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Nonresponse bias in a longitudinal measurement design examining substance use across the transition out of high school

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Abstract

Background—Intensive longitudinal methods are used to study the daily-level, within-person associations between substance use and its predictors and consequences. By definition, these designs require greater respondent effort than more traditional longitudinal designs, the result of which may be increased unit nonresponse and, more importantly, nonresponse bias. The present study contrasts the nonresponse properties of a measurement burst design with those of a single one-year follow-up (“control”) design, with a particular emphasis on the retention of young adults who did not plan to graduate from college.

Methods—High school seniors (N=318) from three Midwestern schools completed an in-school baseline survey in spring 2012. Respondents were then randomized into a measurement burst or control group. Four, eight, and twelve months after baseline, young adults in the measurement burst group received a 30-minute follow-up web survey, followed by 14 days of web-based daily surveys. Young adults in the control group received only a 30-minute follow-up web survey twelve months after baseline. Response rates, predictors of nonresponse, and relative nonresponse biases for measures of sociodemographics, college plans, and substance use were compared between the two measurement groups.

Results—Compared to the control design, the measurement burst design had greater relative nonresponse bias for statistics measuring substance use, but lower relative nonresponse bias for the statistic measuring college plans.

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Conclusion—Intensive longitudinal methods have the potential to retain non-college attending young adults during the transition to adulthood. Nonresponse adjustment weights should be used to correct for any detected bias.

Keywords

nonresponse bias; substance use; intensive measurement; college plans

1. Introduction

The popularity of intensive longitudinal methods has dramatically increased in recent years (Bolger and Laurenceau, 2013). Such methods are appealing because they permit the study of daily life in real-world settings, reduce the effects of recall bias, and permit the study of within-person variation and change (in addition to between-person variation; Bolger and Laurenceau, 2013; Moskowitz et al., 2009; Leigh, 2000; Schwarz, 2012; Stone et al., 1991). Measurement burst designs (Nesselroade, 1991; Sliwinski, 2008) are a particular type of intensive longitudinal design in which short-term longitudinal designs (e.g., daily diaries) are combined with long-term longitudinal designs (e.g., quarterly follow-up).

Substance use researchers use intensive longitudinal methods to examine the within-person associations between substance use and its predictors and consequences. For example, researchers have documented associations between substance use and sexual behaviors (e.g., Kiene et al., 2009; Patrick, 2013; Patrick and Maggs, 2009), affect (e.g., Armeli et al., 2000; Rankin and Maggs, 2006; Simons et al., 2010), and leisure activities (e.g., Finlay et al., 2012). Several of these studies have focused on substance use between the ages of 18 and 25, an important developmental period during which rates of substance use peak (Johnston et al., 2013; Park et al., 2006; Substance Abuse and Mental Health Services Administration (SAMHSA), 2013). Much of the research on substance use during the transition to adulthood applies primarily to young adults who are enrolled full-time at four-year colleges and universities; however, studies that have surveyed more representative samples of young adults have found that substance use varies by college enrollment. For example, in cross-sectional or less intensive longitudinal designs, alcohol use has been found to be greater among college students (Johnston et al., 2013; SAMHSA, 2013; White et al., 2005) whereas marijuana use (Bachman et al., 1997; Gfroerer et al., 1997; Johnston et al., 2013) and cigarette use (SAMHSA, 2013) have been found to be greater among non-students. Thus, it is important that studies of young adults—including studies of daily-level associations—consider young adults on all educational paths in order to get a more complete understanding of substance use during this developmental stage.

By definition, intensive measurement designs place a greater burden on participants than do cross-sectional or less intensive longitudinal designs (Bolger and Laurenceau, 2013; Sliwinski, 2008); thus, it is particularly important to evaluate the effects of these designs on nonresponse. Until recently, achieving a high response rate has been considered synonymous with having high-quality data. Unfortunately, response rates have been declining for some time (Biener et al., 2004; De Leeuw and De Heer, 2002). More importantly, studies have since demonstrated that a high response rate does not guarantee that a given sample represents the target population, nor does a low response rate indicate

that a given sample fails to represent the target population (Groves, 2006; Groves and Peytcheva, 2008). Therefore, a study with a very high response rate might still produce biased results if respondents and nonrespondents differ markedly on key characteristics.

