Essays in Health Economics: Understanding Risky Health Behaviors

Essays in Health Economics: Understanding Risky Health Behaviors

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Abstract

This dissertation presents three papers applying health economics to the study of risky behaviors. The first uses data from the 1979 National Longitudinal Survey of Youth to examine the relationship between adverse events and risky behaviors among adolescents. Substance use responses to experiencing either of two adverse events—violent crime victimization or death of a non-family member one felt close to—explain 6.7 percent of first cigarette use, and 14.3 percent of first use of illegal drugs other than marijuana. Analyses of exercise, a positive coping mechanism, find shock-responses consistent with a coping-response, but not with rational, time-inconsistent, or non-rational drivers considered here. I conclude that distressing events lead to risky behaviors, with a coping response contributing to this effect.

Using National Health Interview Survey data, the second paper considers the mechanism behind growth in smoking's education gradient between 1950 and 1980. Regressions test for education differentials in initiation and cessation responses to cigarette advertising, prices, brand-specific risk information, and public health information on smoking. Differential advertising-responses explain 39 and 27 percent of growth in smoking's education gap among males and females, respectively, while a differential response to brand-specific tar and nicotine information explains a further 13 and 8 percent. Brand-choice analyses find an education gradient among smokers: more educated smokers favor lower risk cigarettes, prefer the more

modern high filtration brand-image, and smoke fewer cigarettes per day. These analyses suggest education differentials in demand for risk-reduction and brand-image responses.

My third paper considers the extent to which gateway effects, dual use, and harm reduction shape the relationship between youth smoking and electronic cigarette use. Using National Youth Tobacco Survey data on high school students ages 14 to 18, the analysis estimates propensities to be a current smoker absent access to electronic cigarettes, and considers the impact of changes in electronic cigarette availability on smoking rates in different propensity groups. Harm reduction is evident in the high propensity group, wherein a one percentage point increase in electronic cigarette use is associated with a 0.5 percentage point drop in the current smoking rate. There is no evidence of a gateway effect.

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Paper I: Adverse Events and Risky Behaviors: Evidence from Adolescents

Risky behaviors such as substance use are a subject of widespread concern. Costs range from adverse effects on health, education, and employment, to spillovers via drunk driving and drug-related violence (DeSimone and Wolaver, 2005; Wolaver, 2007; MacDonald and Shields, 2004; Johansson et al., 2007; DeSimone, 2002; Fryer, Heaton, Levitt, & Murphy, 2005). For youths, the long-run costs may be particularly high, as effects on ongoing brain development can shape impulse control, reward-processing, and behavioral inhibition (Clark, Thatcher, and Tapert 2008; López-Caneda et al., 2014; Wetherill et al., 2013). Despite this, 21 percent of 12 to 17 year olds have smoked cigarettes, 18 percent have used marijuana, and 17 percent have tried another illegal drug (based on 2010 data).[1]

This paper considers whether a relationship between mental distress and risky behaviors helps explain adolescent substance use, focusing on first use of cigarettes, binge drinking, marijuana, and other illegal drugs.[2] Such behaviors entail significant long run costs, but also may offer a distinctive short run benefit: a rapid shift in the user's mental or emotional state. Alcohol and sedatives can dampen/mask painful emotions; stimulants may cause feelings of euphoria; and, hallucinogens can alter one's mental state entirely. Given these effects, such substances could be used to offset immediate mental distress, as a coping response. When distress induces particularly high disutility, the ability to alleviate this, even temporarily, may outweigh a risky behavior's expected long run costs.

[1] U.S. Department of Health and Human Services, 2013

[2] Earlier first use of cigarettes, alcohol, marijuana, and certain harder drugs has been associated with involvement in more risky behaviors—in terms of number (DuRant et al., 1999) and escalation to more dangerous drugs (Agrawal et al., 2006)—as well as a greater likelihood of developing drug or alcohol dependence (Lynsky et al., 2003). Thus, understanding determinants of youths' first use of these substances is of particular interest.

The idea that risky behaviors might be used as coping devices to deal with unexpectedly adverse utility shocks is distinct from other theories of risky behavior. Both rational addiction (Becker and Murphy, 1988; Orphanides and Zervos, 1995) and time-inconsistent models (e.g., Gruber and Köszegi, 2001) explain substance use as a result of intertemporal tradeoffs between current and future utility. Yet a coping response is intratemporal: distress alters the immediate return to risky behaviors within the current period utility function. Non-rational models—often positing that certain factors alter how one makes decisions, as with shifts from deliberative to intuitive decision-making in the System 1-System 2 model (Kahneman, 2003, 2011)—do not predict the use of risky behaviors as a coping device per se, but might amplify an existing coping response (e.g., by reducing attention to less salient costs). Thus, a coping response is distinct from both rational and non-rational models, but not mutually exclusive of either.

I first show this theory's distinction conceptually. To examine the relationship between risky behaviors and mental distress empirically, I consider whether events known to precipitate distress induce changes in substance use, how these effects vary by socioeconomic status, and the extent to which behavior-change around such events is indicative of a coping response. Using data on children of the National Longitudinal Survey of Youth's 1979 cohort, I consider how first-use of four substances—cigarettes, binge drinking, marijuana, and other illegal drugs (downers, uppers, cocaine, and hallucinogens)[3]—is affected by experiencing either of two adverse events: violent crime victimization and the death of a non-family member to whom the respondent felt close. These events are selected because separate research links them to mental distress, and they are both plausibly exogenous and covered in the data.[4] I use first difference

[3] Focusing on first-use sidesteps concerns about how established addiction shapes continued drug use and escalation.

[4] Both events are included in the National Center for Post-Traumatic Stress Disorder's Life Events Checklist (LEC), a validated instrument associated with mental distress and symptoms of post-traumatic stress disorder (Gray et al.,

analyses to consider this relationship, examining the impact of current and lagged events while adjusting for potential confounders, including changes in peer pressure, neighborhood crime and violence, and proxies for substance-access. Falsification tests support the exogeneity assumption.

I find a clear statistically significant increase in first use of cigarettes and of illegal drugs other than marijuana following adverse events. Such events explain 6.7 percent of first cigarette use and 14.3 percent of first illegal drug use. Furthermore, respondents whose mothers graduated college are less likely than others to respond to such events by trying either marijuana or other illegal drugs for the first time. Finally, adverse events also affect a positive coping mechanism: days exercised per week. Respondents living in safe neighborhoods (defined based on their perception of neighborhood crime and violence) exercise more following adverse events, while others respond with a statistically insignificant decrease in exercise. Given evidence that high levels of violent crime in one's neighborhood are associated with reduced adolescent engagement in strenuous exercise, one might expect this response (e.g., if crime victimization raises the stressfulness or perceived danger of outdoor activity in unsafe areas).[5]

This combination of findings is consistent with a coping response to adverse events, but not explained by the other mechanisms considered here. The coping response theory predicts increased exercise following a negative shock, as long as that event does not raise the perceived costs of exercise. Other theories predict decreased exercise in response to adverse events. I conclude that experiencing a highly distressing event leads to risky behaviors, with a coping response contributing to this effect.

2004). Deaths are limited to non-family members to remove genetic signals and reduce the likelihood of changes in other household members' behavior (e.g., reduced parental supervision). Other LEC events are either not covered (e.g., exposure to a toxic substance, a natural distaster) or insufficiently specified in the data (e.g., "life-threatening" illness or injury), or included therein but plausibly caused by respondent substance use (e.g., car accidents).

[5] Gordon-Larsen, McMurray, and Popkin, 2000; Gomez et al., 2004

The paper is structured as follows. Section I outlines a coping response framework and examines the implications of this and other theories for the relationship between risky behaviors and exogenous adverse events. Section II describes the data. Section III presents methodology and results, with subsection A focusing on the effect of adverse events on risky behaviors, while B examines evidence that this relationship reflects a coping response. Section IV concludes.

I. Conceptual Framework

The basic challenge of understanding risky behavior boils down to a question of why one would trade off high long run costs for immediate yet transitory benefits. Unlike models that explain this choice via an intertemporal tradeoff favoring the present or a behavioral decision process (i.e., non-rational utility maximization), a coping response offers an intratemporal explanation: distressing mental states may be sufficiently harmful that the costs of not alleviating them rationalize even high-risk coping mechanisms.[6] Thus, behaviors that rapidly shift one's mental state may be particularly appealing solutions to this short-run problem.[7]

Consider individuals as having lifetime utility over a composite good, x_t, and behaviors, b_t, where the latter include consumption of addictive goods (e.g., cigarettes):

$$W_t = U(b_t, x_t, S_t; H_t) + \Sigma_s \delta^s \cdot U(b_{t+s}, x_{t+s}, S_{t+s}; H_{t+s}).$$

Lifetime utility, W_t, is the present discounted value of utility over the life-course, discounted at rate δ. Health capital, H_t, affects utility by contributing to the production of "healthy time," and is a function of one's endowment of health at birth, as well as depreciation and investment. It is also a constraint: death ensues when H falls below a critical value, H_{min} (i.e., in period N if $H_N <$

[6] The psychology literature documents reliance on coping mechanisms as a means of affect regulation, including costly ones such as self-harm (Laye-Gindhu and Schonert-Reichl, 2005; Nock, 2009).

[7] Research in psychiatry has presented evidence of substance use for this purpose among individuals with concurrent mental health and substance abuse diagnoses, under the label of "self-medication" (Khantzian, 1985, 1997).

$H_{min} < H_{N-1}$) (Grossman, 1972). Past consumption of addictive substances is reflected in one's addictive stock, S_t, which raises the marginal utility of current consumption ($\partial U_b/\partial S_t > 0$, i.e., adjacent complementarity).[8] Individuals choose b_t and x_t to maximize their present discounted value of lifetime utility, accounting for effects on health capital and life expectancy, and subject to a standard budget constraint, ($Y = P_b\, b_t + x_t$, with the price of x_t normalized to 1).

Risky behaviors constitute negative investments in future health capital ($\partial H_{t+s}/\partial b_t < 0$), but may temporarily improve immediate mental health (H_t) in the context of harmful mental distress.[9] Consider such a behavior, b, and an event, Z, which lowers utility by inducing mental distress. If b is no more harmful to future health when consumed under greater distress, increased mental distress will raise the marginal utility of b overall (i.e., $\partial U_b/\partial Z > 0$).[10] Thus, a distressing event may lead to an increase in behavior, b, particularly if its effect on the marginal utility of b (per unit cost) exceeds that on the marginal utility of other goods, x.[11] The intuition here is that mental distress may be sufficiently costly that the value of counteracting it in the short term outweighs potential long-run consequences. An extreme example would be a severely depressed person deciding between trying tranquilizers and committing suicide: to that person, the long run costs of tranquilizers are irrelevant, because there is no long run in the context of the outside option. The risky behavior prevents H_t from dropping below H_{min}. Thus, when the short run costs of inaction are severe, people may find the long run costs of behaviors that mitigate this

[8] As one's addictive stock (S_t) for a given behavior equals zero at first use, I do not discuss S_t's effects in depth.

[9] The impact of b on H_t need not carry over to future health capital stocks: it may fully depreciate by the next period.

[10] This assumption may not hold for certain substances or subgroups. For example, marijuana may pose a greater risk to the long run mental health of those predisposed to certain psychoses (Arseneault et al., 2004; Degenhardt and Hall, 2006). It is not clear whether using this substance when greatly distressed is more dangerous in such cases.

[11] While some have considered traumatic events as a shock to a consumer's level of addiction (less relevant to the first-use decision), O'Donoghue and Rabin (1999) suggest that these might induce "short term increases in the temptation to consume" addictive substances. A coping-response could drive such a shift.

immediate threat to be acceptable.

This theory yields 3 predictions: individuals should be more likely to try a costly behavior after a distressing event; use of positive coping mechanisms should increase after such events; and, more access to low cost coping devices should dampen the risky behavior response.

To consider whether a coping-response shapes substance use, the empirical analysis begins by examining whether adverse events known to cause mental distress are associated with new substance use. I show that they are. Yet increases in risky behavior following an adverse event could be explained by several other mechanisms. To pinpoint these, I apply three models of risky behavior: rational addiction, time inconsistent preferences, and non-rational frameworks.

In models of rational addiction, either the choice to become an addict is the product of rational utility maximization with perfect foresight (e.g., Becker and Murphy, 1988)[12], or the process by which one becomes addicted stems from consumption choices that are utility-maximizing given current beliefs and information (Orphanides and Zervos, 1995). In the latter model, those who do not find addiction desirable *ex ante* may still consume addictive substances (and possibly become addicts) if they underestimate their probability of becoming addicted.[13] Such individuals weigh their expected risk of addiction and associated costs against the drug's immediate benefits, and find that incurring that risk is rational given their priors.

Both of these models rely on intertemporal tradeoffs with costs realized in the future. In this context, a reduced likelihood of experiencing long run costs can incentivize risky behaviors. Adverse events might produce this effect by lowering perceived life expectancy (e.g., in response

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[12] The consumer anticipates adjacent complementarity ($\partial U_b/\partial S_t$), factoring that into his utility maximization.

[13] One's response to (consumed) addictive goods provides imperfect evidence of one's type (potential addict or non-addict), such that the perceived probability of being a potential addict is updated with use. Those who strongly believe that they are non-addicts will experiment, while the less confident abstain. However, not all potential addicts who experiment will become addicts. The more addictive stock a potential addict accumulates before recognizing his true type, the more likely he is to surpass a critical stock in the process (i.e., $S_t > S^c$) and develop an addiction.

to a violent crime). However, reduced life expectancy's effect on behavior depends on the expected cause of death. Expecting a faster depletion of health capital (e.g., a higher perceived risk of heart disease) might motivate reduced risky behavior and increased positive investments in health capital, in order to protect or extend one's length of life (Sloan, Smith, and Taylor, 2003). In contrast, if the expected cause of death is a fatal event outside the individual's control (e.g., car accident, gun violence), a younger death may be anticipated regardless. By lowering the perceived likelihood of realizing later-life costs, this would incentivize increased risky behavior.

Gruber and Köszegi (2001) incorporate time-inconsistency into a rational addiction model by allowing for hyperbolic discounting (Laibson, 1997):

$$W_t = U(b_t, x_t, S_t; H_t) + \beta\, \Sigma_s\, \delta^s \cdot U(b_{t+s}, x_{t+s}, S_{t+s}; H_{t+s}), \text{ where } \beta \in (0,1).$$

This model's policy implications differ markedly from those of Becker and Murphy (1988)[14], as it contradicts the "individuals act in their own best interest" argument for both naïve agents (those unaware of their time-inconsistency) and sophisticated agents who lack effective self-control devices. O'Donoghue and Rabin (1999) also use time-inconsistency in the context of addictive behaviors, but differently: to consider the impact of self control problems on consumption, with β reflecting the value placed on immediate gratification.

Here, adverse events could shift risky behaviors by altering the intertemporal calculation (i.e., δ or β). Stress could affect this physiologically: research on allostatic load indicates that chronic stress impacts neurons in the prefrontal cortex, an area of the brain thought to mediate delayed gratification and influence decision-making (McEwen, 2012; Casey et al., 2011). Alternatively, research indicating that self-control is a limited resource suggests that coping with stress and distress depletes self-control (Muraven and Baumeister, 2000). Thus, stressful or

[14] The two models' forward-looking behavior predictions are sufficiently similar that time-series tests of behavior-change do not distinguish the two.

distressing events may affect intertemporal calculations in a way that increases risky behavior.

Non-rational models have important relevance for risky behaviors, perhaps especially among youth. The System 1–System 2 model of cognition developed by many psychologists (e.g., Jonathan Evans, Steve Sloman, Keith Stanovich, Richard West, and others) provides a framework for understanding a variety of systematic deviations from rational choice (Kahneman 2003, 2011). Here, decision-making involves two cognitive systems: one unconscious and instinctive (System 1), the other conscious and deliberative (System 2). System 1 drives most behaviors, ranging from largely mechanical processes (e.g., breathing) to habitual actions and intuitive judgments, but is highly susceptible to accessibility (i.e., how easily different facets of and associations with the decision at hand come to mind). System 2 monitors System 1 in order to intervene when conscious, more-deliberative processing is called for. Yet using System 2 requires effort, of which individuals have a limited stock, while System 1 is effortless. Thus, if System 2 is heavily engaged on one margin, decisions along another may rely more on System 1, opening the consumer to a variety of non-rational tendencies (e.g., reference dependence).[15]

Applying the System 1–System 2 model to risky behaviors suggests that, when System 2 is otherwise engaged, substance-use decisions may overweight salient and near-term effects (i.e., those that are most accessible). If adverse events absorb effort or cause distress that increases the effort required to engage in basic activities, they would constrain System 2, increasing System 1 style decision-making. Consequent overweighting of salient and near-term outcomes alongside less attention to the long run would dampen perceived disincentives for risky behavior.[16]

[15] As the analysis focuses on events triggering first use of substance, I do not discuss non-rational models of addiction that are predicated on an already-addicted consumer (e.g., Bernheim and Rangel, 2004).

[16] Notably, such behavioral tendencies could amplify a coping response. Reference dependence might increase the perceived benefits of such behaviors in terms of reduced mental distress. Tunnelling—a consequence of scarcity involving single-minded focus on the issue/constraint at hand (Mullainathan and Shafir, 2013)—can produce a non-neutral cost-benefit calculation: costs and benefits related to the focal issue appear larger, while unrelated

Another area of research on non-rational decision making bears mentioning: the impact of incidental affect—one's emotional state at the time of decision-making, unrelated to the decision itself—on choice (Loewenstein and Lerner, 2003; Lempert and Phelps, 2014). Findings of mood congruency—good moods yield more optimistic assessments of probabilities and outcomes, while bad moods produce more pessimistic ones (e.g., Mayer et al., 1992; Wright and Bower, 1992)—suggest that adverse events might reduce risky behaviors. Other experiments find contrasting emotion-specific effects: fear induces optimistic perceptions of risk while anger induces pessimistic ones (Lerner and Keltner, 2001); sadness raises preferences for high risk/reward gambles relative to lower risk/reward options, while anxiety has the opposite effect (Raghunathan and Pham, 1999). These results do not yield a clear hypothesis for how incidental affect due to adverse events might shape risky behavior, in part because I lack information differentiating the specific emotions produced by each event, but also because it is not clear whether results from laboratory experiments with no potential for losses generalize to the context of adolescent risk-taking. Thus, I set this area aside for the remainder of this analysis.

To distinguish a coping response from the other mechanisms—changes in perceived life expectancy, time preferences, or one's decision process—I consider the relationship between adverse events and changes in a positive coping-behavior: exercise. As adolescents associate exercise with long run benefits, less weight on future states (i.e., a lower δ or β) should lower exercise.[17] A shorter life expectancy should have the same effect if one anticipates an outside

consequences appear smaller. If distress captures one's mind like scarcity (e.g., a scarcity of composure), tunneling might lead individuals to underweight longer-run or less-salient costs in pursuing an antidote to their distress.

[17] Benthin et al. (1995) examine adolescents' associations with exercise. Several positive outcomes listed by respondents are consistent with exercise serving as a coping mechanism (e.g., positive changes in affect, stress reduction). Respondents also associate exercise with improved physical health. All listed negative outcomes—pain and fatigue—seem to be short run effects, suggesting that teens view exercise as a net benefit in the long run.

cause.[18] Magnifying salient, near-term outcomes, a shift towards System 1 would be expected to reduce exercise as well, as exercise's long run net benefits suggest that, in equilibrium, short run marginal costs exceed short run marginal benefits. A coping-response, however, predicts increased exercise following an adverse event, as long as that event does not raise the perceived costs of such activity.

II. Data and Preliminary Assessments of the Adverse Event – Behavior Relationship

Empirical analyses use data from the 1979 National Longitudinal Survey of Youth (NLSY), focusing on the young adult children of women in the survey's original cohort. The latter included 6,283 females aged 14 to 22 in 1979, interviewed yearly from 1979 through 1994, and biennially thereafter, allowing me to match mothers' characteristics to their adolescent children. Surveys of female respondents' children began in 1986, and a biennial "young adult survey"—for children turning 15 or older in the interview year—was fielded from 1994-on.

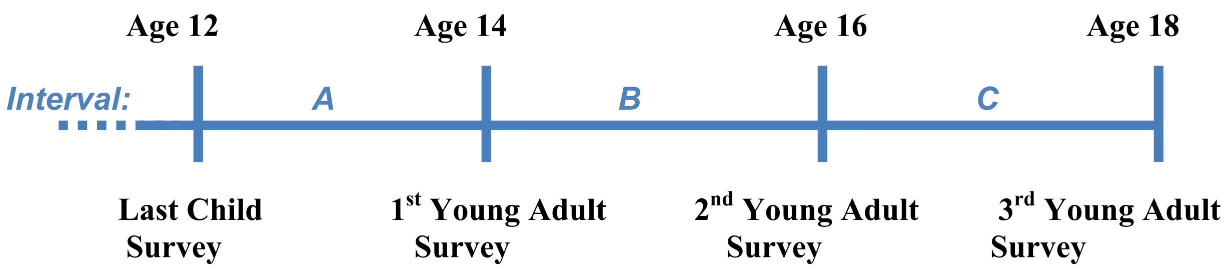
Analyses examine the behavior of young adult interview respondents—these surveys collected extensive data on substance use—between 2002 and 2010, with the sample further limited to those under age 19. The age and year restrictions are due to data limitations: young adult surveys only collected peer pressure data from 2002-onward, and only for those 18-and-under. As regressions use first differences, I further restrict the sample to respondents with at least 2 such interviews. This last requirement reduces the sample from 4691 individuals to 3099. Reassuringly, 80 percent of exclusions are due to age, not attrition: 805 individuals exceeded age 18 by their 2004 interview, while 452 had their first 2002-2010 interview in 2010.[19]

[18] A higher perceived risk of health events related to physical fitness (e.g., heart attack) may motivate exercise as an investment in future health. With non-family deaths and teen respondents, however, this driver seems less likely.

[19] Only 110 young adult respondents (2 percent of the sample) are observed once between 2002 and 2008 at an age younger than 19, and then exit (i.e., are not interviewed again by 2010, even at an age above 18).

For concreteness, Figure 1.1 presents the survey structure for a hypothetical sample member with 3 under-19 young adult interviews between 2002 and 2010. Analyses will examine how first using a substance between two young adult surveys (e.g., interval B, between the age 14 and 16 interviews) is related to experiencing an adverse event in that same interval or in the preceding one (in this example, interval A, between the age 12 and 14 interviews).[20]

Figure 1.1: Timeline for a Hypothetical Respondent's Interviews



Note: Data come from the 1979 National Longitudinal Survey of Youth's child and young adult surveys. Interviews before the year a respondent turns 15 use the "child survey" instrument, with young adult surveys used from that year forward. Thus, the youngest young adult interview age is 14. Regressions consider behaviors reported between 2002 and 2010 in the young adult data, prior to age 19. Controls use both young adult and child survey data, as well as data from surveys of the respondents' mothers.

Table 1.1 presents summary data drawn from each respondent's first post-2000 young adult interview, as well as data on family characteristics from childhood and mothers' surveys.[21] The sample is 48 percent female, 77 percent white, 15 percent black, and 7 percent Hispanic, with 98 percent enrolled in school at baseline. Family income data come from respondents' mothers' interviews during the respondent's early childhood (ages 0 to 5), as earlier surveys have considerably fewer missing income observations. Average early childhood family income is 315 percent of the federal poverty guideline.[22] Mother's highest completed education level is based

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[20] With a two year gap between interviews, I cannot distinguish between immediate and delayed behavior-responses (e.g., trying cigarettes within hours versus months of a shock). Section III discusses implications for directionality.

[21] Variables used as controls but not described in Table 1.1 include census region and urbanicity at place of residence.

[22] The Data Appendix includes more detailed descriptions of how this and other variables are generated.

on the latest observation before the respondent's first young adult interview. All but 13 percent of respondents have mothers who at least graduated high school, with 35 percent high school graduates who did not attend college, 27 percent completing some college but not graduating, and 25 percent graduating college.

Table 1.1: Respondent-Level Summary Statistics

	Full Sample (n=3099)
Number of young adult observations pre-age 19	2.3
Year	2004.6
Age	14.9
Female	48%
Enrolled in school	98%
Race/Ethnicity	
White	77%
Black	15%
Other-race	8%
Hispanic	7%
Mother's highest education-level completed	
Did not finish high school	13%
Graduated high school	35%
Completed some college	27%
Graduated college +	25%
Family Characteristics:	
Family income in childhood (R aged 0 to 5) as percent of the federal poverty guideline	315%
Total net family income, calendar year before 1st young adult interview	$69,735
≥1 Parent knows who R is with when R is not home (R age 12-14)	78%
R has sibling aged ≥ 18	49%
R has sibling aged ≥ 21	26%
Neighborhood Problems:	
Crime and violence	0.24
Parental supervision	0.41
Peer Effects:	
Peer pressure to try cigarettes	8%
Peer pressure to drink alcohol	12%
Peer pressure to use marijuana/other drugs	7%
Peer pressure to work hard in school	50%
Peer pressure to commit a crime or violence	4%

Note: Summary statistics are based on NLSY1979 data on young adult survey respondents who completed at least two surveys between 2002 and 2010 at ages younger than 19. Results are weighted with cross-interview survey weights. Unless otherwise noted, means are based on data from the respondent's first post-2000 young adult interview. Some variables have missing observations; see appendix Table A1.1. Except with income measures, means are taken with missing-observations coded as zeros.

Other family characteristics serve as proxies for substance-access: at baseline, 49 percent of respondents have a sibling who can legally purchase cigarettes (age 18-plus), and 26 percent have a sibling who can legally buy alcohol (age 21-plus). Limitations of the original cohort's substance use questions preclude a recent-maternal-use proxy for access.[23] Access may also be related to supervision. NLSY child surveys ask how often each parent—mother, father, and stepfather—knows "who you are with when you're not home." Based on data from their last child survey, 78 percent indicate at least one parent who "often" knows whom they are with.

Neighborhood factors may be related to both substance-access and risk of experiencing an adverse event (e.g., drug-related crime and violence). These are captured via respondent rankings of the degree to which certain issues are problems in their neighborhood, including "crime and violence" and "too many parents who don't supervise their children." Coding the qualitative indicators as 2 (big problem), 1 (somewhat of a problem), and 0 (not a problem or don't know), mean rankings are 0.2 for crime and violence and 0.4 for parent supervision. Thus, at baseline, most respondents do not consider these issues to be problems in their neighborhood.

Peer pressure data were collected from 2002-onward for young adult survey respondents under age 19. Specific questions ask, "Do you ever feel pressure from your friends" to "try cigarettes" (8 percent indicate yes), "drink beer, wine or liquor" (12 percent), "try marijuana or other drugs" (7 percent), "work hard in school" (50 percent), or "commit a crime or do something violent" (4 percent).[24] As peer pressure is not exogenous, its coefficient estimates may not reflect a pure peer pressure effect (e.g., biased upwards if pressure is related to drug access).

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[23] From 2000 to 2006, current smoking questions were asked with regards to smoking during recent pregnancies and to those with a history of asthma, but not more generally to the full cohort. Questions about marijuana and cocaine were specific to use during or in the 12 months before a recent pregnancy.

[24] At baseline, each peer pressure question has 81 to 89 missing observations. Means code such observations as zero.

Behavior Data

By their first post-2000 interview, many young adult respondents had used substances: 28 percent had smoked a cigarette and 46 percent consumed a full serving of alcohol, while 4 percent had, either at baseline or an earlier survey, indicated a pattern of alcohol consumption consistent with repeated binge drinking in the 30 days before interview (Table 1.2, part A).[25] In terms of illegal drugs, 17 and 2 percent of respondents had tried marijuana and uppers, respectively, with the prevalence for downers, cocaine, and hallucinogens about 1 percent each.

Person-year data on first-use—whether the respondent first tried a substance in the period since the prior interview—reflects initiation during the young adult survey period (Table 1.2, part B). Means are weighted, and taken only over those interviews at which the respondent was eligible for first use (i.e., had never used the substance as of the prior interview). First use of cigarettes, alcohol, and marijuana occur at 14, 36, and 14 percent of eligible observations, respectively. First binge drinking occurs in 6 percent of eligible interviews, with the caveat that prior binge drinking would not be noted if it did not occur in the 30 days before interview.

Data on illegal drugs other than marijuana cover sedatives (downers), stimulants (uppers and cocaine), and hallucinogens. Of these, respondents appear most likely to try stimulants (2.4 percent of person-years, as compared to 1.2 percent for downers and 0.8 percent for hallucinogens). This may be related to greater availability—Ritalin is commonly prescribed to treat attention deficit hyperactivity disorder in teens—or a differential appeal of stimulants' pharmacological effects (e.g., to enhance productivity with school-work, improve mood, etc.).

[25] Binge-drinking is deduced from data on drinks-per-day-drank in the 30 days before interview. Those citing mean consumption of 5 or more drinks per day drank are coded as having engaged in binge drinking. Averaging over multiple drinking days, this variable indicates repeated binge drinking, but may not catch those who binge drank once or twice but usually consumed less alcohol. It may be biased downward by underestimation of serving sizes.

