the time since it occurred, their research demonstrated that respondents tended to remember more salient moves -- those linked with other important life events such as the start of a marriage, birth of a child, change of a job, and moves that lasted a long time. In contrast, respondents were more likely to forget short-duration short-distance migrations. Even when remembered, dating of these events was also not perfect. There was a slight tendency to recall events as having taken place closer to the survey than they did (forward telescoping), especially if these events were salient ones. Finally, education ranked quite high among the respondent attributes that improved both recall that an event took place as well as the date of the event.

Memory decay with time suggests that the probability of reporting an event at all may be higher in the 3-month than in the 6-month sample. Given that an event is reported, the absolute value of the difference between the true and reported date may also be larger in the six-month sample. The impact on reporting in our surveys depends on whether events are forward or backward telescoped. If events are forward telescoped and the extent of forward telescoping increases with time since an event, there are two countervailing effects. Some events will be double counted in the three-month sample, but more events will tend to be telescoped into the survey interval in the six-month sample. If events are backward telescoped and backward telescoping increases with time since the event, both samples will understate the truth, but this understatement will be larger in the six-month sample.

Table 12 lists simple summary statistics about reporting events between waves of the NIS-Pilot survey. For the six-month sample, these represent answers given at six-month follow-up;

for the three-month sample, they combine information available from the three- and six-month waves. For most outcomes listed, responses do differ with more activity reported in the 3-month sample. For example, the fraction of respondents reporting that they attended English language classes is about 50% higher in the 3-month sample (23% to 16%). Given the relatively short time period involved, reasonably large discrepancies also exist between the three-month and six-month samples in the number of different addresses (.35 to .23), the number of trips abroad (.42 to .34), and the fraction attending school (14% to 17%). Three-month sample respondents also report more time out of the U.S. and more hours spent in English classes

The two exceptions to this general rule of greater reporting of activity in the three-month sample are the birth of a child and taking vocational training. In both cases, there are no statistically significant differences between the samples. Both events are relatively salient so that respondents may not only know with some certainty that they engaged in this activity and when it occurred. In fact, there was only one case in the three-month sample where a respondent reported the same birth in the three- and six-month interval. Since there were never any differences between the samples for these two outcomes, the remaining analysis will ignore them.

The three- and six-month sample cases also differ in total elapsed time before completion of the six-month follow-up. The time interval from baseline to six-month completion will generally be longer for the three-month sample. After completion of a three-month case, time was allowed to elapse before beginning to re-contact these cases again for the six-month follow-up. For example, a 3-month case completed in the 5th month after baseline would generally not be contacted again until the 7th or 8th month after baseline.¹⁴ Table 13 does show such differences between the samples in time span to six-month completion. On average, both samples took longer than six months to complete¹⁵; approximately seven months in the six-month sample and

¹⁴ A counteracting force is that cases in the 3-month sample might be easier to find and contact since we contacted them sooner after the baseline.

¹⁵ This is not surprising since the label 6-month more appropriately refers to the month before an attempt to contact started

eight months in the three-month sample.¹⁶ We explore below whether this difference in elapsed time to six-month completion can explain different rates of reporting between the two samples.

We next report results obtained from models of respondents' reports of their activities between the baseline and six-month interview. The outcomes examined include the number of different addresses, the number of foreign trips, whether the respondent attended English language classes, the duration of such instruction, and whether the respondent attended school. Since it indexes the experimental treatment, the key variable in all models is whether the respondent was in the three-month sample. This variable (Got3) measures the effect of inter-round interval length on the propensity to report. The other variables included in these models were respondents' age, sex, schooling, whether married, self-reported difficulty in oral or written English, a new arrival, received an employment visa, or was born in Mexico.

