

Original article

Virginity Pledges Among the Willing: Delays in First Intercourse and Consistency of Condom Use

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Abstract

Purpose: We examine longitudinal relationships between virginity pledging in adolescence and both sexual initiation and condom use. Prior studies have had mixed results and may not adequately control for prepledge differences between pledgers and nonpledgers.

Methods: Data came from a national sample of 12- to 17-year-olds surveyed in 2001 and reinterviewed 1 and 3 years later. Logistic regression models estimated the association between making a pledge and each outcome. Selection bias was reduced through propensity-score weighting and a rich set of demographic and psychosocial covariates.

Results: Pledgers and nonpledgers differed substantially in preexisting characteristics. However, after propensity weighting and statistical controls, pledging was still associated with delayed intercourse. We estimate that in the absence of pledging 42.4% of virgins with characteristics indicating an inclination to pledge initiate intercourse within 3 years; in the presence of the pledge, 33.6% of such youth initiate intercourse. Among those who had sex during this period, pledging was unassociated with condom use. Among those who did not have sex during this period, pledging was unassociated with engagement in noncoital sexual behavior.

Conclusions: Making a virginity pledge appears to be an effective means of delaying sexual intercourse initiation among those inclined to pledge without influencing other sexual behavior; pledging does not appear to affect sexual safety among pledgers who fail to remain abstinent. © 2008 Society for Adolescent Medicine. All rights reserved.

Keywords:

Abstinence; Virginity pledge; Condom use; Adolescent sexual behavior; Sexual initiation; Intercourse initiation; Propensity score

Nearly 800,000 teenagers become pregnant in the United States each year [1], and half of the approximately 19 million new sexually transmitted disease (STD) infections diagnosed annually are among youth 15 to 24 years old [2]. Unplanned pregnancies and STDs are more common among those who begin sexual activity earlier [3]. Delaying sexual activity might therefore have important public health consequences.

One approach to delaying sexual activity is the virginity pledge—a public declaration to abstain from sex until marriage. The first virginity pledge program was True Love Waits, founded by the Southern Baptist Church in 1993. The movement has since grown to include hundreds of churches, schools, and colleges across the country. Among adolescents in the United States, it is estimated that 23% of females and 16% of males have made a virginity pledge [4]. From a theoretical perspective, pledging may work by enhancing social norms to abstain from sex [5]; pledgers may believe, more so than others, that they will be held responsible to their peers or important adults to follow through on their public commitment. Through processes of cognitive

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dissonance or self-perception [6,7], pledging may also lead youth to internalize the commitment or values expressed in the pledge.

Although these programs may delay sex, those who make a virginity pledge but fail to keep it may actually increase their risk for unintended pregnancy and STDs because abstinence messages often provide incomplete and/or misleading information about condom use, exclude information about birth control and STDs, or lead youth to view such information as personally irrelevant, leaving them unprepared when they do have sex [8]. It has also been suggested that virginity pledges may encourage risky noncoital sexual behaviors (e.g., oral sex) as adolescents attempt to preserve their “virginity” and yet still be sexually active [9].

Two recent studies found little support for a delay in intercourse initiation among those making a formal virginity pledge [10,11]. However, in an analysis of data from the National Longitudinal Study of Adolescent Health (Add Health), 16–17-year-old teens who made a virginity pledge delayed the initiation of sexual intercourse on average about one-third longer than similarly aged teens who had not made a virginity pledge [4]. Among teens younger than 16 years, results were mixed. This study also found that pledgers who had sex were less likely to use a condom at first sex. However, in a follow-up analysis, Brückner and Bearman [9] found that pledgers who became sexually active were as likely as nonpledgers were to use condoms at their most recent intercourse.

These prior studies use covariates in linear models to control for the possibility that youth who make a pledge differ from those who do not in ways that predispose them to delay intercourse, which may not be sufficient if the groups differ substantially. Our study uses propensity scores to reexamine longitudinal relationships between virginity pledging and both sexual intercourse initiation and condom use. In addition, we investigate whether, among youth who do not initiate intercourse, pledging is associated with engagement in noncoital sexual behavior. The use of propensity scores to equate pledgers and nonpledgers on observed characteristics can reduce confounding when random assignment is not feasible [12–14], and can remove substantially more self-selection bias than covariates alone (sometimes eliminating 90% of bias or more) [12].

