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How strong is the "fake ID effect?" An examination using propensity score matching in two samples

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Abstract

BACKGROUND—Underage college students who obtain and use false identification (fake ID) are at risk for negative outcomes. However, it is currently unclear how uniquely the fake ID itself serves as a vehicle to subsequent harm (i.e., the "fake ID effect") over and above general and trait-related risk factors (e.g., deviant peers, low self-control).

METHODS—In order to investigate whether the "fake ID effect" would hold after accounting for phenotypic risk, we utilized propensity score matching (PSM) in a cross-sectional sample of n=1,454 students, and a longitudinal replication sample of n=3,720 undergraduates. Individuals *with* a fake ID were matched with individuals *without* a fake ID, in terms of a number of trait-based and social risk factors. These matched groups were then compared on five problematic outcomes (i.e., frequent binge drinking, alcohol-related problems, arrests, marijuana use, and hard drug use).

RESULTS—Findings showed that "fake ID effects" were substantially—though not fully—diminished following PSM. The "fake ID effect" remained strongest for alcohol-related arrests. This may relate to issues of enforcement and students' willingness to engage in deviant behavior with a fake ID, or it may be a function of combined processes.

CONCLUSIONS—Overall, the findings suggest that interventions should not only be aimed at reducing fake ID-related alcohol access specifically, but should also be aimed more generally towards at-risk youths' access to alcohol. Future research might examine whether fake IDs have their strongest potency as moderators of the effects of risky traits—such as impulsiveness—on drinking outcomes.

Keywords

False identification; Fake IDs; underage alcohol use; heavy episodic drinking; binge drinking

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Introduction

Fake IDs, a unique mode of alcohol access, are increasingly sought after as individuals near the minimum legal drinking age (Martinez et al., 2007; Wagenaar et al., 1996). These forms of false identification may be borrowed (or duplicated) from an older peer or sibling (Myers et al., 2001), or they may be a specially crafted document obtained locally or from an online vendor (Murray, 2005). Regardless of their source, there appears to be a bidirectional relation between heavy drinking and fake IDs, such that (1) heavy drinking predicts subsequent obtainment of a fake ID, and (2) "ownership" (i.e., possession) of a fake ID predicts subsequent frequency of heavy drinking (defined as 5+ drinks per occasion; Martinez, et al., 2007).

This bidirectional relation not only illustrates the public health risks of this mode of alcohol access, but begs the question of whether it is more the case that a fake ID itself serves as a vehicle to subsequent harm (i.e., the "fake ID effect") or whether such harms and outcomes are predominantly driven by a general level of phenotypic risk on the part of the fake ID "owner" (e.g., deviant peer associations, low self-control). Although general alcohol access theories might support the former hypothesis almost entirely (namely, fake ID possession increases alcohol access and subsequent harm; see Gruenewald, 2011), general criminological theories of phenotypic risk support the latter (namely, that broad categories of risk—or *propensities* to engage in risky behavior—are the true cause of harm; see Pratt & Cullen, 2000). Certainly, such propensities might be what predicts fake ID obtainment in the first place, and although the strength of the fake ID effect appears to increase over time, it is greatly diminished after controlling for sex, Greek status, and pre-college rates of drinking (Martinez et al., 2007). In sum, it is unclear how strong the fake ID effect might be after accounting for individuals' levels of phenotypic or *propensity* risk—though this question has bearing on prevention and policy initiatives, which may focus on either strengthening enforcement of fake ID laws themselves, increasing resources for trait-based at-risk youth programs, or a community-driven combination of both (see Fell, Thomas, Scherer, Fisher & Romano, 2015; Fell, Scherer, Thomas & Voas, 2016; Fell, Scherer & Voas, 2015; Grube, 1997) .

Thus, in order to investigate the strength of the fake ID effect, we matched students with and without fake IDs on a number of risk-based covariates using propensity score matching (PSM) techniques. We first compared matched groups' drinking- and drug-use-related outcomes in a cross-sectional sample of n=1,454 college students at a large Southeastern university. We also compared matched groups in an additional longitudinal replication sample of n=3,720 undergraduates at a large Midwestern university. We hypothesized that the effects of fake ID ownership on outcomes would be greatly diminished by—and therefore largely attributable to—the pre-existing trait-based factors on which fake ID owners and non-owners could be matched. These comparisons can inform the extent to which the relationship between negative outcomes and false identification ownership are attributed to selection factors, which again, may have practical application for intervention and policy.

Methods

Procedure and Participants

Two samples were separately investigated following Institutional Review Board (IRB) approval: (1) A cross-sectional sample of *n*=1,454 underage college students from a large Southeastern University (IRB Protocol H12032) and (2) a prospective replication sample of *n*=3,720 undergraduates under the minimum legal drinking age from a large Midwestern university (IRB Protocol 01-01-001). Of note, both samples offer unique insights into the relationship between false identification use and negative outcomes. More specifically, the cross-sectional study includes items that distinguish between the use of fake IDs in different situations (at bars, at grocery stores, etc.) and the longitudinal study offers insight into the potential effects of fake ID ownership over time and establishes temporal order.