If this treatment was random as intended, the estimated size of Got3 should not be affected by inclusion of variables measuring respondent attributes. Using OLS estimation, the first row of Table 14 lists estimated differences between the three- and six-month samples (with the associated 't') while the second row provides estimates obtained in models including respondent attributes. In all cases, greater activity is reported in the three-month than in the six-month sample. The inclusion of personal attributes in models did not noticeably affect the estimated reporting differences between the two samples. It should be noted that sample sizes involved in this experiment were not large (approximately 430 in each sample) so differences between the samples must also be large in order to find statistically significant results. These modest sample sizes limit our ability to reach stronger conclusions when effects are only moderate in size.

Table 15 lists estimated coefficients obtained for these outcomes. Since the time from baseline to six-month completion is not the same for the two samples, a variable (span6) was included measuring the number of days from the baseline to six-month completion. Unlike personal attributes, span6 can and does alter the size of the Got3 variable. Statistical models

¹⁶ 75% of cases in the six-month sample were completed within 8 months and three-quarters of cases in the three-month sample were completed within 9 months.

more appropriate to each outcome were used--probits for attending school or attending English language instruction, tobit for total hours of English language instruction, and negative binomials for number of addresses and number of foreign trips.

Since our interest in these models is pretty single minded (Got3), results obtained for other covariates will be described very briefly. Between wave foreign mobility increases with age and is higher among those immigrants who are new arrivals and among those who came on employment visas. Not surprisingly, the likelihood of attending English language classes increases among those who self-rate their oral or verbal English language skills as fair or poor. In addition, the probability of attending English language classes declines with age, is higher among new arrivals, and is lower among immigrants with employment visas. Finally, the probability of attending school between waves is lower among less schooled, older immigrants, those with English language difficulty, immigrants from Mexico, and new arrivals.

With the exception of the number of foreign trips taken, our finding of more activity reported for respondents in the 3-month sample is maintained in multivariate models. Follow-up interviews with greater periodicity may lead to more activity being reported, but is this reporting more or less accurate? Respondents could forward telescope into the second component of the three-month sample so that the same event is reported twice. However, it is more likely in our view that on net the six-month sample underreports events. For example, even though the time span covered is less, the fraction of people reporting school attendance or English language classes is as high in the first component on the three-month sample as in the six-month sample.

These models indicate that the time span between surveys also matters especially for mobility outcomes. Longer time between waves of this survey leads to a larger number of addresses and a greater number of trips abroad. Virtually all surveys have varying between wave interval lengths for respondents due in part to planned survey logistics and in part to difficulties in re-contacting. These differences in inter-round time span are typically ignored by researchers, but at least for some outcomes our results may indicate that may not be a wise decision.

The reporting consequences of more frequent interviews on the type of responses given

may vary by respondent attributes and behaviors. For example, more educated respondents or those with greater facility in English may understand questions better and have less need of frequent re-interviewing. Similarly, such respondents must be able to distinguish -- especially in time sequencing -- events that took place before the baseline interview from those that took place after it. To the extent that there are many similar events, it may be more difficult to make distinctions in timing. The latter point is particularly important for migration histories if respondents changed addresses frequently or engaged in many foreign trips prior to the baseline. To test these ideas, we explored a number of interactions with participation in the three-month sample. A number of plausible interactions such as education, age, or sex of the respondent never mattered and no interaction ever mattered in either the schooling or language models.

Two aspects that did matter for certain outcomes were self-reported English language difficulty and similar activity reported before the baseline interview. Table 16 lists estimated effects of these interactions with participation in the 3-month sample. Those with self-rated English language difficulty report more change of addresses in the three-month sample while those who had taken more trips outside the U.S. prior to the baseline reported more foreign trips in the 3-month sample. This confusion caused by foreign trips before the baseline interview by itself does not tell us which of the two samples provided more accurate answers.

REFERENCES

Smith, James P., and Duncan Thomas. Unpubl. "Remembrances of Things Past: Test-Retest Reliability of Retrospective Migration Histories. Rand Working Paper, September 1998.