Methods

Procedure

We conducted a national telephone survey in 2001 (T1) and reinterviewed the same group 1 and 3 years later, in 2002 (T2) and 2004 (T3).

Sample

Participants were recruited from a purchased national list of households with a high probability of containing a mem-

ber aged 12–17. The sampling frame was stratified by census tract race/ethnicity to produce nationally representative proportions of minority and non-Hispanic white youth. We mailed parents in these households an explanation of the study in advance, and obtained verbal consent via telephone from a parent or guardian just prior to conducting an interview with a randomly selected teen from the household. Teens provided verbal assent. Our refusal rate at baseline was 36%, and modeling indicated some selection associated with enrollee characteristics. There was a trend toward higher response rates (1) in census tracts with higher proportions of African Americans, (2) among households where a teen aged 12 to 14 was present but not randomly selected, and (3) when girls of any age or boys aged 14 or younger were randomly selected for sampling. Seventy-three percent of the baseline sample ($N = 1,461$) participated in the T3 survey, and regression modeling also indicated some selectivity in attrition. Overall, attrition was higher among all races for teens over 14 at baseline, boys, and those whose parents had greater educational attainment. Among African Americans, attrition was also higher among those with the least sexual activity at baseline and was lower among those who, as of baseline, had not engaged in intercourse but had engaged in genital noncoital sexual activity. Thus, inverse-probability enrollment and attrition weights, as well as poststratification weights to correct some baseline differences compared to 1999 Current Population Survey demographics, were created and combined to produce analytic weights. All analyses employed these weights, appropriately accounting for their effects on standard errors.

After applying these weights, 47% of T3 respondents were female, 68% were white, 14% African American, 12% Hispanic, and 6% “other.” At least one parent had a college degree for 33% of the sample; 59% had a parent who had been otherwise educated beyond high school.

Measures

Questions assessed sexual behavior with someone of the opposite sex. Intercourse experience at each time point was measured with the item “Have you ever had sex with a boy/girl? By sex we mean when a boy puts his penis in a girl’s vagina” (yes/no). We used this information to determine whether or not a respondent initiated sexual intercourse between T1 and T3. We also measured, at each survey, lifetime levels of noncoital experience with a scale developed for this study based on a measure used by Miller et al [15]. Adolescents indicated whether they had ever (1) kissed,* (2) “made-out (kissed for a long time),” (3) touched a breast/had their breast touched,* (4) touched genitals/had their genitals touched, (5) given oral sex or received oral sex. Items with an asterisk were asked of all youth, others were asked only if the response was “yes” to the item listed immediately before it. Participants received a score of 0–4, reflecting the highest level of noncoital behavior experi-

enced; adolescents who reported none of the noncoital behaviors were included in the lowest category, along with those who had only kissed.

At T3, respondents who reported having sex in the past year were asked how often they used a condom when they had sex in the past 12 months (0 = *always*, 1 = *sometimes*, 2 = *never*). We combined “sometimes” and “never” responses to create a dichotomous indicator of less than consistent condom use in the past year.

Virginity pledge status was measured at baseline with the following question from Add Health: “Have you made a public or written promise to not have sex before marriage (yes/no)?”

Covariates were measured at baseline. In addition to age, gender, and race/ethnicity, we included indicators known to predict sexual initiation, virginity pledging, or both. A list of covariates is shown in Table 1. Details regarding the measurement of the first 13 are provided in a previous paper [16]. Each of these was a strong predictor of intercourse initiation in prior analyses of this data set [16]. In addition to these factors, we measured peer descriptive norms by asking respondents, “How many of your friends have had sexual intercourse (1 = *none* to 5 = *all*)?” and measured peer prescriptive norms (perceived peer approval of sex) by asking respondents, “How would your friends feel if you had sexual intercourse in the next year (1 = *disapprove a lot* to 5 = *approve a lot*)?” We also measured respondents’ amount of participation in community, school, or church activities or clubs (0 = *none* to 3 = *3 or more hours per day*), which was a strong predictor of making a virginity pledge in Bearman and Brückner’s [4] study.