With regard to the cross-sectional sample, during the academic year 2011–2012, participants were recruited from forty randomly selected large (>99 students) and moderate enrollment (30–99 students) classes. Participants completed a one-page informed consent document in the selected classes before being given a six-page paper survey about college life and behaviors to complete with pencil or pen. Participants were not compensated. All enrolled students were invited to participate and the response rate was high at 80.4% (Stogner & Miller, 2013; 2014; Hart et al., 2014). After those above the legal drinking threshold were removed, the analytic sample was *n*=1,454 underage individuals. The sample was largely representative of the university with regard to demographics and was specifically 51.6% female, 68.9% White/non-Hispanic, with an average age of 18.95 (*SD*=.795). Although this sample is cross-sectional, establishing temporal ordering of the covariates and fake ID ownership is largely inconsequential for the majority of covariates as many are immutable (age, race, gender) or outside of the individual's control (home location, parental income, sexual orientation, etc.).

The longitudinal sample also utilized a self-report survey methodology. All incoming students in 2002 were recruited to complete an instrument during the summer prior to university entrance using paper and pencil and then were asked to complete online surveys each semester for the next four years (a total of eight semesters). Students provided informed consent and were compensated $25 in each wave. After excluding the *n*=35 who were of age, 88% of the eligible entering class completed the survey (n=3,720). The sample was 53.7% female, 90.3% White/non-Hispanic, and averaged 17.9 (*SD*=.36) years of age (reflecting demographics that are representative of the university as a whole [University Registrar, 2013]). Students were traditionally aged; by the start of their junior year, only one-third of the sample had reached the minimum legal drinking age, climbing expectedly to 99.7% by the final semester of college, Sample retention was good, ranging from 69% to 87% of baseline respondents participating at each subsequent wave. Retention biases were low, though individuals were more likely to remain in the sample if they were females (OR=2.33) and were less likely to remain in the sample if they were frequent binge drinkers (OR=.88; Sher & Rutledge, 2007). By the final time-point, the sample size was *n*=2,250, though 90% of students participated in two or more assessment waves and 82% participated in three or more waves. The longitudinal PSM presented within the text utilized the first two

years of college only (i.e., the first four semesters, when the overwhelming majority of participants were underage) and, consistent with most PSM research, only created matches *between* individuals in a manner which is directly comparable to the analysis performed with the cross-sectional sample.[1]

Measures

For the purposes of replication, it was important that the measures used in both the cross-sectional and longitudinal studies stayed as similar as possible. For ease of presentation, measures are organized in terms of their conceptual importance to the overall study with cross-sectional and longitudinal measures explained together in each section. Timing of the longitudinal measures was considered important and is described as is appropriate. Specifically, though the eight-wave longitudinal sample included multiple measurements of many covariates across time, the primary longitudinal PSM only utilized measurements as they would be expected to occur if observing a "fake ID effect" over a logical progression of time (i.e., Trait/propensity measures were measured at Wave 1 and used to predict fake ID ownership at Wave 2 which in turn assessed as a predictor for outcome measures at both Waves 3 and Wave 4). The second semester of college (Wave 2) was chosen as the singular target time-point at which fake ID ownership (or the "fake ID effect") was measured, because it is thought to be a peak time of risk for negative drinking-related outcomes and false ID ownership (see Martinez et al., 2008).

Main Outcomes—Five outcomes related to substance use were explored. First, a measure of *frequent binge drinking* was created in both samples. A six-option ordinal item asked respondents how many days in the last month did they consume five or more alcoholic drinks. A sex-specific binge drinking measure was not available. Those selecting either of the two highest frequency options (10–19 days and 20+ days) were classified as frequent binge drinkers while all others were not. This dichotomous item represents binge drinking more than ten days in the last month. Second, we utilized an instrument created by Maney, Higham-Gardill, and Mahoney (2002) to represent *alcohol-related problems* in the cross-sectional sample. This ten-item scale assesses the degree to which the individual feels that alcohol use has created relationship, family, health, behavioral, and professional/school problems in the last year and shows adequate reliability (α=.822). In the longitudinal sample, this scale was approximated from ten items taken from the Young Adult Alcohol Problems Screening Test (YAAPST; Hurlbut & Sher, 1992) with adequate reliability (α=.848 in second-year fall and α=.846 in second-year spring). A dichotomous *alcohol-related arrest/citation* measure was created within both samples using items that asked respondents if they had ever been arrested or cited for driving under the influence, underage drinking, public disorder (due to alcohol), being drunk in public, or an open container violation in the last year. The final two outcomes were both dichotomous and measured similarly in each sample; *marijuana use* and *hard drug use* represent whether the respondent self-reported any use of marijuana and cocaine, heroin, and/or methamphetamine, respectively, in the last year.