A key measure of sample representativeness is nonresponse bias, which is a characteristic of a specific survey statistic (e.g., proportion of participants using alcohol) that is a function of the proportion of nonrespondents (i.e., the nonresponse rate) and the difference between respondents and nonrespondents on the variables used to create the statistic of interest (Groves, 1989). For example, when respondents and nonrespondents report meaningfully different rates of alcohol use *and* when the nonresponse rate is high, the estimated proportion of people using alcohol will be biased.

Several studies have considered issues related to nonresponse in intensive measurement studies about substance use (e.g., Gerstel et al., 1980; Litt et al., 1998); cross-sectional studies of substance use (e.g., Maclennan et al., 2012); and traditional longitudinal studies of substance use among the general population (e.g., Studer et al., 2013), among college students (e.g., Cranford et al., 2008), and among young adults in the U.S. military (Cunradi, et al., 2005). In this body of related literature, however, no study has included an experimental manipulation permitting the evaluation of a measurement burst design in a study of substance use among young adults regardless of their college plans.

The purpose of the current study was to examine the nonresponse properties of two measurement designs: (a) a measurement burst design and (b) a one-year follow-up (“control”) design. Specifically, response rates, predictors of nonresponse, and relative nonresponse biases for measures of sociodemographics, college plans, and substance use were compared between designs. The study had five aims. We began by evaluating earlier phases of the study’s design that had the potential to affect the interpretation of the relative nonresponse bias results: (Aim 1) differential participation in the longitudinal phase of the study and (Aim 2) baseline equivalence of measurement groups after randomization. We then compared (Aim 3) response rates, (Aim 4) predictors of nonresponse, and (Aim 5) relative nonresponse biases between designs. We hypothesized that (1) Wave 3 response rates would be lowest in the measurement burst group; (2) men and students with greater substance use would be more likely to drop out of the study, regardless of design; and (3) the measurement burst design would do a better job than the control design of retaining young adults who did not plan to attend college.

2. Methods

2.1. Enrollment procedures, information, and consent

The study began with the administration of a paper-and-pencil baseline survey to all participants. In March, 2012, three high schools in the Midwest, selected to represent urban, suburban, and rural communities, were recruited. All schools received an honorarium. Principals received study information and worked with study staff to select 12th grade classrooms based on study guidelines and finalize plans for the in-school group administration of the baseline questionnaire. Schools sent parents/guardians information on the study and provided the opportunity for them to deny permission for their children to

participate in the study. Immediately after administration of the baseline survey, participants were asked to provide their contact information in order to be invited into the longitudinal phase of the study.

After administration of the baseline survey, study staff randomized approximately two thirds of baseline participants into the measurement burst group and the remaining into the control group. Groups were disproportionately assigned so that there would be greater power to evaluate within-person associations between substance use and its predictors and consequences. In addition to the methodological questions described in the current paper, the substantive questions of interest were key aims of the broader study. Four, eight, and twelve months after baseline (Wave 1: September 2012, Wave 2: January 2013, and Wave 3: May 2013, respectively), young adults in the measurement burst group received a 30-minute follow-up web survey, followed by 14 days of web-based daily surveys. Young adults in the control group were invited to participate in only the final 30-minute follow-up web survey administered in May 2013 (Wave 3).

Follow-up surveys were sent only to participants who were at least 18 years of age at the time of each follow-up survey. Study staff made reminder telephone calls to nonrespondents. Participants in the measurement burst group received the following post-paid incentives for the successful completion of each component of the study: \$20 for completing the Wave 1 follow-up survey, \$20 for completing the Wave 2 follow-up survey, \$25 for completing the Wave 3 follow-up survey, and \$2 for completing each diary day with a \$2 completion bonus for completing all days within a 14-day measurement burst. Participants in the control group received a \$25 postpaid incentive for completing the Wave 3 follow-up survey. All procedures were approved by the university's IRB.