Table 1.2: Adverse Events & Substance-Use Summary Statistics		
A. Respondent-Level Data at Baseline	Mean	Count
Childhood Adverse Events		
Death of non-family member R was close to	2.0%	3053
Victim of a violent crime	1.7%	3036
Either shock	3.6%	3035
Ever-Used Substance:		
Cigarettes	28.0%	3099
Full serving of alcohol	46.1%	3087
Binge drinking	4.1%	2888
Marijuana	16.6%	3099
Downers	0.7%	3098
Cocaine	1.0%	3099
Uppers	1.9%	3099
Hallucinogens	0.6%	3099
B. Person-Year Data	Mean	Count
Adverse Events since Last Interview		
Victim of violent crime	1.8%	6896
Death of non-family member R was close to	4.9%	6921
Either shock since last interview	6.5%	6896
1st Use Occurred since Last Interview		
Cigarettes	13.8%	5237
Full serving of alcohol	36.0%	4331
Binge drinking	6.3%	6484
Marijuana	13.8%	5905
Downers	1.2%	6948
Stimulants (uppers or cocaine)	2.4%	6897
Hallucinogens	0.8%	6941
Illegal drug besides marijuana	2.5%	6877
Number of Days Exercised / Week		
Strenuous exercise > 15 min (2008-2010 data)	3.4	2407

Note: These use NLSY1979 young adult survey data on respondents who completed at least two such surveys before age 19 (3099 respondents, 7017 person-years in total). Means are weighted using 2002-2010 cross-interview survey weights. "Baseline" observations refer to those at the respondent's first post-2000 young adult interview. "Childhood Adverse Events" are those that occurred prior to the last child-survey interview. First-use data is out of incident cases, such that counts reflect behavior when an individual was eligible for first use (i.e., had never used the substance as of the prior interview). The 2008 to 2010 data include 2408 person-years.

Exercise data allow for consideration of a positive coping mechanism. The NLSY began collecting such information in 2004, but changed the question in 2008. This analysis uses data on the 2008 to 2010 survey question—the average number of days per week a respondent engaged in “strenuous exercise for more than 15 minutes during free time”—as this focuses on free-time behaviors, suggesting that externally-imposed exercise (e.g., required by a school gym class) would not qualify.[26] These data yield 2407 observations with a mean of 3.4 days per week.[27]

Adverse Events

The adverse events considered here are selected based on the National Center for Post-Traumatic Stress Disorder’s Life Events Checklist (LEC), an instrument validated as reflecting mental distress and associated with symptoms of post-traumatic stress disorder (Gray et al., 2004). The NLSY includes data on two plausibly exogenous LEC events: having been the victim of a violent crime—arson, physical assault, sexual assault, or robbery—and the death of someone the respondent felt close to. For the latter event, analyses restrict consideration to non-family deaths, as deaths in the family may be anticipated, affect the behavior of others in the respondent’s household (e.g., supervision), or convey family-specific risk information.

Considering all shocks before age 19, 6 percent of respondents were victims of a violent crime while 11 percent had lost a non-family member they felt close to. These correspond to 176 and 342 individuals, respectively, including 35 people who experienced both events. Shock data are missing for 56 respondents (1.8 percent).[28] Of course, identifying variation stems from shocks during the period of analysis: 15 percent of respondents (474 people) report one or both of these events at a young adult interview prior to age 19 (Figure 1.2); 54 respondents cannot be classified due to missing data (1.74 percent). This corresponds to an adverse event incidence of

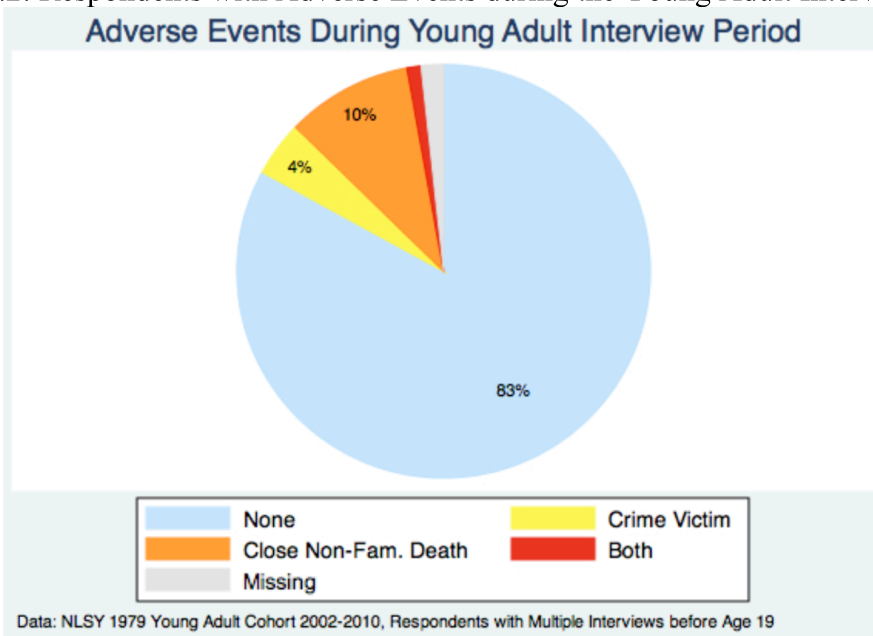
[26] The 2004 and 2006 survey questions ask about “exercise that lasts 30 minutes or more.”

[27] The sample includes 2408 person-year observations from the 2008 and 2010 surveys.

[28] Missing shock-data involves a respondent who either lacks data on both shocks, or is missing data on one shock while the other is coded as a 0 (such that he or she cannot be placed in the neither-shock category with certainty).

6.5 percent (of interviews) over the young adult interview period (Table 1.2, part B).[29]

Figure 1.2: Respondents with Adverse Events during the Young Adult Interview Period



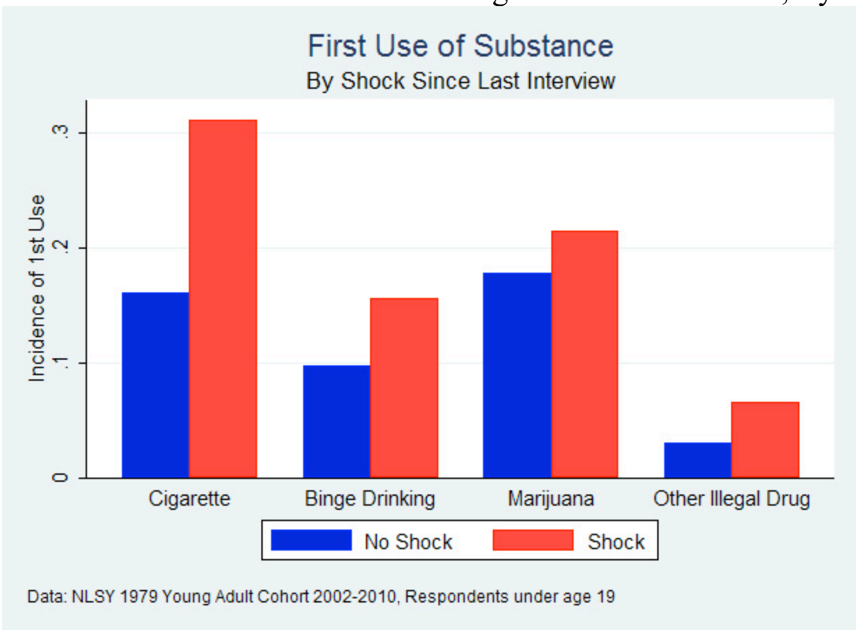
Notes: The "Young Adult Interview Period" refers to surveys completed the year a respondent turned 15 or older. Adverse events in the young adult interview period are those occurring after the respondent's last childhood interview (i.e., in Figure 1.1's interval A or later, such that the first survey completed post-shock was a Young Adult Interview). All events considered here occur prior to age 19.

As Figure 1.3 shows, first use of each substance is noticeably higher in periods with an adverse event. Relative to periods without a shock, this incidence is 92 percent larger for first cigarette use, 60 percent larger for first binge drinking, 20 percent larger for first marijuana use, and 115 percent larger for first illegal drug use. Figure 1.4 considers this comparison separately for those whose mothers did and did not graduate college. For cigarettes and binge drinking, these education groups show similar patterns: in both cases, first use is much higher in periods with an adverse event, particularly for cigarettes. Yet, for illegal drugs, the groups' patterns differ. For those whose mothers did not graduate college, shocks are associated with greater first use of marijuana (18 percent with a shock versus 15 percent without one) and of other illegal drugs (8 versus 3 percent). Yet children of college graduates show almost no difference in first

[29] Events are in the "young adult interview period" if one's first post-event interview is a young adult survey.

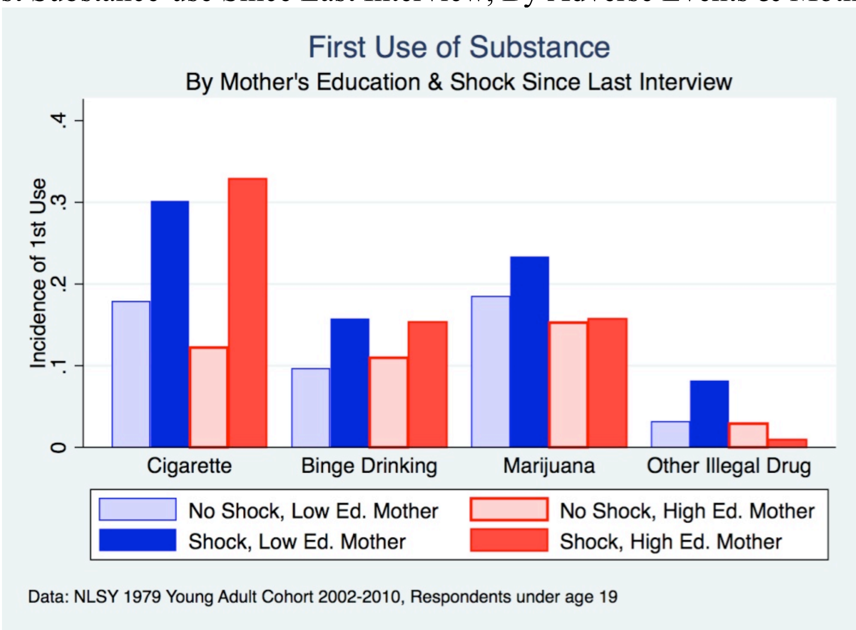
marijuana use by shock (15.7 percent with a shock versus 15.2 percent without one) and lower first use of other illegal drugs in periods with an adverse event (1 versus 3 percent).

Figure 1.3: Incidence of First Substance-use Occuring Since Last Interview, By Adverse Events



Notes: This figure depicts the incidence of first substance use—percent who first tried the drug since their prior interview out of those who had not previously used it—by whether the respondent experienced an adverse event—crime victimization or death of a non-family member they felt close to—in that interval.

Figure 1.4: First Substance-use Since Last Interview, By Adverse Events & Mother's Education



Notes: This figure depicts the incidence of first substance use—percent who first tried the drug since their prior interview, out of those who had not previously used it—by whether (a) one experienced an adverse event in that interval, and (b) one's mother graduated college ("High Ed.") or not ("Low Ed.").

Drawing causal inferences from such comparisons would require that the adverse events be exogenous to the behaviors. This assumption is violated if, *ex ante*, individuals who experience shocks differ from those who do not in a manner related to their likelihood of engaging in risky behaviors. Essentially, this is a "type" story: if teens of one type are both more likely to experience adverse events and to engage in risky behaviors, adverse events will be associated with greater risk-taking (and vice versa), even without a causal relationship. In regressions controlling for observable differences, this introduces confounding if "type" is unobserved. Before proceeding with regression analyses, the next section will consider a falsification test tailored to this concern: conditional on observables, do next period shocks predict current period behaviors?

III. Methods and Results

The analysis proceeds in two parts. First, I consider whether adolescents respond to adverse events with increased involvement in risky behaviors. Second, I examine whether the shock-behavior relationship demonstrated here reflects a coping response. The methods and results for each section are described in turn.

A. Do Individuals Increase Risky Behaviors in Response to Adverse Events?

Focusing on the relationship between distressing events and shifts in behavior suggests a first difference analysis as the natural regression model:

$$\Delta B_{it} = \beta_1 \cdot \Delta Shock_{it} + \beta_2 \cdot \Delta Shock_{i,t-1} + \lambda \cdot X_{it} + \gamma_t + \varepsilon_{it}. \quad (1)$$

The subscript t refers to a biennial survey, such that 1 unit of t corresponds to a two-year gap. The dependent variable, ΔB_{it}, captures changes in behavior (B_{it} - $B_{i,t-1}$). Most analyses focus on first-use, with B_{it} a dummy variable for whether respondent i tried behavior B for the first time between interviews t-1 and t. Similarly, $Shock_{it}$ is a binary indicator for "recent shocks," equal to one if an adverse event occurred since the prior interview (i.e., between t-1 and t), whereas $Shock_{i,t-1}$ signifies "lagged shocks" (equal to one if a shock occurred between t-2 and t-1).[30] The corresponding independent variables are specified as changes: $\Delta Shock_{it} = Shock_{it} - Shock_{i,t-1}$, $\Delta Shock_{i,t-1} = Shock_{i,t-1} - Shock_{i,t-2}$. Thinking of adverse events as triggering lasting mental distress, these change-in-shock variables indicate whether the period since the last interview was marked by greater distress than the period before that. This specification is preferable to a differenced ever-shock variable, as the latter would not capture later events among those with multiple-shocks.[31] It also allows the shock-behavior relationship to change with time: β_1 captures the recent shock effect, while the long run effect is the linear combination, $\beta_2 - \beta_1$ (because a change in lagged shocks equal to 1 induces a change in recent shocks equal to -1). For clarity, regression tables will present shock effects in a separate section, below coefficient estimates.

The remaining terms include controls, specified as either changes or levels (X_{it}), and survey-year fixed effects (γ_t). Controls specified as changes include binary indicators for immediate- and extended-family deaths, geographic factors (census region, urbanicity), peer pressure (to work hard in school, to commit a crime or violence, and to use the substance under consideration), and proxies for substance-access (e.g., having a sibling aged 18-plus or, for drinking behaviors, aged 21-plus), as well as respondent-rankings of their neighborhood (with respect to parent supervision, and to crime and violence), the interview date in months—as changes, this controls for the number of months since last interview—and age-attained indicators

[30] For example, if t corresponds to the age-16 interview in Figure 1, then $Shock_{it}$ equals one if an adverse event occurred in interval B, whereas $Shock_{i,t-1}$ equals one if an adverse event occurred in interval A.

[31] Concerns about differencing between two consecutive periods with adverse events are minimal, as only 35 respondents experience multiple shocks before age 19, and not necessarily in consecutive periods. Differencing a total-shocks-ever variable instead is not an option, as timing data on crime victimizations are not reported for every incident (only the first and most-recent victimizations).

for ages 16, 17, and 18 (e.g., age 16-plus). As certain time-invariant factors may influence the evolution of behavior over time, I also include level-effects for demographic traits (gender, race, Hispanic ethnicity), SES proxies (mother's education, early-childhood family income), perceived parental supervision at the last childhood interview (i.e., Figure 1's age 12 interview), and childhood adverse events (i.e., shocks occurring prior to one's last childhood-interview).

Survey-year fixed effects account for time trends in prices, as well as access to or information about various substances. While prices may vary geographically (particularly due to variation in state tax rates), it is not clear why these would bias the estimated response to adverse events in a first difference analysis.[32] Absent state identifiers, I specify census region controls as changes to help capture shifts in tax rates related to moving (since state tax rates exhibit regional similarities). Substance-access outside of formal purchasing may also vary by individual, particularly with age. The attained-age indicators noted above help control for this.

First-use analyses sidestep questions about the impact of pre-existing addiction on continued-use. As first-use of a given substance can only occur once, these regressions drop respondents post-initiation, yielding a binary dependent variable. They are evaluated via logistic regression. Results present both odds ratios and, for interpretability, average marginal effects.[33] Standard errors are clustered at the unit of survey randomization: mother's 1979 household.

To address concerns that unobserved factors may be associated with both new substance use and the likelihood of experiencing an adverse event, I consider a falsification test: are those who will undergo a shock in the next period more likely to try a substance now? Replacing the baseline model's recent and lagged shocks with a change-in-future-shock term—ΔB_{it} =

[32] An upward bias would require shocks to be more common in states and years with fewer or lower price increases.

[33] Marginal effects for interaction terms are estimated as if the interaction-term is unrelated to the relevant freestanding variables (i.e., not as a derivative taken with respect to each element of a factor variable).

$\beta_1 \cdot (Shock_{i,t+1} - Shock_{it}) + \lambda \cdot X_{it} + \gamma_t + \varepsilon_{it}$—$\beta_1$ indicates whether, conditional on controls, a future-period shock predicts current initiation among those with no adverse event in the prior period.

This specification presents a practical problem: it requires three observations per individual (t-1, t, and t+1, a prerequisite met by only 819 sample-members) and limits outcomes to first-use observed at the second survey, meaning that substances people tend to try at later ages are not as well represented. To address this, I include adverse event data reported at ages 19 and 20 in defining future shocks ($Shock_{i,t+1}$). This yields 1959 observations[34] for first cigarette, 2395 for first binge drinking, 2225 for first marijuana, and 2662 for first other illegal drug.

Table 1.3: Falsification Test: Is First-Use Predicted by Future Shocks? Odds Ratio/(t-stat)

	1st Cigarette	1st Binge Drinking	1st Marijuana	1st Illegal Drug
Independent Variables	(1)	(2)	(3)	(4)
Δ Shock in future	0.763	0.746	1.156	1.093
	(-0.90)	(-1.16)	(0.61)	(0.18)
N	1959	2395	2225	2662
Mean(Δ Behavior)	0.178	0.093	0.189	0.037

Note: Illegal drugs (in the final specification) include downers, uppers, cocaine, and hallucinogens. These results are from logistic regressions evaluating the following model: $B_{i,t} - B_{i,t-1} = \beta_1 \cdot (Shock_{i,t+1} - Shock_{i,t}) + \lambda \cdot X_{it} + \gamma_t + \varepsilon_{it}$. B_{it} is a binary indicator for whether first-use of a given substance occurred between interview t and the prior survey, and is dropped from the regression once a respondent is ineligible for initiation (i.e., had used that substance before as of the prior interview). $Shock_{i,t+1}$ indicates whether an adverse event occurred between t+1 and t. Additional controls (X_{it} and γ_t) are as follows: Controls Specified As-Changes: death in immediate-family since last interview, death in extended-family since last interview, sibling aged-18-plus (21-plus for binge drinking), geographic controls (census region, urbanicity), age-indicators (attained-age 16, 17, 18), neighborhood rank (crime and violence; parent supervision), interview date in-months (i.e., ΔDate equals months since past interview), & peer pressure (use the substance in question; work hard in school; commit a crime or violence). Substance-specific peer pressure variables are pressure to try cigarettes in the cigarette regression, to drink alcohol in the binge drinking analysis, and to use marijuana or other drugs in the marijuana and other illegal drug analyses. Level-Effect Controls: Fixed effects for survey-year, sex, race (plus missing race), ethnicity, childhood adverse event (plus missing-observation indicator), perceived parental supervision at latest childhood survey, mother's education, and family income in early childhood as percent of the federal poverty line (plus missing-observation indicator). Current and lagged missing-data indicators: family-death since last interview, peer pressure, neighborhood, & geographic controls. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

[34] That is, individuals with data from 2 surveys in the under-19 sample as well as a future shock observation. Counts vary by substance because first-use regressions omit those who have already tried the substance in question.

Table 1.3 presents the falsification test results. In all cases, the change-in-future-shock effect is statistically insignificant, with odds ratios of 0.76 for cigarettes, 0.75 for binge drinking, 1.16 for marijuana, and 1.09 for other illegal drugs. These findings alleviate concerns that shocks may be associated with unobserved factors related to substance use initiation.

Returning to the equation 1 specification, interpreting β as the causal effect of prior adverse events on behavior still faces two key threats to validity. The first is directionality. For recent shocks, these regressions identify a relationship between adverse events experienced between t-1 and t, and changes in behavior in that same period. While the adverse event occurred since the prior interview, it is not clear exactly when it occurred relative to first substance use. This is less of an issue for certain behaviors: binge-drinking and most dose behaviors are coded based on the 30 days before interview[35], such that shocks occurring since the prior interview most likely preceded this measurement period. Remaining directionality concerns are addressed by examining the impact of lagged shocks, and controlling for factors that might induce behavior-change (e.g., proxies for changes in access, such as a sibling reaching the legal purchasing age or a change in one's neighborhood's rank with respect to parental supervision).[36]

The second threat to validity involves third-factor drivers: outside events that might raise the likelihood of both experiencing a shock and involvement in risky behaviors. Two types of potential confounders stand out here: changes in neighborhood factors (either in crime/violence or in parental supervision, both potentially related to access and shock-risk), and changes in peer effects (e.g., if befriending a high-risk peer group raises pressure to engage in substance use and

[35] E.g., "How many drinks did you usually have in a day on the days that you drank during the past 30 days?"

[36] A particular reverse causation concern is that substance use might raise one's risk of experiencing an adverse event (e.g., being robbed by one's drug dealer). This argument is more plausible for marijuana and illegal drug use than cigarettes or binge drinking. However, regressions indicate a long run shock effect on first illegal drug use: the clear directionality contradicts a reverse causation argument.

likelihood of experiencing an adverse event). To address these concerns, regressions control for changes in the respondent's ranking of his or her neighborhood in terms of crime and violence, and in terms of parental supervision, as well as changes in peer pressure to use the substance in question (i.e., to try cigarettes, to drink alcohol, or to use marijuana or other drugs).

A second specification further addresses concerns that differential incidence of adverse events by substance-access or neighborhood factors might drive the estimated shock effects. Expanding the baseline specification, equation 2 adds eight terms: four interacting the change in recent shocks with lagged proxies for access (having a sibling aged 18-plus, peer pressure to use the substance in question) and neighborhood-problem rankings (crime and violence (NeighCV), lack of parental supervision (NeighSup)), plus a stand-alone control for each lagged variable:

$$\begin{aligned}\Delta B_{it} = {} & \beta_1\cdot\Delta Shock_{it} + \beta_2\cdot\Delta Shock_{i,t-1} + \beta_3\cdot\Delta Shock_{it}\cdot Sibling18plus_{i,t-1} + \beta_4\cdot Sibling18plus_{i,t-1} + \\ & \beta_5\cdot\Delta Shock_{it}\cdot PeerPressure_{i,t-1} + \beta_6\cdot PeerPressure_{i,t-1} + \beta_7\cdot\Delta Shock_{it}\cdot NeighCV_{i,t-1} + \\ & \beta_8\cdot NeighCV_{i,t-1} + \beta_9\cdot\Delta Shock_{it}\cdot NeighSup_{i,t-1} + \beta_{10}\cdot NeighSup_{i,t-1} + \lambda\cdot X_{it} + \gamma_t + \varepsilon_{it}. \qquad (2)\end{aligned}$$

A statistically significant β_1 indicates a shock-behavior relationship distinct from any related differential in pre-shock peer pressure, potential access via a sibling of legal purchasing age, neighborhood crime and violence, or neighborhood parental supervision.

Notably, even if adverse events lead to increased substance use, an intermediate factor might drive this change. In particular, a shock could facilitate access to certain drugs (e.g., prescribed sedatives one might use as downers) or prompt a change in peer-pressure (either within the same peer group or via movement to a new peer group). The first scenario does not apply to analyses of non-prescribed substances (e.g., cigarettes, alcohol). I address the second by adding a term to equation 1, interacting changes in recent shocks with changes in peer pressure:

$$\Delta B_{it} = \beta_1\cdot\Delta Shock_{it} + \beta_2\cdot\Delta Shock_{i,t-1} + \beta_3\cdot\Delta Shock_{it}\cdot\Delta Pressure_{it} + \lambda\cdot X_{it} + \gamma_t + \varepsilon_{it}. \qquad (3)$$

A statistically significant β_1 indicates an effect of adverse events on substance use distinct from any shock to peer pressure relationship.

Analyses of these specification checks focus on first cigarette use. An adverse event can only trigger first use of a substance if the consumer is able to access that drug, and cigarettes are easily accessible relative to the other drugs considered here. For first use, they are also fairly inexpensive if not free. Thus, analyses of first cigarette use seem the most likely to exhibit a shock-effect, if it exists.

Table 1.4 examines first cigarette use. The first specification is the baseline analysis described in equation (1). New shocks are associated with a statistically significant 12 percentage point increase in one's likelihood of trying cigarettes for the first time (OR=2.7), with the long run shock effect a statistically insignificant 6 percentage point increase (OR=1.6). Thus, the effect of adverse shocks on first cigarette use declines over time, as one might expect.[37] Mother's education has the expected effect, with children of college graduate mothers less likely to try cigarettes (a 7 percentage point drop).[38] To address the possibility that peer effects may drive both behaviors and adverse events, this regression controls for changes in peer pressure to try cigarettes (associated with a 13 percentage point increase in probability of first use), to work hard in school (a statistically significant 3 percentage point decrease), and to commit a crime or violence (a statistically insignificant 3 percentage point increase). Access is another potential third factor, if it is related to shock-risk. Controls for changes in neighborhood rankings of crime and violence (yielding a statistically insignificant 0.6 percentage point increase) and parents'

[37] If risky behaviors are a coping response to distress, one might expect larger responses to more recent shocks. Yet the events considered here can be particularly traumatic, and have been linked to post-traumatic stress disorder. If such shocks elicit lasting distress, long run effects on behavior are plausible, though they might be more evident with illicit drugs (i.e., if individuals try less costly coping devices first, or if illicit drugs take more time to acquire).

[38] Childhood family income is not listed in Table 1.4. It has a statistically significant zero effect in all specifications.

supervision of their children (a statistically significant 3 percentage point increase) address this.

The recent-shock effect remains positive and statistically significant despite these controls, suggesting that the relationship between first cigarette use and adverse events does not stem from access or peer effects acting as a third factor driver. Further, the effect is large: adverse events explain 6.7 percent of first cigarette use.

Column 2 considers whether this result is explained by differentials in access or neighborhood effects among those who experience adverse events. It includes a series of terms interacting the change in recent shocks with either lagged proxies for substance access (including peer pressure) or lagged neighborhood characteristics. Each of these interaction terms is statistically insignificant, with negative effects estimated in all but one case (where the odds ratio is 1.0). Concurrently, the impact of a recent shock is statistically significant and larger than in the baseline regression, now indicating a 14 percentage point increase in probability of first cigarette use (OR=3.2). Thus, the effect of recent adverse events does not seem to stem from differentials in access, peer pressure, or neighborhood characteristics among those experiencing such shocks.

The final column of Table 1.4 considers whether peer-pressure acts as an intermediary driving the relationship. That is, do adverse events lead to a shift in peer pressure that, in turn, drives changes in substance use? To test this, I add an additional term to the baseline equation: an interaction between the change in recent shocks and change in peer pressure to use cigarettes. This term's estimated effect is statistically insignificant and negative (OR=0.4), contradicting the intermediary driver hypothesis. The recent shock effect remains statistically significant and matches its baseline magnitude: a 12 percentage point increase in probability of first use.

.

Table 1.4: Effect of Adverse Events on 1st Cigarette Use

	Baseline		Access & Neighborhood Factors		Peer Pressure Driver	
	(1)		(2)		(3)	
Parameter Estimates	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*
Δ Shock since last interview	2.685*** (3.89)	*12.2%*	3.245*** (2.97)	*14.0%*	2.711*** (3.97)	*12.3%*
Lag: Δ Shock since last interview	4.361*** (3.14)	*18.2%*	3.674*** (2.68)	*15.5%*	4.187*** (2.96)	*17.7%*
Δ Peer pressure to try cigarettes	2.829*** (3.14)	*12.9%*	5.685*** (6.39)	*20.7%*	2.967*** (3.18)	*13.4%*
Mother graduated college	0.544** (-2.47)	*-7.4%*	0.559** (-2.25)	*-6.7%*	0.544** (-2.47)	*-7.4%*
Δ Shock since last interview $\cdot$ Peer pressure$_{t-1}$			0.732 (-0.40)	*-3.7%*		
Δ Shock since last interview $\cdot$ Sibling age 18+$_{t-1}$			0.779 (-0.55)	*-3.0%*		
Δ Shock since last interview $\cdot$ Neighborhood crime/violence$_{t-1}$			0.799 (-0.51)	*-2.7%*		
Δ Shock since last interview $\cdot$ Neighborhood supervision$_{t-1}$			1.027 (0.08)	*0.3%*		
Δ Shock since last interview $\cdot$ Δ Peer pressure					0.372 (-1.13)	*-12.2%*
N	2411		2411		2411	
Shock Effects						
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	*12.2%**** (3.89)		*14.0%**** (2.97)		*12.3%**** (3.97)	
Long-run ($\beta_{\text{Lag}(\Delta\text{Shock})} - \beta_{\Delta\text{Shock}}$)	*6.0%* (1.23)		*1.5%* (0.21)		*5.4%* (1.04)	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates, as AMEs. Additional controls are described in the note to Table 3. Column 2 also controls for the level effect in recent-shock interaction terms (lagged peer pressure, lagged sibling-age-18+, and lagged neighborhood ranks for crime/violence & parental supervision). Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

Overall, the results indicate an increase in first cigarette use following an adverse event, not explained by proxies for differential access or neighborhood risk factors among affected respondents, and not attributable to changes in peer pressure acting as an intermediary driver.

Table 1.5 presents the baseline specifications for first binge drinking, marijuana, and other illegal drug use, where the latter includes downers, uppers, cocaine, and hallucinogens.[39] For both binge drinking and marijuana, recent shock effects are positive but statistically insignificant, indicating a 3 and 5 percentage point increase in the probability of first use, respectively. Based on coefficient estimates, adverse events explain 3 percent of first binge drinking and 1 percent of first marijuana use. With respect to first binge drinking, peer pressure to drink alcohol also explains little, associated with only a statistically insignificant 1 percentage point increase in first use. This is perhaps unsurprising, though, given the difference between drinking any alcohol and binge drinking. For marijuana, peer pressure appears to exert substantial influence: new pressure to use marijuana or other drugs is associated with a 19 percentage point increase in probability of first use (OR=4.3, p-value < 0.01). Such a large effect is consistent with peer pressure variables capturing a variety of factors, such as greater access to a difficult-to-acquire drug, or a tendency to cite peer pressure as an *ex post* justification for substance use.[40]

[39] Tests of the peer-pressure mechanism for other behaviors are included in appendix Table A3, with a caveat: due to prices (especially for binge drinking) and the illegality of certain substances, access-restrictions are expected to be more influential on first-use here than with cigarettes. If lack of access suppresses consumption and increased peer-pressure reflects greater access, these specifications will underestimate the effect of adverse events on demand.