[1]A second, exploratory PSM approach with the longitudinal sample was also conducted. This analysis used all eligible data points from the eight waves (i.e. waves in which the respondent had yet to turn 21) and allowed both inter- and intra-individual matches. The results from this analysis are presented in the subsequent footnote.

False Identification—Current fake ID "ownership" was assessed dichotomously in both samples (0=No, 1=Yes). The cross-sectional study also included additional items that asked respondents whether they had used the fake ID in a bar or club and whether they had used it in a store to purchase alcohol.

Trait and risk factor (matching) covariates—Fifteen variables were used in propensity score matching in the cross-sectional sample and fourteen were used in the longitudinal sample. Variables were selected due to their inclusion in both datasets and previous research suggesting that they may be related to the propensity to own a fake ID and experience one of the five outcomes. These matching variables are: (1) age, (2) age of alcohol use onset, (3) employment status, (4) exposure to substance use, (5) family income, (6) gender, (7) GPA, (8) Greek membership, (9) health, (10) low self-control, (11) peer substance use, (12) race, (13) rural home location (only measured in the cross-sectional study), (14) sexual orientation (1=LGBT), and (15) subjective distress.

Eight of the fifteen variables were measured identically and a ninth was measured nearly identically. Among those identically measured were age, age of first alcohol use, employment status (0= not employed; 1=employed), gender (0=female; 1=male), self-reported grade point average (GPA), membership in a campus Greek organization (0=non-member; 1=member), race (0=white, 1=non-white), and sexual orientation (0=heterosexual; 1=lesbian, gay, bisexual, or other). Self-reported health was measured with an item that asked respondents to rate their own health—the cross-sectional study offered responses ranging from 1 (poor) to 4 (excellent) whereas the longitudinal study options ranged from 1 (poor) to 5 (excellent).

The cross sectional study utilized four-item measures adapted from Lee, Akers, and Borg (2004) to represent exposure to substance use (α=.786) and peer substance use (α=.801). As the longitudinal data did not include similarmeasures, each of these constructs was represented by a single item rather than a four-item scale. The first (exposure) was measured dichotomously while the second (peer substance use) was measured on a six-option ordinal scale. Low self-control was operationalized using the 24-item Grasmick et al. (1993) scale (α=.889) in the cross-sectional study and the NEO Five Factor Inventory conscientiousness scale (reverse-coded) in the longitudinal sample (α=.844; Costa & McCrae, 1992). Subjective distress was measured using Cohen and Williamson's (1988) ten-item perceived student stress scale (α=.814) in the cross-sectional study and the Global Severity Index from the Brief Symptom Inventory-18 in the longitudinal sample (Derogatis, 2000). Higher values on these scales represent more exposure to substance use, a larger portion of peers that use substances, lower self-control, and more subjective distress, respectively.

Both studies included a single-item family socioeconomic status measure. In the cross-sectional study a measure of family income was used. Participants chose between options ranging from under $10,000 per year (coded 1) to over $175,000 per year (coded 9). An item assessing whether or not students were the first in their family to attend college (0=No, 1=Yes) was utilized in the longitudinal study.

Rural home location was used in the cross-sectional study, but no similar measure was available in the longitudinal data. This variable was important to include despite creating differing matching criteria due to the characteristics of the study area. The cross-sectional sample was drawn from an area that is very rural with the exception of one major city; thus, a dichotomous item representing whether the student grew up in an urban / suburban area (coded 0) or a rural one (coded 1) was included. By comparison, this was not a special consideration for the longitudinal sample, which originated from a university of 35,000 that draws students from two large neighboring cities and its own moderately large population.

Analysis

First, we estimated the proportions of fake ID ownership in both the cross-sectional and longitudinal samples. In addition to possession of a fake ID, the cross-sectional sample also documented participants' using of the fake ID in bars/clubs and stores. We estimated the bivariate associations of fake IDs with the five specified substance use outcomes in both samples—a rudimentary "fake ID effect."

Next, to better determine the strength of the "fake ID effect" after accounting for trait measures, propensity score matching (PSM) was used for both samples. PSM offers a clearer picture of the relationship between two variables than bivariate analyses which may yield spurious results (Guo & Fraser 2009) and has been used to assess issues related to substance use (Miller et al., 2011). Also of note, PSM is preferable to multivariate regression models in instances such as this where the variable of interest may not be independently connected to the dependent variable, but is likely correlated with those that are and also occurs more proximally. The propensity matching techniques developed by Rosenbaum and Rubin (1983, 1985) can be used to create a sample with two groups that are similar in all relevant variables except for the "treatment" (i.e., fake ID possession). While their techniques do lead to a reduction in size of analytic sample (often leading PSM to be referred to as resampling), they are effective at creating a situation whereby the effect of "treatment" can be estimated as the average difference between those exposed to the treatment and "counterfactuals," defined as the anticipated outcomes were it not for exposure to the treatment (Guo & Fraser 2009). In this case, the PSM method creates analytic groups whereby differences other than false identification use are minimalized.