Participants’ intentions to marry and have children before age 22 were assessed at baseline via the following items: Do you think you’ll get married/have children?,” and, for those responding “yes,” “Do you think you’ll get married/have your first child at age 17 or younger, age 18 to 21, or 22 or older?” We derived dichotomous variables to distinguish those who intended to marry and those who intended to have children prior to age 22 from all others providing a rough indicator of intent to marry/have children prior to the completion of college.

Sexual knowledge was assessed with true–false items that covered pregnancy, contraception, HIV/STD transmission and prevention. We summed responses to these 11 items and transformed this variable to a standard normal distribution. Expected positive consequences of having sex were measured with a three-item scale (e.g., “If you had sex, it would make you feel more mature or grown up”). Because these three items had different response scales, they were first standardized and then averaged ($\alpha = .57$). Perceived risk of engaging in unprotected sex was measured with three items that asked about the likelihood of getting pregnant, getting AIDS, and getting an STD by having sex one time without using a condom (1 = *no chance* to 4 = *more than a 50/50 chance*; $\alpha = .62$).

Missing data imputation

Covariates were missing in small numbers of cases (0.5%–2% for five variables, <0.5% for six variables). To avoid bias that list-wise deletion of cases with missing data might introduce in our results, we imputed missing data [17]. Imputation was based on random draws corresponding to model-based predicted probabilities. We did not impute missing values on virginity pledge status or on any of the outcome variables.

Analysis

Following Rosenbaum and Rubin [12] and others [13,14], we used a “counterfactual” framework to define the effects of making a virginity pledge. Within this framework, we define the effect of making a pledge as the difference between what actually occurred among pledgers and nonpledgers (e.g., intercourse initiation, condom use) and what would happen if each individual’s pledge status were the opposite of what was observed (i.e., in a hypothetical counterfactual scenario). In particular, we estimated the average treatment effect on the treated (ATT) [18], which is the effect of the virginity pledge (the “treatment”) on individuals who resemble those who pledge, rather than the effect of pledging on the entire population (including the types of individuals who are very different from those who pledge). The ATT is of particular interest in the virginity pledge scenario because pledging is expected to be most (perhaps solely) effective in delaying the sexual initiation of youth when undertaken voluntarily [4]. Were it possible to coerce youth who would be unlikely to pledge to do so, it seems unlikely that such a coerced pledge would have much effect on later behavior.

Using logistic regression, we estimated, for each participant, the probability of pledging, p_i , from the 22 baseline characteristics described above. To estimate average treatment effects on the treated, we assigned each pledger a weight of 1 and each nonpledger a weight of $p_i/(1 - p_i)$ [19]. This approach gives greater weight to the survey responses of nonpledgers in the dataset who have characteristics similar to those of pledgers, and less weight to nonpledgers who are unlike people who pledge. Thus, we compare pledgers to similar nonpledgers to estimate the effects of virginity pledges.

A key standard for evaluating the success of propensity score models is the extent to which treated and nontreated groups, in this case pledgers and nonpledgers, have balanced or similar covariate distributions after weighting or matching. To assess covariate balance, we computed the standardized difference (d) for each of the characteristics described above before and after propensity-score adjustment. A general rule of thumb is that a standardized difference greater than 10 represents meaningful imbalance in a given covariate [20–22].

After verifying that our propensity-score weights bal-

anced covariates between pledgers and nonpledgers, we used propensity-score-weighted logistic regression models to estimate the association between making a virginity pledge and each of our outcomes. These models also included covariates, as is commonly recommended [23,24].