2.2. Setting and participants

Of the 440 students eligible to participate in the baseline survey, 318 (72.3%) completed it, 104 (23.6%) were absent on the day of baseline administration, 3 refused (0.7%), and 15 of their parents refused (3.4%). Of the 318 baseline survey respondents, 300 provided their contact information on a separate form so that they were eligible to participate in the longitudinal phase of the study. Approximately two thirds of baseline participants ($n=202$) were randomized into the measurement burst group and the remaining ($n=98$)² were randomized into the control group. Response rates for the baseline survey and all follow-ups are shown in Table 1. Among baseline respondents eligible for the follow-up data collection ($N=299$), 42.8% were male ($n=6$ missing sex), 74.6% were White ($n=1$ missing race/ethnicity), 64.2% had parents with at least some college education, 50.5% were from a rural school, 33.8% were from a suburban school, and 15.7% were from an urban school. (One baseline participant was deceased (and thus ineligible) at the time of the Wave 3 follow-up.)

The average age at baseline was 18.3 years ($SD = 0.53$). Because we were interested in how well the survey design retained young adults with and without plans to graduate from college, it is relevant to note that, at baseline, 46.5% ($n=14$ missing) said they definitely would graduate from a four-year college.

2.3 Measures

2.3.1. College plans—By definition, actual Wave 3 college status is not known for Wave 3 nonrespondents; thus, a baseline measure was used as a proxy. Specifically, participants were asked how likely it is that they would graduate from college (four-year program). Responses to the four-point scale (definitely won't, probably won't, probably will, definitely will) were dichotomized to differentiate between participants who said they definitely will and participants who provided some other response. This specific dichotomization was selected because it was most highly correlated with actual college status measured at Waves 1, 2, and 3.

2.3.2. Alcohol use—The following measures of alcohol use were obtained at baseline: the number of occasions on which participants had more than just a few sips of alcohol in their lifetimes (“lifetime”) and in the last 12 months (“last 12 months”) and the number of times participants had five or more drinks in a row in the last two weeks (“binge”). A drink was defined as a half-ounce of absolute alcohol with the following examples provided: a 12 – ounce can (or bottle) of beer, a 5-ounce glass of wine, or a drink containing 1 shot of liquor or spirits. Responses for all items were dichotomized to differentiate between drinkers and abstainers (lifetime and last 12 months) or binge drinkers and non-binge drinkers (binge).

2.3.3. Marijuana use—The baseline survey assessed the number of occasions on which participants had used marijuana or hashish in their lifetimes (“lifetime”) and in the last 12 months (“last 12 months”). Responses were dichotomized to differentiate between users and abstainers.

2.3.4. Other illegal drug use—The baseline survey assessed the number of occasions on which participants had used any other illegal drugs in their lifetimes (“lifetime”) and in the last 12 months (“last 12 months”). Responses were dichotomized to differentiate between users and abstainers.

2.3.5. Sociodemographics—The following demographic information was obtained at baseline: month and year of birth (used to calculate age), sex (male, female), race/ethnicity [White, other (Black or African American, Mexican American or Chicano, Puerto Rican, Other Hispanic or Latino, Asian American, American Indian or Alaska Native, Native Hawaiian or Other Pacific Islander)], and parental education reported as the highest level of schooling completed by at least one parent (proxy for socioeconomic status; dichotomized to compare those with at least some college education to others). Additionally, an indicator of high school location was coded as urban, rural, or suburban.

2.4. Statistical analyses

To address Aim 1, whether students who provided contact information at baseline (and were, thus, eligible for participation in the longitudinal phase of the study) differed from students who did not provide contact information at baseline, we conducted a series of chi-square analyses comparing baseline measures of sociodemographics, college plans, and substance use between the two groups. To address Aim 2, whether the randomization to measurement group resulted in equivalent distributions of measures of interest, we

conducted a series of chi-square analyses comparing the same baseline measures between the control and measurement burst groups. Next, we compared nonresponse rates (Aim 3) and predictors of nonresponse (Aim 4) (the two components of nonresponse bias) between the two measurement groups. Specifically, to address Aim 3, we estimated a chi-square test to compare response rates between the measurement groups. To address Aim 4, we estimated a series of chi-square tests to evaluate predictors of nonresponse within each measurement group. We also estimated a series of logistic regressions (predicting nonresponse) that included a measurement group interaction term to evaluate whether predictors of nonresponse differed by measurement group. To address Aim 5, we calculated the relative nonresponse bias for all measures. All analyses were conducted using SPSS v. 20 (IBM Corporation, 2011).