[40] The latter tendency would bias coefficients on other predictors of substance use towards zero.

Table 1.5: First-Use Analyses for Binge Drinking, Marijuana, and other Illegal Drugs

	1st Binge Drinking		1st Use of Marijuana		1st Other Illegal Drug	
	(1)		(2)		(3)	
Parameter Estimates	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*
Δ Shock since last interview	1.427 (1.39)	*3.0%*	1.430 (1.46)	*4.6%*	2.042 (1.64)	*2.2%*
Lag: Δ Shock since last interview	1.911 (1.30)	*5.5%*	0.982 (-0.04)	*-0.2%*	8.683*** (3.52)	*6.6%*
Δ Peer Pressure to use substance	1.150 (0.83)	*1.2%*	4.297*** (6.15)	*18.8%*	1.365 (0.56)	*1.0%*
Mother graduated college	1.813* (1.86)	*4.5%*	0.764 (-1.14)	*-3.4%*	0.501* (-1.67)	*-2.3%*
N	2962		2732		3327	
Mean(Δ Behavior)	0.082		0.167		0.031	
Shock Effects						
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	*3.0%* *(1.39)*		*4.6%* *(1.46)*		*2.2%* *(1.64)*	
Long-run ($\beta_{\text{Lag}(\Delta\text{Shock})}$ - $\beta_{\Delta\text{Shock}}$)	*2.5%* (0.65)		*-4.9%* (-0.80)		*4.4%**** (2.92)	
Share of first use attributable to adverse events:	3.0%		1.1%		14.3%	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates (as AMEs), as well as the percent of first-use attributable to shocks (estimated using regression coefficients to predict first use with the observed data, and again with recent and lagged shocks coded as zero). Other illegal drugs include downers, uppers, cocaine, and hallucinogens.
Additional controls are described in the note to Table 3. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

With illegal drugs other than marijuana, adverse events show a statistically significant long run effect, raising the probability of first illegal drug use by 4 percentage points. Notably, this addresses directionality concerns, as lagged shocks explicitly predate initiation. Peer pressure to use marijuana or other drugs exhibits a statistically insignificant effect (a 1 percentage point increase in probability of first use). The share of first illegal drug use attributable to adverse events is 14 percent.

Overall, the first-use regression results from Tables 4 and 5 reveal a statistically significant relationship between adverse events and first-use of two types of substances: cigarettes, and illegal drugs other than marijuana.

Alongside an SES gap in the incidence of traumatic events, this relationship may help explain some of the SES gap in substance use. Respondents whose mothers graduated college are 5 percentage points less likely to try cigarettes and 1 percentage point less likely to try an illegal drug other than marijuana. Using the baseline regression coefficients and shock incidence by subgroup, the shock-differential's contribution to the SES gap can be estimated.

However, the gaps in adverse-event incidence across SES groups are quite small. For recent shocks, the gap between those whose mothers did and did not finish college is 1.5 percentage points, while the gap between those above and below the median income is only 0.2 percentage points. Consequently, a substance use response to differentially distributed shocks explains little of the SES gap in these behaviors. Indeed, the differential distribution of adverse events between those whose mothers are and are not college graduates explains 0.2 percentage points of the 5.4 percentage point gap in first cigarette use, and less than 0.1 percentage points of the gaps in first use of other substances (See Table 1.6). The gap between those with above- and below-median family income in childhood is under 0.02 percentage points in all cases.

Table 1.6: SES Gaps in First Use: Percentage Point Gaps Observed & Attributable to Differentials in Shock Incidence				
	1st Cigarette	1st Binge Drinking	1st Use of Marijuana	1st Other Illegal Drug
A. Gaps By Mother's Education: College Grad. vs. Not				
Observed Gap	5.36%	-1.05%	3.60%	0.94%
Gap Attributable to Differential Shock-Incidence	0.18%	0.04%	0.07%	0.03%
B. Gaps By Childhood Family Income: ≤ vs. > median				
Observed Gap	0.90%	-2.86%	-0.23%	-0.03%
Gap Attributable to Differential Shock-Incidence:	0.019%	0.005%	0.007%	0.003%

Note: The gap in adverse event incidence between respondents whose mothers did and did not graduate college ($\text{Shock}_{\text{Low Ed}} - \text{Shock}_{\text{High Ed}}$) equals 1.47 percentage points for recent shocks, and 0.35 for lagged shocks. Comparing respondents whose childhood family incomes (measured as percent of the federal poverty guideline) were above versus below the median yields corresponding incidence gaps of 0.16 and 1.11 percentage points. This table's estimates of the percentage point gap in first use that can be attributed to differential shock-incidence are estimated via recent shock data and coefficients from the baseline regressions, as follows: $\text{AME}_{\Delta\text{Shock Since Last Interview}} \cdot [\Pr(\text{Shock})_{\text{Low-SES Proxy}} - \Pr(\text{Shock})_{\text{High-SES Proxy}}]$. Note that the observed gaps in binge drinking are negative. This is consistent with the literature, which generally finds more binge drinking among higher income groups.

An SES differential might also stem from differential responsiveness to adverse events, if SES modifies the strength of the shock-to-behavior relationship (e.g., if high-SES teens have access to more effective low-risk coping mechanisms). This can be tested by interacting the shock variables with a proxy for high-SES.

Table 1.7 considers this using whether the respondent's mother graduated college as a high-SES proxy. High-SES respondents' recent and long-run shock effects are statistically insignificant for both first cigarette use and first binge drinking. The results for first marijuana use are somewhat more nuanced: the high-SES long run shock effect is statistically significant and large, indicating a 24 percentage point drop in the probability of first marijuana use relative to the full sample long run effect (a statistically insignificant decrease of 1 percentage point). Recent shock effects on first illegal drug use have similar implications: while the overall recent shock effect is a 3 percentage point increase in probability of first use, those whose mothers graduated college exhibit a relative reduction of 8 percentage points (both statistically significant). Thus, it seems that children of higher education mothers are less likely to respond to adverse events with first use of either marijuana or other illegal drugs.

Table 1.7: Differential First-Use-Responses to Adverse Events by Mother's Education

	1st Cigarette		1st Binge Drinking		1st Marijuana		1st Other Illegal Drug	
	(1)		(2)		(3)		(4)	
Parameter Estimates	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*
Δ Shock since last interview	2.271*** (2.78)	*10.1%*	1.503 (1.40)	*3.5%*	1.502 (1.40)	*5.3%*	2.665** (2.19)	*3.0%*
Lag: ΔShock since last interview	4.523*** (2.88)	*18.6%*	2.445 (1.49)	*7.6%*	1.363 (0.53)	*4.0%*	8.222*** (2.93)	*6.4%*
Mother graduated college	0.533** (-2.47)	*-7.6%*	1.899** (1.99)	*4.9%*	0.795 (-0.96)	*-2.9%*	0.517 (-1.55)	*-2.2%*
Δ Shock since last interview · Mother graduated college	1.764 (1.05)	*7.0%*	0.772 (-0.45)	*-2.2%*	0.780 (-0.47)	*-3.2%*	0.066** (-2.35)	*-8.3%*
Lag: ΔShock since last interview · Mother graduated college	0.547 (-0.61)	*-7.4%*	0.342 (-1.19)	*-9.1%*	0.126** (-2.24)	*-26.7%*	0.221 (-1.05)	*-4.6%*
N	2411		2962		2732		3327	
Mean(Δ Behavior)	0.160		0.082		0.167		0.031	
Shock Effects								
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	*10.1%**** *(2.78)*		*3.5%* *(1.40)*		*5.3%* *(1.40)*		*3.0%**** *(2.19)*	
Added recent shock effect if mother graduated college	*7.0%* *(1.05)*		*-2.2%* *(-0.45)*		*-3.2%* *(-0.47)*		*-8.3%*** *(-2.35)*	
Long-run ($\beta_{Lag(\Delta Shock)}$ - $\beta_{\Delta Shock}$)	*8.5%* (1.55)		*4.1%* (0.90)		*-1.3%* (-0.18)		*3.4%** (1.87)	
Added long-run shock effect if mother graduated college	*-14.5%* (-1.42)		*-7.0 %* (-0.96)		*-23.5%*** (-2.17)		*3.7%* (1.26)	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates (as AMEs). Additional controls are described in the note to Table 3. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

Table 1.8 describes results for a similar regression using above-median childhood family income (253 percent of the federal poverty guideline) as a high-SES proxy. For every substance, the high-SES response to recent and lagged shocks is statistically insignificant. One possible explanation is that the above-median-income proxy includes too many middle-income youths, obscuring a differential response. To address this, I repeat the analysis with a 75th percentile income cutoff (372 percent of the federal poverty guideline). In all cases, high-SES responses to both recent and lagged shocks are statistically insignificant (Appendix Table A1.3). This suggests that family income in early childhood and one's mother having graduated college capture different elements of socioeconomic status, at least as they affect harmful behaviors.

While the analyses focus on first use outcomes, I also consider two change-in-dose regressions: cigarette packs per day and drinks per day drank, both averaged over the 30 days before interview. These regressions are less appealing than the first-use analyses, because they pick up addictedness as well as incidence. Further, prices and income are likely to be more important factors in continued use than initiation. Finally, an addict's demand for their drug of choice could drop in response to a shock: if a drug's marginal utility is decreasing in dosage, an adverse event may motivate initiation of new drug. Still, these analyses are worth considering. Specified as in the baseline regressions above (equation 1) and evaluated using ordinary least squares, they find statistically insignificant increases in dosage following recent shocks: an additional 0.05 packs per day and 0.15 drinks per day drank (See Appendix Table A1.4).

Table 1.8: Differential First-Use-Responses to Adverse Events by Family Income in Childhood

	1^{st} Cigarette		1^{st} Binge Drinking		1^{st} Marijuana		1^{st} Illegal Drug	
	(1)		(2)		(3)		(4)	
Parameter Estimates	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*
Δ Shock since last interview	2.566***	*11.7%*	1.449	*3.2%*	2.033**	*9.2%*	2.033**	*0.8%*
	(2.69)		(1.09)		(2.01)		(2.01)	
Lag: ΔShock since last interview	3.462**	*15.4%*	1.610	*4.1%*	1.155	*1.9%*	1.155	*5.2%*
	(2.05)		(0.63)		(0.20)		(0.20)	
Δ Shock since last interview ·	1.098	*1.2%*	1.016	*0.1%*	0.512	*-8.6%*	2.382	*2.6%*
Family income > median	(0.20)		(0.03)		(-1.36)		(1.12)	
Lag: ΔShock since last interview	1.756	*7.0%*	1.517	*3.6%*	0.738	*-3.9%*	3.167	*3.5%*
· Family income > median	(0.61)		(0.43)		(-0.30)		(1.08)	
Δ Shock since last interview ·	1.154	*1.8%*	0.524	*-5.5%*	0.292*	*-15.9%*	0.749	*-0.9%*
Missing family income	(0.15)		(-0.79)		(-1.67)		(-0.27)	
Lag: ΔShock since last interview	1.668	*6.3%*	0.375	*-8.4%*	0.298	*-15.6%*	0.105*	*-6.9%*
· Missing family income	(0.28)		(-0.96)		(-1.23)		(-1.94)	
N	2411		2962		2732		3327	
Mean(Δ Behavior)	0.160		0.082		0.167		0.031	
Shock Effects								
Recent ($\beta_{\Delta \text{Shock since last interview}}$)	*11.7%****		*3.2%*		*9.2%***		*0.8%***	
	(2.69)		(1.09)		(2.01)		(2.01)	
Added recent shock effect if	*1.2%*		*0.1%*		*-8.6%*		*2.6%*	
family income > median	*(0.20)*		(0.03)		(-1.36)		(1.12)	
Long-run ($\beta_{\text{Lag}(\Delta\text{Shock})}$ - $\beta_{\Delta\text{Shock}}$)	*3.7%*		*0.9%*		*-7.3%*		*4.4%***	
	(0.57)		(0.15)		(-0.80)		(1.97)	
Added long-run shock effect if	*5.8%*		*3.4%*		*4.7%*		*0.9%*	
family income > median	(0.58)		(0.45)		(0.38)		(0.33)	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates (as AMEs). Additional controls are described in the note to Table 3. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

B. Evidence for a Coping Response Mechanism

Several mechanisms could drive increases in first substance use following adverse events. To differentiate among these pathways, I consider a behavior that should change differently in response to a shock if driven by a coping response versus a decrease in life expectancy, time preferences (δ or β), or reliance on System 2: days exercised per week.

Specifically, a coping response predicts increased exercise in response to adverse events, as long as they do not raise the cost of exercising. Yet greater present-bias predicts decreased exercise due to less value placed on long run benefits, and reduced life expectancy has a similar effect because the improved future state is less likely to be realized.[41] A shift towards System 1 should also reduce exercise, as more distant benefits tend to be less accessible mentally.[42]

To test for a coping response, I estimate the effect of adverse events on the change in days exercised per week. Evaluated using OLS, this regression is specified as in equation 1, but with three extra terms: an indicator for having ranked one's neighborhood as having no problem with crime or violence at interview t ($Safe_{it}$), plus terms interacting this with each shock variable:

$$\Delta Exercise_{it} = \beta_1 \cdot \Delta Shock_{it} + \beta_2 \cdot \Delta Shock_{i,t-1} + \beta_3 \cdot \Delta Shock_{it} \cdot Safe_{it} + \beta_4 \cdot \Delta Shock_{i,t-1} \cdot Safe_{it} + \beta_5 \cdot Safe_{it} + \lambda \cdot X_{it} + \gamma_t + \varepsilon_{it}. \qquad (4)$$

If events like crime victimization raise the perceived costs of outdoor activity in higher crime areas, these may reduce exercise among shock-victims living in such neighborhoods.[43] Thus, a

[41] This assumes that the expected cause of death is an outside event, as expecting an earlier death from an internal health event (e.g., updated genetic risk for heart disease) may increase exercise as an investment in health capital. Excluding family deaths reduces the likelihood that the latter interpretation drives this analysis's results.

[42] Benthin et al.'s (1995) findings indicate that adolescents view exercise as having long run health benefits, as well as short run associations consistent with recognizing exercise as both a coping device—positive changes in affect, stress reduction—and a source of short run costs: fatigue and pain. Coping motives aside, a present-focused decision process should thus reduce exercise by underweighting future gains and raising attention to short run discomfort.

[43] This is consistent with evidence of a negative association between neighborhood violent crime and adolescent strenuous or outdoor exercise (Gordon-Larsen, McMurray, and Popkin, 2000; Gomez et al., 2004).

coping response may yield non-positive β_1 and β_2 values alongside positive values of β_3 and β_4.

The first column in Table 1.9 finds exactly that: while a recent shock induces a 1.4 day drop in days exercised per week in the full sample, respondents who live in safe neighborhoods respond with an additional 1.8 day increase in days exercised (both statistically significant). Notably, the full recent shock effect for those in safe neighborhoods is only 0.3 days (the sum of these two effects). The implication is that individuals who experience an adverse event tend to exercise less if they live in an area they consider unsafe, but not if they live in a safe area.

However, closer consideration of the exercise data calls for an adjusted specification: above 1 day per week, days-exercised is coded in groups of 2.[44] Consequently, some top-coded respondents—those exercising "6 or 7" days per week—could increase their weekly exercise during the survey period without the data capturing it (i.e., from 6 to 7 days). To avoid biasing results downwards, I add three further controls: a binary indicator for listing the highest exercise level at the prior interview ($\text{MaxEx}_{i,t-1}$), and terms interacting this with each shock variable:

$$\Delta\text{Exercise}_{it} = \beta_1 \cdot \Delta\text{Shock}_{it} + \beta_2 \cdot \Delta\text{Shock}_{i,t-1} + \beta_3 \cdot \Delta\text{Shock}_{it} \cdot \text{Safe}_{it} + \beta_4 \cdot \Delta\text{Shock}_{i,t-1} \cdot \text{Safe}_{it} + \beta_5 \cdot \text{Safe}_{it} + \beta_6 \cdot \Delta\text{Shock}_{it} \cdot \text{MaxEx}_{i,t-1} + \beta_7 \cdot \Delta\text{Shock}_{i,t-1} \cdot \text{MaxEx}_{i,t-1} + \beta_8 \cdot \text{MaxEx}_{i,t-1} + \lambda \cdot X_{it} + \gamma_t + \varepsilon_{it}. \quad (5)$$

Presented in column 2 of Table 1.9, these results further support the coping response theory, finding a statistically insignificant recent shock effect (-0.5 days exercised per week) alongside a statistically significant relative increase in exercise among those in safe neighborhoods (+ 1.3 days). The linear combination of these effects shows a statistically significant 0.7 day increase in days exercised per week among safe neighborhood respondents who have experienced a shock.

[44] Respondents code one value for exercising "2 or 3 times per week", another for "4 or 5," and another for "6 or 7." Exact values are coded for responses of 0 or 1 day per week. I recode the exercise variable for grouped responses to reflect the implied average days exercised per week (i.e., 2.5 if "2 or 3," 4.5 if "4 or 5," and 6.5 if "6 or 7").

Table 1.9: Adverse Events and Changes in Days of Strenuous Exercise per Week
Coefficient/(t-statistic)

	Baseline	Top-Coded Exercise Controls	Change in Sports Controls
Parameter Estimates	(1)	(2)	(3)
Δ Shock since last interview	-1.419**	-0.524	-0.464
	(-1.99)	(-1.01)	(-0.88)
Lag: ΔShock since last interview	-1.901**	-1.039	-0.994
	(-2.01)	(-1.35)	(-1.31)
Δ Shock since last interview ˙ Safe neighborhood$_t$	1.766**	1.273**	1.249**
	(2.20)	(2.21)	(2.16)
Lag: ΔShock since last interview ˙ Safe neighborhood$_t$	2.530**	1.713**	1.689**
	(2.46)	(1.97)	(1.98)
Safe neighborhood$_t$	-0.831**	-0.472	-0.469
	(-2.57)	(-1.61)	(-1.61)
Δ Shock since last interview ˙ MaxExercise$_{t-1}$		-1.527***	-1.642***
		(-2.67)	(-2.91)
Lag: ΔShock since last interview ˙ MaxExercise$_{t-1}$		-0.663	-0.708
		(-0.77)	(-0.86)
MaxExercise$_{t-1}$		-2.839***	-2.956***
		(-12.75)	(-12.61)
Δ Belongs to school clubs or teams			-0.191
			(-0.74)
Δ How often plays/practices sports after school			0.139
			(1.09)
Δ Usually goes to sports facility/court/field after school			-0.376
			(-1.28)
N	782	782	782
R-squared	0.081	0.316	0.322
Mean(Δ Days exercised per week)	-0.246	-0.246	-0.246
Shock Effects			
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	-1.419**	-0.524	-0.464
	(-1.99)	(-1.01)	(-0.88)
Recent if living in safe neighborhood	0.347	0.749**	0.785**
($\beta_{\Delta\text{Shock since last interview}} + \beta_{\Delta\text{Shock since last interview} \cdot \text{Safe}}$)	(0.87)	(2.15)	(2.21)

Safe neighborhood$_t$ is a 0-1 indicator for reporting that one's neighborhood has no problem with crime or violence. MaxExercise$_{t-1}$ is a 0-1 indicator for reporting the maximum exercise level in the prior period, such that one's change-in-exercise variable is top-coded. Additional controls are described in the note to Table 3, aside from current and lagged missing-observation indicators for the third specification's sports controls. Standard errors are clustered by the unit of survey randomization: mother's 1979-household.
*** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

As expected, having a top coded exercise-level in the prior period is associated with a statistically significant drop in days exercised per week. Both top coding and regression towards the mean could explain this. The interaction between this indictor and the recent shock term also yields a statistically significant drop in days exercised per week (-1.5 days). This reduction makes sense if individuals at or near the top of their potential exercise consumption *ex ante* have a lower marginal return to additional days of exercise (e.g., in terms of distress-reduction) and thus respond to a new shock by initiating a new coping behavior that crowds out exercise (e.g., by absorbing leisure time) or raises the discomfort involved in working out (e.g., smoking).[45]

To address the possibility that differential effects by neighborhood safety stem from differential access to sports teams or clubs, particularly through school, the final column in Table 1.9 adds three different controls for changes in involvement in sports or access to sports facilities: the change in a binary indicator of involvement in school clubs/teams, change in frequency of practicing or playing sports after school, and change in a binary indicator for whether the respondent is usually at a sports facility, field, or basketball court between school and dinner. The recent shock effects, both overall and for those in safe neighborhoods, retain similar sizes and statistical significance as in column 2, yet all sports variables are statistically insignificant. Thus, changes in access to or involvement in sports do not explain the observed changes in exercise following adverse events.

Overall, these analyses' findings—increased exercise following adverse events only among those living in safe neighborhoods—are consistent with the coping response framework, but not with the other shock-to-behavior pathways considered here.

IV. Discussion and Conclusion

This study finds that adverse events are associated with an increased probability of first

[45] Increased exercise-intensity (e.g., longer runs) among high-exercisers or athletes could also contribute to this result, as more intensive workouts can require low impact or off days in order to recover/avoid injury.

cigarette use and first use of an illegal drug other than marijuana for adolescents under age 19. A falsification test supports the exogeneity of these events, and thus a causal interpretation of the observed relationship. Controls and specification checks indicate that these findings do not stem from changes in peer pressure or neighborhood characteristics acting as third factor drivers, or from differential responses by baseline peer pressure, substance access, or neighborhood crime and violence. The long run effects of shocks on first illegal drug use corroborate directionality.

As SES gaps in the incidence of these events are small in this dataset, they explain little of the corresponding gaps in initiation rates. However, respondents whose mothers graduated from college show a lower probability of responding to adverse events with first use of either marijuana or other illegal drugs. Such differentials are not evident for those with above-median family incomes in childhood, suggesting a more nuanced driver than income alone. This is consistent with a large and ongoing literature on the protective effects of maternal education.[46] The coping response theory may explain this if children of more educated mothers have better access to or knowledge of low cost coping devices. The specific mechanism merits further study.

Finally, the influence of adverse events on changes in days exercised per week indicates a coping response to mental distress: adolescents living in neighborhoods they consider safe—that is, having no problem with crime or violence—show a statistically significant increase in exercise following adverse events, while others exhibit a statistically insignificant reduced-exercise response. These findings are predicted by the coping response framework, but inconsistent with alternative shock-response theories considered here.

This paper has several limitations. While the question of interest is whether increased

[46] Studies of adopted children indicate that both biological and adopted parents' educations affect child educational attainment, indicating roles for both nature (i.e., genetics) and nurture (Tsou, Liu, and Hammit, 2012; Björklund, Lindahl, and Plug, 2006). Chen and Li (2009) find that a nurture-effect contributes to the impact of mother's education on child health, while Glewe (1999) presents evidence that the impact of maternal education on child health is mediated by mothers' health knowledge.

distress raises demand for substance use, empirical analyses focus on two particularly traumatic events, suggesting that results may not be generalizable to more common or chronic sources of distress (e.g., daily hassles). Basing identification on exogenous changes in mental distress prevents estimation of such effects here.[47] Focusing on adolescents further constrains generalizability. Yet, given high adolescent initiation rates and costs of early substance use, understanding first use in this demographic is vital and thus a logical focus of study. The lack of price data poses a third drawback. Though it is not clear whether adolescents pay for such substances at first use, the gap between changes in demand and consumption suggests a particular importance to factors that restrict access, including price.

Results from one further regression add another layer to this analysis. Column 3 of Appendix Table A1.4 examines changes in the number of partners a respondent had sex with in the 12 months before interview. OLS estimates indicate a statistically significant increase in number of partners (+0.24) associated with recent shocks.[48] This raises an important point: while the empirical analyses herein focus on substance use, a variety of behaviors influence one's mental state. For example, sexual intercourse affects dopamine and other neurochemicals. Thus, the relationships discussed above need not be limited to addictive substances. Further work should consider this.

This paper's contributions span several literatures. Research in psychiatry and psychology has considered whether self-medication helps explain the relationship between substance use and mental illness (Khantzian, 1985, 1997). Yet, as such work often focuses on individuals with concurrent substance abuse and mental illness diagnoses, issues of directionality

[47] While compelling work has shown increased adolescent smoking following parental divorce (see Fletcher and Sindelar, 2012), disentangling the causal mechanism driving this change in behavior remains a challenge.

[48] As the number of partners question caps responses at 4, the true effect may be larger.

and confounding tend to prevent causal conclusions. Using longitudinal data on first substance use and acute events known to cause mental distress, this paper provides compelling evidence of a causal effect of mental distress on risky behaviors. Moreover, these results are evident in a broad population of adolescents, weighted to yield nationally representative estimates. Findings on how exercise changes following these events support the coping response interpretation.

This study offers further implications for both research and policy. Evidence that mental distress influences substance use suggests a need to explicitly account for mental health in models of risky behaviors and interventions targeting them. Moreover, the coping response hypothesis implies that access to lower risk coping mechanisms could affect use of more costly behaviors. Better understanding the relationships between these different coping devices/behaviors is an important area for future research, with potentially valuable policy implications.

Implications for welfare analyses related to substance use merit further comment, as the latter often focus on one drug at a time. The coping response framework suggests that, in the context of mental distress, losing access to one substance may motivate substitution towards the next lowest cost option. For the consumer, this new option will be costlier than the first (otherwise they would have used it *ex ante*), and possibly even more so from a societal perspective. Thus, welfare analyses of policies restricting access to specific drugs should account for users' outside options. Not all restrictions on relatively safe substances need be beneficial.

Finally, evidence of engagement in risky behaviors as a coping response suggests a different type of policy intervention than non-rational or naïve-choice characterizations of such behaviors. Specifically, initiation of a new substance as an attempt to cope implies painful distress alongside a lack of access to less costly alternative coping devices. The situation and set of options create a context in which the costly behavior is a solution to an immediate problem, albeit one with long-term consequences. To the extent that context induces risky behaviors, policy interventions focusing on contextual factors (e.g., access to less costly coping mechanisms) may be particularly promising.

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Paper II: Explaining the Education Gradient in Smoking: The Impact of Advertising and Information on Smoking Behaviors

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In the past half-century, smoking rates among the more and less educated have diverged dramatically. In 1955, male college graduates smoked 26 percent less than their peers who did not finish high school. By 2011, the gap was 82 percent. For women, the gap increased from 3 percent to 85 percent. As we show below, most of the growth in this gap occurred in the 1960s and 1970s. This differential change in smoking rates has major consequences. It has been linked to differential trends in life expectancy by education (Cutler et al., 2011) as well as differential rates of low birth-weight (Martensen et al. 2009) and old age disability (Cutler & Lleras-Muney, 2010a). The greater reduction in smoking for the more educated has been noted by several authors but never fully explained (See Pierce et al. 1989, Gilpin & Pierce 2002, De Walque 2010; Cutler & Lleras-Muney 2010b).

An obvious theory for differences in consumption by education groups over time is different price elasticities. Not surprisingly, the less educated are more price elastic than the more educated (Evans et al., 1999; Hersch, 2000; Gruber and Köszegi, 2004). But cigarette prices were essentially constant in real terms from 1955 and 1980, the period when most of the increase in the smoking gradient took place. Income differences could also be important, if higher income is causally associated with reduced smoking. However, within-individual income gradients in cigarette consumption appear to be positive (Apouey and Clark, 2010; Kenkel, Schmeiser, and Urban, 2012).

Given the lack of importance of price and income, researchers have turned to other explanations for the growing smoking gradient. Education differentials have been variously attributed to discounting (Farrell & Fuchs 1982), impulsivity (Khwaja, Silverman, & Sloan 2007), cognitive ability (Cutler & Lleras-Muney 2010b), and years of schooling itself (Grimard & Parent 2007, De Walque, 2007). Yet explaining growth in the smoking gradient would require drastic shifts in these factors over time. While schooling did increase over this period, studies attributing education differentials to schooling per se have not established the specific mechanism(s) behind this, many of which may not be monotonically increasing in years of schooling (e.g., education quality, school-based supervision of behaviors, fostered attitudes towards scientific research, etc.).

In this paper, we focus on two explanations that could account for substantial growth in smoking's education gradient over time: new information about smoking's harms, and significant changes in cigarette advertising. Specifically, if more and less educated individuals respond differently to such information or advertising, trends in these factors could yield a growing gap in smoking rates by education.

Information about cigarette smoking's health effects evolved over time. From the 1940s through the 1960s, cigarettes went from a good with uncertain health impacts to one with large and clear consequences. Given this change in information, a greater demand for health among the better educated (as in Grossman 1972) or a differential ability to process such information (Kenkel 1991) would predict growth in smoking's education gradient.

Advertising also changed markedly over time. Cigarettes were, and remain, among the most marketed products in the economy. Sometimes, the marketing is informational (focusing on product safety, primarily), but often it is designed to associate a specific cigarette brand with social traits considered desirable by the targeted market segment (e.g., masculinity and virility).[49]

[49] For example, Marlboro advertisements (after 1955) zeroed in on masculinity with the slogan, "Where there is a man, there's a Marlboro," alongside images of a rugged or well-dressed man with a tattoo on his hand and a woman

It is possible that tobacco companies found it easier to portray cigarettes in a fashion that appealed more to less educated groups, such that a growing advertising volume led to differential smoking trends.

Empirically, this paper considers the role of health information and advertising in explaining the growing gap in smoking by education. Our data come from responses to retrospective smoking questions in the 1978, 1979, 1980, and 1987 National Health Interview Survey (NHIS). Using these data, we reconstruct individuals' smoking histories from 1950 to 1980 – the period of the largest increase in the smoking gradient. We conduct two empirical exercises with these data. First, we estimate time series models for smoking initiation and cessation, matching these behaviors for different education groups to the nature and quantity of cigarette advertising, new public health information, and cigarette taxes. Second, we use newly available data on cigarette brands smoked by 1978-1980 NHIS respondents – provided by the National Center for Health Statistics (NCHS) at our request – to consider education differentials in cigarette brand choice.