Table 1
Number of Sampled Immigrants Aged 18+, by Response Category and Interview Round

	Baseline (%)	6-month (%)
A. Distribution of Outcomes		
Contacted		
Completed interview	1130 (61.4)	1034 (91.5)
Refusal by respondent	170 (9.2)	96 (8.5)
Refusal by respondent proxy	22 (1.2)	---
Not Contacted		
No field work	226 (12.3)	---
No telephone number	105 (5.7)	---
Moved prior to contact	126 (6.8)	---
Other no contact	60 (3.3)	---
Total	1839 (100)	1130 (100)
B. Contact Summary		
Contacted	1322 (71.9)	1130 (100)
Not Contacted	517 (28.1)	---
Total	1839 (100)	1130 (100)
C. Completion Summary Among Contacted Immigrants		
Completed	1130 (85.5)	1034 (91.5)
Refusal by respondent	170 (12.9)	96 (8.5)
Refusal by respondent proxy	22 (1.7)	---
Total	1322 (100)	1130 (100)

Table 2
Comparisons of NIS-P with Other Surveys

A. Baseline Comparisons

	HRS	AHEAD	PSID	PSID-Immigrant
Complete	80.5	80.0	76	18.5 (72.3)
Respondent Refusal	9.3	13.8	NA	6.9 (27.0)
Other refusal	5.0	2.2	NA	0.2 (0.8)
No contact	0.1	UNK	NA	63.3
Language Barrier	1.2	UNK	NA	NA
Unknown Eligibility	2.4	0.9	NA	11.3
Other	1.5	3.1	NA	NA

B. Attrition Comparisons

	HRS1-2	AHEAD1-2	PSID1-2	PSID 96-97
Complete	91.7	92.5	88.5	96.6
Respondent Refusal	4.8	3.8	9.1 ^a	1.5
Other Refusal	1.0	1.2		
No contact	1.8	0.8	1.6 ^a	0.7
Other	0.7	1.7	0.0	1.2

^aPSID numbers combine respondent and other refusal and no contact equals moved. See Fitzgerald, Gottschalk, and Moffitt (1998). We appreciate the assistance of Dan Hill and Bill Shay in providing numbers for these tables. UNK is unknown and NA is not available.

Table 3
 Characteristics of Sampled and Interviewed Immigrants Aged 18+ Years, Baseline

	Sampled	Interviewed
Personal characteristics		
Mean age (years)	36.4	35.4
Percent male	47.0	47.2
Percent married	72.7	71.0
Percent in visa category		
Spouse of U.S. citizen	21.4	22.1
Parent of (adult) U.S. citizen	8.1	6.4
Sibling	6.2	4.3
Employment	31.8	32.6
Refugee/asylee	7.2	8.7
Diversity	5.6	4.6
Pre green-card characteristics		
Mean years in the U.S.	2.3	2.4
Percent in illegal status	12.2	11.4
Place of birth (%)		
Africa	7.2	6.2
North and Central America	28.8	28.9
South America	7.6	8.5
Asia	38.7	35.8
Europe	17.4	20.2
Oceania	.38	.35
Mexico	13.1	12.1
Philippines	7.1	6.7
Former Soviet Union	5.8	7.5

Table 4
 Characteristics of Baseline Respondents and Differences in Characteristics Between Baseline Respondents and Non-respondents, by Category