Exact nonresponse bias is defined as

$$\overline{y}_r - \overline{y}_t = \frac{m}{n}(\overline{y}_r - \overline{y}_m)$$

where $\overline{y}_r$ is the respondent mean

$\overline{y}_t$ is the full sample mean

$\overline{y}_m$ is the nonrespondent mean

m is the number of nonrespondents

n is the sample size (Groves, 1989).

In this study, the “full” sample is measurement-group specific. Specifically, it is the set of participants randomly assigned to the control or measurement burst group who were eligible to participate in the Wave 3 follow-up survey ($N=97$ and $N=202$, respectively).

When full sample means vary substantially by statistic (e.g., proportion of binge drinkers vs. proportion of other illegal drug users), it is difficult to compare the exact nonresponse biases. Instead, the relative nonresponse bias, which adjusts for the full sample mean, is calculated. Specifically, relative nonresponse bias is defined as

$$\frac{100 * |\overline{y}_r - \overline{y}_t|}{\overline{y}_t}$$

where $\overline{y}_r$ is the respondent mean and $\overline{y}_t$ is the full sample mean (Groves and Peytcheva, 2008). The relative nonresponse bias of a particular statistic indicates whether and in what direction inferences based on unadjusted respondent means will be biased.

For example, suppose that the full sample means of lifetime alcohol use and lifetime marijuana use are 0.5 and 0.2, respectively. Further, suppose that the respondent means of lifetime alcohol use and lifetime marijuana use are 0.4 and 0.1, respectively. For both

substances, the difference between the full sample and respondent means (i.e., the exact nonresponse bias) is 0.1; however, the relative nonresponse biases are 20% and 50%, respectively. In short, the relative nonresponse bias accounts for the prevalence of the measure such that the same level of nonresponse bias has a larger effect on less prevalent measures (here, lifetime marijuana use) than on more prevalent measures (here, lifetime alcohol use).

3. Results

3.1. Aim 1: Initial self-selection into the study

Eighteen of the 318 baseline participants did not provide contact information and, thus, were not included in the longitudinal phase of the study. As illustrated in Table 2, participants who provided contact information and participants who did not provide contact information did not differ by race/ethnicity, parental education, school location, college plans, last 12 month alcohol use, last two week binge drinking, or lifetime and last 12 month other illegal drug use. The only characteristics that differed significantly based on provision of contact information were sex, lifetime alcohol use, and lifetime and last 12 month marijuana use such that students who did not provide contact information were more likely to be male and to report greater use.

3.2. Aim 2: Baseline equivalence of measurement groups

Baseline equivalence of the two measurement groups on measures of sociodemographics, college plans, and substance use was assessed. As illustrated in Table 3, groups did not differ by sex, race/ethnicity, parental education, school location, college plans, all measures of alcohol and other illegal drug use, or last 12 month marijuana use. The only statistically significant difference was that participants assigned to the measurement burst group were less likely to report lifetime marijuana use than participants assigned to the control group.

3.3. Aim 3: Response rates

The Wave 3 response rates for the control and measurement burst groups were 40.2% and 34.2%, respectively. Although the measurement burst group response rate was slightly lower than the control group response rate, the response rates did not differ significantly ($\chi^2(1, 299) = 1.039, p = .308$).

3.4. Aim 4: Predictors of nonresponse

Next, we evaluated whether sociodemographics, college plans, or baseline substance use predicted Wave 3 nonresponse. As illustrated in Table 4 (“ χ^2 ”), respondents and nonrespondents in either group did not differ significantly on race/ethnicity, school location, college plans, alcohol use, or last 12 month marijuana or other illegal drug use. In both measurement groups, Wave 3 nonrespondents were more likely than Wave 3 respondents to have parents with a high school education or less. In the measurement burst group, nonrespondents were more likely than respondents to be male and more likely to report lifetime marijuana use at baseline. Predictors of nonresponse did not differ by measurement group as indicated by non-significant interaction terms (e.g., parental education by measurement group, “Odds Ratio for Group by Characteristic Interaction”).