We find statistically significant and substantively large education differentials in both genders' initiation responses to cigarette advertising expenditure, as well as a differential cessation response among men. When advertising expenditure increases, smoking rises more among the less educated. These differential responses explain 39 percent of growth in the gap in smoking rates between male high school dropouts and college graduates from 1950 to 1980, and 27 percent of growth in the gap among women.

We show that smoking is information-responsive as well. The better educated were much more likely to choose safer cigarettes than the less educated, and they smoked fewer cigarettes.

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in the background. Similarly, Virginia Slims played on themes of female independence and success ("You've come a long-way baby.").

In our analysis of brand choice, we find that health information is the primary driver of differences in brands smoked between the better educated and the less educated. Image is also important, but quantitatively smaller.

The paper proceeds as follows. Section I discusses the potential influence of advertising and information on smoking's education gradient. Section II describes our data. Section III outlines the historical factors shaping both cigarette advertising and the dissemination of public health information about cigarettes. Section IV presents the initiation and cessation analyses, and section V presents the brand choice results. The last section concludes.

I. Information, Advertising-Induced Tastes, and Smoking Gradients

To understand the role of information and advertising in explaining the smoking gradient, we start with a basic intertemporal utility function:

$$W_t = \sum_s \rho^s \, S(t+s \mid t) \, U(Cig_{t+s}, \mathbf{X}_{t+s}; \beta) \tag{1}$$

where Cig_{t+s} is consumption of cigarettes s years in the future and X_{t+s} is consumption of other goods. The probability that a person is alive at t+s conditional on being alive at t, $S(t+s \mid t)$, is a parameter we denote μ_s, which depends on the history of cigarette consumption: $\mu_s(\mathbf{Cig})$. The individual's perception of this variable is □□, which may vary over time. ρ is the discount rate, and β is the taste parameter governing the utility tradeoff between cigarettes and other goods.[50]

Adding in a standard budget constraint ($Y = P_c \, Cig_t + X_t$, with exogenous income Y), optimal cigarette consumption can be expressed as $C = C(Y, P_c, \beta, \rho,$ □$)$. Cigarette consumption will be positively associated with Y and β (normalizing β so that a higher value corresponds to a greater utility parameter on cigarettes), and negatively associated with P_c and □. Lower discount

[50] In an addiction model, the utility of current consumption will also depend on past consumption, and thus current consumption will depend on past consumption (Becker and Murphy, 1988).

rates will negatively affect cigarette consumption, provided people know that smoking is harmful.[51]

Information, whether scientific or from advertising, may affect utility in three ways. First, information can change the perceived impact of smoking on survival, ⍰. If cigarettes are initially thought to be safe and later learned to be harmful, people who incorporate such information will choose to cut back on smoking. This may mean quitting smoking entirely, reducing the number of cigarettes smoked per day, or switching to less harmful cigarettes. Second, such information might motivate individuals to take steps to shift their discount factor, as in endogenous models of time preferences (Becker and Mulligan, 1997). By shifting the utility derived from future states, this could fuel different behavior, particularly with respect to choices that offer long run costs or benefits. Finally, advertising may affect the utility of smoking, β. Notably, the effect we discuss here is different from taste-shifting as addressed in Becker and Murphy (1993)[52]: if smokers have preferences over cigarette attributes, and advertising can change the bundle of attributes embodied in a particular brand (including the individual's preferred brand), then advertising can shift the utility an individual derives from smoking.

Education may be correlated with each of these responses. A variety of evidence suggests that better educated people respond more to scientific information than less educated people (Cutler and Lleras-Muney, 2010b; Lichtenberg and Lleras-Muney, 2010), perhaps because better educated people are also more trusting of science. In addition, more educated people may be better at using new information to enact behavioral changes (Grossman, 1972; Kenkel, 1991).

Advertising about product attributes is particularly important for cigarettes. Experiments

[51] This is a rational model of smoking. There are a variety of other models that might explain smoking without invoking rationality assumptions. We do not pursue such models here.

[52] An older literature assumes that advertising changes consumer tastes, framing this as a shift in preferences (Dixit and Norman, 1978). We do not pursue this path.

in the early 1940s demonstrated that smokers could not identify major brands, or even their preferred brand, in a blind comparison (Littell, 1942). Thus, branding focused on "image wants." In an internal marketing document from 1978, RJR – one of the leading tobacco companies – defined image wants as varying by gender ("mainly by men," "more by men," "more by women," and "mainly by women"), age ("younger adults" vs. "older adults"), occupation (blue-collar vs. white-collar), smoker type (heavy vs. light), physical characteristics ("rugged" vs. "gentle"), stylishness, modernity (modern vs. traditional), and package attractiveness. Examples of advertising built around image wants include Marlboro's 1950s shift from a more-feminine to a masculine image via the "Marlboro Man" campaign, and Virginia Slims' projection of a "modern independent woman" persona.

There is no theoretical reason why cigarette companies should be better able to target image characteristics appealing to more or less educated individuals. Empirically, however, it may be that image-associated utility is stronger for one education group than another. For example, more rebellious people may pursue less education, and a rebellious image may be easier to project than an attitude of conformity. Information sources may also differ across groups, making the less educated easier to reach with advertising. Or perhaps high educational achievement per se is a strong image-signal, reducing the value of signals sent via cigarette use.

Before proceeding with our empirical analyses examining the response to health information and image advertising, we remark on the relevance of education. In our examination of education and smoking, we consider the full range of education outcomes, up to college graduate. As we show below, many smoking decisions are made before education is completed, often in the teenage years. Thus, we do not interpret the education interactions as a causal impact of achieving that level of schooling on smoking. For now, we remain agnostic about whether higher levels of education proxy for better quality education at younger ages, or whether these reflect some other consumer-attribute such as impulsivity or discounting, which might explain the increasing gradient through a separate interaction.

II. Smoking Data and Trends

We assemble a time series of smoking histories using data from the National Health Interview Survey's (NHIS) 1978, 1979, and 1980 Smoking Supplements, as well as its 1987 Cancer Control Supplement.[53] In each of these years, individuals were asked their age at smoking initiation (when they first began smoking regularly) and time since cessation (when they last smoked regularly).[54] Using this information, we assemble a pseudo-panel data set covering respondents ages 25 to 64 at interview, including person-year observations from age 14 through interview. The age-25 lower bound ensures that we have a good proxy for completed education, while the upper bound addresses differential mortality (i.e., smokers dying before non-smokers). The age-14 cutoff marks the 10th percentile for initiation ages, and addresses concerns about the accuracy of reports citing particularly young initiation ages. We also exclude those whose surveys were completed by a proxy (due to concerns about accurately reported smoking histories). Finally, we only extend our analyses back to 1950, as earlier years yield progressively younger samples that are not comparable with later years' data. Even with these exclusions, our sample includes more than 45,000 respondents and over 1 million person-years.[55]

While the obvious issue with such data is the accuracy of retrospective self-reports,

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[53] De Walque (2010) also reconstructs smoking histories using NHIS data, including a number of post-1987 surveys that we do no use. We are grateful to him for sharing his data.

[54] In all cases, smoking refers to cigarette smoking only. By the mid-20th century, cigarettes accounted for the vast majority of tobacco consumption (Brandt, 2007).

[55] See the data appendix for a more detailed description of our data.

evidence suggests that such accounts are reasonably accurate. Comparing longitudinal records with subjects' recall of their smoking status 20 (32) years prior, Krall et al. (1989) found correct recall among 90 (87) percent of subjects, with no apparent differences in accuracy by gender or smoking status at interview.[56]

In addition to asking about smoking histories, the 1978 -1980 and 1987 NHIS asked people what brand of cigarette they usually smoked. Historically, this information has been suppressed in the public use files. In response to our request, the National Center for Health Statistics released these data publicly. Our analysis of brand choice focuses on the 1978-1980 brand data, as changes in available brands between then and 1987 complicate comparisons.

In each survey, the NHIS inquires about completed education. We code people into five groups based on the highest level of education completed: less than 8 years of school; at least 8 years of school but not a high school graduate; high school graduate; some college but no college degree; and college graduate.[57] Additional questions cover race (black, white, other), Hispanic ethnicity, veteran status (including the specific war), urbanicity, and census region. We code these for each individual in the appropriate year (e.g., for war service), as well as indicators based on imputed birth year for whether a male respondent had ever been draft eligible.

The education distribution changed considerably from 1950 to 1980. To present more meaningful trends, we reweight each year of data to match 1980 population totals by five-year age group, sex, and education. These weights are applied in all analyses of the historical data.[58]

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[56] We also undertook a more limited analysis, comparing 1978 smoking rates as reported by 1978 survey respondents to those derived from the 1987 survey for individuals from the same cohort. The implied 1978 smoking rate for the 1987 cohort is 43 percent, whereas the actual smoking rate for that group in 1978 was 41 percent.

[57] College graduates are defined as those who completed at least 4 years of college.

[58] Such reweighting guarantees that our means and trend data depict changes in smoking behavior not due to shifts in the population's education-distribution over time. In regressions, it accounts for heterogeneity in the average response in the population and corrects for within-group heterogeneity (Solon, Haider, and Wooldridge, 2013).

Table 2.1: Summary Statistics for NHIS Data, 1950-80		
Variable	**Male**	**Female**
Age	30.8 (12.1)	31.0 (12.1)
Ever-Married	90.4%	92.3%
Smoking Variables		
Ever-Smoked	60.5%	39.5%
Age at Initiation (if smoked)	17.5 (4.3)	19.8 (5.7)
Ever Tried to Quit (if smoked)	45.2%	44.4%
Ever-Quit (if smoked)	16.4%	11.8%
Age at Cessation (if quit)	38.0 (11.7)	37.5 (12.0)
Number of cigarettes smoked daily (if current smoker) #	23.4	19.4
Education		
<8 years of School	4.3%	3.6%
Did Not Graduate High School	13.6%	14.3%
High School Graduate	36.6%	42.2%
Completed Some College	21.0%	20.3%
College Graduate +	24.5%	19.7%
Military Service		
Ever Draft Eligible	76.2%	0.0%
Unknown If Served	0.1%	0.2%
Veteran, Other Service	5.3%	0.9%
Veteran, WW2	19.4%	0.5%
Veteran, Korea	10.7%	0.2%
Veteran, Vietnam	7.4%	0.2%
Race/Ethnicity		
Hispanic	5.0%	5.0%
White	89.4%	88.1%
Black	8.4%	9.9%
Other-Race	2.3%	1.9%
Urbanicity		
SMSA	73.0%	72.3%
Non-SMSA, Non-Farm	24.4%	25.2%
Non-SMSA, Farm	2.6%	2.5%
N	469,635	577,989

Note: Data are reweighted to the age-sex-education distribution of the population in 1980 and cover only those respondents who were ages 25-64 when interviewed, and were 14 or older in the year in question. Numbers in (.) are standard deviations. The sample sizes are the same other than for the smoking variables, which are asked to subsets based on prior smoking and quit status.

Measured only in survey year.

Table 2.1 shows summary statistics for the smoking histories data. Our sample is largely white, non-Hispanic, and urban, with about 58 percent of person-years among individuals educated at or below the high school graduate level, 14 percent having dropped out of high school, and 4 percent with fewer than 8 years of school. About half the sample smoked regularly at some point in their lives, with that proportion higher among men than women.

Figure 2.1 shows a respondent-level histogram of age at smoking initiation, with figure 2.2 showing the corresponding figure for cessation. Almost all smoking initiation decisions are made in adolescence, with substantial spikes at ages 16 and 18. Quitting is common: at interview, 41 percent of males and 33 percent of females who had ever smoked had quit, with almost 69 percent of ever-smokers having engaged in at least one serious quit attempt. Age at cessation is more diffuse than age at initiation, with 10^{th} and 90^{th} percentiles at 23 and 53, respectively. Since the older population is only represented in later years, we cannot examine the time series of cessation at older ages. We thus look at cessation for individuals aged 14-46, covering 80 percent of cessation in this sample.

Figure 2.1: Age at Smoking Initiation

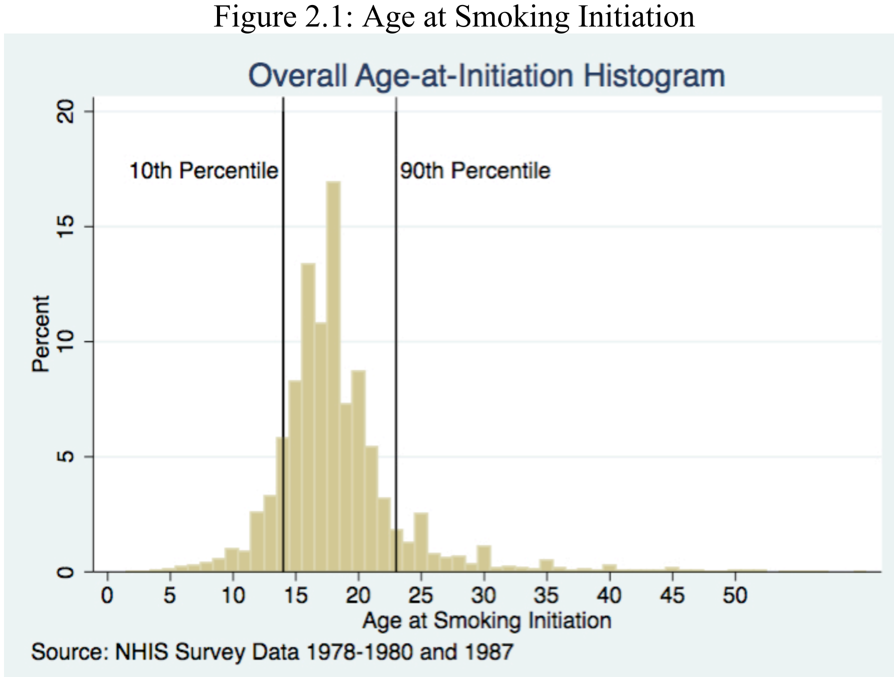


Figure 2.2: Age at Smoking Cessation

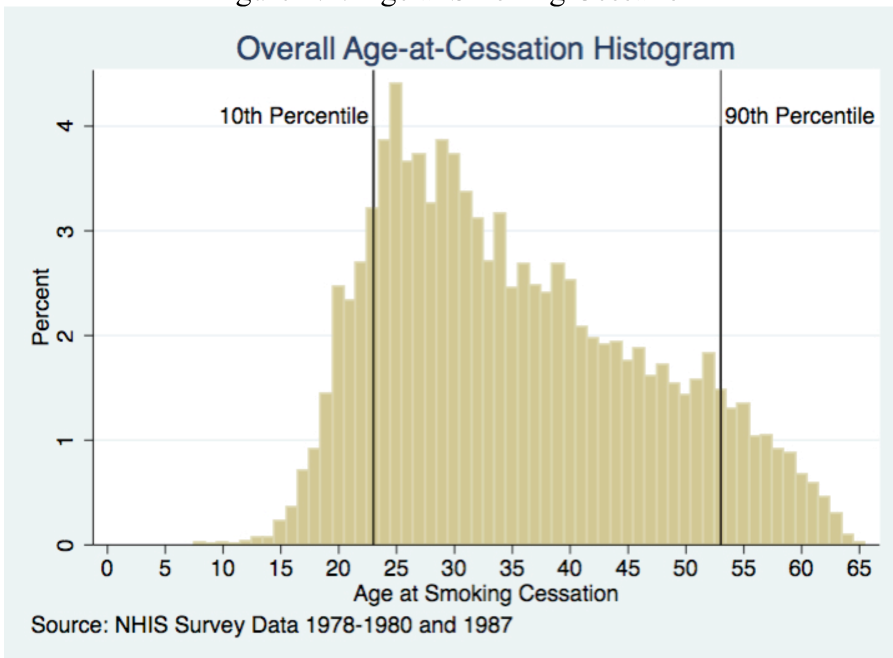
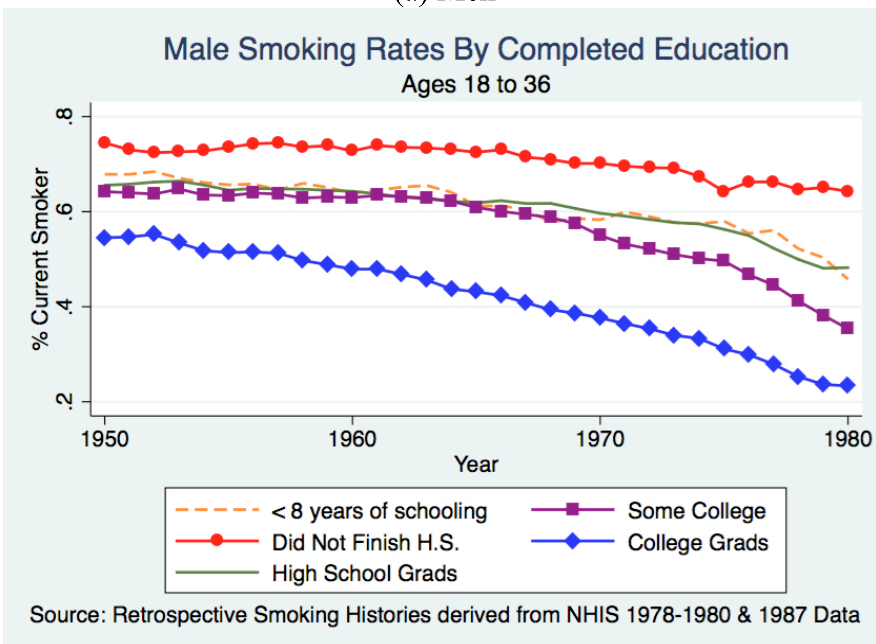


Figure 2.3 plots smoking rates by education for prime age men (the upper figure) and women (the lower figure) from 1950-80. To maintain a relatively constant age distribution over time, these samples are limited to people aged 18 to 36. Among men, high school dropouts exhibit the highest smoking rates and college graduates the lowest in every year, with the other three education groups – fewer than 8 years of schooling, high school graduates, and college dropouts – in between. Male smoking rates began decreasing in the mid-1960s for all education groups except college graduates, whose decline appears to have started around 1952. Moreover, smoking rates among college graduates drop much more steeply than any of the other groups, such that the gap in smoking rates between high school dropouts and college graduates grows almost monotonically from the early 1950s through 1980.

Among women, overall smoking rises over the 1950s, though rates for college attendees (graduates and dropouts) are relatively flat over this period and generally decline from 1958-onwards. Concurrent with this decline, the growth in smoking rates for those with fewer than 8 years of schooling levels off somewhat, while rates continue to rise among high school dropouts and, less steeply, high school graduates. Post-1964, smoking rates decline in all groups except high school dropouts. The under-8-years of education group aside, these trends are consistent with women's smoking habits approaching those of men in the same education group, though remaining 10 to 15 percentage points lower.

Figure 2.3: Smoking Rates by Completed Education
(a) Men



(b) Women

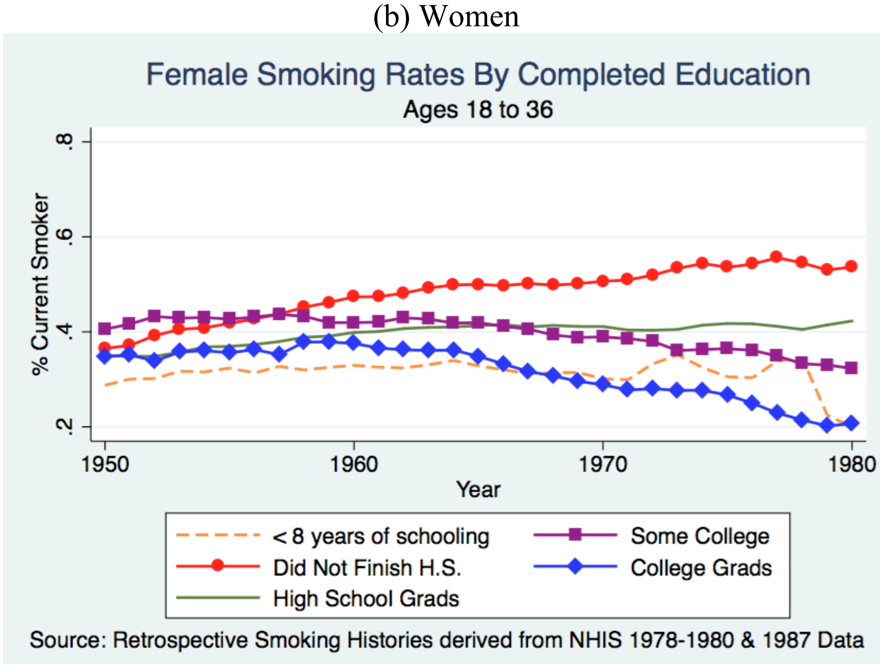
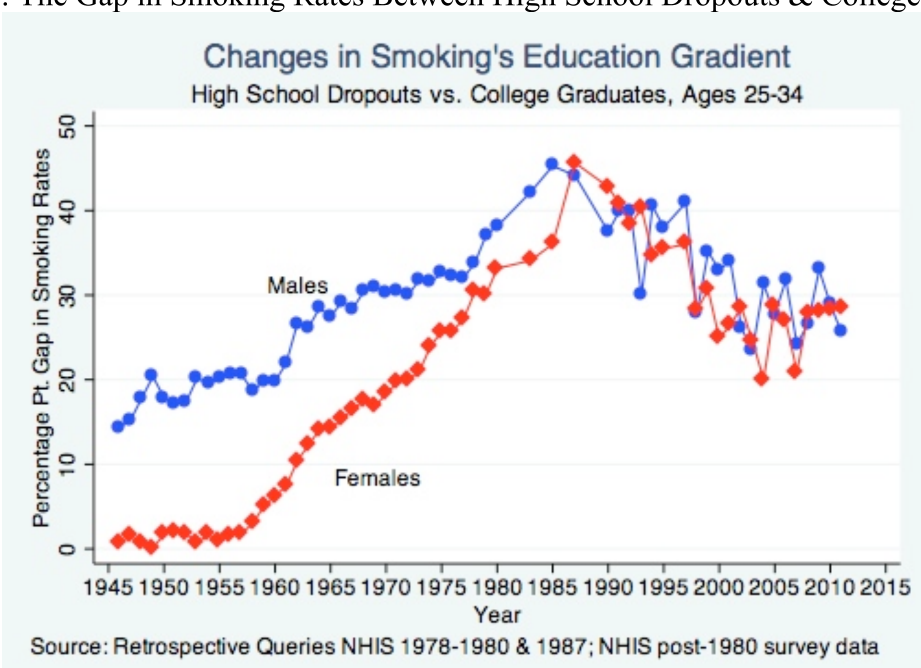


Figure 2.4 shows the implied gap in smoking rates between college graduates and high school dropouts. We use the NHIS smoking history data described above through 1980, and supplement that with year-of-interview data from later surveys.[59] For age comparability, we consider people aged 25-34 in each year. Between 1946 and the mid-1980s, the education differential in smoking rises markedly. For men, the gap increases from 13 percentage points in 1946 to 38 percentage points in 1980, peaking at 45 percentage points in 1985. For women, the increase is even greater, jumping from essentially no gap in 1946 to 46 percentage points at its peak in 1987. Thereafter, there is a modest decline in the gap for both men and women. Even in 2011, however, we see 26 and 28 percentage point differences in smoking rates by education, for males and females respectively.

Figure 2.4: The Gap in Smoking Rates Between High School Dropouts & College Graduates



Note: Data for 1980 and earlier are smoking histories derived from retrospective smoking questions in the 1978-1980 NHIS Smoking Supplements and the 1987 Cancer Control Supplement, all reweighted to reflect the sex-by-education-by-age-group distribution in 1980. Post-1980 observations are year-of-interview data on smoking status from the 1983, 1985, 1987, 1990-1995, and 1997-2011 NHIS. The noise in the post-1980 data reflects the fact that the corresponding surveys cover different individuals each year.

[59] These include data from 1983, 1985, 1987, 1990-1995, and 1997-2011. Because the year-of-interview data cover different respondents in each year, post-1980 trends are noisier than those before 1980.

Initiation and Cessation

To understand smoking's education gradient, it is helpful to differentiate contemporaneous smoking rates into differences in initiation and cessation. Figure 2.5 depicts trends in ever smoking from 1936 to 1980, for males and females separately. Each figure plots, by ultimate-education, the percent of individuals aged 18-22 who had ever smoked regularly as of that year.[60] Given the ages in question, this is effectively a measure of initiation.

For men, there is a clear education differential in initiation, generally reflecting the traditional gradient (i.e., lower initiation rates at higher education levels) for all but the least educated. Male smoking initiation rose during the US involvement in World War II in all education groups except high school dropouts, an unsurprising finding given the latter group's already high smoking rates and the fact that cigarettes were included in field rations. Among college graduates, smoking initiation fell soon after the war's end, whereas other groups' initiation rates dipped slightly in the late 1940s and then rebounded. Rates for high school dropouts climbed slowly thereafter, while the other non-college-graduates' smoking rates remained relatively constant from the mid-1950s through the 1964 Surgeon General's Report on Smoking and Health (SGR). Thereafter, initiation declined in all groups, with the drop particularly steep among those who would attend or complete college.

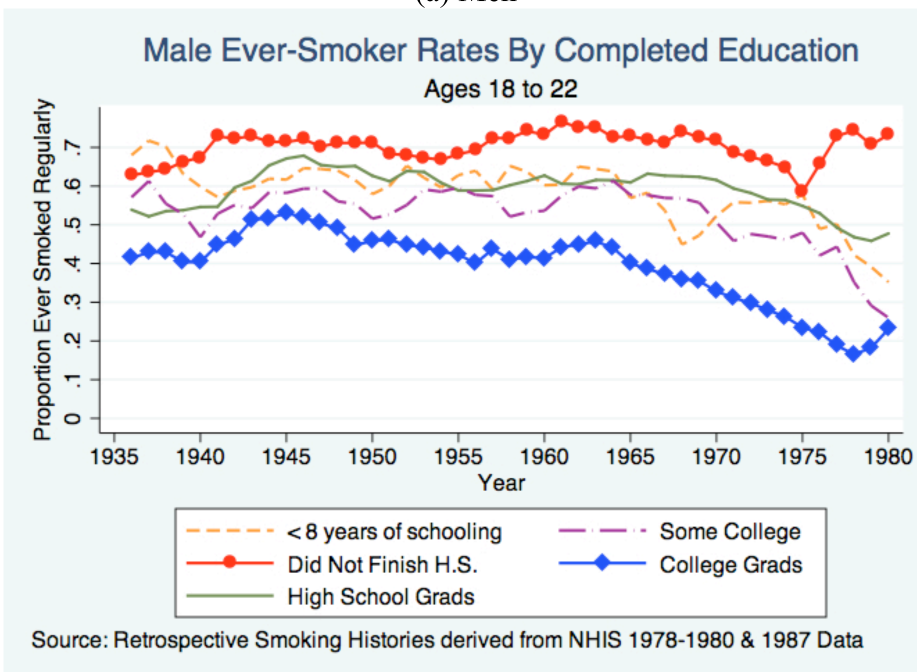
The pattern of initiation among women differs noticeably from that for men. Not only were initiation rates much lower in the 1930s and 1940s, but they continued to rise after World War II. The increase was especially pronounced for high school dropouts, though notable for high school graduates as well. Again, the Surgeon's General's report appears to shift trends. After 1964, female initiation declines noticeably among the highest and lowest education groups,

[60] Trends are plotted for ages 18-22 because the equivalent plots for ages 14-22 miss individuals who are aged 14-17 in 1980, 14-16 in 1979, etc. (Recall that we only know completed education for those aged 25 and older at interview.) Our regressions control for both age and year fixed effects to counter this imbalance.

and somewhat less steeply among college dropouts, while the other groups level off through the remainder of the 1960s. While initiation rates among high school dropouts and graduates rebound somewhat in the mid-1970s, those of college graduates fall.