As suggested by Rosenbaum and Rubin (1983, 1985), we utilized logistic regression to estimate a propensity score for each participant in each analytic sample. Fake ID possession was regressed on 15 covariates in the cross-sectional sample (age, age of firsts alcohol use, employment status, exposure to substance use, family income, gender, grade point average, membership in a campus Greek organization, self-assessed health, low self-control, peer substance use, race, size of home community, sexual orientation, and subjective distress) and 14 similar variables (size of home community excluded, as explained above) in the main longitudinal PSM analysis (i.e. fake ID possession measured at the second semester and outcomes evaluated in the third and fourth semesters). Using these models, each participant's propensity score was then calculated as their conditional probability of having a fake ID. Following an assessment of regions of common support, we created comparison groups within each sample using a two-to-one nearest neighbor matching algorithm with a

caliper calculated as .25σ of the propensity scores (see Guo & Fraser 2009). This caliper was .0725 in the cross-sectional sample and .0426 in the longitudinal sample. This matching technique led to the expected decrease in sample size (n=817 and n=518, respectively) but a sufficient number of cases were retained for statistical comparisons.

Results

Rates of fake ID ownership

Rates of fake ID ownership were quite high, particularly in the cross-sectional sample. That is, of the 1,454 underage alcohol consumers in the cross-sectional sample, 583 or 40.1% own or have owned a fake ID, 560 (38.5%) have used a fake ID at a bar, and 460 (27.8%) have used the ID to purchase alcohol at a store. Prevalence rates of false ID use in the Midwestern sample changed over time. Fake ID ownership among students under 21 peaked during the third year of college (pre-college=12.5%, first-year fall=17.1%, first-year spring=21.4%, second-year fall=28.1%, second-year spring=32.2%, third-year fall=34.9%, third-year spring=39.0%, fourth-year fall=38.1%, fourth-year spring= fewer than ten students were below the minimum legal drinking age).[2]

The “fake ID effect” prior to matching

Table 1 presents mean scores for five substance use outcomes for fake ID owners and non-owners in both samples (outcomes at both Wave 3 and 4 are reported for the longitudinal sample). Average scores for each outcome (frequent binge drinking [10 or more days in the last month], self-reported alcohol related problems, alcohol-related arrests, marijuana use, and hard drug use) are presented for those that have and have not owned a fake ID, used a fake ID at a bar/club (cross-sectional sample only), and used a fake ID at a store (cross-sectional sample only). Independent samples t-tests were conducted to determine whether, on average, differences exist between fake ID users and non-users. Each of the tests reached significance. Regardless of whether the focus was ownership of a fake ID or using it at a specific type of outlet, the results were consistent. At the bivariate level, more individuals with false identification engage in frequent binge drinking, have been arrested/cited for an alcohol violation, engage in marijuana use, and use hard drugs. Individuals with fake identification also, on average, report more alcohol-related problems. These results would indicate that fake IDs are a vehicle of risk. However, it is possible that fake ID ownership (and associated risks) are more a function of underlying risky traits.

Propensity score matching (PSM)

Due to the consistency in the findings to this point regardless of false identification measure (ownership, bar use, and/or store use), the additional analyses with each sample utilizes only one false identification measure, possession of a false ID. We carried out PSM analyses in both samples, to examine whether individuals with and without fake IDs continue to differ on these outcomes after being matched on substantively important traits. Table 2 shows that individuals with and without fake IDs indeed differed from each other on these trait

[2]These percentages represent the number of students under 21 that owned a fake ID at the time of that wave’s data collection. Students above the minimum legal drinking age are excluded.

propensity variables, suggesting that it is these variables which may ultimately be driving the fake ID effect. Table 2 also shows that the PSM technique worked well in both samples, consistently reducing bias associated with the statistically significant differences between those with and without fake IDs by more than 50% on all but one variable. Though three significant differences still remained in the cross-sectional sample (age of alcohol use onset, Greek affiliation, and having been raised in a rural area), the magnitude of the differences in age of onset and Greek affiliation were minor compared to pre-matching. In this, matching was equally, if not more, successful in the longitudinal sample. It should be noted that matching did yield a reduction in sample size. Overall, however, in both samples matching appears to have created treatment and comparison groups that are more equivalent and more appropriate for comparison than the unmatched data.

The propensity scores that were calculated for each case are graphically displayed in Figure 1. As can be seen in the figure, a region of common support exists, but very few with low propensity scores had a fake ID and very few with high propensity scores did not.