We excluded from all analyses participants who already had intercourse by the time of the baseline survey ($n = 334$) as well as participants for whom we were missing information either on baseline sexual initiation status ($n = 75$), virginity pledge status ($n = 71$), or both ($n = 6$). Thus, the final (unweighted) sample size for estimating the propensity score model was 1,517. Of these 1,517 participants, 76% ($n = 1,154$) participated in the T3 survey. Analyses predicting intercourse initiation from virginity pledge status were restricted to the 96% ($n = 1,105$) of these 1,154 participants for whom intercourse initiation status at T3 was known. Of these 1,105 participants, 53% ($n = 591$) reported not having sex by the T3 survey. Of these 591 participants, 98% ($n = 579$) provided complete data on noncoital sexual behavior at T1 and T3 and were thus included in the analyses predicting noncoital sexual behavior. Of the 514 participants who did initiate intercourse in the year prior to the T3 survey, 94% ($n = 484$) provided data on condom use at

T3 and were thus included in the analyses predicting less than consistent condom use.

All analyses were conducted in Mplus 3.12 using maximum likelihood for parameter estimation [25]. To be certain that our estimates were robust to violations of the assumption of multivariate normality, we estimated standard errors using a sandwich estimator and tested the significance of coefficients with the Yuan-Bentler [26] T2* test statistic.

Results

In our baseline sample of virgins, 23.8% reported having made a virginity pledge. This is comparable to the rate of pledging observed in Add Health's sample of teens [4,9]. In our weighted longitudinal sample, 17% had ever had intercourse at baseline, 29% at T2, and 53% at T3. Of teens who reported having intercourse in the year prior to the T3 survey, 42% reported less than consistent condom use.

Treatment selection model

Table 1 presents results of the multivariate logistic regression model predicting virginity pledging at T1. The concordance (c) value for this model is 0.8. Making a pledge was associated with older age, greater parental monitoring, greater perceived parental disapproval of sex, lesser perceived peer approval of sex, greater religiosity, greater participation in activities or clubs, lower sex self-efficacy ($p = .06$), greater knowledge about sex ($p = .06$), and less positive expectations about the outcomes of having sex.

Table 2 shows the percent standardized differences between pledgers and nonpledgers before and after adjustment by the propensity score. Before applying propensity score weights the two groups differed (by greater than 10%) on 15 of the 22 variables. The strongest differences (greater than half a standard deviation) were that, compared to nonpledgers, pledgers perceived less approval of sex from their peers, were more religious, participated in activities and clubs more often, and expected fewer positive outcomes from sex. After we applied weights derived from the propensity score model to nonpledgers, between-group differences were almost completely eliminated, with no standardized difference exceeding 5.5%. Thus, there is strong evidence that the propensity score weights correct for bias associated with our extensive set of observed covariates.

Outcomes analysis

Table 3 shows estimates of the effects of virginity pledging on intercourse initiation, noncoital sexual behavior, and less than consistent condom use, adjusted by propensity score weighting and controlling for covariates. As the table shows, making a virginity pledge was negatively associated with intercourse initiation and not associated with noncoital sexual behavior (among those who did not initiate intercourse) or with condom use (among those who did initiate

Table 1
Multivariate logistic regression predicting making a virginity pledge at T1 ($N = 1517$)

Predictor variables	<i>b</i>	SE	<i>p</i>
Age, years	.18	.06	.002
Female ^a	.02	.16	.92
Race/ethnicity ^b			
African American ^a	-.13	.26	.62
Hispanic ^a	-.22	.28	.42
Other ^a	-.17	.33	.61
Lives with both parents ^a	.17	.20	.40
Has at least one older sibling ^a	.24	.15	.11
Parent education	-.02	.15	.65
Parental monitoring	.31	.13	.01
Perceives parent disapproval of sex ^a	.97	.49	.04
Has mostly older friends	.16	.22	.47
Religiosity	.95	.13	<.001
Sensation seeking	-.02	.07	.81
Deviant behavior ^c	.04	.05	.45
Self-esteem	.15	.15	.32
Perceived prevalence of sex among peers	-.18	.11	.11
Perceived peer approval of sex	-.21	.08	.01
Participation in activities or clubs	.36	.07	<.001
Intention to marry before age 22 ^a	.08	.23	.72
Intention to have children before age 22 ^a	-.14	.42	.73
Low sex self-efficacy	.23	.12	.06
Sexual knowledge	.15	.08	.06
Positive sex expectancies	-.40	.13	.001
Perceived risk of unprotected sex	-.05	.15	.73

Note: All predictor variables measured at T1.