Figure 2.5: Percent of People 18-22 Who Ever-Smoked by Completed Education

(a) Men



(b) Women

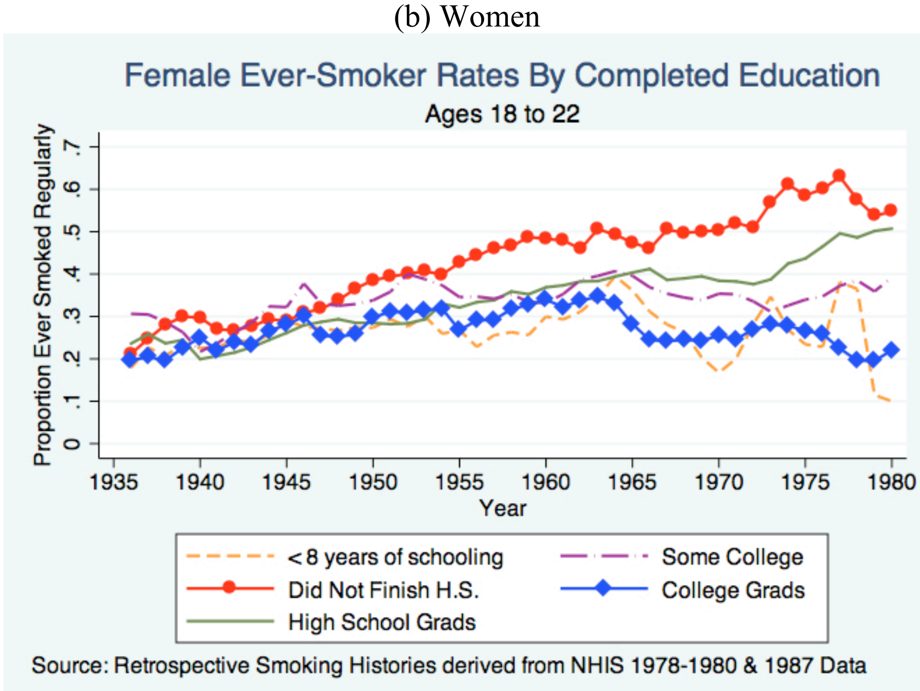
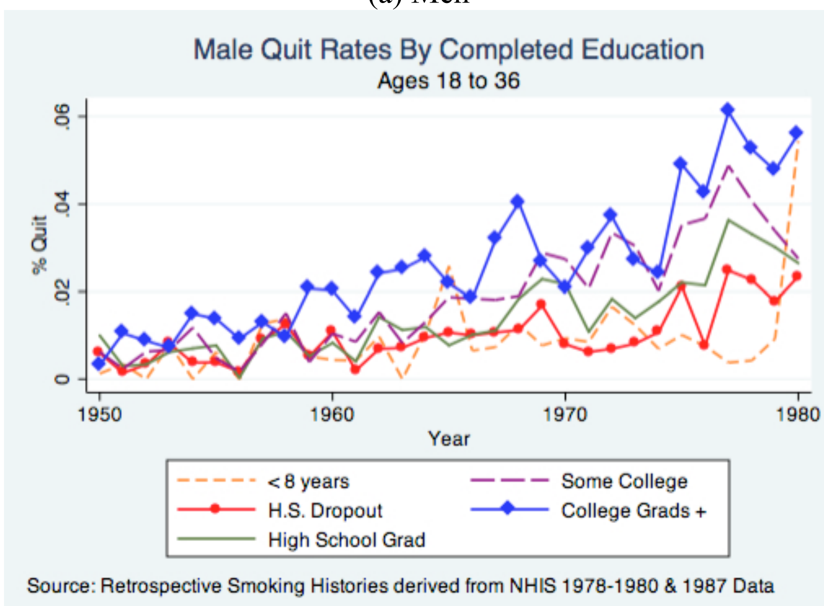


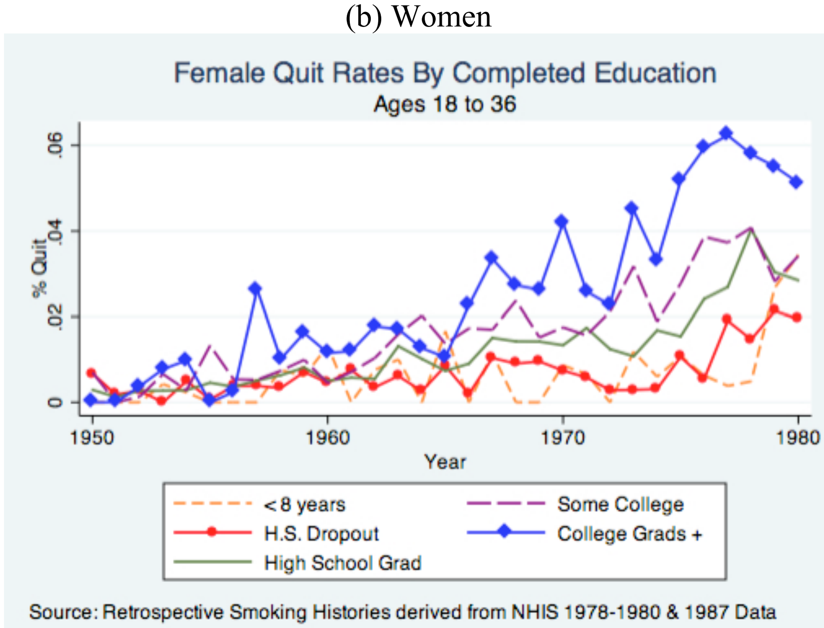
Figure 2.6 shows the annual quit rates for people aged 18-36 who smoked in the prior year. The trends by education are fairly similar for males and females. In the 1950s, cessation rates moved similarly for all education groups. Starting in the late 1950s/early 1960s, the rates diverge, with cessation increasing most among the better educated. By the late-1970s, college graduates' cessation rates are 2 to 3 times higher than those of high school dropouts.

Figure 2.6: Annual Quit Rate by Completed Education, Ages 14-46

(a) Men



(b) Women



To compare the contributions of initiation and cessation trends to growth in smoking's education gradient, we consider a simple decomposition. Note that

$$\Pr(\text{Smoke}_t) = \Pr(\text{EverStart}_t) * (1-\Pr(\text{EverQuit}_t \mid \text{EverStart}_t=1)). \quad (2)$$

Hence, the change in the probability of smoking over time is given by:

$$\Delta\Pr(\text{Smoke}) \approx \Delta\Pr(\text{EverStart})*(1-\Pr(\text{EverQuit}_0)) + \Pr(\text{EverStart}_0)*\Delta(1-\Pr(\text{EverQuit})), \quad (3)$$

a weighted average of the change in initiation and cessation rates.

Table 2.2 shows the results of this decomposition, comparing the shift in smoking rates from 1950-1952 to 1978-1980 among high school dropouts and college graduates ages 25 to 36. Over this time period, smoking rates among male high school dropouts declined by 11 percentage points (first column, first row), primarily due to increased cessation. Among college graduates, the decline was 19 percentage points greater, reflecting both reduced initiation and increased cessation. The differential between these two groups, shown in the last rows of the table, suggests that greater reduction in initiation explains 75 percent of the differential change between the groups (14.1% ÷ 18.7%). Increased cessation explains 52 percent (9.7% ÷ 18.7%).[61] Results for women are similar in the final implication – initiation explains about 74 percent of the differential change versus closer to 34 percent for increased cessation – with the initiation effect due almost entirely to increased initiation among female high school dropouts.[62] Given these results, we include both behaviors in our analysis, with somewhat more of a focus on initiation.

[61] The residual is the covariance term.

[62] We estimated a similar decomposition for people aged 25-64; initiation and cessation's contributions to the differential change in smoking rates were similar to those reported in Table 2.2.

Table 2.2: Decomposing the Change in the Education Gradient in Smoking, Ages 25-36
(Percentage point change)

	Men	**Women**
High School Dropouts		
Change in smoking rate	-11.1%	15.0%
Effect of change in initiation	-2.3%	20.3%
Effect of change in cessation	-9.1%	-3.5%
College Graduate +		
Change in smoking rate	-29.8%	-14.3%
Effect of change in initiation	-16.7%	-1.4%
Effect of change in cessation	-18.8%	-13.6%
Differential Change		
Change in smoking rate	-18.7%	-29.3%
Effect of change in initiation	-14.1%	-21.7%
Effect of change in cessation	-9.7%	-10.1%

This table shows the change in smoking rates from the early part of the sample (1950-52) to the later part of the sample (1978-80) for respondents ages 25-36.

Brand Choice by Education

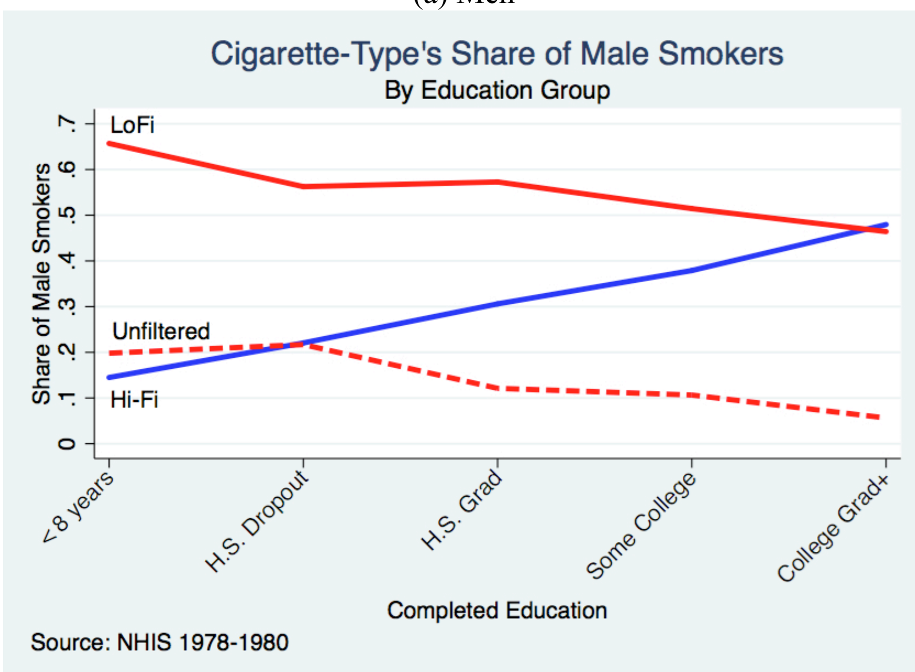
The first cigarettes were unfiltered – there was no filter to reduce tar and nicotine intake. In the 1950s, several tobacco companies responded to increasing concern about smoking's health risk by adding filters to existing cigarettes and introducing new filtered-cigarette brands (e.g., Winston). Over time, stronger filters were engineered, especially after the 1964 Surgeon General's report. Industry documents from the 1970s describe three broad categories of cigarettes based on both the presence of a filter and the cigarette's tar and nicotine content: straights (unfiltered cigarettes); Hi-Fi (i.e., high filtration, meaning filtered cigarettes with much lower tar and nicotine), and Lo-Fi (i.e., low filtration cigarettes). To the extent that consumers viewed these categories as signals of a cigarette's relative health risk,[63] brand choice offers an

[63] Beyond a general awareness of these categories, Hi-Fi cigarettes' advertising often emphasized these brands' relative health benefits: "New low tar entry packs taste of cigarettes having 60% more tar" (Merit); "Out of 122

important indicator of smoker responses to health information. We assign each cigarette to a filtration category using industry documents from the time period (see the data appendix).

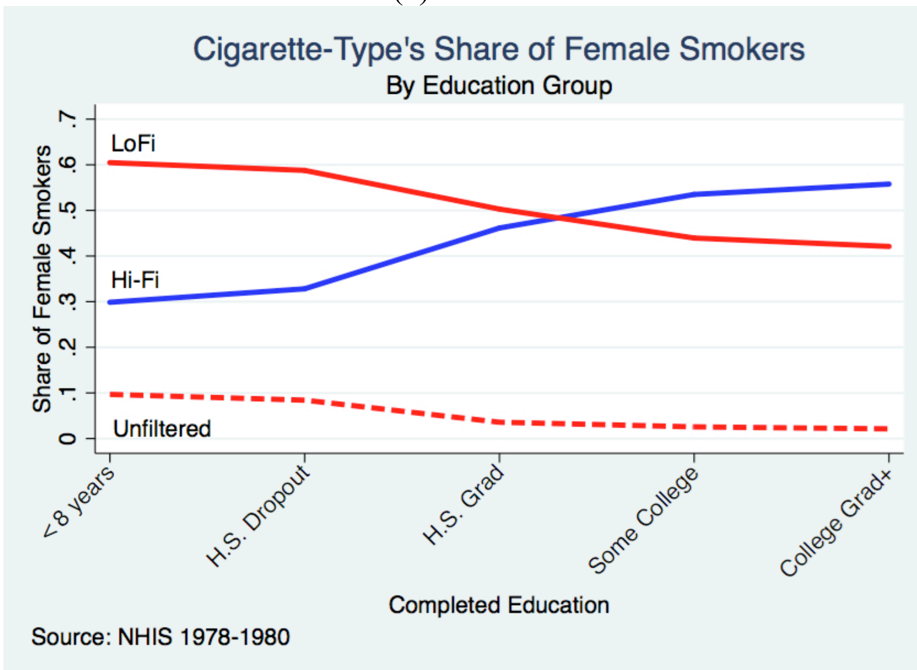
Figure 2.7 plots the share of smokers in each education group who use Hi-Fi, Lo-Fi, and unfiltered cigarettes, for males and females respectively. Use of Hi-Fi cigarettes is strongly related to education; Hi-Fi cigarettes are the most common brand for college graduates but the lowest among less educated men (below even unfiltered cigarettes) and well below Lo-Fi cigarettes among less educated women.

Figure 2.7: Cigarette-Category Market Shares by Education
(a) Men



varieties of cigarettes, the U. S. Government lists Carlton as lowest in tar with only 4 mgs. '43%, lower in tar than the brand I thought was lowest.'"; "Micronite filter. Mild, smooth taste. For all the right reasons" (Kent).

Figure 2.7 (Continued)
(b) Women



Smokers might also reduce their risk by smoking fewer cigarettes. The average smoker in the late 1970s smoked 22 cigarettes per day. College graduates smoked 2 fewer cigarettes per day than high school graduates, but the difference is not statistically significant.

III. Cigarette advertising and health information

The question we ask is whether these levels and trends can be explained by differential responses to changes in information and image/advertising. From 1950 to 1980, cigarette advertising and information regarding smoking's consequences evolved substantially. Importantly, many key changes stemmed from non-market processes such as new research findings and government regulation. In this section, we explain these changes briefly; more detailed discussions can be found elsewhere (Brandt, 2007; Calfee, 1985; Friedman, 2014).

To some extent, the health risks of smoking were long known. Many states banned the sale of cigarettes to minors as early as 1900 and to adults in the first decade of the twentieth century. Cigarettes had developed a reputation as “coffin nails” by the 1920s. Thus, firms always faced a choice between advertising for brand image and advertising for relative health.

Unconstrained, tobacco companies competed over health concerns. In the 1920s and 1930s, Old Gold advertised that there was “Not a Cough in a Carload” (1927-1935), and Kool proclaimed that, “If you want to guard that throat of yours, then KOOL it is” (1936). The high volume and perceived success of such advertising brought regulatory attention to these claims. In the late 1930s, the Federal Trade Commission (FTC) decided that unsubstantiated health claims in cigarette advertisements were unacceptable, and began issuing cease-and-desist orders for such ads. In 1941 and 1942, the FTC initiated legal cases against several major cigarette companies, leading to a general pause on such advertising until the litigation concluded in 1951.

In 1950, the first controlled epidemiological studies linking smoking and lung cancer were published. Their findings reached the popular press in the form of a *Reader’s Digest* article, “Cancer by the Carton,” published in December 1952, and a follow-up article in 1954. *Readers Digest* and *Consumer Reports* published brand-specific tar and nicotine levels in 1952 and 1953.

Responding to these events, tobacco companies built new advertising campaigns around health claims. Advertisements stated that: “Nose, Throat, and Accessory Organs not Adversely Affected by Smoking Chesterfields,” (1952); “the difference in protection is priceless,” (Kent, 1952); and “Takes the Fear out of Smoking,” (Philip Morris, 1953). Yet such “fear advertising” may have hurt the industry more than it helped: per capita cigarette sales fell 10 percent over the 1952-54 period, the industry’s largest decline until the last decades of the 20th century.

Recognizing the threat health fears posed to the industry, the tobacco companies formed the Tobacco Industry Research Committee in 1953. Among its first acts was to publish “A Frank Statement to Cigarette Smokers,” assuring smokers that the industry was interested in their health, did not believe that their products were injurious to it, and would contribute funding to research focused on the relationship between tobacco and health. The goal, of course, was to sow sufficient doubt about cigarettes' health effects that people would continue smoking (U.S. House of Representatives, 1994).

In 1955, the FTC released its Cigarette Advertising Guides, which barred all health claims from cigarette advertisements, positive or negative, and prohibited tar and nicotine claims until "competent scientific proof" established both the health claim's veracity and the significance of any difference between the product in question and its competitors.[64] Advertising shifted to focus on taste, along with highlighting a filter's presence but not its efficacy (such a claim would violate the FTC guidelines). Ironically, the FTC's actions may have led to an adverse effect on smokers, as barring health claims effectively prohibited fear advertising and hindered the growth of lower tar and nicotine cigarettes introduced in the early 1950s, since firms could no longer advertise these characteristics. Cigarette sales rebounded in this period.

In the summer of 1957, however, the head of Sloan-Kettering's Institute for Cancer Research testified to Congress, stating that substantive tar and nicotine reductions were both feasible and likely to reduce smokers' cancer risks. Tobacco firms interpreted this testimony as satisfying the 1955 guidelines' requirement of "competent scientific proof" that tar and nicotine impact health outcomes, and resumed advertising their brands' tar and nicotine levels. The period of advertising between 1957 and 1959 is sometimes referred to as the "Great Tar Derby." Moreover, *Reader's Digest* and *Consumer Reports* again published brand-specific tar and nicotine levels. As in the earlier time period, average nicotine levels dropped with this advertising, falling approximately 30 percent from 1957 to 1960 (Calfee, 1985).

[64] The FTC informed major tobacco companies regarding the Guides a year in advance. Industry cooperation was to some extent voluntary, as enforcing a number of the proposals was beyond the FTC's legal powers. Yet, by the time the Guides went into effect, the vast majority of cigarette advertising met the new standards.

Policy ended this era, too. At the end of 1959, the FTC announced that advertising reduced tar and nicotine levels would be considered a health claim and that such claims were only allowable if scientific evidence established the associated health benefits. As lower-tar cigarettes had not been available long enough to allow evaluation of their long-term health effects, this requirement effectively halted advertising of tar and nicotine levels. By 1960, the FTC had negotiated an industry-wide ban on nicotine and tar claims.[65] Advertising returned to themes of filters and flavor, and cigarettes' tar and nicotine content stopped falling.

The 1960s saw enormous attention to the health consequences of smoking. In 1962, the British Royal College of Physicians concluded that smoking caused both lung cancer and chronic bronchitis, and most likely contributed to heart disease. In 1964, the U.S. Surgeon General's Advisory Committee on Smoking and Health reached similar conclusions. Legislative and private actions followed. In April 1964, the tobacco industry announced a Cigarette Advertising Code, banning advertising and promotion aimed at individuals younger than 21 as well as in school and college publications. Federal law in 1965 codified a warning on cigarette packages: "CAUTION: Cigarette Smoking May Be Hazardous to Your Health." By 1966, a variety of health groups, including the American Cancer Society, were encouraging smokers to choose lower tar cigarettes.

Mid-way through 1966, the FTC announced that it would no longer consider tar and nicotine claims in cigarette advertising to be misleading. Moreover, the FTC began producing its own brand-specific tar and nicotine estimates in 1967, releasing them annually.

Regulation continued as well. In 1967, the Federal Communications Commission concluded that the Fairness Doctrine required broadcasters to donate time for one anti-smoking

[65]A variety of organizations on both sides of the issue objected to this agreement, including the American Cancer Society and manufacturers of low-tar and low-nicotine cigarettes.

announcement for approximately every three cigarette commercials aired. A substantial body of research has shown that this policy reduced cigarette consumption (Warner, 1977; Lewit, Coate, and Grossman, 1981). Partly as a result, tobacco companies came to support an even stronger policy – a ban on broadcast advertisements for cigarettes, which took effect on January 2, 1971. By removing cigarette advertising from the airwaves, anti-tobacco advertising slots allotted via the Fairness Doctrine were dropped as well.

Additional restrictions followed. In 1972, tobacco companies agreed to publish health warnings on all cigarette advertisements. In 1975, Minnesota became the first state to ban smoking in a variety of public places, and other states soon followed.

Figure 2.8 summarizes this history in two dimensions: health information disseminated to the general public, and the nature of advertizing – image only, or also based on health claims. Between 1950 and 1980, public health information changed markedly, with three key events: the Reader's Digest article in December 1952; the reports on smoking from the Royal College of Physicians and the Surgeon General; and the Fairness Doctrine. Over this same period, advertising shifted back and forth between two modes: unconstrained, in which many tobacco companies emphasized the relative healthiness of their product and information on brand-specific tar and nicotine levels was generally available, and constrained, when tobacco companies were prohibited from making health claims and current brand-specific tar and nicotine information was not available.

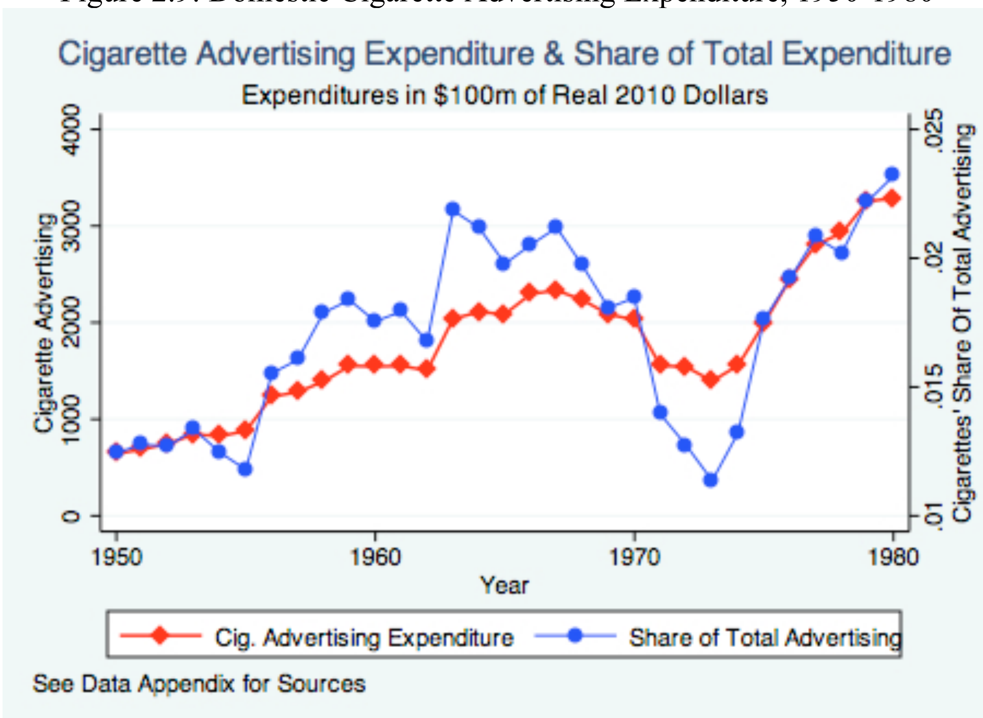
.

Figure 2.8: Advertising and Health Information

	1950s										*1960s*										*1970s*	
	'50	*'51*	*'52*	*'53*	*'54*	*'55*	*'56*	*'57*	*'58*	*'59*	*'60*	*'61*	*'62*	*'63*	*'64*	*'65*	*'66*	*'67*	*'68*	*'69*	*'70*	*'71-'80*
Tar & Nicotine Availability	*No (Health-Claims Litigation)*		*Yes*				*No (FTC Guides)*		*Yes*		*No (FTC-negotiated ban on all nicotine and tar claims in cigarette advertising)*							*Yes*				
Public Health Information				*Cancer by the Carton*									*U.K. & U.S. government reports conclude that smoking causes lung cancer*					*Fairness Doctrine (Anti-smoking PSAs)*				*Broad-cast Ban*

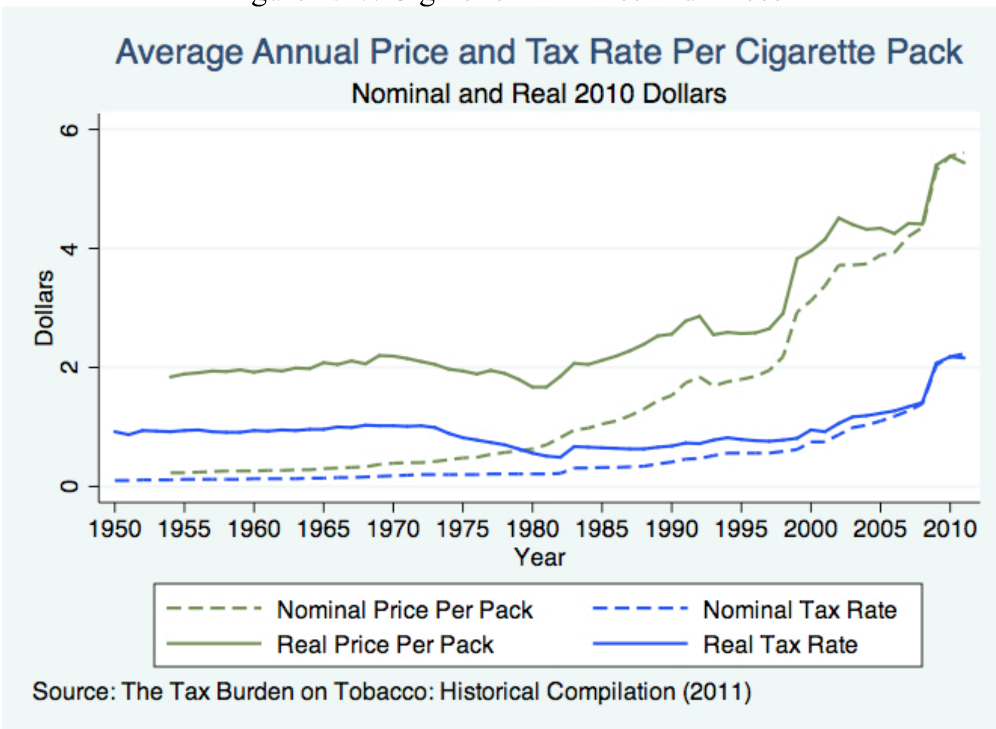
Partly as a result of these changes, advertising spending for cigarettes was quite variable. Figure 2.9 shows that real cigarette advertising grew rapidly from 1950 through the mid-1960s before declining markedly with the fairness doctrine and the broadcast ban. Spending rose again in the mid-1970s, as firms substituted into print media and other marketing (e.g., event sponsorships, sample distribution). This variation in advertising expenditure helps us in our time series analyses.

Figure 2.9: Domestic Cigarette Advertising Expenditure, 1950-1980



Prices varied too, as taxes on cigarettes were alternately increased and then eroded in real terms. Figure 2.10 shows real cigarette tax rates and prices from 1950 to 2011. In real terms, prices rose only mildly in the 1960s (if at all), and then fell noticeably over the 1970s. The pre-1980 price and tax trends are virtually parallel, allowing us to use real tax rates to capture price variation in a manner exogenous to demand.

Figure 2.10: Cigarette Tax Rates and Prices

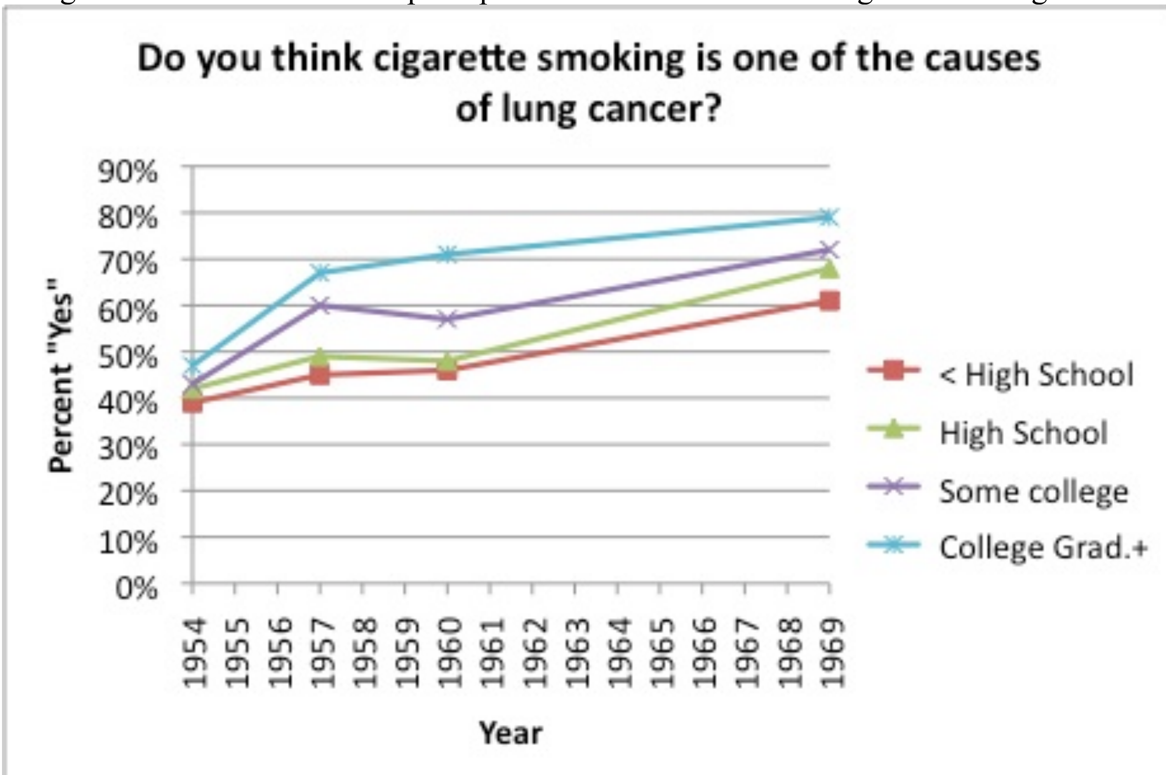


Knowledge about the Health Implications of Smoking

Not surprisingly, understanding of smoking's health effects spread over this time period. A 1954 Gallup survey showed that, in each education group, about 40 to 50 percent of people thought that smoking was one of the causes of lung cancer (See Figure 2.11). Just three years later, knowledge of this link had increased 20 percentage points among college graduates, but less than 10 percentage points among high school dropouts or graduates, yielding a 20 percentage point knowledge gap between the most and least educated. This gap persisted over the 1960s, as knowledge expanded among all education groups.

Lining up these trends with the incidence and cessation data in figures 5 and 6 suggests a possible impact of knowledge on behavior. As with the knowledge gap, cessation rates by education begin diverging in the mid-to-late 1950s, with initiation trends also growing apart in these years. Our time series analysis considers this more fully.