Comparing fake ID owners and non-owners after PSM

Cross-sectional sample—After matching, false identification owners and non-owners were compared on each of the five substance use related outcomes. While significantly more of those with fake IDs in the cross-sectional sample were frequent binge drinkers prior to matching (t=9.81, df=815), the groups were no longer significantly different after matching (t=1.81, df=815) and the average treatment effect (ATE; e.g., differences in group means), as displayed in Table 3, was reduced by 59.2%. Similarly, prior to matching, fake ID owners had significantly higher scores on the alcohol related problems scale than non-owners (t=9.83, df=815), but the groups were no longer significantly different after matching (t=1.31, df=815) and the ATE was reduced by 63.4%. However, in terms of alcohol-related arrests, the two groups were still significantly different and the ATE effectively remained unchanged. As was the case for the first two outcomes, fake ID possession was associated with marijuana use prior to matching (t=9.36), but not after (t=1.52; ATE reduced by 60.4%). Finally, hard drug use was associated with fake ID possession both before (t=7.26, df=815) and after matching (t=2.29, df=815), but the ATE was reduced by 38.4%.

Longitudinal sample—As was the case in the cross-sectional sample, propensity score matching led to a substantial decrease in the ATE for four of the five outcomes (Table 3, columns 3–6). However, unlike the cross-sectional sample, ATEs remained significant for alcohol related problems (t=4.00 wave 3; t=4.17, wave 4, df=516) and marijuana use (t=4.13, wave 3; t=2.58, wave 4, df=516) after propensity score matching. The ATE also remained significant for frequent binge drinking (t=3.26, df=516, wave 3) and hard drug use (t=2.06, df=516, wave 4) at one wave but not the other. Again, these effects sizes were substantially reduced, but in the aforementioned cases, not eliminated. As was the case in the cross-sectional sample, propensity score matching had little influence on the ATE on alcohol-related arrests.[3]

[3]To reinforce these findings, we utilized all eight waves of the longitudinal sample and allowed individuals to serve as the comparison for their own data points at different waves (inter- and intra-individual matching). Though PSM is not routinely used in this way as doing so may undermine its assumptions about the independency of data points, we explored this option in order to demonstrate the

Conclusions

This study's initial results are consistent with previous research—a substantial number of underage students have fake IDs and are at higher risk for binge drinking, alcohol-related problems, alcohol related arrests, and other substance use (see Arria et al., 2014; Martinez & Sher, 2010; Nguyen et al., 2011). Yet our work also showed that for some outcomes, it seems that what initially might have appeared to be a "fake ID effect" is largely the result of factors that influenced both the acquisition of the false ID and the outcome. The significant relationship between fake ID use and other substance use outcomes often remained after PSM, but the magnitude of these relationships were substantially diminished, most by over 40%. Alcohol-related arrests were an exception as the relationship was unaffected by PSM (i.e., after matching, those with a fake ID were still at similarly high levels of risk for alcohol-related arrests [DUIs, open container, etc.]). The reason this outcome is distinct from the others is not readily clear; perhaps law enforcement officers are more likely to issue citations or arrests for other substance-related offenses when an individual is also found with a fake ID. If this is the case, the "effect" would not appear smaller in propensity score models as the difference would be driven by officers' reactions to the fake ID rather than individuals' underlying propensity.

The pattern that emerges from Table 3 seems to indicate that non-matched samples may have overestimated the effect of false identification use on negative outcomes, but that fake ID ownership has an effect that extends beyond shared causal factors. This specific remaining "fake ID effect" could indeed support the idea that the fake ID itself serves as a type of threshold into other forms of deviant behavior, where those who are willing to acquire fake IDs become increasingly willing to violate other laws (see Ruedy et al., 2013; Winograd et al., 2014). But in light of the other findings, it is more likely that fake IDs more generally moderate the effects of risky traits on behavior. For example, fake IDs may have the highest potency of effect via providing impulsive individuals with additional means and opportunity for problematic behaviors that they would not otherwise have engaged in. Indeed, underlying trait risks are often incorporated into opportunity-theory-related examinations of crime (Grasmick et al., 1993; Lagrange & Silverman, 1999).