3.5. Aim 5: Relative nonresponse bias

Last, we calculated the relative nonresponse bias for each statistic and each measurement design. In the absence of a statistical test for relative nonresponse bias, we interpret a relative nonresponse bias of greater than 10% to be meaningful (see also Studer et al., 2013). This level of bias is analogous to having a sampling margin of sampling error (MSOE) of $\pm 10\%$; thus, it is a conservative cut-off for identifying particularly biased estimates (common MSOEs are $\pm 2\%$ or $\pm 3\%$; American Association for Public Opinion Research, 2014). Each level of a categorical variable has its own relative nonresponse bias estimate; however, for the sake of brevity, we highlight the relative nonresponse bias of groups that are usually underrepresented in survey research (i.e., sex: male, race: other than White, parental education: high school or less, school location: urban, college plans: did not definitely plan to graduate from a 4-year college, and substance users). Relative nonresponse bias estimates for all categories are reported in Table 4 (“Relative Bias %”). Neither measurement group showed meaningful relative nonresponse bias for statistics measuring lifetime or last 12 month alcohol use. Both measurement groups had meaningful relative bias for race/ethnicity, parental education, school location, last 12 month marijuana use, and last 12 month other illegal drug use. Specifically, relative to estimates based on the respective full sample (i.e., the 97 or 202 participants randomly assigned to the control or measurement burst group and eligible for the Wave 3 follow-up survey), estimates based on the set of Wave 3 respondents for race/ethnicity status other than White, parental education of high school or less, urban high school location, and last 12 month other illegal drug use were biased downwards. Although there was meaningful relative nonresponse bias for last 12 month marijuana use, the direction differed by measurement groups. Specifically, relative to the estimates based on the respective full sample, estimates based on the set of Wave 3 respondents for last 12 month marijuana use were biased upwards for the control group and biased downwards for the measurement burst group.

For the measurement burst group only, meaningful relative bias was found for sex, last two week binge, and lifetime marijuana and other illegal drug use. Specifically, relative to estimates based on the full sample, estimates based on the set of Wave 3 respondents for male and lifetime marijuana and other illegal drug use were biased downwards; the estimate of last two week binge drinking was biased upwards.

For the control group only, meaningful relative bias was found for college plans. Specifically, relative to the estimate based on the full sample, the estimate based on the set of Wave 3 respondents for not having definite plans to graduate from a four-year college was biased downwards. With respect to our research questions, it is notable that the control design had greater relative nonresponse bias with respect to college plans than the measurement burst design.

4. Discussion

As the popularity of intensive measurement designs for the study of substance use increases, it is important to understand the effects of such methods on sample composition. To the extent that the methods yield a nonrepresentative sample of participants, the generalizability of subsequent results is questionable. Furthermore, because there is limited daily-level

information about substance use among young adults not in college, it is important to determine whether research designs intended to collect daily-level data are able to retain these individuals.

The results support all three of our hypotheses. Specifically, response rates did not differ significantly by measurement group nor did predictors of Wave 3 nonresponse. These results provide evidence that nonresponse in longitudinal designs is not necessarily influenced by the measurement intensity of the design. Although Wave 3 respondents in both measurement groups did not differ significantly from Wave 3 nonrespondents with respect to definite plans to graduate from a four-year college, the relative nonresponse bias exceeded 10% for the control group but did not for the measurement burst group. Because the relative nonresponse bias analyses identified differences between the measurement groups that were not detected in Aim 4, it is possible that the Aim 4 analyses were underpowered. Together, these findings provide preliminary evidence that measurement burst designs, despite their increased respondent burden, can engage young adults regardless of their college plans as well as or better than more traditional longitudinal designs.