^a Dichotomous predictor.

^b The comparison group was non-Hispanic white.

^c Defined as being sent out of class, skipping school, cheating on a test, stealing, intentionally damaging property, or breaking into a house, school or place of business.

Table 2
Percent standardized differences (*d*) between virginity pledgers and nonpledgers before and after propensity score weighting

Covariate	Before weighting			After weighting		
	Pledger (mean or %)	Nonpledger (mean or %)	<i>d</i>	Pledger (mean or %)	Nonpledger (mean or %)	<i>d</i>
Age, years	15.31	15.19	7.48	15.31	15.29	1.33
Female	61.1%	45.3%	32.07	61.1%	58.4%	5.51
White	81.2%	78.3%	7.22	81.2%	80.7%	1.27
African American	8.2%	9.4%	-4.24	8.2%	8.8%	-2.15
Hispanic	6.6%	7.7%	-4.27	6.6%	6.9%	-1.20
Other	4.1%	4.6%	-2.45	4.1%	3.6%	2.60
Lives with both parents	85.4%	80.6%	12.80	85.4%	83.6%	4.98
Has at least one older sibling	68.2%	64.1%	8.67	68.2%	67.7%	1.07
Parent education	3.90	3.85	3.43	3.90	3.88	1.76
Parental monitoring	4.33	4.05	43.76	4.33	4.31	2.53
Perceives parent disapproval of sex	98.7%	91.9%	32.55	98.7%	98.7%	0.00
Has mostly older friends	13.3%	11.5%	5.46	13.3%	13.8%	-1.46
Religiosity	3.74	3.09	85.61	3.74	3.73	2.08
Sensation-seeking	3.95	3.78	17.90	3.95	3.93	2.43
Deviant behavior	7.19	7.68	-28.07	7.19	7.21	-1.04
Self-esteem	3.49	3.39	22.11	3.49	3.48	1.12
Perceived prevalence of sex among peers	1.61	1.85	-30.10	1.61	1.63	-2.29
Perceived peer approval of sex	2.00	2.67	-59.14	2.00	2.06	-5.20
Participation in activities or clubs	1.70	1.10	64.15	1.70	1.72	-2.15
Intention to marry before age 22	10.2%	10.9%	-2.28	10.2%	10.5%	-0.98
Intention to have children before age 22	2.7%	4.6%	-10.14	2.7%	2.7%	0.00
Low sex self-efficacy	1.98	1.80	25.96	1.98	1.99	-0.81
Sexual knowledge	0.22	0.14	8.58	0.22	0.18	3.94
Positive sex expectancies	-0.28	0.04	-50.85	-0.28	-0.26	-3.99
Perceived risk of unprotected sex	3.10	3.04	12.31	3.10	3.11	-1.29

intercourse). Figure 1A presents estimated rates of intercourse initiation among pledgers and nonpledgers. Two estimates are shown for nonpledgers: one derived from a model that adjusts for covariates only, and another derived from a model that adjusts for covariates and uses propensity scores to weight nonpledgers to match pledgers. As the figure shows, the net difference in intercourse initiation between pledgers and nonpledgers from the model that adjusts for covariates only is almost three times as large as the net difference from the propensity-adjusted model, suggesting that selection bias would have greatly overstated the true average treatment effect of virginity pledges on treated pledgers in the absence of propensity score weighting. Nonetheless, although removing selection bias associated with our observed covariates greatly diminishes the relationship between making a virginity pledge and intercourse initiation, it does not eliminate that relationship. Figure 1B presents comparable estimates for less than consistent condom use. In neither the covariate-adjusted model nor the covariate-adjusted and propensity-score-weighted model do estimates of consistent condom use differ between pledgers and nonpledgers. We ran these same models treating condom use as a three-level (0 = *always*, 1 = *sometimes*, 2 = *never*) ordinal variable and found remarkably similar results. In the propensity-score-weighted ordinal logistic regression model, the log odds for virginity pledging on

condom use was $-.42$ (SE = $.37$) and the associated *p*-value was $.25$. A test of the proportional odds assumption confirmed that it is reasonable to assume a constant odds ratio across each of the categories of the three-level condom variable, $\chi^2(24) = 21.46$, *p* = $.61$.