As such, these findings have practical implications. Though increased server training, fake ID production/supplier laws, and liability laws are an important means of addressing the risks of fake IDs as a form of alcohol access (Fell, Scherer, Thomas & Voas, 2014; Yörük, 2014), fake-ID related outcomes may also partly be a function of trait risks that can

robustness of our findings and also to incorporate all available data in a single analysis (see Leon et al., 2012). A data file was created where each observation was coded as a separate case; all observations in which the respondent was at least 21 years old were eliminated from the file. Then, a multilevel random-effects logistic regression model was estimated using thirteen of the previously described risk-factors as level-1 variables (employment status was excluded as it was not measured in all waves) and the respondent identifier as a level-2 variable. The resulting model was used to create a propensity score for each observation. Similar to previous analyses, regions of common support were assessed and a two-to-one nearest neighbor matching algorithm with a .0577 caliper (calculated as .25 σ of the propensity scores) was utilized. Thus, treatment observations were allowed to match with the two comparison cases with the nearest propensity scores regardless of whether those were from the same person or from the same data collection wave. Results generally mirror those of previous analyses. There is a significant association between fake ID ownership and each of the five outcomes prior to matching. After matching, the magnitude of these associations drastically decreased (the association with binge drinking was reduced by 17.7%, alcohol-related problems by 43.3%, alcohol-related arrests by 43.4%, marijuana use by 33.8%) with hard drug use being an exception (3.9% reduction). The ATE for each remained significant (although this is not unsurprising given the large number of treatment observations included in this analysis [2,027 matched with 4,054 comparison cases]).

additionally be addressed with intervention. One way to begin addressing this combination of factors may be via motivational, normative feedback-based, or skills interventions that are specifically aimed at decreasing the likelihood that at-risk students obtain a fake ID (see Fromme & Orrick, 2004; Larimer & Cronce, 2007). Moreover, a fake ID obtainment-aimed intervention might possibly be broadly incorporated into interventions that are especially tailored toward addressing both individuals' risky traits and their resulting behaviors (see Conrod et al., 2006).

Although a great strength of this study rests in the similar findings found with two unique college populations, these findings may not be generalizable to non-college attending populations. Additionally, fake ID policies, enforcement, and fear of sanctions may vary substantially in different localities (Erickson, Lenk, Toomey, Nelson & Jones-Webb, 2016). For example, some drinking establishments may be lenient in their carding policies, intentionally accept false identification, and/or not be subject to rigorous regulatory enforcement (Murray, 2005). Also, penalties for possessing and/or using a fake ID to purchase alcohol varies substantially from state to state including the type of offense, amount of fine, suspension of driver's license, and the possibility of probation or jail time. Future research should evaluate the impact of fake ID relative to differential policies and enforcement of the minimum legal drinking age, including community efforts (Grube, 1997). Further, while fifteen distinct traits were included in the matching process, there remains the possibility that additional factors not measured in our data would affect both the willingness to access a fake ID and the outcome measures. If this is the case, the "fake ID effect" may be even smaller than our matching models suggested.

In concluding that the "fake ID effect" is mainly a function of phenotypic risk, fake ID ownership may serve as an indicator of heightened risk for more severe drinking related problems. Although most penalties for fake ID ownership are punitive (fines, probation/jail, and/or loss of driver's license), policy-makers, university officials, and practitioners should target fake ID owners for intervention strategies aimed at reducing high-risk drinking behaviors (and other problematic behaviors linked to phenotypic risk). Although increased penalties and enforcement of the minimum legal drinking age has the potential to reduce fake ID ownership, we caution policy-makers to evaluate and consider the negative consequences of moving college drinking away from regulated establishments where security and emergency services are more readily available (see Baldwin et al., 2012; 2014). Although our findings found that fake ID possession (regardless of individual risk characteristics) increased the risk for alcohol related arrests, drug use, and alcohol related problems (Midwest sample only), we did not assess victimization and other harms associated with excessive alcohol consumption that could increase in spaces not subject to regulatory controls (Miller, Levy, Spicer & Taylor, 2006). Future research is needed to evaluate the impact that fake ID enforcement may have on problematic drinking both in regulated and unregulated spaces.