Figure 2.11: Percent of Gallup Respondents Who Think Smoking Causes Lung Cancer



Source: Gallup Organization (1954, 1957, 1960, 1969)

IV. Time Series Evidence

Our first empirical analysis relates smoking initiation and cessation to the factors noted above: advertising, public health information, and cigarette prices. Since our focus is on education differentials in the impact of these variables, we estimate models interacting these variables with a dummy variable for high education (some college, college graduate).[66] Our regressions are of the form:

$$\text{Smoke}_{it} = \beta_1 \text{Ad\$}_t{*}\text{HighEd}_i + \beta_2 \text{TN}_t{*}\text{HighEd}_i + \beta_3 \text{PH}_t{*}\text{HighEd}_i + \beta_4 \text{Tax}_t{*}\text{HighEd}_i + X_{it}\beta + \gamma_t + \varepsilon_{it}. \qquad (4)$$

[66] We also considered a specification with an added set of low-education (did not graduate high school) interactions. These results had the same implications as the one-interaction analysis. We favor the latter because it is both easier to interpret and, with high school graduates in the reference group, a more conservative approach.

The dependent variable is an indicator for smoking initiation or cessation for person *i* in year *t*. $Ad\$_t$ refers to real cigarette advertising expenditure (in hundreds of millions of 2010 dollars); TN_t is a dummy variable indicating whether tar and nicotine levels were available and could be advertised in year *t*; PH_t is a dummy representing the dissemination of new public health information; and Tax_t gives the real cigarette tax rate, in 2010 cents. The PH dummy is defined as in Figure 1.8, with dummies for 1953 (the *Readers' Digest* article), 1962-64 (the British and American government reports), and 1967-70 (the Fairness Doctrine).[67] TN takes on the value of 1 in 1952-54, 1957-59, and 1967-onwards.

The year dummy variables are shown as γ_t. Year effects pick up the trends for those with a high school degree or less. Other controls (X_{it}) include binary indicators for education (fewer than 8 years, high school dropout, some college, college graduate), age, decade of birth, survey year, ever draft eligible, race (black and other non-white), Hispanic ethnicity, Census region, urbanicity, veteran-status-by-war, and indicators for missing ethnicity and veteran status.

The regressions for initiation focus on individuals aged 14-22 and cover 1950 through 1980. Once a respondent has begun smoking regularly, they are dropped from the sample. The cessation analysis is limited to ever-smokers ages 14-46 over the same period. Individuals are dropped once they quit. Since people are included in multiple years, we cluster the standard errors by respondent.

[67] A debate exists on how the dissemination of health information should be modeled in the context of cigarettes, particularly with respect to the 1964 Surgeon General's report and the Fairness Doctrine (e.g., Schneider, Klein, and Murphy, 1981). We assume a contemporaneous effect, in part because lag structures are not easily distinguished in such a short panel. We do not use filtered or low tar cigarettes' market share as a proxy for health knowledge for a specific reason: this conflates the effect of health information and events on consumer knowledge with the impact on supplier constraints and behavior. Specifically, the FTC prohibited advertising of cigarettes' tar and nicotine levels in several periods between 1950 and 1970, with this restriction predicated on the idea that such information could not be advertised unless the related health claims had been substantiated. This restriction ended two years after the 1964 SGR, but it was not in effect for 2 to 3 years in the late 1950s either (the "Great Tar Derby"). Unsurprisingly, cigarettes' tar and nicotine levels fell and the low tar market share grew in both periods: firms were able to advertise these attributes, and they did so. Thus, using increased low tar market share as a proxy for changes in consumer information due to the Surgeon General's Report alone seems misleading. The former may not have occurred absent the change in FTC restrictions and its effect on cigarette advertising.

A central issue in equation (4) is the timing of the information, advertising, and prices, relative to the decision to initiate or quit smoking. In the models we present, we assume the link is contemporaneous. We have experimented with lags of the independent variables. Given the strong time series correlations and the relatively short panel we employ, it is difficult to tell apart any particular lag structure.

The first two columns of Table 2.3 present average marginal effects (AMEs) for smoking initiation models, separately by gender.[68] The results show statistically significant effects of advertising dollars on initiation, with AMEs indicating initiation reductions of 0.13 and 0.14 percentage points for every $100 million in advertising spending, among high education males and females, respectively. Thus, advertising dollars have a smaller impact on initiation for more educated people than for less educated people. We consider the magnitude in more detail below. The reason for this differential is clear from Figure 2.5. Cigarette advertising declined markedly with the fairness doctrine and the first years of the broadcast ban (late 1960s and early 1970s), before rebounding and climbing quickly in the mid to late-1970s. In the same period, smoking initiation among high school dropouts (of both sexes) and female high school graduates shows a similar rebound, while rates for college graduates continue to fall.

The availability of tar and nicotine information also affects smoking initiation, again more so for the better educated. AMEs indicate that, when this information is available, high education men and women show initiation reductions of 1.0 and 0.7 percentage points, respectively.

Somewhat surprisingly, the impact of public health information and taxes on smoking initiation does not differ greatly by education for men, and shows a counterintuitive-response among women: initiation is 1.1 percentage points *higher* among more educated women in

[68] See Appendix Table A2.1 for the corresponding odds ratios.

periods when significant public health information was disseminated to the general public. An *ex ante* education differential in awareness of smoking's health risks may explain this finding: if most highly educated individuals knew of smoking's health risks prior to the Surgeon General's report and Fairness Doctrine (as indicated in Figure 2.11), we might expect a greater change in behavior among the less-educated in response to these events, as the AMEs suggest.

Table 2.3: Explaining Smoking Initiation and Cessation, 1950-1980
Average Marginal Effects (t-statistic)

	Initiation, Ages 14-22		**Cessation, Ages 14-46**	
Independent Variable	**Men**	**Women**	**Men**	**Women**
Cig-Ad$_t$ $ * > HS Grad	-0.0013*	-0.0014*	0.0003*	-0.0002
	(-2.84)	(-4.40)	(2.05)	(-0.72)
Yes Tar-Nic. * >HS Grad	-0.0102*	-0.0070*	-0.0004	0.0020
	(-2.20)	(-2.06)	(-0.28)	(1.07)
Public Health Info * >HS Grad	-0.0006	0.0107*	-0.0050*	0.0005
	(-0.10)	(2.56)	(-2.72)	(0.24)
Cigarette Tax Rate$_t$ * >HS Grad	0.0002	-0.0005**	0.0002*	0.0000
	(0.44)	(-1.95)	(2.40)	(0.14)
Education Groups (ref: H.S. Graduates)				
< 8 years of School	-0.0025	-0.0157*	-0.0051*	-0.0064*
	(-0.24)	(-2.00)	(-2.67)	(-2.87)
Did Not Graduate High School	0.0362*	0.0230*	-0.0053*	-0.0074*
	(7.83)	(7.84)	(-5.17)	(-6.55)
Completed Some College	-0.0057	0.0706*	-0.0210*	0.0042
	(-0.13)	(2.64)	(-1.99)	(0.29)
College Graduate +	-0.0424	0.0534*	-0.0154	0.0097
	(-0.97)	(2.00)	(-1.46)	(0.68)
N	81,492	119,962	212,738	181,303
Pseudo-R^2	0.046	0.040	0.051	0.064

Note: All regressions include fixed effects for year, race, ethnicity (including a missing ethnicity indicator), Census region, urbanicity, Veteran status (by war), ever draft eligible, survey year, and decade of birth. Cigarette advertising expenditure is in hundred-millions of 2010 dollars. Tax rates are in 2010 cents. The sample for the cessation analyses is individuals who smoked in the prior year (i.e., potential quitters). ** (*) denotes statistical significance at the 10% (5%) level.

Both genders exhibit small, statistically insignificant initiation responses to cigarette taxes. This may stem from the fact that most U.S. initiation occurs among teenagers, before income differences associated with education are realized. As younger teens are less responsive to cigarette taxes than older teens, initiation responses to tax rates may be further dampened among this subgroup (Gruber and Zinman, 2001).

The second set of columns in Table 2.3 presents results for cessation regressions. Notably, women exhibit no statistically significant cessation differentials, perhaps because the change in women's smoking over this period was driven primarily by initiation (See Table 2.2). Among men, advertising dollars appear more effective at retaining less educated smokers than more educated ones. The corresponding AME indicates a statistically significant 0.03 percentage point increase in cessation among high education males per $100 million of advertising expenditure. In contrast, public health information has a larger effect on cessation for less educated males: high education males show a 0.5 percentage point decrease in cessation associated with such events. As with the female initiation response to such information, this might be explained by the better educated learning about public health information earlier or in other venues, such that the events captured in our public health variable transmitted information that was not new to as many high education smokers. Indeed, the reduction in smoking among high education males begins in the late 1940s, before "Cancer by the Carton" was published. Finally, cigarette taxes have a larger effect on cessation among more educated males. However, as real cigarette tax rates decreased over the period in question, this AME would counteract growth in the smoking gap. Thus, the only statistically significant positive contributor to growth in the male cessation gradient is the increase in advertising dollars over time.

To examine the quantitative importance of these coefficients, we conduct a simulation exercise. We use coefficient estimates from the Table 2.3 regressions and data on pre-1951 advertising expenditure, tax rates, and information to predict the probability of ever having been a regular smoker as of 1950 for individuals who turned age 14 in or before that year.

We then estimate the empirical derivative of the initiation decision as we change each of the independent variables: advertising, tar and nicotine information, public health information, and cigarette taxes. We use this to empirically predict the change for the 1980 population, assuming they were otherwise the same as the 1950 population but for changes in the policy variable. We conduct an analogous estimation for cessation. Together, the initiation and cessation results allow us to estimate, for each education group, an expected 1980 smoking rate due to differential responses to each independent variable, which we compare to the actual change in smoking over time.

Table 2.4 presents our estimates of the percent of observed changes in the education gap between high school dropouts and college graduates that can be explained by each factor. The first row shows the actual change in the education gap over time, and the second the predicted change given the average initiation and cessation rates by age in the relevant years. The remaining rows show the decomposition.

Table 2.4: Decomposing the Changing Gap in Smoking Rates, 1950 to 1980
(Δ = High School Dropout - College Grad)

	Men		**Women**	
	Percentage Points	**Percent of Observed Change**	**Percentage Points**	**Percent of Observed Change**
Observed $\Delta Gap_{1980-1950}$	18.5		25.1	
Predicted $\Delta Gap_{1980-1950}$	18.7		25.1	
ΔGap Explained by Differential Initiation and Cessation Responses to:				
Advertising	7.32	39.2%	6.73	26.8%
Tar & Nicotine Information	2.41	12.9%	1.93	7.7%
Tax Rates	0.07	0.4%	-2.67	-10.6%
Public Health Info	-0.16	-0.8%	-0.80	-3.2%

Note: The table shows how advertising dollars, the availability of tar and nicotine information, cigarette taxes, and public health information affect the differential in smoking between high school dropouts and college graduates. Results are based on the regressions in Table 2.3.

Our estimates attribute 39 percent of growth in smoking's education gap among men and

27 percent among women to differential responses to advertising. Given the predominant targeting of cigarette advertising to men over this time period, the larger effect among males makes sense. Differential responses to tar and nicotine information also contribute to the gradient, accounting for 13 percent of observed growth in the smoking gap for men and 8 percent for women. Neither differential tax rates (recall that these were not statistically significant for women) nor differences in public health information explain a large share of the gap, though health information surely contributes to the impact of tar and nicotine information.

Overall, then, our time series analysis suggests that both advertising and tar and nicotine information explain a substantial amount of the reduction in smoking over time. These effects are evident for both genders and all act through differential initiation responses, except for an additional differential cessation response to advertising among men. This second response contributes to the larger impact of advertising on growth in the smoking gap among men.

IV. Brand Choice and Number of Cigarettes

We can further examine advertising and health information by considering the cigarettes that smokers choose to smoke. By the late 1970s, cigarettes were known to fall into one of three categories: straights (unfiltered); Lo-Fi (low filtration) and Hi-Fi (high filtration). To examine the separate roles of advertising and information, we split the Hi-Fi market into two subgroups. The first group represents freestanding brands, whose parent brand includes only Hi-Fi cigarettes (e.g., Carlton and Merit). Marketing for these cigarettes generally focused on their relative safety. The second group is line extensions – cigarettes for whom the parent brand includes a Lo-Fi and/or unfiltered base-cigarette, but with a Hi-Fi extension (e.g., Marlboro Lights). The idea behind Hi-Fi line extensions was to shore up market share against losses to lower-tar brands.[69] Thus, firms built their line extension brand images and slogans off of the base brand, with catch phrases like, "The spirit of Marlboro in a low tar cigarette," for Marlboro Lights (1975), or "Taste Winston Lights. The low tar cigarette that's all Winston. All taste." for Winston Lights (1978).

The distinction between cigarettes whose only marketing constraint is their health profile, versus those who also come with a parent brand profile, allows us to contrast the importance of health information and brand image. If health information is the important variable driving differentials in cigarette choice, better educated people will choose Hi-Fi cigarettes but will be no more likely to choose freestanding Hi-Fi's over line extension Hi-Fi's. If brand image is important, however, we expect less educated people to favor those with a pre-existing brand image over freestanding Hi-Fi's.

We model this using a standard framework. Suppose that utility for person *i* in considering each brand choice *j* is expressed as:

$$U_{ij} = \beta_1 * \text{FreestandingImage}_j * \text{HighEd}_i + \beta_2 * \text{Safe}_j * \text{HighEd}_i + X_{ij}\beta + \varepsilon_{ij}. \quad (5)$$

FreestandingImage is a binary indicator signifying that the brand's image was not anchored to a pre-existing parent brand image. Safe is an indicator for Hi-Fi cigarettes. If the more and less educated differ primarily in the importance they attach to brand image, β_1 will differ from 0. Conversely, if the more and less educated differ primarily in the importance they attach to risk reduction, β_2 will differ from 0.

Note that price is not included in equation (5). A 1979 duty-free price schedule shows identical prices for 21 of the top cigarette brands, including representatives from each major

[69] For example, see R.J. Reynolds' (1977) characterization of their decision to launch Salem Lights as a "defensive opportunity" to address "switching losses to Hi Fi brands" (p.138).

category (straight, Lo-Fi, and Hi-Fi; see NCL, 1979). Thus, we cannot estimate cross-brand price elasticities.

Equation (5) lends itself naturally to an alternative specific conditional logit model. We estimate such a model using the 1978-80 brand choice data. Rather than estimating by brand, however, we estimate consumption of one of three brand groupings: straight or Lo-Fi brands, Hi-Fi brand extensions, and freestanding Hi-Fi brands. The reason for this is that we do not have data on the specific marketing characteristics of each cigarette (e.g., its appeal to ruggedness or independence). We have very good data, however, on how safety is presented. Effectively, we are assuming that consumers first choose a nest based on safety vs. image profile, and then choose a specific brand within the relevant nest.

This analysis omits Lo-Fi and unfiltered cigarettes whose brands do not have a Hi-Fi line-extension. Cigarettes within a given Hi-Fi line-extension can be categorized as offering smokers one brand-image (often originating in the 1950s or earlier) at several risk levels. Freestanding Hi-Fi brand images can be contrasted with those of line extension Hi-Fi's: firms could tailor freestanding brand images without constraints to suit a Hi-Fi market, whereas cigarettes from the latter group were constrained by a broader parent brand image. This allows us to contrast line extension brands' images more generally with a distinct "freestanding image" arising from the common risk-profile, market, and context (i.e., lack of constraints) of freestanding Hi-Fi cigarettes. It does not, however, apply to non-Hi-Fi freestanding cigarettes. Freestanding Lo-Fi and unfiltered brands range from long-established unfiltered cigarettes (e.g., Chesterfield) to more recent Lo-Fi entries (e.g., More), to Lo-Fi cigarettes initially marketed as Hi-Fis (e.g., Lark). Such broad variation pushes against assigning these cigarettes to a single image indicator. Moreover, less that 5 percent of smokers in our sample favor these brands. Thus, we omit them from the safety-vs.-image analysis entirely.[70]

In addition to the interaction terms noted above, we include a variety of other control variables (X): race, Hispanic ethnicity, survey year, Census region, SMSA status, veteran by war, ever draft eligible, and a fourth-order polynomial in age, as well as an indicator for ever married and missing marital status, and a series of dummy variables for interview-year income earned by the household's 'breadwinner.' To account for omitted sources of heterogeneity, we cluster standard errors by census-region.

Table 2.5: Brand Choice Analyses, Alternative Specific Conditional Logistic Regressions (Base=Unfiltered/Lo-Fi Line-Extension Cigarettes), Relative Risk Ratio (t-statistic)

	Men	**Women**
Differential Response to Brand Safety		
Hi-Fi * > HS Grad	1.6591*	1.3757*
	(6.05)	(3.49)
Differential Response to Brand Image		
Freestanding Hi-Fi * > HS Grad	1.2797*	1.2507
	(2.31)	(1.26)
N		
Number of Observations	16,932	15,906
Number of Cases	5,644	5,302

Notes: The sample in each case is smokers ages 25-64 who list a cigarette brand that is a Hi-Fi and/or belongs to a parent brand with a Hi-Fi line-extension. In addition to the variables listed above and a fourth-order polynomial in age, the following controls are included these regressions, all as dummy variables: income of household breadwinner (by NHIS grouping), survey year, census region, urbanicity, Veteran status, Hispanic ethnicity, non-white race, ever married, and an indicator for missing marital status. ** (*) denotes statistical significance at the 10% (5%) level.

Table 2.5 presents the regressions described above. A clear education differential is evident in demand for safer cigarettes, with statistically significant odds ratios of 1.66 and 1.38 for men and women, respectively. While higher education smokers also exhibit a greater demand for the freestanding brand image, these effects are smaller—1.28 and 1.25 for men and women,

[70] A multinomial logistic analysis including these omitted observations is presented in the Appendix as Table A2. That regression focuses exclusively on whether there is an education gradient in the choice of cigarette-type (i.e., Hi-Fi or Lo-Fi, relative to unfiltered), since we cannot assign the non-line-extension Lo-Fi and unfiltered cigarettes to a single image indicator. It finds a clear education differential in the choice of lower risk cigarettes, though we cannot (from that analysis) attribute the findings to differential demand for safety versus the freestanding-Hi-Fi brand image.

respectively—and only statistically significant for men. The effect of the risk-reduction response among men is more than twice the corresponding brand-image response.

These results indicate that high education smokers exhibited greater demand for both safer cigarettes and the newer freestanding brand-image, with the former a substantially larger influence than the latter. This demonstrates another dimension to smoking's education gradient along the intensive margin: education differentials in brand choice shaped by both differential demand for risk reduction and differential brand-image appeal.

Number of Cigarettes Smoked

Another way to estimate the importance of risk reduction is to examine daily cigarette consumption. If better educated smokers are more worried about the harms of cigarettes, they should smoke fewer cigarettes. The NHIS asks about cigarettes smoked per day. While this analysis is informative, it is not definitive. In particular, we do not know how much people would have smoked in the absence of health and advertising information, or whether/how certain smokers changed their behavior to alter nicotine doses (e.g., by smoking more of the cigarette or inhaling more deeply). Still, the analysis is suggestive.

We estimate three specifications for the cigarettes per day analysis: one including all current smokers, the second limited to current smokers who indicate a usual cigarette brand, and a third using the latter sample and controlling for cigarette type (Hi-Fi, Lo-Fi, or Straights).

Table 2.6 considers the education gradient in daily cigarette consumption, controlling for the same variables as in Table 2.5. The first three columns are for men, and the second three are for women. In every specification and for both male and female smokers, college graduates smoke statistically significantly fewer cigarettes in a day. The decline is about 1.5 cigarettes for men and 2.2 cigarettes for women. Controlling for filtration category has little to no effect on these results. Indeed, use of filtered cigarettes is associated with neither a statistically significant reduction in cigarettes smoked (as a complement to smoking safer cigarettes) nor increased daily consumption (to make up for lower tar and nicotine in each cigarette).

Table 2.6: Cigarettes Per Day Among Current Smokers, Coefficient (Standard Error)

	Men			Women		
Sample Limitations:	Full	Brand-Listed	Brand-Listed	Full	Brand-Listed	Brand-Listed
	(1)	(2)	(3)	(4)	(5)	(6)
Education						
< 8 years of School	-1.05	-0.92	-0.90	-1.18	-1.12	-1.18
	(-0.71)	(-0.85)	(-0.80)	(-1.11)	(-1.11)	(-1.16)
High School Dropout	1.11**	1.09	1.04	1.06	1.12	1.08
	(-0.47)	(-0.50)	(-0.49)	(-0.98)	(-0.98)	(-0.93)
College Dropout	0.34	0.40	0.36	-0.34	-0.22	-0.23
	(-0.59)	(-0.54)	(-0.53)	(-0.20)	(-0.22)	(-0.25)
College Grad. +	-1.51*	-1.51*	-1.51*	-2.23*	-2.16*	-2.17*
	(-0.43)	(-0.42)	(-0.37)	(-0.33)	(-0.25)	(-0.29)
Cigarette brand						
Hi-Fi			-0.80			-1.45
			(1.01)			(0.80)
Lo-Fi			-1.31			-1.61
			(0.85)			(1.15)
N	5585	5435	5435	5378	5301	5301
R^2	0.115	0.117	0.118	0.078	0.080	0.080
F-test: H.S. Drop = College Grad.+	0.007*	0.005*	0.001*	0.036*	0.033*	0.032*

Note: The sample is smokers ages 25-64. Additional controls include fixed effects for age, income of the household breadwinner (by NHIS bin), race, ethnicity, ever married, Census region, urbanicity, Veteran-status (by war), ever draft eligible (if male), and survey year, as well as indicators for missing ethnicity and unknown if served). ** (*) denotes statistical significance at the 10% (5%) level.

Summary

Together, our findings on brand choice and daily cigarettes consumption support the conclusions that higher education smokers derive greater utility from risk reduction, and that brand-specific factors such as brand-image and marketing also seem to contribute to education differentials in brand choice.

The brand choice findings may help explain the relatively low contribution of cessation to education differentials in smoking. While a non-smoker can respond to a change in his demand for smoking-related risk reduction by not initiating, smokers can adjust behavior on the intensive as well as the extensive margin.

If these findings generalize to the nonsmoking population, they may help explain the differential initiation and cessation responses documented in Table 2.3. Specifically, differential demand for brand image and risk reduction could drive differential responses to cigarette advertising as well as tar and nicotine information.

V. Conclusion

This analysis offers three key findings. First, we find that the education differences in individuals' responses to cigarette advertising as well as brand specific tar and nicotine information contributed to growth in smoking's education gradient from 1950 to 1980. Respectively, these differential advertising and information responses explain 39 and 13 percent of growth in the gradient for males, and 27 and 8 percent for females, operating through differential smoking initiation for both genders, as well as a differential cessation response to advertising among men. Second, education differentials in smoking extend beyond initiation and cessation, to shape cigarette brand choice and daily cigarette consumption. Third, education differentials in brand choice appear to be the product of both brand marketing/image and differential demand for risk reduction, with the latter exhibiting a significantly larger effect.

Our results offer a variety of promising directions for future research. Perhaps the lowest hanging fruit would be examining whether education differentials in other health behaviors, including drinking, using safety devices, and seeking preventive care, also appear to operate through a modifier effect of education on responses to specific stimuli. Similarly, considering education differentials in response to media or stimuli not present during our period of analysis (e.g., the internet) would be quite valuable.

This paper also offers a suggestion on how we might identify the impact of traits associated with education (e.g., conscientiousness, impulsivity) on smoking differentials. Specifically, testing whether there are trait differentials in smoking responses to advertising or information, as well as the size of these effects relative to the estimated education differential, could clarify the extent to which these third factors shape education gradients.

This analysis also has a number of limitations. The information acquired by different education groups might vary in content or timing. In this case, our estimated differential initiation or cessation responses to "information" may merely be responses to different information. Similarly, we cannot analyze differential advertising responses based on actual exposure to cigarette advertising, only total expenditure.[71] One particularly interesting story that we can neither prove nor disprove concerns the relationship between education differentials in smoking's extensive versus intensive margins. Specifically, smokers may switch to Hi-Fi cigarettes as part of a longer-run cessation strategy: the lower nicotine dose could allow them to slowly reduce nicotine consumption, while cost concerns might limit compensating increases in daily cigarette consumption.

The implications of our results for current policy are substantially limited by the period of analysis. However, there is one potentially useful policy implication: to the extent that smokers choose "safer" cigarettes out of a higher demand for risk reduction, brand choice could be used as a means of targeting marginal quitters for cessation interventions. Of course, the cigarette

[71] For 1950 to 1980, there are no data on exposure to cigarette advertising that we could find.

categories referenced here are not used today, and brands currently perceived as “safer” are unlikely to match those of the late 1970s. Still, to the extent that such targeting raises the success rate of antismoking interventions, it may be a path worth pursuing.

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Paper III: Electronic Cigarettes and Adolescent Smoking: Differentiating Gateways, Dual-use, and Harm Reduction

On April 24th, 2014, the FDA released its proposed electronic cigarette regulations for public comment.[72] Ranging from health warnings to a ban on vending machine sales and sales to minors, these regulations are motivated by concern over e-cigarettes' potential long run health effects and, in particular, their possible impact on cigarette smoking. As regular use of e-cigarettes can be less expensive than a smoking habit, is thought to be less risky, and delivers the same addictive substance, some argue that e-cigarettes will reduce smoking by leading smokers and would-be smokers to substitute away from cigarettes (harm reduction).[73] Others contend that these products may exert a gateway effect on smoking or perpetuate the habit via dual use—smokers' use of e-cigarettes as a complement to regular smoking (e.g., to reduce nicotine cravings where smoking is prohibited).

If harm reduction or gateway effects exist and operate via changes in smoking initiation, one might expect to see these among adolescents, as this age group accounts for the vast majority of U.S. smoking initiation. Teens may also be more likely to show harm reduction via cessation: e-cigarette use is associated with a greater intention to quit among smokers in high school, but not those in college (Lee, Grana, and Glantz, 2013; Dutra and Glantz, 2014; Sutfin et al., 2013), and does not appear to be associated with smoking cessation among adults (Grana, Popova, and

[72] When the user inhales on an electronic cigarette, a cartridge containing nicotine, propylene glycol, and other chemicals is heated, emitting both nicotine and vapor that mimics the mouth-feel and look of smoking (hence, "vaping"). These products do not contain tar, the key carcinogenic component of traditional cigarettes.

[73] Consider an August 2009 web-archived post on blu e-cigarettes, citing the price of a blu starter kit as $59.99. Beyond the chargers, batteries, and atomizer, this includes 25 cartridges (a "carton"), which is described as equivalent to 350 cigarettes (Blu Electronic Cigarette Products, 2009). At the average 2009 price of $5.68 per pack, 350 cigarettes would cost $99.40, over 165 percent of the starter kit's price (Orzechowski and Walker, 2012).

Ling, 2014; Adkison et al., 2013).[74] Yet existing research on this cohort has been hampered by a lack of longitudinal data, a focus on population average effects (obscuring differential effects in low versus high propensity to smoke subgroups), and the possibility of third factors driving both e-cigarette use and smoking. Focusing on high school students, this paper addresses these issues and provides first evidence distinguishing harm reduction and gateway effects of e-cigarettes on smoking among high school students.

To address the issues outlined above, I estimate propensities to smoke absent e-cigarettes via a logistic regression of current smoking using data from 2006, the year before e-cigarettes entered the U.S. market.[75] Applying that equation to later years' data, I estimate each respondent's counterfactual propensity to smoke, and use this to assign individuals to propensity to smoke quantiles, allowing me to both estimate within-quantile changes in smoking rates and e-cigarette use over time, and examine whether low, middle, and high propensity to smoke groups differ in their indications of harm reduction or gateway effects.[76] By estimating the effects of changes in e-cigarette advertising on the change in cigarette smoking and on the change in e-cigarette use for each group (adjusted for linear group-specific time trends), and taking the ratio of these, I am able to identify the impact of the change in e-cigarette use on changes in smoking, without inducing concerns about reverse causation or selection based on

[74] Several early studies found increased cessation among adult e-cigarette users, but used non-random samples, such that selection may drive their results. For example, Etter (2010) uses a self-selected sample of individuals who indicate use of e-cigarettes to quit smoking, while Etter and Bullen (2011) recruit their sample via postings on a smoking cessation website. Yet smokers who use e-cigarettes in order to quit are not representative of the broader smoking population: in a study of 1567 adult smokers, Pokhrel et al. (2013) find that those who use e-cigarettes specifically to quit smoking exhibit longer recent quit durations and report higher motivation to quit.

[75] Current smoking is defined as having smoked at least 100 cigarettes and having smoked in the past 30 days.

[76] Note that these regressions are not causal estimations, and thus include factors that may drive or derive from recent smoking as independent variables, as long as these do not fully identify current smoker status—having both smoked 100 cigarettes in one's life and smoked in the past 30 days. The goal here is simply to generate a measure of underlying propensity to be a current smoker absent e-cigarettes, in order to classify respondents into low, middle, and high propensity groups. However, a key assumption (addressed in several specification checks) is that these independent variables are not endogenous to trends in adolescent smoking.

observed e-cigarette use. The identifying assumption here is that group specific linear pre-trends explain variation in smoking and e-cigarette consumption due to outside factors.[77] For clarity, I will begin with a graphical analysis of the data before proceeding with regressions.

Both visual and statistical analyses point to harm reduction among those with the highest propensity to be current smokers, but find no evidence of gateway effects. Multiple specifications find that a 1.0 percentage point increase in ever use of e-cigarettes is associated with an approximately 0.5 percentage point drop in the current smoking rate in the high propensity group. In 2012, this group accounted for about 10 percent of high school students ages 14 to 18, but, depending on how the propensity to smoke regression is specified, comprises 34 to 82 percent of current smoking and 36 to 87 percent of dual use. In the high propensity to smoke group, dual use is negatively associated with cessation: the cessation rate among ever smokers who had ever used e-cigarettes is 1.2 percent, versus 2.0 percent among never-users.