Discussion

The primary question that our study addresses is whether, among adolescents who have characteristics associated with being inclined to make a virginity pledge, making a virginity pledge delays sexual initiation. That is, among a group of youth who are already inclined to delay intercourse relative to other youth, does the act of making a virginity pledge further delay their debut? Our analysis suggests that it does (and without influencing noncoital sexual behavior). According to our propensity score model, 42.4% of adolescent virgins aged 12 to 17 who are the type inclined to make a virginity pledge would initiate sexual intercourse within 3 years in the absence of making a pledge. In the presence of the pledge, 33.6% initiate sexual intercourse in that same interval. Thus, making a pledge may lessen the likelihood of sexual initiation by almost 9 percentage points among this group of low-risk youth. This size reduction is comparable to the reduction observed for other interventions that have been shown to impact adoles-

Table 3
Propensity score analysis of the effect of making a virginity pledge at T1 on intercourse initiation (N = 1105), noncoital sexual behavior (N = 579), and less than consistent condom use (N = 484) at T3

Predictor variable	Intercourse initiation			Noncoital sexual behavior ^{cd}			Less than consistent condom use ^e		
	<i>b</i>	SE	<i>p</i>	<i>b</i>	SE	<i>p</i>	<i>b</i>	SE	<i>p</i>
Virginity pledge ^a	-.50	.22	.02	-.36	.23	.11	-.40	.34	.25
Covariates									
Age, years	.10	.11	.36	.10	.09	.24	.58	.14	<.001
Female ^a	.40	.26	.12	.04	.26	.88	1.21	.41	.003
Race/ethnicity ^b									
African American ^a	.85	.52	.10	-.08	.46	.87	.35	.55	.53
Hispanic ^a	-.29	.52	.58	-.43	.42	.30	-.51	.88	.56
Other ^a	1.39	.45	.001	-.18	.63	.77	-.57	.69	.41
Lives with both parents ^a	-.11	.35	.76	.09	.33	.79	-.73	.44	.10
Has at least one older sibling ^a	-.19	.27	.48	.35	.25	.15	.53	.36	.15
Parent education	-.16	.08	.04	-.01	.08	.92	.13	.12	.28
Parental monitoring	-.31	.22	.14	.17	.24	.48	.03	.31	.93
Perceives parent disapproval of sex ^a	1.47	1.09	.18	-.71	1.27	.58	1.34	.87	.12
Has mostly older friends ^a	.20	.31	.53	-.45	.40	.26	-.18	.48	.72
Religiosity	-.21	.20	.30	-.13	.24	.59	.39	.30	.21
Sensation seeking	-.20	.12	.10	-.24	.11	.04	-.15	.20	.46
Deviant behavior	.14	.08	.11	-.10	.10	.33	.00	.09	.96
Self-esteem	.05	.26	.86	-.19	.26	.46	-.12	.42	.78
Perceived prevalence of sex among peers	.34	.25	.17	-.19	.21	.37	-.22	.24	.35
Perceived peer approval of sex	.29	.12	.02	.05	.14	.71	.62	.18	<.001
Participation in activities or clubs	-.04	.37	.79	-.11	.13	.41	-.09	.16	.60
Intention to marry before age 22 ^a	-.03	.37	.93	.45	.36	.21	.95	.51	.06
Intention to have children before age 22 ^a	.33	.56	.56	-.14	.85	.87	1.37	.81	.09
Low sex self-efficacy	-.52	.21	.01	-.07	.18	.70	.58	.29	.05
Sexual knowledge	.04	.13	.75	-.03	.12	.79	-.02	.26	.95
Positive sex expectancies	.20	.20	.32	.40	.22	.07	-.31	.29	.28
Perceived risk of unprotected sex	.43	.24	.07	.06	.25	.80	.50	.38	.18

Note: All predictor variables measured at T1.