	Contact with Respondent			No Contact with Respondent			
	Interviewed	Refusal by Respondent	Refusal by Proxy	No Field Work	No Telephone	Moved	Other
Personal characteristics							
Mean age (years)	35.4	2.49 (2.28)	5.35 (1.87)	2.60 (2.69)	1.18 (0.88)	1.14 (0.91)	7.99 (4.55)
Percent male	47.2	.479 (0.12)	-6.26 (0.58)	-2.04 (0.56)	-.501 (0.10)	2.04 (0.43)	-.501 (0.08)
Percent married	71.0	9.61 (2.63)	10.8 (1.13)	6.46 (1.99)	-1.45 (0.32)	-1.13 (0.27)	2.36 (0.40)
Percent in visa category							
Spouse of U.S. citizen	22.1	-5.65 (1.68)	-8.49 (0.96)	3.10 (1.04)	-4.98 (1.19)	-.695 (0.18)	-3.79 (0.70)
Parent of (adult) U.S. citizen	6.4	.0989 (0.04)	7.26 (1.24)	6.02 (3.04)	4.10 (1.48)	3.95 (1.55)	12.0 (3.32)
Sibling	4.3	5.66 (2.86)	-4.34 (0.84)	5.40 (3.07)	2.33 (0.95)	6.77 (2.99)	5.66 (1.77)
Employment	32.6	12.1 (3.19)	3.80 (0.38)	-13.1 (3.88)	1.72 (0.36)	-3.99 (0.92)	-4.23 (0.69)
Refugee/asylee	8.7	-2.79 (1.31)	-8.67 (1.56)	-4.25 (2.25)	-3.91 (1.48)	-4.70 (1.94)	-.339 (0.10)
Diversity	4.6	-1.07 (0.57)	9.03 (1.82)	3.36 (2.00)	5.87 (2.50)	4.13 (1.91)	.398 (0.13)
Pre green-card characteristics							
Mean years in the U.S.	2.4	.294 (1.04)	.456 (0.62)	-.471 (1.88)	-.0707 (0.20)	-.131 (0.40)	-1.010 (2.22)

Percent in illegal status	11.4	-2.00 (0.75)	11.3 (1.61)	4.96 (2.08)	4.77 (1.43)	1.28 (0.42)	-4.75 (1.10)
Place of birth (%)							
Africa	6.2	3.80 (1.79)	-1.65 (0.30)	.885 (0.47)	4.28 (1.63)	2.54 (1.05)	3.80 (1.11)
North and Central America	28.9	-3.06 (0.82)	-15.3 (1.57)	3.36 (1.02)	-4.18 (0.90)	.427 (0.10)	2.73 (0.46)
South America	8.5	-4.97 (2.28)	-3.95 (0.69)	-3.19 (1.65)	1.98 (0.73)	1.03 (0.41)	-5.16 (1.47)
Asia	35.8	11.2 (2.81)	32.3 (3.10)	8.41 (2.38)	4.16 (0.84)	-.920 (0.20)	7.49 (1.17)
Europe	20.2	-7.82 (2.52)	-11.1 (1.36)	-9.56 (3.48)	-5.89 (1.53)	-2.72 (0.77)	-8.51 (1.70)
Oceania	.35	.822 (1.62)	-.354 (0.27)	.0885 (0.20)	-.00354 (0.56)	-.354 (0.61)	-.354 (0.43)
Mexico	12.1	-3.89 (1.41)	-7.58 (1.05)	11.8 (4.82)	1.21 (0.35)	-1.81 (0.57)	1.21 (0.27)
Philippines	6.7	3.27 (1.55)	6.91 (1.25)	.354 (0.19)	2.80 (1.07)	.417 (0.17)	-6.73 (1.98)
Former Soviet Union	7.5	-2.23 (1.17)	-7.52 (1.50)	-5.75 (3.40)	-6.57 (2.77)	-4.35 (1.99)	-2.52 (0.82)

Notes: Absolute values of *t*-ratios beneath parameter estimates. Estimates pertain to sampled respondents aged 18 or more years.

Table 5
 Multinomial Logit Estimates: Determinants of Non-response at Baseline (N=1613)