The success of the measurement burst design with respect to the retention of young adults with different college plans might be attributable to two features of the specific design used in the current study: the number of contacts and the use of monetary incentives. First, by definition, the measurement burst design involved a greater number of contacts with sample members than did the control design. As documented in the panel survey literature, regular contact with sample members is essential for sample retention (Elliott et al., 2008). Second, given that participants were asked to complete a series of 14 consecutive daily surveys, the measurement burst design used in the current study was arguably more burdensome than a traditional longitudinal survey. Because researchers commonly use monetary and non-monetary incentives to compensate and/or motivate participants (Conner and Lehman, 2012), participants in the current study were compensated for completing each survey component, resulting in eligibility for greater monetary incentives. Therefore, it is possible that the current study's use of monetary incentives throughout the data collection period helped with sample retention for specific subgroups, including those who did not have definite plans to graduate college. That is, because the composition of Wave 3 respondents with respect to college plans was biased in the control design but not in the measurement burst design, these results suggest that more frequent contacts and monetary incentives might be more effective for students who do not plan to graduate from college.

With respect to substance use measures, the measurement burst group had greater relative nonresponse bias estimates than the control group. Specifically, estimates of substance use based on the set of measurement burst group respondents tended to be biased downwards; thus, there is evidence that substance users might be more likely than non-users to drop out of intensive longitudinal studies evaluating substance use. Similar studies might consider the use of data collection techniques (e.g., tailoring requests to substance users) that might mitigate this tendency.

Future research can extend this work in two important ways. First, in the current study, relative nonresponse bias was evaluated by comparing the distribution of baseline

characteristics between respondents and nonrespondents one year after baseline (Wave 3). For characteristics that are not expected to change (e.g., sociodemographics), the assumption that the characteristics are identical or even highly similar at those two time points is reasonable. For characteristics that might change over time (e.g., college plans and substance use), the validity of this assumption depends on the correlations of the measures at the two time points. To overcome this measurement limitation, future studies should consider conducting a nonresponse follow-up survey to obtain even a limited set of measures from Wave 3 nonrespondents. Second, to better understand how specific survey design features like the use of incentives or the number and frequency of contacts affect sample retention within the measurement burst design, future research should experimentally manipulate these characteristics.

In sum, we found that measurement burst designs can effectively retain high school students who do and do not plan to graduate from college. Given that substance users disproportionately dropped out of the measurement burst design (compared to the control design), researchers using intensive designs should consider the use of nonresponse weighting adjustments to correct for any bias in inferences based on respondent means. Further, researchers interested in only between-person differences in substance use between young adults who do and do not plan to graduate from college in a more traditional longitudinal design should consider whether increasing the number of contacts between baseline and follow-up surveys or adjusting monetary incentives improves the retention of sample members of interest.

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Highlights

- We examined the nonresponse properties of a measurement burst and control design.
- Response rates to the one-year follow-up survey did not differ by design.
- Predictors of nonresponse did not differ by design.
- Relative nonresponse biases differed by design.
- The measurement burst design successfully retained non-college attending young adults.

Table 1
Response rates^a

	Baseline	Wave 1 ^b	Wave 2 ^c	Wave 3 ^d
Measurement Burst Group		45.1%	34.8%	34.2%
Control Group				40.2%
Overall	72.3%			36.1%

^a Response rate (RR2) = number of complete interviews divided by the number of eligible participants (The American Association for Public Opinion Research (AAPOR), 2009)

^b The denominator for the response rate calculation is 193 and reflects the fact that nine baseline participants were not eligible to participate because they were under age 18.

^c The denominator for the response rate calculation is 201 and reflects the fact that one baseline participant was not eligible to participate because he/she was under age 18. Note that the seven participants who refused participation during Wave 1 are included in the denominator.

^d The denominator for the intensive measurement response rate calculation is 202. Note that seven participants who refused participation during Wave 1 and the four participants who refused participation during Wave 2 are included in the denominator. The denominator for the control group response rate calculation is 97 and reflects the fact that one baseline control group participant was deceased and was thus not eligible for participation in Wave 3.