^a Dichotomous predictor.

^b The comparison group was non-Hispanic white.

^c Among those not initiating intercourse.

^d Controls for level of noncoital sexual behavior at T1.

^e Among those initiating intercourse.

cent sexual initiation (e.g., [27–30]; for a review, see [31]). Of course, because not all youth have characteristics that incline them to pledge, the reduction in rates of sexual initiation that are likely to be attainable in the broader youth population will be lower. Nonetheless, virginity pledges appear to be as effective among the applicable segment of the youth population as are other interventions that effectively postpone adolescent sexual intercourse.

Our finding that making a virginity pledge is associated with a delay in sexual initiation replicates the findings of Bearman and Brückner [4], and extends them in an important way. In particular, our use of an unusually rich set of psychosocial correlates of both virginity pledging and sexual initiation in combination with a robust statistical method more convincingly addresses the question of self-selection bias that was the primary limitation of previous evidence of the effectiveness of virginity pledge programs.

Another question that our study addresses is whether youth who make a virginity pledge but then have sexual

intercourse are more likely to be inconsistent condom users than comparable youth who do not make a pledge. That is, does pledging impede readiness for responsible sex, should pledgers fail to meet their abstinence till marriage goals? If so, it might be unwise from a public health standpoint to promote pledging, given the high rates of sex among pledgers (33% in our analysis). We found that teens who pledged were marginally more knowledgeable about sex than teens who did not pledge and not less likely to use condoms consistently. Perhaps pledging makes teens less likely to use a condom the first time they have sex, as Bearman and Brückner [4] found, but as soon as they initiate they educate themselves and begin to carry and use condoms for subsequent encounters. Indeed, Brückner and Bearman [9] showed that Add Health pledgers were as likely as nonpledgers were to use condoms at their most recent intercourse. Still, failure to use a condom at first sex is important, as the risk of teen pregnancy is considerably higher among those who do not use condoms at first sex [32].