	Refusal by Respondent	No Telephone	Moved	Refusal by Proxy	Other
Married	.394 (1.68)	-.137 (0.53)	-.220 (0.89)	.513 (0.86)	-.257 (0.75)
Age	.00718 (0.84)	.00115 (0.11)	-.0004 (0.04)	.0369 (1.74)	.0346 (2.93)
Sibling	1.19 (3.08)	.912 (1.80)	1.38 (3.35)	-.035 (0.01)	.933 (1.52)
Employment	.676 (2.48)	.517 (1.58)	.250 (0.81)	.140 (0.23)	.511 (1.06)
Spouse of U.S. citizen	-.00523 (0.02)	.111 (0.29)	.393 (1.15)	-.704 (0.92)	.646 (1.22)
Parent of (adult) U.S. citizen	.231 (0.50)	.926 (1.76)	.925 (1.90)	-.131 (0.14)	.598 (0.97)
Refugee/asylee	-.058 (0.14)	-.127 (0.23)	-.389 (0.74)	-.034 (0.04)	.330 (0.52)
Diversity	.239 (0.48)	1.25 (2.90)	1.01 (2.41)	1.25 (1.56)	.468 (0.65)
Years in the U.S.	.0331 (1.34)	-.0171 (0.46)	.0051 (0.16)	-.0103 (0.17)	-.130 (1.88)
Illegal status	-.133 (0.42)	.743 (2.16)	.268 (0.80)	1.14 (1.76)	.312 (0.52)
Male	.0286 (0.17)	.0897 (0.43)	-.0876 (0.45)	.373 (0.82)	-.0019 (0.01)
No field-visit area	.670 (2.58)	.0268 (0.07)	-.485 (1.11)	.776 (1.21)	-.555 (0.91)
Constant	-2.97 (7.97)	-2.84 (6.59)	-2.37 (6.15)	-6.07 (5.91)	-4.33 (7.42)

Table 6
 Multinomial Logit Estimates: Determinants of Non-response at Baseline for Male Immigrants
 Aged 18+ (N=762)

	Refusal by Respondent	No Telephone	Moved	Refusal by Proxy	Other
Occupational income (x10 ⁻⁴)	.293 (2.16)	.232 (1.30)	.116 (0.68)	-.446 (0.63)	.314 (1.47)
Missing occupational income	1.12 (2.71)	.773 (1.47)	1.11 (2.49)	.399 (0.27)	-.116 (0.16)
Married	.184 (0.50)	-.695 (1.63)	-.482 (1.21)	1.41 (1.09)	-1.03 (1.83)
Age	-.00027 (0.02)	.0158 (0.97)	-.0073 (0.49)	.0162 (0.43)	.0232 (1.16)
Spouse of U.S. citizen	-.694 (1.27)	-.614 (0.88)	-.0236 (0.04)	-3.95 (0.01)	1.44 (1.69)
Parent of (adult) U.S. citizen	-.445 (0.56)	.239 (0.29)	.0966 (0.12)	.792 (0.46)	1.54 (1.53)
Sibling	.755 (1.28)	.604 (0.85)	.885 (1.38)	-3.4 (0.02)	.218 (0.18)
Employment	.270 (0.64)	-.0550 (0.11)	.180 (0.41)	.699 (0.55)	.637 (0.85)
Refugee/asylee	-.184 (0.34)	-1.73 (1.58)	-.864 (1.25)	-.341 (0.04)	.418 (0.43)
Diversity	.119 (0.18)	1.39 (2.57)	.795 (1.37)	1.89 (1.34)	.843 (0.87)
Years in the U.S.	.021 (0.59)	-.0177 (0.31)	.0120 (0.27)	-.0272 (0.30)	-.169 (1.55)
Illegal status	-.305 (0.65)	.270 (0.51)	-.123 (0.27)	1.26 (1.26)	.0100 (0.12)
No field-visit area	.612 (1.51)	-.543 (0.72)	.0834 (0.17)	1.41 (1.57)	-.120 (0.16)
Constant	-2.82 (5.47)	-2.99 (4.90)	-2.14 (4.09)	-6.02 (3.27)	-4.05 (4.74)

Table 7
Number of Contacted Respondents 18+, by Reservation Price Interval and Survey Round

Reservation price interval	Baseline	6-month
$\rho \leq \$5$	1082	543
$\rho \leq \$15$	-	471
$\$5 < \rho \leq \25	-	3
$\$5 < \rho \leq \100	48	8
$\$15 < \rho \leq \25	-	1
$\$15 < \rho \leq \100	-	8
$\rho > \$5$	57	-
$\rho > \$25$	-	64
$\rho > \$100$	135	32
Total	1322	1130