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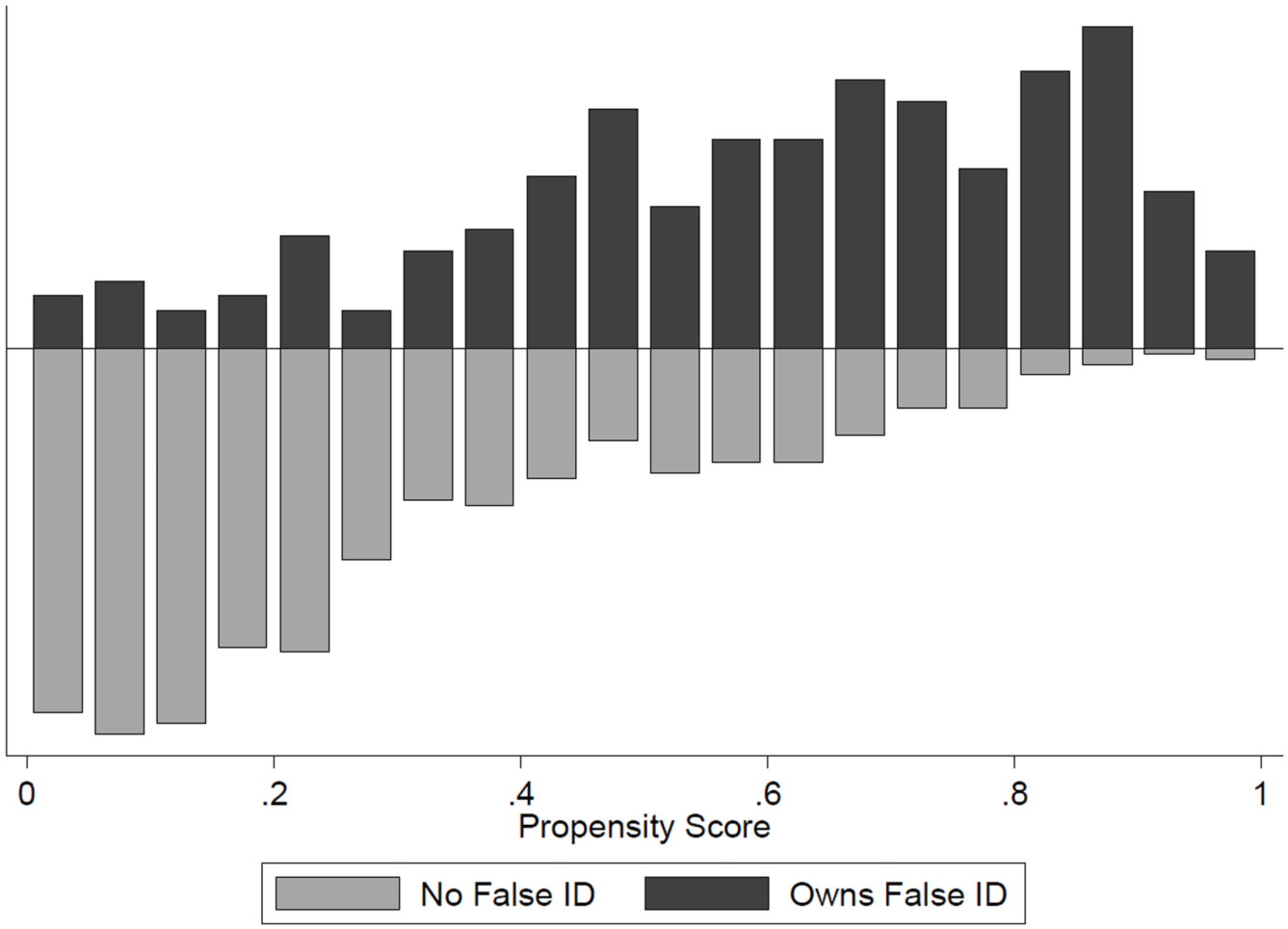

Figure 1.
Propensity scores among students with and without false identification.

Table 1

The “fake ID effect” prior to propensity score matching (PSM)

	No Fake ID *M (SD)*	Fake ID *M (SD)*	Independent samples *t*
Cross-sectional sample (*n*=1,454)			
Fake ID Ownership			
Frequent binge drinking	.09 (.29)	.31 (.46)	9.81*
Alcohol-related problems	1.37 (.53)	1.68 (.63)	9.83*
Alcohol-related arrests	.05 (.22)	.20 (.40)	8.18*
Marijuana use	.57 (.50)	.79 (.41)	9.26*
Hard drug use	.03 (.18)	.15 (.36)	7.26*
Fake ID Use at a Bar			
Frequent binge drinking	.10 (.30)	.31 (.46)	9.18*
Alcohol-related problems	1.37 (.54)	1.69 (.62)	9.75*
Alcohol-related arrests	.05 (.22)	.21 (.40)	8.05*
Marijuana use	.58 (.49)	.80 (.40)	9.30*
Hard drug use	.03 (.18)	.16 (.36)	7.48*
Fake ID Use at a grocery or convenience store			
Frequent binge drinking	.11 (.32)	.35 (.48)	8.96*
Alcohol-related problems	1.40 (.54)	1.74 (.65)	9.08*
Alcohol-related arrests	.07 (.26)	.22 (.41)	6.59*
Marijuana use	.60 (.50)	.82 (.38)	9.26*
Hard drug use	.04 (.20)	.18 (.38)	6.66*
Longitudinal sample, Fake ID Ownership (*n*=3,720)			
Second-year fall (Wave 3) outcomes			
Frequent binge drinking	.05 (.21)	.17 (.37)	8.36*
Alcohol-related problems	.63 (.70)	1.28 (.81)	15.37*
Alcohol-related arrests	.04 (.19)	.13 (.34)	7.61*
Marijuana use	.32 (.47)	.60 (.49)	10.01*
Hard drug use	.05 (.22)	.10 (.30)	3.47*
Second-year spring (Wave 4) outcomes			
Frequent binge drinking	.04 (.19)	.14 (.35)	7.52*
Alcohol-related problems	.62 (.66)	1.20 (.80)	14.38*
Alcohol-related arrests	.04 (.20)	.14 (.35)	6.85*
Marijuana use	.34 (.47)	.60 (.49)	9.57*
Hard drug use	.06 (.23)	.12 (.32)	4.03*

Note: For longitudinal sample, outcome comparisons are made as a function of first-year spring (Wave 2) fake ID possession, the peak time of risk for negative drinking-related outcomes (see Martinez, Sher & Wood, 2008).