Table 2

Self-selection (N=318)

	Provided Contact Information (n=300) %	Did Not Provide Contact Information (n=18) %	$\chi^2(df)$	Φ_C	N
SOCIODEMOGRAPHICS					
Sex (male)	43.7	72.2	5.572 (1)*	0.134	311
Race/ethnicity (White)	74.6	94.1	3.331 (1)	0.103	316
Parental education (some college or more)	64.0	66.7	0.053 (1)	0.013	318
School location					
Rural	50.3	72.2			
Suburban	33.7	22.2	3.453 (2)	0.104	318
Urban	16.0	5.6			
COLLEGE PLANS					
Graduate from 4-year college					
Definitely will	48.6	58.8	0.671 (1)	0.047	303
SUBSTANCE USE					
Alcohol					
Lifetime	72.1	94.4	4.326 (1)*	0.119	305
Last 12 months	67.2	77.8	0.861 (1)	0.053	305
Binge, last 2 weeks	26.2	50.0	4.802 (1)	0.125	308
Marijuana use					
Lifetime	51.9	77.8	4.583 (1)*	0.121	315
Last 12 months	41.0	66.7	4.568 (1)*	0.121	313
Other illegal drug use					
Lifetime	12.5	22.2	1.413 (1)	0.067	314
Last 12 months	7.1	16.7	2.167 (1)	0.083	312

Note. Control group valid sample sizes range from 91 to 97; measurement burst valid sample sizes range from 193-202. R = respondent, NR = nonrespondent. $\chi^2(df)$ = chi-square test statistic and degrees of freedom for test of differences between respondents and nonrespondents. Φ_C = Cramér's V coefficient (an effect size measure that is equivalent to the Phi coefficient (Φ) in the case of a 2x2 contingency table).

* $p<05$

100>*p*

10<*p*
**

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Table 3

Baseline equivalence of measurement groups (N=300)

	Control Group (n=98) %	Measurement Burst Group (n=202) %	χ^2 (df)	Φ_C	N
SOCIODEMOGRAPHICS					
Sex (male)	40.6	45.5	0.612 (1)	0.046	294
Race/ethnicity (White)	80.4	71.8	2.575 (1)	0.093	299
Parental education (some college or more)	66.3	62.9	0.342 (1)	0.034	300
School location					
Rural	53.1	49.0			
Suburban	33.7	33.7	0.895 (2)	0.055	300
Urban	13.3	17.3			
COLLEGE PLANS					
Graduate from 4-year college					
Definitely will	45.7	50.0	0.472 (1)	0.041	286
SUBSTANCE USE					
Alcohol					
Lifetime	76.3	70.1	1.218 (1)	0.065	287
Last 12 months	72.3	64.8	1.646 (1)	0.076	287
Binge, last 2 weeks	27.7	25.5	0.152 (1)	0.023	290
Marijuana use					
Lifetime	61.9	47.0	5.774 (1)*	0.139	297
Last 12 months	47.4	37.9	2.451 (1)	0.091	295
Other illegal drug use					
Lifetime	9.3	14.1	1.369 (1)	0.068	296
Last 12 months	4.1	8.6	1.989 (1)	0.082	294

Note. Control group valid sample sizes range from 91 to 97; measurement burst valid sample sizes range from 193-202. χ^2 (df) = chi-square test statistic and degrees of freedom for test of differences between respondents and nonrespondents. Φ_C = Cramér's V coefficient (an effect size measure that is equivalent to the Phi coefficient (Φ) in the case of a 2x2 contingency table).

* $p < .05$
 ** $p < .01$

100 > d'

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Table 4
Relative nonresponse bias and its components for sociodemographics, college plans, and substance use by measurement group