Methods and Data

To begin distinguishing harm reduction, gateway effects, and dual use with the currently available data, I consider how changes in smoking within groups who have a similar *ex ante* propensity to smoke is related to changes in e-cigarette sales and advertising from 2004 to 2012. Growth in both domestic sales and e-cigarette advertising were highly nonlinear from 2004 to 2012, due in part to non-market events (See Figure 3.1). Electronic cigarettes entered the U.S. in 2007, the same year that Ruyan, the Chinese company that invented e-cigarettes, received an international patent. However, the U.S. Food and Drug Administration's (FDA) barred e-

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[77] Changes in both domestic e-cigarette advertising and total domestic e-cigarette sales are markedly non-linear over the period of analysis, due at least in part to a variety of non-market events. The drastic increase in e-cigarette advertising from 2011 to 2012 is entirely accounted for by blu eCigs (Herzog and Gerberi, 2013). Lorillard's acquisition of blu in April of 2012 is the likely driver of this increase, an entrance which came one week after blu settled a longstanding patent infringement claim made against it.

cigarette imports starting in 2008, based on concerns about the product's safety.[78] After the FDA blocked a shipment by Sottera, Inc. in April of 2009, the importer filed suit, challenging e-cigarettes' regulation as drug-device combinations (the legal basis for the FDA ban). The case would last over a year and a half. In the meantime, Paypal cancelled e-cigarette sellers' accounts, and Amazon.com began prohibiting e-cigarette sales on its website. Finally, in December of 2010, the U.S. Court of Appeals found that e-cigarettes should not be regulated as drug-device combinations, but could be regulated as tobacco products under the 2009 Family Smoking Prevention and Tobacco Control Act (Riker et al., 2012).[79] Within a month of this decision, Ruyan announced its intention to sue U.S. companies for patent infringement. Blu eCigs settled Ruyan's claim against it in April 2012 and, within days, was acquired by Lorillard, marking Big Tobacco's entrance into the e-cigarette market on April 24th of 2012.[80] With Big Tobacco's entrance, advertising expenditure on "smoking material and accessories," a category that includes e-cigarettes, grew rapidly, from $2.7 million in 2010 to $7.2 million in 2011, and $20.8

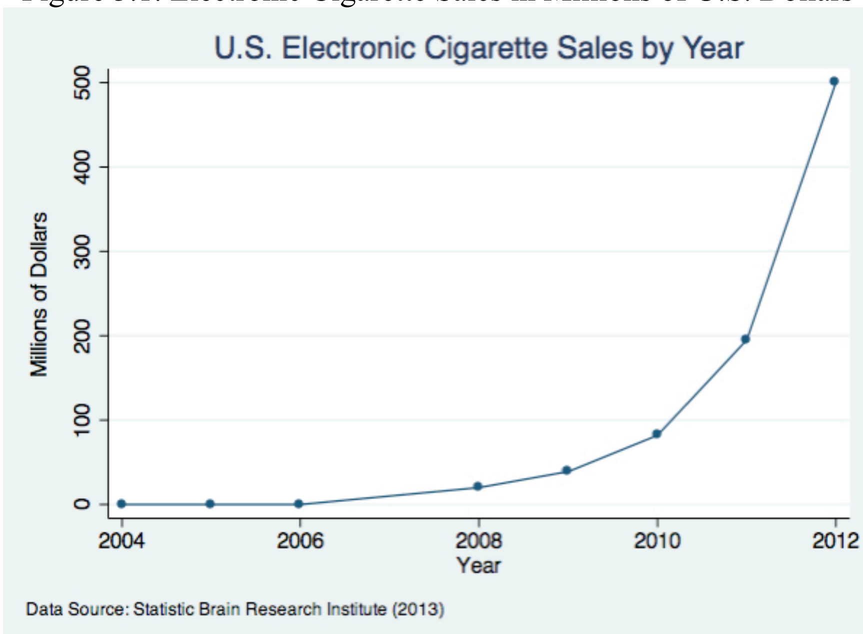
[78] Considering two prominent e-cigarette brands, a 2009 FDA study found substantial variation in cartridge contents. Most cartridges contained potentially harmful tobacco-specific alkaloids, with known carcinogens found in about half of them. All but one of the no-nicotine cartridges contained low levels of nicotine. A single cartridge contained high levels of diethylene glycol, a known toxin (FDA 2009).

[79] While the FDA did not release its proposed e-cigarettes regulations until April of 2014, several states and localities issued their own policies years earlier. For example, state bans on e-cigarette sales to minors went into effect in New Jersey, New Hampshire, and Minnesota in 2010, Colorado and Tennessee in 2011, and Wisconsin, Idaho and Maryland in 2012 (GASP, 2014).

[80] That July, R.J. Reynolds announced its development of Vuse e-cigarettes (Craver, 2012). In 2013, both Altria Group (owner of Philip Morris cigarettes) and British American Tobacco announced plans to introduce their own brands. The logic behind Big Tobacco's entry is fairly straightforward, especially given the industry advantage in navigating tobacco control legislation. Even beyond that, controlling a large share of the e-cigarette market facilitates a wider array of profit maximization strategies for cigarette producers. If the products are complements, the firm can reinforce both brands and further secure its consumer base (e.g., by branding its e-cigarettes to match the target market and brand preferences of its existing cigarettes). If the products are substitutes, it has the added advantage of potentially insulating the firm from switching losses. Indeed, the companies took a similar approach to High Filtration (Hi-Fi) cigarettes' introduction in the mid-20th century, with the largest brands introducing Hi-Fi line extensions (e.g., Marlboro Lights) as a means of shoring up their market share against losses from more health conscious/concerned smokers switching to lower risk brands (Cutler and Friedman, 2014).

million in 2012 (Elliot, 2013).[81] Indeed, advertising by blu eCigs explains the entire 2011 to 2012 increase in total e-cigarette advertising expenditure (Kim, Arnold, and Makarenko, 2014).

Figure 3.1: Electronic Cigarette Sales in Millions of U.S. Dollars



Source: Statistic Brain Research Institute (2013)

The impact of the FDA ban, Sottera v FDA, and Ruyan's patent infringement suit on sales and market entry, alongside the fact that middle and high school students only accounted for 7 percent of ever-use of e-cigarettes in 2011 (as compared to a 2010 to 2011 increase in total U.S. e-cigarette sales from \$82 million to \$195 million), suggests that youth e-cigarette use did not drive total U.S. e-cigarette sales and advertising over the period in question.[82] Notably, analyses of current smoking will control for a linear time-trend, such that reverse causation is not a concern as long as youth smoking does not drive the *nonlinearities* in total e-cigarette sales and advertising.

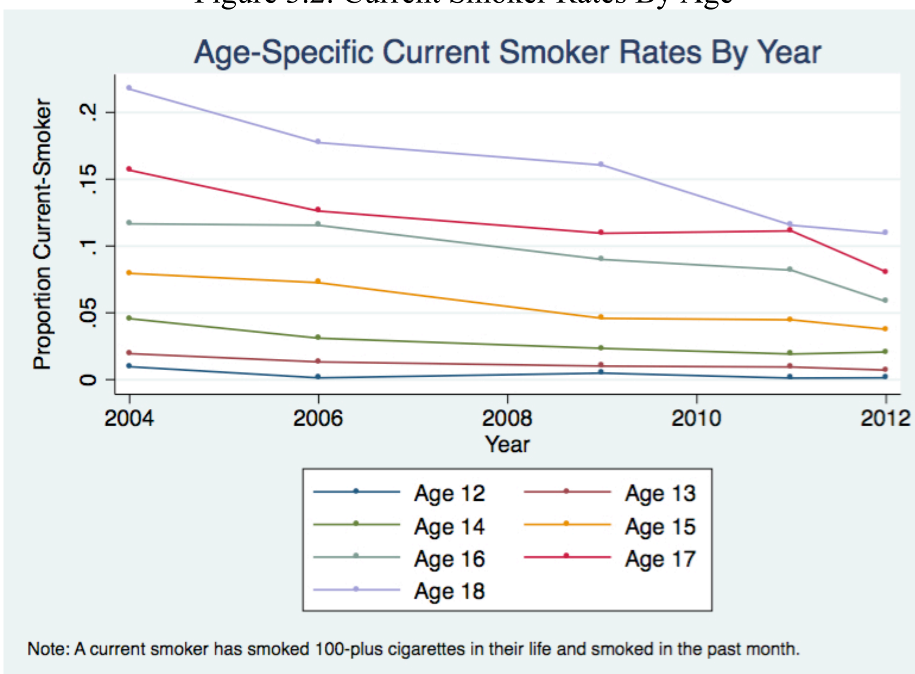
[81] E-cigarette marketing utilizes a variety of promotion platforms long restricted or banned for cigarettes, often due to a concern about targeting youth (e.g., television advertising, cartoon characters, free-sample distribution). And, while brands' tag-lines often focus on the reduced guilt (e.g., blu's "Freedom to have a cigarette without the guilt" slogan, or Fin e-cigarettes' "it's O.K. to smoke again" pitch), a recent ad in *Sports Illustrated*'s swimsuit edition makes it clear that more traditional messaging (i.e., sex sells) is also in play (Elliott, 2014).

[82] This percentage is based on census data on cohort size and 2011 rates of ever use of e-cigarettes among middle and high school students (from 2011 National Youth Tobacco Survey data) and among adults (King et al, 2013).

Using variation to differentiate harm reduction, gateway effects, and dual use, however, requires distinguishing the subgroups that might respond in each way, as the average population effect could easily disguise such variation. Those with a low propensity to smoke would not be expected to develop a cigarette smoking habit absent e-cigarettes, so increased smoking in this subgroup concurrent with a rise in e-cigarette use would suggest a gateway effect. Among those with a high propensity to smoke, reduced smoking alongside increased e-cigarette use would suggest harm reduction, while increased smoking would be consistent with dual-use bolstering smoking habits. Individuals with a middle propensity to smoke would likely include marginal smokers (those for whom smoking's perceived costs barely outweigh its benefits, or vice-versa). Economic theory suggests that marginal smokers would be the most likely to take up e-cigarettes and exhibit gateway effects (e.g., if e-cigarettes raise the benefit from smoking by fomenting nicotine addiction), but also the most likely to show harm reduction (e.g., substituting e-cigarettes for cigarettes, particularly if withdrawal symptoms or losing smoking's social benefits are key costs of cessation). Dual use may be evident here as well. Thus, I estimate propensity to be a current smoker in a manner exogenous to trends in youth smoking, define low, middle, and high propensity groups, and examine within group changes in smoking and e-cigarette use.

Analyses use repeated cross-section data from the 2004, 2006, 2009, 2011, and 2012 National Youth Tobacco Survey (NYTS), a nationally representative survey of students in grades 6 through 12 that collects information on use of various tobacco products, including electronic cigarettes in the 2011 and 2012 surveys. Figure 3.2 presents weighted current smoking rates—having both consumed 100-plus cigarettes in one's life and smoked in the past 30 days—by age and year, limiting the sample to those ages 12 to 18 in grades 7 through 12. As expected, current smoking rates increase with age, with particularly low and invariant rates among 12 and 13 year olds. In part, this may stem from the conventional current smoking definition[83]: 12 and 13 year olds who smoke daily are more likely to have initiated recently, and thus may not have consumed 100 cigarettes yet. To consistently identify current habitual smoking, analyses focus on respondents ages 14 to 18.

Figure 3.2: Current Smoker Rates By Age



Source: National Youth Tobacco Survey data from 2004, 2006, 2009, 2011, and 2012.

Table 3.1 presents weighted summary statistics for 14 to 18 year old high school students by NYTS survey year. These indicate similar demographic traits over time—a mean age of 16, around 50 percent female, 66 to 72 percent white, 16 to 19 percent black—with a marked increase in the percent Hispanic from 2004 (11 percent) to 2012 (20 percent). Experimentation with regular cigarettes (i.e., having tried even one puff) fell 16 percentage points from 2004 to 2012; ever-smoker rates—having smoked 100-plus cigarettes in one's life—dropped 7 percentage points; and, current smoker rates declined 6 percentage points. The similar sizes of the reductions in ever- and current-smoking stem from low adolescent cessation rates: 89 to 91

[83] The smoking literature, particularly in economics, generally distinguishes a "smoker" (whether past or current) from someone who has merely tried a few cigarettes by requiring the former to have smoked at least 100 cigarettes.

percent of ever-smokers had smoked in the past 30 days.

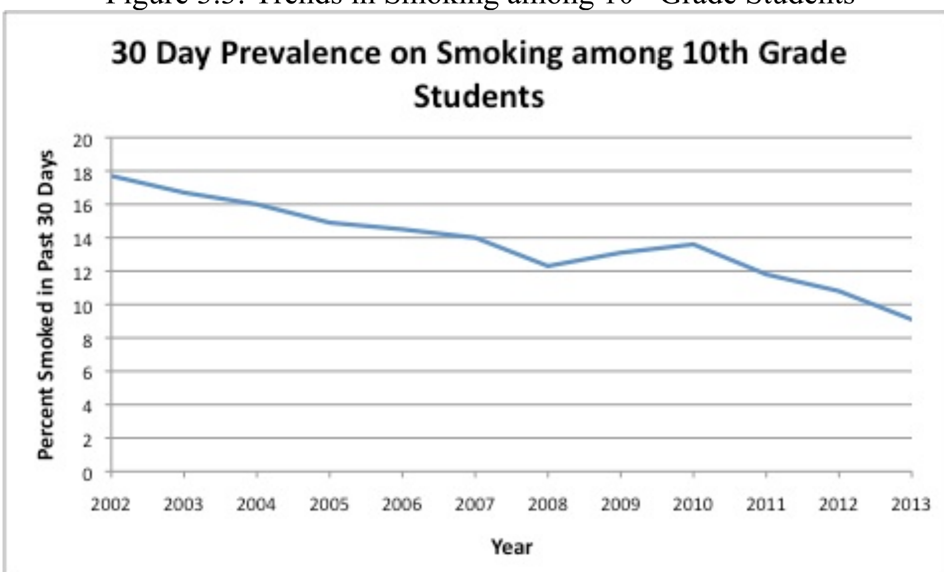
Table 3.1: National Youth Tobacco Survey Summary Statistics, Weighted Means by Survey Year

	2004	2006	2009	2011	2012
N	13,413	13,431	12,093	9,477	12,695
Demographics					
Age	16.0	16.0	16.0	16.1	16.1
Female	51%	51%	49%	49%	49%
Hispanic	11%	13%	17%	19%	20%
Race: White	72%	71%	66%	67%	66%
Race: Black	16%	17%	19%	18%	19%
Race: Asian	5%	4%	5%	5%	6%
Race: American Indian or Alaska Native	3%	4%	4%	5%	5%
Race: Native Hawaiian or Pacific Islander	2%	2%	2%	2%	2%
Smoking behaviors					
Ever tried cigarettes	52%	48%	42%	39%	36%
Ever-smoker: Smoked 100+ cigarettes in life	14%	12%	10%	9%	7%
Current smoker: Ever smoker + Smoked in past 30 days	12%	11%	8%	8%	6%
Ever-smoker who has not smoked in past 30 days	1.2%	1.3%	1.0%	0.9%	0.7%
Ever tried electronic cigarettes	–	–	–	4.5%	10.0%
Used electronic cigarettes in past 30 days	–	–	–	1.4%	2.8%
Smoking-related factors					
Lives with someone who smokes cigarettes	39%	38%	33%	33%	32%
Lives with someone who chews tobacco	9%	11%	10%	9%	9%
How often see actors using tobacco? –Does not watch TV or movies	3%	3%	3%	2%	3%
How often see actors using tobacco? –Never	3%	3%	3%	6%	6%
How often see actors using tobacco? –Rarely	10%	10%	12%	16%	17%
How often see actors using tobacco? –Sometimes	46%	47%	49%	41%	42%
How often see actors using tobacco? –Most of the time	37%	36%	29%	34%	30%
Smoking makes people look cool? –Yes	10%	9%	8%	8%	8%
Smoking makes people look cool? –Probably	12%	12%	12%	12%	10%
Smoking makes people look cool? –Probably not	19%	18%	18%	21%	20%
Smoking makes people look cool? –No	58%	60%	59%	59%	61%
Smoke cigarette if friend offered it? –Yes	3%	3%	3%	3%	4%
Smoke cigarette if friend offered it? –Probably	6%	6%	6%	7%	8%
Smoke cigarette if friend offered it? –Probably not	14%	13%	13%	14%	15%
Smoke cigarette if friend offered it? –No	76%	77%	75%	70%	71%

Source: NYTS data on high school students ages 14 to 18. All statistics are weighted.

Notably, over a quarter of the drop in current smoker rates occurred between 2004 and 2006, prior to both the Great Recession and the 2009 increase in the federal excise tax on cigarettes. Considering Monitoring the Future data on 30 day prevalence of cigarette use among 10th graders, smoking declines at a fairly constant rate from 2002 through 2006, suggesting that controlling for a linear pre-trend will be necessary to account for the fact that cigarette smoking was falling prior to e-cigarettes' U.S. debut (See Figure 3.3).

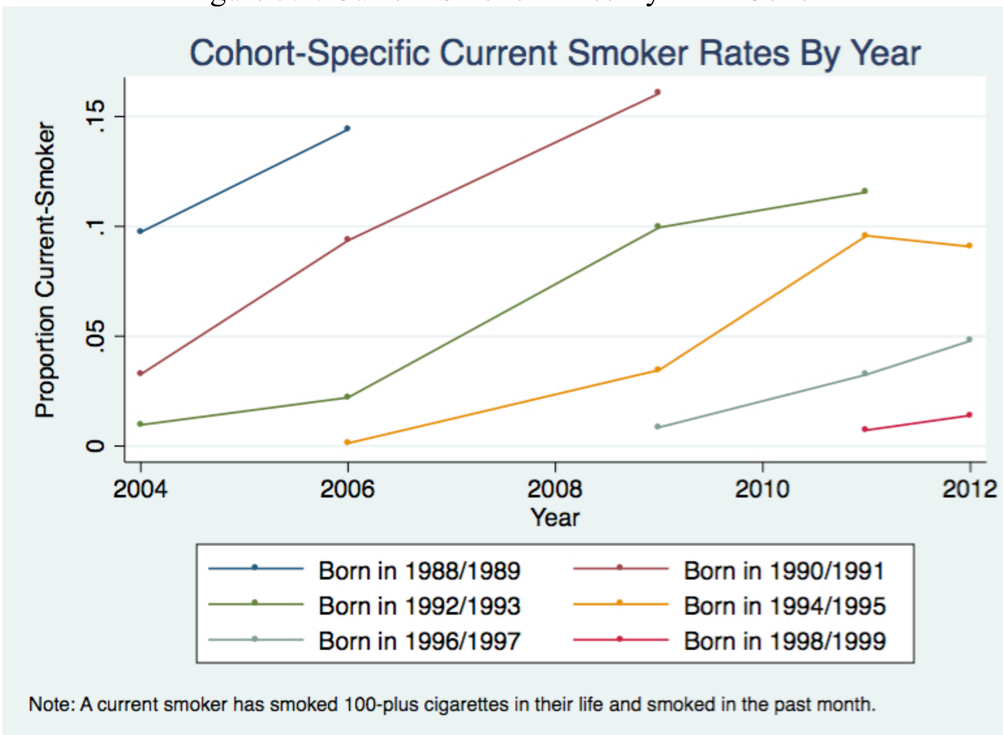
Figure 3.3: Trends in Smoking among 10th Grade Students



Source: Monitoring the Future data (Johnston, O'Malley, Bachman, and Schulenberg, 2013)

The 2009 federal cigarette tax increase also appears important. Figure 3.4 uses NYTS data to plot current smoking by birth cohort. Successive cohorts' smoking trends are largely horizontal shifts of the prior cohort's trend, as expected given age effects. In 2009, however, the 1992/1993 birth cohort shows a distinct kink. The 1994/1995 birth cohort does not show this kink until 2011, when their age distribution would have matched that of the earlier cohort in 2009. This is consistent with evidence that older teenagers' smoking participation responds to cigarette tax rates, while younger teens' does not (Gruber and Zinman, 2001).

Figure 3.4: Current Smoker Rates By Birth Cohort



Source: National Youth Tobacco Survey data from 2004, 2006, 2009, 2011, and 2012

Even post-2009, e-cigarette use rose at different rates over different intervals: from 2011 to 2012, rates of having tried e-cigarettes and current use both doubled, growing as much in a single year as they had since the product's introduction.

To estimate smoking behavior absent e-cigarette access, I calculate a predicted propensity to smoke based on logistic regression analysis of the 2006 NYTS data. Independent variables include binary indicators for age, grade, gender, race/ethnicity, how often the student sees cigarette smoking by actors on television or in movies, whether they live with someone who smokes cigarettes, whether someone they live with chews tobacco, how likely they would be to smoke a cigarette if a friend offered it to them, and whether they think smoking makes people look cool or fit in. The resulting coefficient estimates are given in Appendix Table A3.1 for five binary outcome variables: ever tried cigarettes, ever tried a non-cigarette tobacco product[84],

[84] This is defined as ever use of smokeless tobacco, pipes, bidis, kreteks, or cigars.

having used a non-cigarette tobacco product in the past 30 days, being an "ever-smoker" (i.e., having smoked 100-plus cigarettes in one's life), and being a current smoker, the latter for both the full 14 to 18 year old sample and an under 18 subsample. These regressions yield pseudo R-square values of 0.33, 0.27, 0.28, 0.48, 0.55, and 0.56, respectively.[85] Notably, as whether one would accept a cigarette if one's best friend offered it could be endogenous to trends in youth smoking rates (e.g., if the answer depends on the proportion of one's friends who smoke), specification checks also consider an adjusted propensity to be a current smoker estimation that omits indicators for responses to this question (See Appendix Table A3.2).

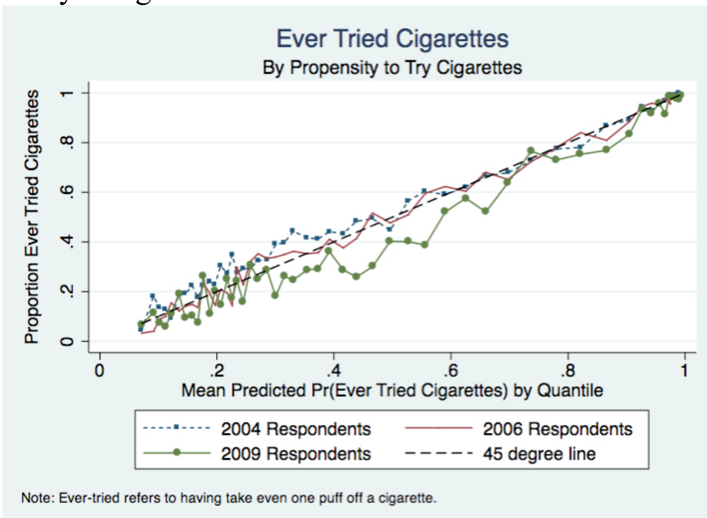
Figure 3.5 allows for visual assessment of how well these equations predict actual behavior among the 2004, 2006, and 2009 NYTS respondents (i.e., prior to the steep post-2010 increase in e-cigarette availability). Dividing predicted behavior into 50-quantile bins, each bin's mean observed behavior is plotted against the mean prediction. In all cases, both the 2006 and 2004 trends closely overlap the 45-degree line, especially for the ever- and current smoker plots. However, in every plot except B (ever used non-cigarette tobacco products), the 2009 trend shows a clear dip in observed rates relative to predicted rates for those with propensities between 0.3 and 0.9. The fact that this dip is not evident for non-cigarette tobacco products suggests that it may be related to the 2009 federal cigarette tax increase (from 39 to 101 cents per pack).[86] Overall, the close match between observed and predicted behaviors supports using the 2006 equations to estimate *ex ante* propensities to smoke in later years' data.

[85] I do not discuss individual coefficients, as the equations' purpose is to provide a means of estimating propensity to smoke absent e-cigarettes, using later years' data.

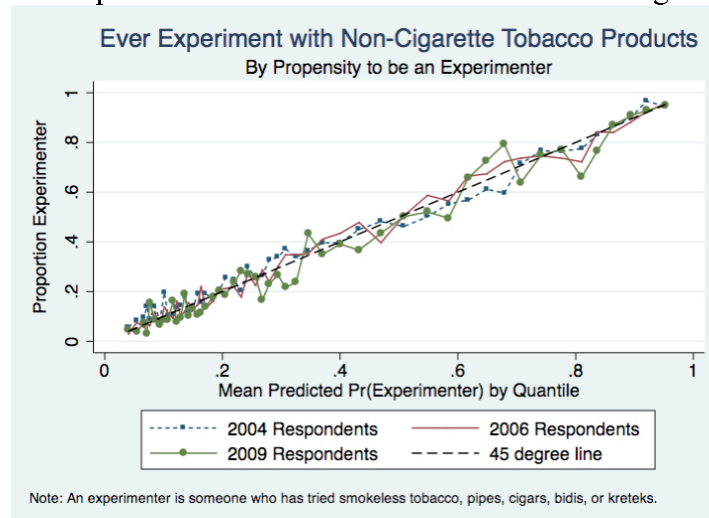
[86] Changes in the chewing tobacco, snuff, and pipe tobacco taxes were much smaller, increasing by 31 cents, 93 cents, and 173 cents *per pound*, respectively (Alcohol and Tobacco Tax and Trade Bureau, 2012). Plotting current use of non-cigarette tobacco products by propensity for such use, Figure A3.1 also finds no dip in the 2009 trend.

Figure 3.5: Actual Use by Quantile of Predicted Use

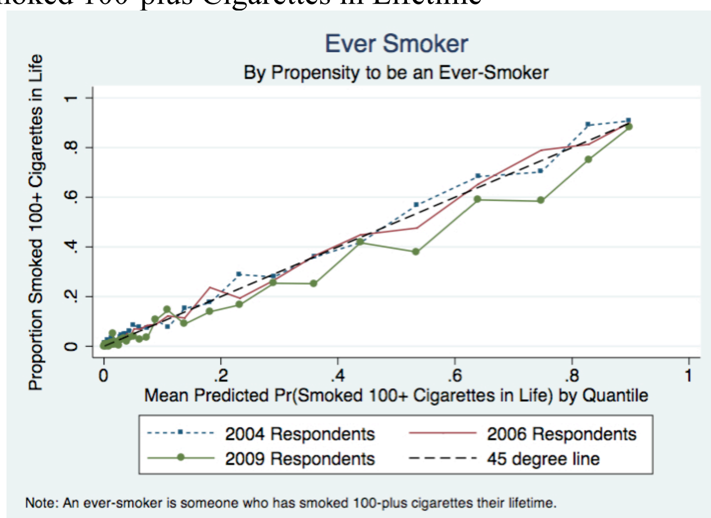
A. Ever Try a Cigarette



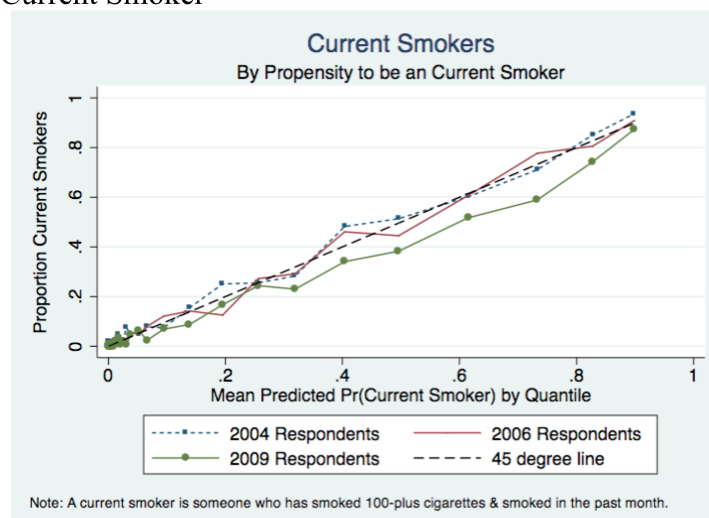
B. Ever Experiment with Tobacco Products besides Cigarettes



C. Smoked 100-plus Cigarettes in Lifetime



D. Current Smoker



Notes: All figures use the National Youth Tobacco Survey data from 2004, 2006, and 2009. Applying Table A3.1 coefficients to these data yields the predicted propensities. Each figure divides its predicted propensities into 50 quantiles, plotting the observed-behavior mean for each against its mean predicted value. All means are weighted using survey weights.

To examine how increased e-cigarette access affects the change in current smoking rates for different smoking propensity groups, I first consider cigarette demand, which is assumed to be shaped by income, prices, tastes, and information. With the NYTS data (which lacks income information), one might describe this as follows:

$$\Pr(\text{CurrentSmoker}_{it}) = \beta_0 + \beta_1\text{Price}_t \cdot \text{G}_i + \beta_2\text{ECigs}_t \cdot \text{G}_i + \beta_3\text{TT}_t \cdot \text{G}_i + \lambda\text{X}_i + \gamma\text{Year}_s + \varepsilon_{it}, \qquad (1)$$

where t indicates year and G is a vector of three binary indicators for whether one falls in the low, middle, or high propensity to smoke group, reflecting tastes for smoking. This equation assumes that the impacts of cigarette price (Price_t) and e-cigarette availability (ECigs_t) on cigarette smoking differ across these groups, and that the influence of one's propensity group may itself change over time (hence the time-trend-by-group interactions, $\text{TT}_t \cdot \text{G}_i$, with $\text{TT}_t = \text{year}_t - 2000$).[87] Year fixed effects (Year_s) absorb the reference group's responses to such time-varying factors, as well as any year-specific changes in information. Tastes for smoking may also vary by individual characteristics, X_i.[88]

To identify off of measures of cigarette price and e-cigarette availability that are not driven by adolescent smoking, I use the federal cigarette tax rate in place of price, and consider two proxies for the latter: annual U.S. e-cigarette sales and advertising expenditure on "smoking material and accessories," a category that includes e-cigarettes. Note that, while advertising is not typically a measure of "availability," it is likely to reflect the ability of production to meet demand as well as youth awareness of e-cigarettes via information or salience effects (i.e., availability in the behavioral sense). Due to data limitations, advertising expenditure analyses are

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[87] As propensity groups are estimated based on regression analysis of 2006 data, time-trend interactions also reflect the fact that these groups may be better descriptions of smoking preferences in years closer to 2006 than much later.