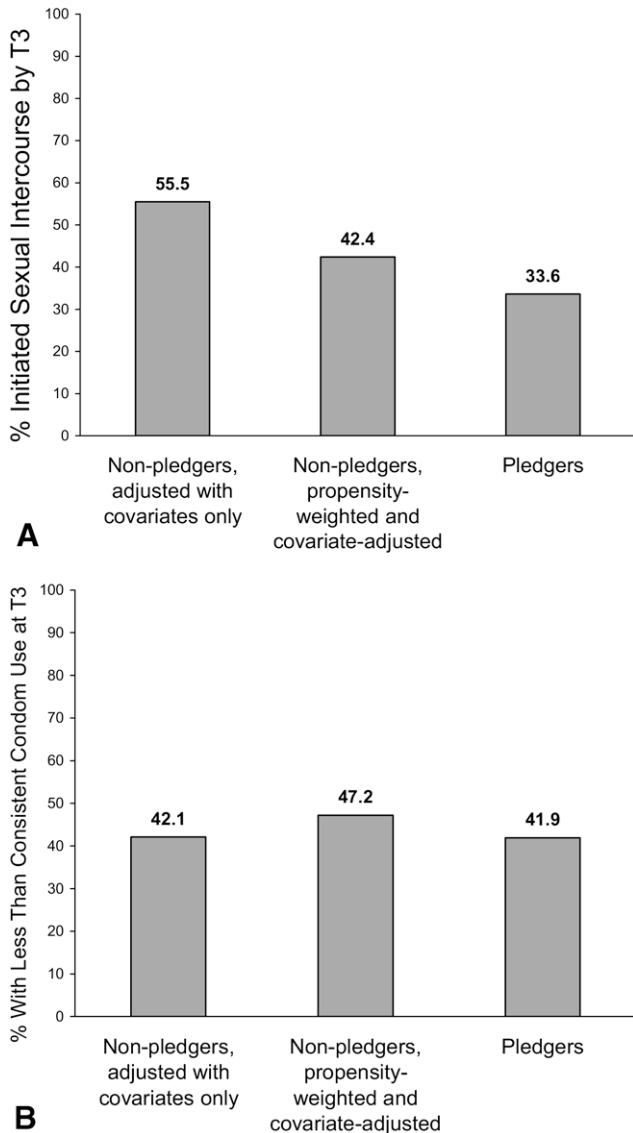


Figure 1. (A) Estimated rates of intercourse initiation among virginity pledgers and nonpledgers. The leftmost bar shows estimates for nonpledgers based on a model that is adjusted with covariates only. The middle bar shows estimates for nonpledgers from a model that is adjusted with covariates and uses propensity scores to weight nonpledgers to match pledgers. (B) Estimated rates of less than consistent condom use among virginity pledgers and nonpledgers. The leftmost bar shows estimates for nonpledgers based on a model that is adjusted with covariates only. The middle bar shows estimates for nonpledgers from a model that is adjusted with covariates and uses propensity scores to weight nonpledgers to match pledgers.

Our study also sheds new light on the characteristics that distinguish pledgers from nonpledgers. Compared to youth who do not pledge, pledgers are more strongly religious, their parents more closely monitor their behavior, their friends and parents are more often opposed to their having sex, they are more likely to join clubs and participate in community activities, they are less likely to expect that having sex will lead to positive consequences, and they have lower sex self-efficacy. Although pledgers are predisposed

to later sexual initiation even in the absence of pledging, it appears that pledging further delays sex among these youth, a finding that is consistent with psychological theories of behavior [6,7].

Our findings should not be taken as evidence that virginity pledges should be imposed upon adolescents. For youth who want to have sex and whose social environments support doing so, pledging is not likely to be an effective means of delaying sexual initiation (and it is doubtful that sincere pledges could be elicited from such youth). These youth need sex education that helps reduce sexual risk taking and unintended pregnancy, as do the substantial number of pledgers who eventually have sex [33]. Moreover, it is questionable whether being coerced into making a pledge will be effective even for teens who have characteristics like those of teens who pledge voluntarily. Psychological theory would suggest that pledging will have an effect on behavior to the extent that the pledger believes he or she freely chose to make a pledge [6]. Thus, it may be important and useful to have both pledge programs and comprehensive sex education available to provide the most appropriate intervention for all types of teens.

Our study may be limited by its reliance on self-report measures of pledging and sexual behavior. Some youth who made a pledge—particularly those for whom it was less meaningful—may have forgotten doing so or been unwilling to later admit to doing so. Also, teens who made a pledge and broke it may not have been completely forthcoming about their sexual behavior. Rosenbaum [34] found that about half of youth from Add Health who reported making a pledge at the first survey did not report having done so a year later at the second survey [34]. Rosenbaum also found that pledgers who reported initiating intercourse between the two surveys were three times less likely to report having made a pledge as those who did report intercourse initiation.

It is also possible that we have omitted important variables from our model. The propensity score method, like all observational approaches, is limited by the availability of variables that can be used to model self-selection; if critical self-selection factors are omitted, then the model will not fully eliminate self-selection bias from estimates. Nonetheless, even imperfect propensity score weights increase the robustness of multivariate regression estimates to the misspecification of the regression model; similarly, the use of even imperfect covariates increases the robustness of estimates to misspecification of the selection model [23]. Finally, it is important to note that our findings may be irrelevant to nonheterosexual adolescents, as we did not ask participants about same-gender sexual behavior.

These potential limitations are balanced by important strengths. We used data from one of the few national data sets apart from Add Health that contain longitudinal data on adolescent sexual behavior and a rich source of predictors of that behavior. Also, this study used the most appropriate

method for drawing causal inferences from observational data, and found that pledging may work for those who are prone to pledging, and does not seem to have long-term effects on sexual safety among those pledgers who fail to remain abstinent.

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