Table 2

Comparison of matched and non-matched samples (restricted to underage drinkers).

	Prior to Propensity Score Matching			After Matching		
	No Fake ID	**Fake ID**	**p**	**No Fake ID**	**Fake ID**	**% bias reduction**
Cross-sectional Sample	***N*=1454** (583 of which had fake IDs)			***N*=817** (359 of which had fake IDs)		
Age	18.95	19.06	.007	19.06	19.14	8.0%
Age of Alcohol Use Onset	16.26	15.38	<.001	15.13	15.35	78.6%*
Employed	.22	.24	.321	.20	.24	NA
Exposure to Substance Use	3.68	4.22	<.001	4.28	4.25	94.7%
Family Income	4.90	5.91	<.001	5.87	5.87	99.2%
Gender (1=Male)	.42	.50	.002	.52	.49	75.6%
GPA	3.00	3.02	.466	3.11	3.05	NA
Greek membership	.09	.35	<.001	.33	.38	81.3%*
Health (self-assessed)	3.33	3.44	.001	3.50	3.44	52.3%
Low Self-Control	2.13	2.24	<.001	2.25	2.24	86.0%
Peer Substance Use	2.42	2.90	<.001	2.94	2.93	98.2%
Race (1=Nonwhite)	.43	.11	<.001	.12	.10	94.9%
Rural Home Location	.34	.38	.114	.47	.38	NA*
Sexual Orientation (1=LGBT)	.03	.03	.869	.01	.03	NA
Subjective Distress	3.07	3.05	.608	3.06	3.06	NA
Longitudinal Sample	***N*=2392** (510 of which had fake IDs)			***N*=518** (244 of which had fake IDs)		
Age	18.31	18.34	.340	18.34	18.34	NA
Age of Alcohol Use Onset	11.60	13.77	<.001	13.94	13.95	99.8%
Employed	.41	.31	<.001	.29	.33	36.3%
Exposure to Substance Use	.14	.16	.494	.11	.15	NA
Socioeconomic Status	.30	.25	.039	.27	.27	90.5%
Gender (1=Male)	.38	.37	.569	.32	.29	NA
GPA	3.10	2.95	<.001	3.04	3.08	63.8%

	Prior to Propensity Score Matching			After Matching		
	No Fake ID	**Fake ID**	**p**	**No Fake ID**	**Fake ID**	**% bias reduction**
Greek membership	.21	.56	<.001	.56	.55	97.0%
Health (self-assessed)	3.91	4.04	.001	4.04	4.04	98.4%
Low Self-Control	17.34	16.70	.062	15.48	15.66	NA
Peer Substance Use	2.54	3.72	<.001	3.69	3.65	96.2%
Race (1=Nonwhite)	.11	.06	<.001	.07	.06	81.7%
Sexual Orientation (1=LGBT)	.06	.05	.657	.02	.03	NA
Subjective Distress	.53	.56	.481	.55	.56	NA

Note: values listed are means of the groups prior to and following matching. Significant p values in the non-matched sample indicate those with and those without fake identification were different prior to matching. The percent reduction in bias is listed in the final column for each of the factors that were previously significantly different.

An * in the final column denotes the factors that remain significantly different between the groups even after matching.

Table 3

Average treatment effects (ATE) prior to and after propensity score matching.

	Cross-sectional sample		Longitudinal sample, wave 3 outcomes		Longitudinal sample, wave 4 outcomes	
	ATE	**% red.**	**ATE**	**% red.**	**ATE**	**% red.**
Frequent binge drinking in the last month						
Unmatched	.201*	59.2%	.122*	24.6%	.091*	48.4%
Matched	.082		.092*		.047	
Alcohol related problems						
Unmatched	.268*	63.4%	.602*	54.7%	.564*	50.5%
Matched	.098		.273*		.279*	
Alcohol related arrest or citation						
Unmatched	.172*	0.6%	.063*	6.3%	.090*	-2.2%
Matched	.171*		.059*		.092*	
Marijuana use						
Unmatched	.222*	60.4%	.288*	33.7%	.246*	50.8%
Matched	.088		.191*		.121*	
Hard drug use						
Unmatched	.114*	38.6%	.048*	18.8%	.053*	3.8%
Matched	.070*		.039		.051*	

* indicates p<.05