	Control Group (n=97; Response Rate=40.2%)						Measurement Burst Group (n=202; Response Rate=34.2%)						Odds Ratio for Group by Characteristic Interaction
	Full Sample %	R %	NR %	χ²	Φ	Relative Bias %	Full Sample %	R %	NR %	χ²	Φ	Relative Bias %	
SOCIODEMOGRAPHICS													
Sex													
Male	40.0	38.5	41.1			3.8	45.5	33.3	51.9			26.8	0.516
Female	60.0	61.5	58.9	0.065	0.026	2.5	54.5	66.7	48.1	6.276*	0.178	22.4	
Race/ethnicity													
White	81.2	84.6	78.9			4.2	71.8	76.8	69.2			7.0	1.007
Other ^a	18.8	15.4	21.1	0.485	0.071	18.1	28.2	23.2	30.8	0.253	0.080	17.7	
Parental education													
Some college or more	67.0	84.6	55.2			26.3	62.9	72.5	57.9			15.3	0.428
High school or less	33.0	15.4	44.8	9.145**	0.307	53.3	37.1	27.5	42.1	4.131*	0.143	25.9	
School location													
Urban	12.4	10.3	13.8			16.9	17.3	11.6	20.3			32.9	0.720
Rural	53.6	61.5	48.3	1.649	0.130	14.7	49.0	49.3	48.9	2.920	0.120	0.6	0.569
Suburban	34.0	28.2	37.9			17.1	33.7	39.1	30.8			16.0	2.170
COLLEGE PLANS													
(Graduate from 4-year college)													
Definitely will	46.2	57.9	37.7			25.3	50.0	52.2	48.8			4.4	0.505
Other	53.8	42.1	62.3	3.619	0.199	21.7	50.0	47.8	51.2	0.205	0.033	4.4	
SUBSTANCE USE													
Alcohol: Lifetime													
Drinker	77.2	79.5	75.5			3.0	70.1	70.3	70.0			0.3	0.806
Abstainer	22.8	20.5	24.5	0.206	0.047	10.1	29.9	29.7	30.0	0.002	0.003	0.7	
Alcohol: Last 12 months													
Drinker	73.1	79.5	68.5			8.8	64.8	64.1	65.1			1.1	0.536
Abstainer	26.9	20.5	31.5	1.386	0.122	23.8	35.2	35.9	34.9	0.021	0.010	2.0	
Alcohol: Binge, last 2 weeks													

	Control Group (<i>n</i> =97; Response Rate=40.2%)						Measurement Burst Group (<i>n</i> =202; Response Rate=34.2%)						Odds Ratio for Group by Characteristic Interaction
	Full Sample %	R %	NR %	χ^2	Φ	Relative Bias %	Full Sample %	R %	NR %	χ^2	Φ	Relative Bias %	
Binge drinker	28.0	28.2	27.8				0.7	25.5	33.8	21.4			32.5
Non-binge drinker	72.0	71.8	72.2	0.002	0.005		0.3	74.5	66.2	78.6	3.556	0.135	11.1
Marijuana: Lifetime													
User	62.5	61.5	63.2				1.6	47.0	35.3	53.0			24.9
Abstainer	37.5	38.5	36.8	0.026	0.016		2.7	53.0	64.7	47.0	5.668**		22.1
Marijuana: Last 12 months													
User	47.9	53.8	43.9	0.925	0.098		12.3	37.9	31.3	41.2	1.838	0.096	17.4
Abstainer	52.1	46.2	56.1				11.3	62.1	68.7	58.8			10.6
Other illegal drug: Lifetime													
User	9.4	10.3	8.8				9.6	14.1	7.4	17.6			47.5
Abstainer	90.6	89.7	91.2	0.060	0.025		1.0	85.9	92.6	82.4	3.855	0.139	7.8
Other illegal drug: Last 12 months													
User	4.2	2.6	5.3				38.1	8.6	4.5	10.8			47.7
Abstainer	95.8	97.4	94.7	0.422	0.066		1.7	91.4	95.5	89.2	2.220	0.106	4.5

Note. Control group valid sample sizes range from 91 to 97; measurement burst valid sample sizes range from 193-202. R = respondent, NR = nonrespondent. χ^2 (*df*) = chi-square test statistic and degrees of freedom for test of differences between respondents and nonrespondents. Φ C = Cramér's V coefficient (an effect size measure that is equivalent to the Phi coefficient (Φ) in the case of a 2x2 contingency table).

^a Black or African American, Mexican American or Chicano, Cuban American, Puerto Rican, Other Hispanic or Latino, Asian American, American Indian or Alaska Native, Native Hawaiian or Other Pacific Islander

* $p < .05$

** $p < .01$

*** $p < .001$