Table 8
Average Number of Days to First Contact, by Interval and Round

Interval	Sample	Mean (s.d.)	N
From immigration date to first contact attempt at baseline	Adult immigrants	167.7 (75.8)	1505
From baseline interview date to first contact attempt, “6-month”	Baseline, 6-month only	209.1 (52.4)	636
From “3-month” interview date to first contact attempt, “6-month”	Baseline, 3-month	98.1 (44.8)	430
From last contact to first contact attempt, “6-month”	Baseline, 3-month+6-month	169.4 (76.1)	1106
From immigration date to first contact attempt at “6-month”	Baseline	418.3 (94.5)	1106

Table 9
Interval Regression: Determinants of the log of the Interview Reservation Price at Baseline

	All Contacted Immigrants	Contacted Male Immigrants
Days from immigration to first contact attempt	.0689 (1.68)	.114 (1.79)
Days to contact squared	-.000113 (1.39)	-.000248 (1.80)
Occupational income (x10 ⁻⁴)	-	2.51 (2.24)
Missing occupational Income	-	9.30 (2.56)
Married	3.71 (2.07)	2.90 (1.09)
Age	.117 (1.47)	-.0129 (0.14)
Spouse of U.S. citizen	.485 (0.20)	-4.46 (1.14)
Parent of (adult) U.S. citizen	1.84 (0.53)	1.22 (0.23)
Sibling	5.69 (2.01)	5.45 (1.23)
Employment	4.28 (2.26)	2.28 (0.75)
Refugee/asylee	1.50 (0.52)	.469 (0.12)
Diversity	.835 (0.24)	-1.60 (0.34)
Years in the U.S.	.198 (0.91)	.138 (0.47)
Illegal status	-2.06 (0.81)	-4.86 (1.33)
σ	14.6 (6.80)	13.5 (4.77)
E(ρ) (s.d)	\$.00048 (.005)	\$.024 (.27)
Number	1203	568

Table 10
Interval Regression: Determinants of the log of the Interview Reservation Price, “6-month” Round

	(1)	(2)
Days from last contact (interview) to first contact attempt, 6-month round	.119 (3.05)	.119 (3.04)
Days from last contact squared	-.000141 (2.24)	-.000142 (2.23)
Days from immigration to first contact attempt, 6-month round	.0372 (3.40)	.0386 (3.40)
3-month interview	3.98 (1.92)	3.87 (1.87)
\$5 < ρ \leq \$100 at baseline	-	-2.19 (0.62)
Married	-2.92 (1.56)	-2.95 (1.54)
Age	-.0206 (0.27)	-.0179 (0.24)
Spouse of U.S. citizen	-2.15 (0.86)	-2.11 (0.84)
Parent of (adult) U.S. citizen	2.86 (0.72)	3.04 (0.76)
Sibling	7.80 (2.06)	7.86 (2.07)
Employment	.300 (0.15)	.334 (0.16)
Refugee/asylee	2.47 (0.84)	2.54 (0.86)
Diversity	-4.93 (0.99)	-5.03 (1.01)
Pre-immigration years in the U.S.	.114 (0.52)	.107 (0.49)
Illegal status	7.26 (2.60)	7.27 (2.60)
σ	12.3 (4.64)	12.3 (4.64)
E(ρ) (s.d)	\$3.37 (66.6)	-
Number	1106	1106

Table 11
Determinants of the Contact Delay, “6-month” Round

	(1)
3-month interview	-91.3 (24.6)
Days from immigration to first contact attempt, baseline	-.103 (4.20)
\$5 < ρ \leq \$100 at baseline	-10.9 (1.21)
Married	-2.52 (0.53)
Age	.201 (1.07)
Spouse of U.S. citizen	-1.42 (0.23)
Parent of (adult) U.S. citizen	-1.92 (0.19)
Sibling	-8.69 (0.86)
Employment	3.83 (0.71)
Refugee/asylee	-7.60 (1.00)
Diversity	-1.57 (0.17)
Pre-immigration years in the U.S.	-.209 (0.34)
Illegal status	3.85 (0.58)
Constant	222.2 (26.6)
R ²	.382
Number	1106