[88] Coded as binary indicators, these controls include sex, race, ethnicity, year of age, and grade, as well as whether the respondent would smoke a cigarette if a friend offered it, lives with someone who smokes, lives with someone who uses smokeless tobacco, believes smoking makes people look cool or fit in, and how often he or she sees actors using tobacco on TV or in movies.

limited to post-2009 changes in smoking.[89]

To consider how e-cigarettes influence adolescent smoking, I focus on changes in current smoker rates over time. Using the 2006 regressions discussed above, I estimate counterfactual behavior (probability of current smoking absent e-cigarettes' introduction), group consumers by centile of propensity to smoke, and examine within-centile changes in smoking with a differenced version of the above equation, as follows:

$$\Delta \text{CurrentSmoker}_{qs} = \beta_0 + \boldsymbol{\beta_1} \text{G}_q' \Delta \text{CigTax}_s + \boldsymbol{\beta_2} \text{G}_q' \Delta \text{ECigs}_s + \boldsymbol{\beta_3} \text{G}_q' \Delta \text{TT}_s + \gamma \text{Year}_s + \varepsilon_{qs}. \qquad (2)$$

The dependent variable, $\Delta\text{CurrentSmoker}_{qs}$, is the within quantile (q) change in current smoker rates between survey s and the prior survey. G_q is a vector of three dummy variables indicating whether $\Pr(\text{Smoker})_{qs}$ falls into the low, middle, or high propensity to be a current smoker ranges. These groups are defined as those quantiles covering the 20th to 80th percentile of propensity to smoke estimates, 80th to 90th percentile, and above the 90th percentile, respectively.[90] I interact these indicators with changes in the federal cigarette tax rate (ΔCigTax_s) and domestic e-cigarette sales (ΔECigs_s), as well as a change in time-trend (ΔTT_s). Year fixed effects are retained to absorb time trends in the reference group. This allows for more flexible variation than a stand alone change in time-trend variable, addressing the possibility of non-linear shocks that may have affected all groups similarly (e.g., the recession). This specification has the benefit of differencing out time invariant effects, though a specification check will include every control used to estimate propensity to be a current smoker interacted with change

[89] Advertising expenditure on "smoking material and accessories" grew from $2.7 million in 2010 to $7.2 million in 2011, and $20.8 million in 2012 (Elliot, 2013). As I was unable to locate an expenditure value for 2009, I use the 2010 level in its stead. This seems reasonable if Sottera v. FDA's effect on such advertising in 2009 was similar to that in 2010. The steep 2012 jump reflects Big Tobacco's entrance into the e-cigarette market.

[90] These percentiles reflect the fact that the vast majority of 14 to 18 year olds are not current smokers, such that the middle and high propensity to smoke groups are a smaller fraction of the cohort than the low propensity group. Regression results are not noticeably affected by redefining these groups such that more of the low propensity to smoke group is included in the middle propensity group.

in time trend ($X_{qs}{}^{'}\Delta TT_s$), in case their relationship to current smoking evolved over time.[91]

The lack of geographic identifiers poses a weakness for this approach. Specifically, tax rate variation is limited to the 2009 federal cigarette tax change, despite concurrent changes in state tax rates. Thus, the change in tax rates is non-zero for 2009 only, meaning that the corresponding interaction terms will absorb group-specific variation in smoking changes from 2006 to 2009 that is not already explained by the time-trend and change in e-cigarette sales.[92] This may be a good thing: as the 2006 to 2009 period overlaps the Great Recession, the $\boldsymbol{\beta_1}$ tax coefficients will absorb differential changes in cigarette smoking due to this economic change, raising our confidence that the e-cigarette interaction coefficients are not confounded by the recession. However, this means that $\boldsymbol{\beta_1}$ cannot be interpreted as a pure tax response. Further concerns about tax variation are addressed by repeating the regressions without 18 year olds in the sample—existing work suggests that younger teenagers do not respond to cigarette taxes[93]—and by considering only post-tax-change shifts in smoking rates (i.e., from 2009 to 2011, and 2011 to 2012).

Equation 2 is essentially a triple-difference analysis, considering whether larger increases in the availability of e-cigarettes are associated with larger changes in current smoking for

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[91] In the 2006 propensity to smoke regression, all controls are coded as binary indicators (See Appendix Table A1).

[92] Notably, the Children's Health Insurance Program Reauthorization Act of 2009, which established this tax, was signed on February 4th, with the tax going into effect on April 1st. However, the 2009 surveys were administered from February 9th through June 2nd of that year. Lacking data on each respondent's survey date, I cannot distinguish interviews conducted before versus after the tax change. Thus, since every interview took place after the law was signed, and over half of the survey administration period is after April 1st, I treat all 2009 surveys as post-tax. While the idea that teens anticipated a tax increase when it was inevitable but not yet in effect is questionable, this specific tax change was the largest federal cigarette tax increase to date, and thus covered widely online and in the news that February and March, suggesting that it may have been salient in those months.

[93] Gruber and Zinman (2001) show that high school seniors exhibit statistically significant smoking responses to cigarette taxes, while younger high school students do not, repeating their analysis with several data sets. Proposed explanations for this finding tend to rest on 18 year olds' greater tendency or ability to buy their own cigarettes, or a change in the budget constraint of teens preparing to leave their parents' household. Thus, omitting 18 year olds from regression analyses should reduce concerns about confounding due to changes in cigarette tax rates.

different propensity to smoke groups. The 2004 to 2006 change in smoking acts as a pre-period ($\Delta ECigs_{2006} = 0$), with the propensity group interaction terms capturing group-specific responses to subsequent growth in e-cigarette availability, detrended via the group by change in time trend terms ($G_q \cdot \Delta TT_s$). To identify the relationship between smoking and e-cigarette use, I take the ratio between the $\boldsymbol{\beta_2}$ values estimated using the change in e-cigarette advertising and an analogous $\boldsymbol{\beta_2}$ estimated from a change in e-cigarette use regression, as follows:

$$\Delta EverECig_{qs} = \beta_0 + \boldsymbol{\beta_1} G_q \cdot Year_{2011} + \boldsymbol{\beta_2} G_q \cdot \Delta ECigs_s + \gamma Year_{2011} + \varepsilon_{qs}. \quad (3)$$

This regression considers changes in ever-use of e-cigarettes ($\Delta EverECig_{qs}$) from 2006 to 2011, and 2011 to 2012, as e-cigarette use data is not available for 2009. In place of equation 2's terms interacting each propensity group with the change in taxes and change in time trend, I merely interact them with an indicator for 2011. Given that the analysis considers only two sets of changes, from 2006 to 2011 and 2011 to 2012, these interactions cannot be included alongside a change in time trend by group due to collinearity. The year by group interaction is preferred as the recession and 2009 tax change may affect smoking differently in the earlier period. Thus, the $\boldsymbol{\beta_2}$ estimates will reflect group-specific changes in e-cigarette use associated with changes in e-cigarette advertising expenditure (relative to the reference group's changes), allowing for differential responses in each group to influential factors occurring between 2006 and 2011 (captured by $\boldsymbol{\beta_1}$). Notably, these regressions use Kim, Arnold, and Makarenko's (2014) estimates of e-cigarette advertising expenditure in place of Elliot's (2013) expenditures on "smoking materials and accessories," such that \$0 of advertising expenditure can be assumed for 2006.

Results

Figure 3.6A plots ever use of e-cigarettes in 2011 and 2012 against propensity to try cigarettes, while 3.6B plots these rates against the propensity to experiment with non-cigarette tobacco products. At propensities to try cigarettes or experiment below 0.15, rates of e-cigarette use are almost 0, with little to no increase from 2011 to 2012. As the propensities rise from there, however, e-cigarette use increases. Moreover this rise in e-cigarette use steepens around 0.6, particularly in the 2012 data. Thus, those more likely to try tobacco products *ex ante* are more likely to try e-cigarettes, with e-cigarette use rising fastest in the highest propensity range.

Figure 3.6: Who tries electronic cigarettes?

A. Ever-use by propensity to try cigarettes

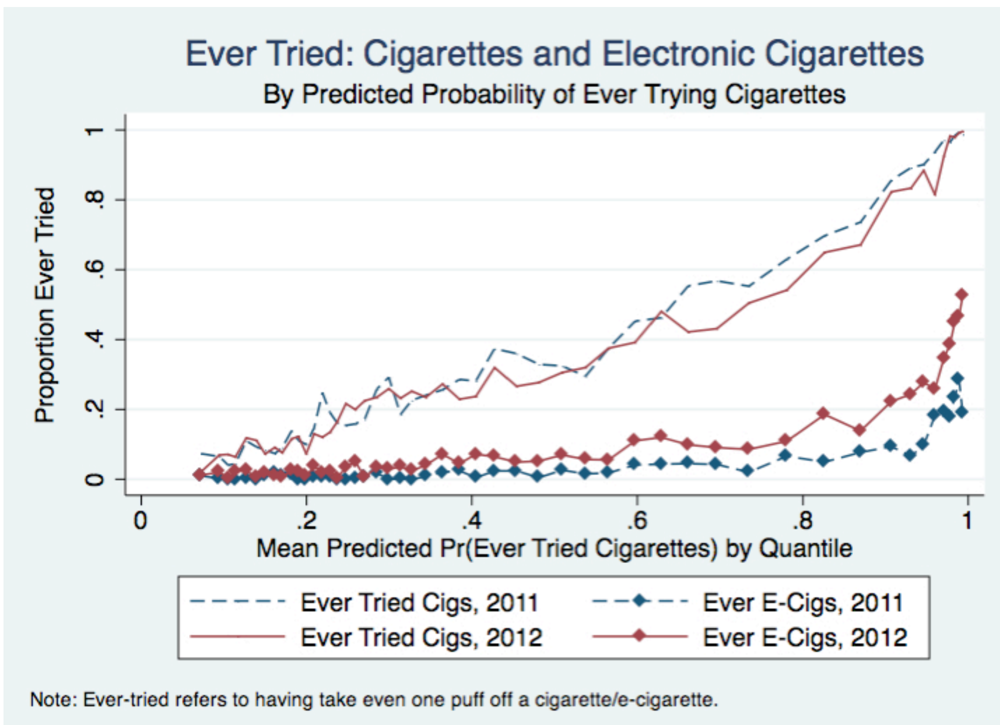
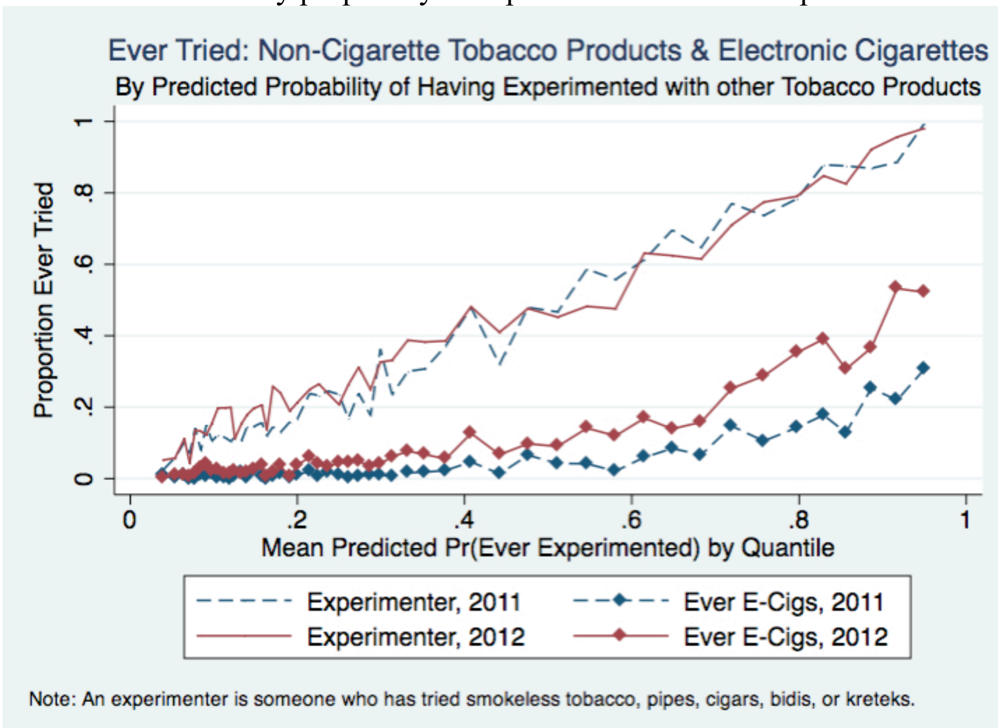


Figure 3.6 (Continued)
B. Ever-use by propensity to experiment with tobacco products

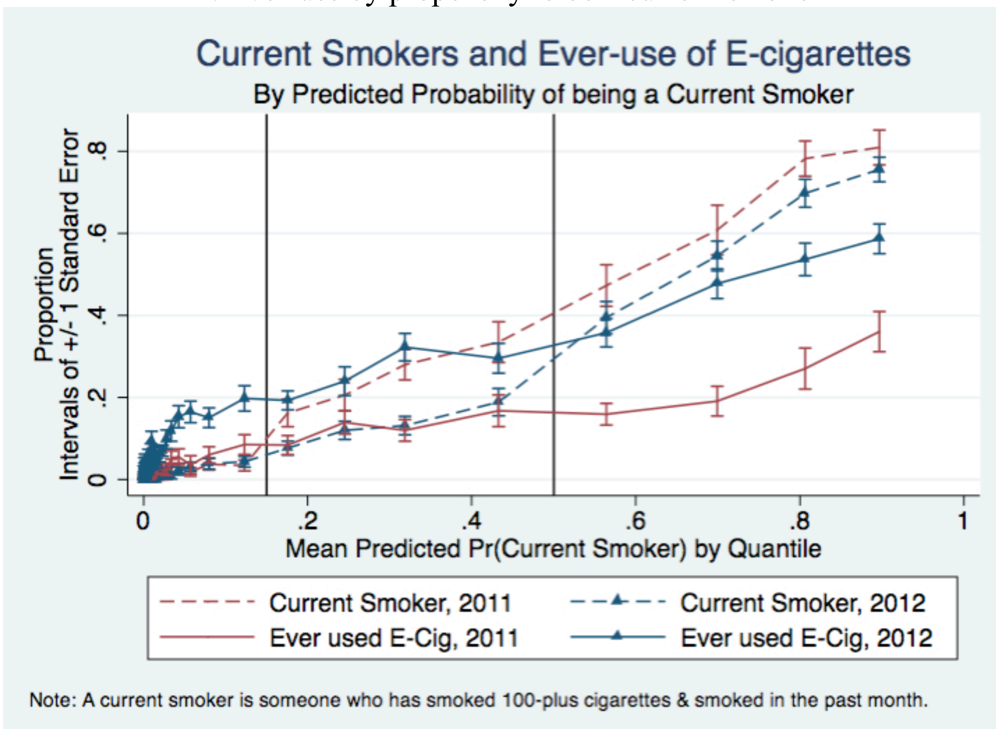


Notes: Figures plot weighted means of data from the 2011 and 2012 NYTS. Applying Table A3.1 coefficients to these data yields the x-axis predicted propensities, which are divided into 50 quantiles. Quantile means for the observed-behaviors are plotted against the mean predicted propensity.

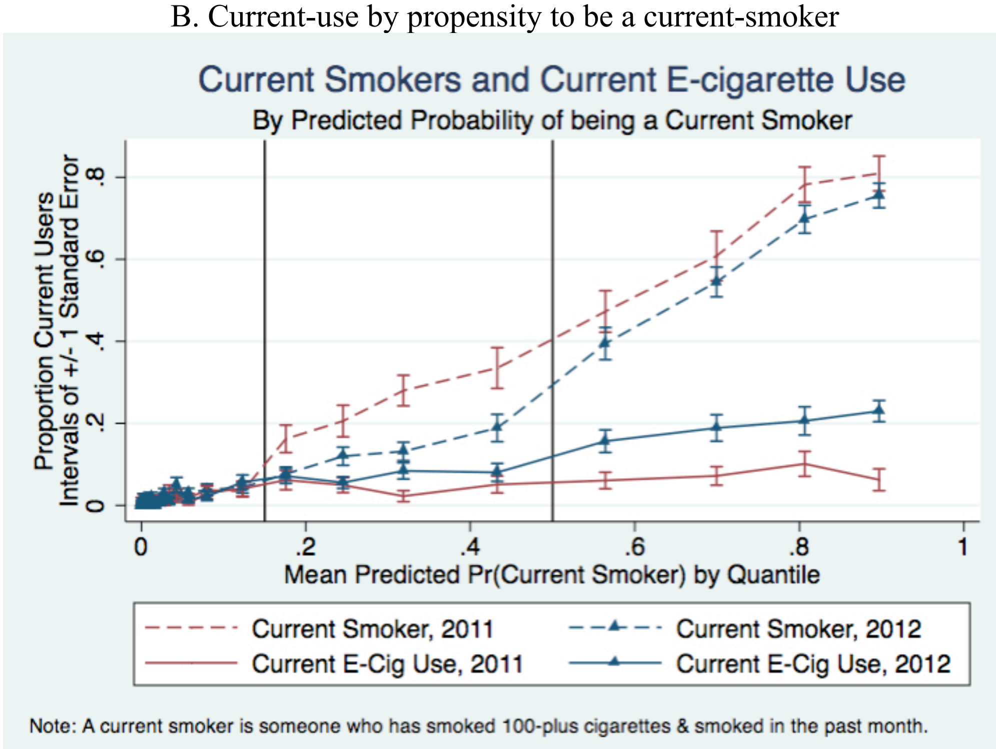
Figure 3.7A plots ever use of e-cigarettes against propensity to be a current smoker, along with intervals of one standard error above and below the mean for each quantile. Based on the current smoking trends, there appear to be three groups: low propensity to be a current smoker—Pr(Current Smoker)<0.15—wherein there is no change in current smoker rates from 2011 to 2012, a middle propensity group—Pr(Current Smoker) $\in$ [0.15, 0.5)—that shows a widening gap in current smoking, and a high propensity group—Pr(Current Smoker)≥0.5—for whom the 2011 to 2012 current smoking gap is relatively constant as rates increase. These cutoffs are shown on Figures 3.7 and 3.8, categorizing the top ten percent of respondents (approximately) as high propensity to smoke, the next highest 10 percent as middle propensity, and the remainder as low propensity. Thus, regression analyses use 80th and 90th percentile cutoffs to define the middle and high propensity to smoke groups.

Figure 3.7: Electronic cigarettes and habitual smoking

A. Ever-use by propensity to be a current-smoker

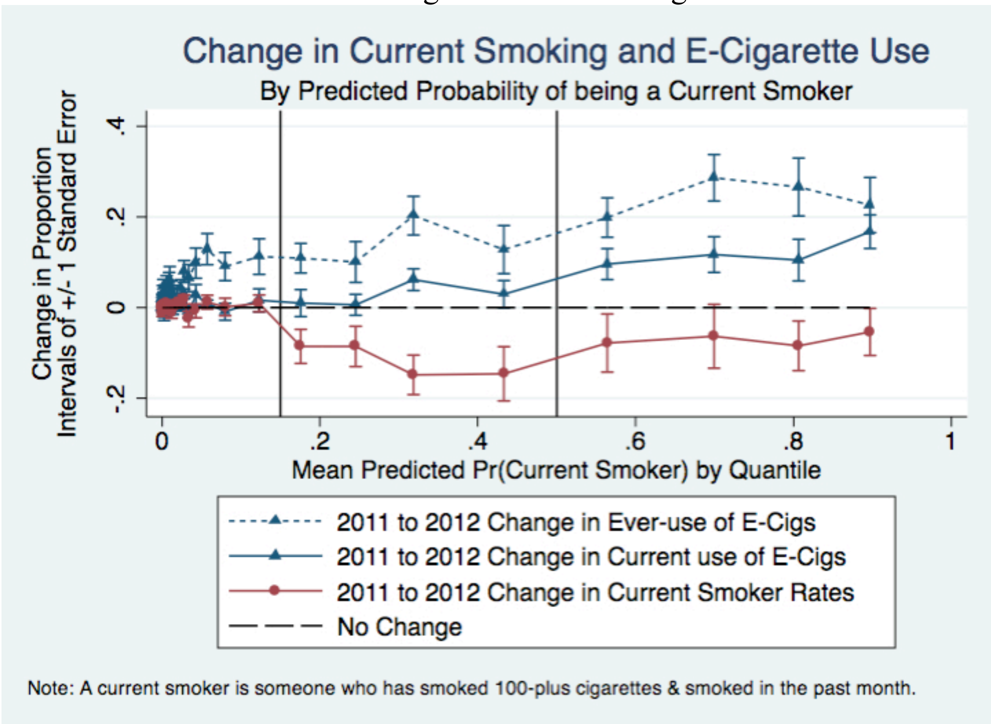


B. Current-use by propensity to be a current-smoker

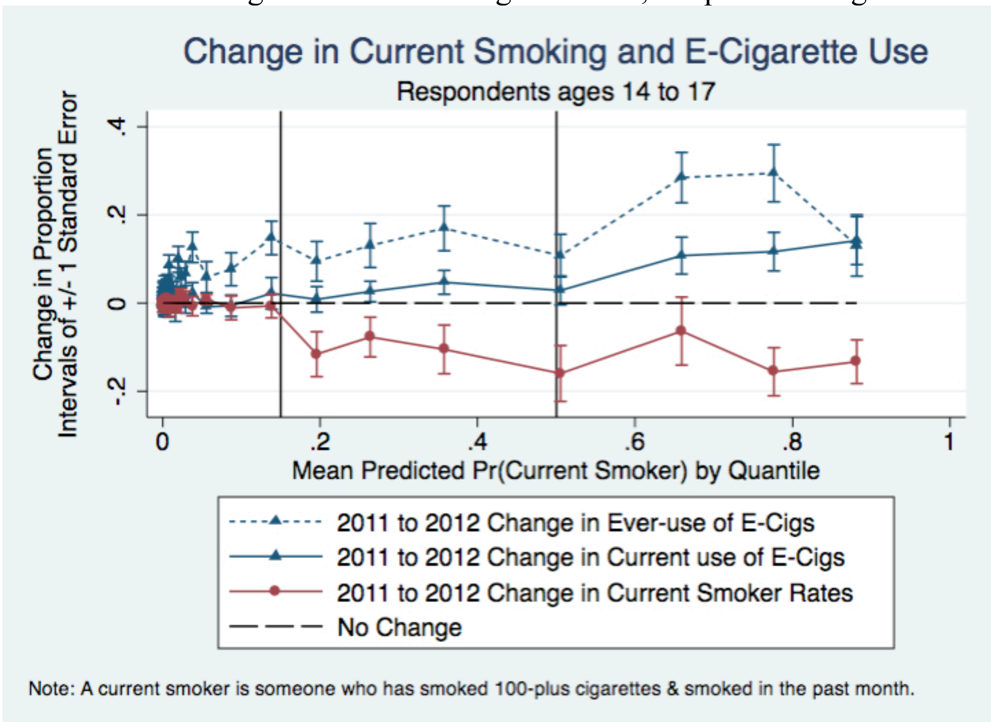


Notes: Figures plot weighted means of data from the 2011 and 2012 NYTS. Applying Table A3.1 coefficients to these data yields the x-axis predicted propensities, which are divided into 50 quantiles. For each quantile, means for observed-behaviors and the corresponding range of one standard error above and below each mean are plotted against the mean predicted propensity to be a current smoker.

Figure 3.8: Changes in Current Smoking and Electronic Cigarette Use by Propensity to Smoke
A. Current Smoking and Electronic Cigarette Use



B. Current Smoking and Electronic Cigarette Use, Respondents Ages 14 –17



Notes: Data are from the 2011 and 2012 NYTS, using survey weights throughout. Applying the Table A3.1 current smoker regression coefficients to these data yields the x-axis predicted propensities, which are divided into 50 quantiles. For each quantile, I calculate the difference between each behavior's observed 2011 and 2012 mean, and its corresponding standard error. These differences, plus a range of 1 SE above and below each, are plotted against each quantile's mean propensity to be a current smoker.

In the low propensity group, one notes a steep jump in e-cigarette use from 2011 to 2012, despite essentially no change in current smoking rates. Plotting current e-cigarette use instead of ever-use, Figure 3.7B shows essentially no change in current e-cigarette use in this subgroup, suggesting that the low propensity group may try e-cigarettes, but neither tends to adopt them for continued use nor exhibits increased current smoking. Thus, in this subgroup, simply trying e-cigarettes does not seem to induce a gateway effect on cigarette smoking.

The middle-propensity group (0.15, 0.5) tells a different story: current smoker rates drop an average of 12 percentage points concurrent with a mean 14 percentage point increase in ever-use of e-cigarettes (Figure 3.7A). Recall, however, that even the 2009 trend shows a drop in current smoking for those with propensities above 0.3, potentially related to the 2009 cigarette tax increase (see Figure 3.5D). Plotting the changes in smoking rates and e-cigarette use, Figure 3.8A shows that the 2011 to 2012 drop in smoking is distinguishable from 0 and clear even among those with current smoker propensities between 0.15 and 0.3, a group that does not exhibit the 2009 dip in current smoking. Moreover, while these respondents display increased ever-use of e-cigarettes, they do not show a rise in current e-cigarette use. This is consistent with a harm reduction story in which some marginal smokers try e-cigarettes instead of cigarettes, perhaps as a lower risk form of teenage rebellion, but do not develop an e-cigarette or smoking habit.

For propensities between 0.3 and 0.5, cigarette smoking falls further, perhaps partially in response to the tax increase. To mitigate the tax's influence, I reproduce this figure for those under age 18 (Figure 3.8B). Here, the 0.3 to 0.5 fall in cigarette smoking seems no greater than that in the 0.15 to 0.3 range: standard error intervals around each quantile mean suggest that the observed decline in current smoking is not statistically distinguishable from that in the 0.15 to

0.3 propensity interval. Overall, the middle propensity group shows a drop in cigarette smoking alongside increases in ever- and current-use of e-cigarettes, whether the trends exclude 18 year olds or not. This suggests harm reduction via substitution towards e-cigarettes.

Interestingly, ever- and current-use of e-cigarettes increase the most among those with the highest propensity to be current smokers (≥0.5), but the mean reduction in smoking seems no greater than that observed in the middle propensity group. These findings are consistent with the idea that the high propensity group contains fewer marginal quitters than the middle group, and exhibits greater dual use. Whether such dual use is harmful depends on a number of factors, including the effect on total cigarette consumption, how long one continues to smoke (i.e., do people delay full cessation longer), and levels of nicotine dependence. I do not attempt to evaluate changes in cigarettes per day concurrent with dual use as a means of clarifying this, since that would require a way to distinguish selection (e.g., heavier smokers disproportionately relying on e-cigarettes to sooth nicotine cravings) from the effects of e-cigarettes per se.[94]

To consider how changes in e-cigarette access influence current smoking rates among those with different propensities to smoke, Table 3.2 presents the equation 2 regressions for ages 14 to 18. As the signs and significance for all e-cigarette coefficients match those in the age 14 to 17 regressions, the latter are presented in the Appendix and not discussed here (see Table A3.4).

The baseline regression omits e-cigarette sales interactions, and finds a statistically significant reduction in current smoking rates associated with the 2009 federal cigarette tax increase in the high propensity to smoke group. Recall, however, that the tax change coefficients should not be considered causal as they may reflect a smoking response to the recession as well as the tax.

[94] My approach to estimating propensity to be a current smoker is less feasible with the NYTS data on cigarettes per day: the latter are grouped in a manner that reduces observable variation, and only 12 percent of the 2006 sample are current smokers, restricting statistical power. Given these limitations, I leave this question for future research.

Table 3.2: OLS analysis of Change in Current Cigarette Smoking, Coefficients/(Standard Error)

	Δ Current Smoker, Ages 14 to 18			
Years considered:	2004-2012		2009-2012	
Specification:	Baseline	ΔE-cig sales	ΔE-cig sales	ΔE-cig ad spending
Low Pr(Smoker) · ΔCig. tax (in dollars)	0.0044	0.0056		
	(0.0245)	(0.0254)		
Mid Pr(Smoker) · ΔCig. tax (in dollars)	-0.0299	-0.0846*		
	(0.0363)	(0.0377)		
High Pr(Smoker) · ΔCig. tax (in dollars)	-0.1046**	-0.1683**		
	(0.0352)	(0.0366)		
Low Pr(Smoker) · ΔE-cig. sales (in $100m)		0.0004	0.0010	
		(0.0037)	(0.0049)	
Mid Pr(Smoker) · ΔE-cig. sales (in $100m)		-0.0203**	-0.0313**	
		(0.0055)	(0.0073)	
High Pr(Smoker) · ΔE-cig. sales (in $100m)		-0.0237**	-0.0397**	
		(0.0054)	(0.0071)	
Low Pr(Smoker) · ΔE-cig. ads (in $1m)				0.0002
				(0.0010)
Mid Pr(Smoker) · ΔE-cig. ads (in $1m)				-0.0063**
				(0.0015)
High Pr(Smoker) · ΔE-cig. ads (in $1m)				-0.0079**
				(0.0014)
Constant	0.0003	-0.0057	0.0003	0.0003
	(0.0069)	(0.0077)	(0.0087)	(0.0087)
N	400	400	200	200
Adjusted R-square	0.130	0.208	0.262	0.262
Mean (Δ Current Smoking)	-0.010	-0.010	-0.009	-0.009

Notes: Observations are year-specific quantiles of predicted propensity to be a current smoker in the absence of e-cigarettes, estimated using logistic regression analysis of current smoking in the 2006 data (see Appendix Table A3.1) and applying the resulting equation to later years' data. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. Regressions use 2004, 2006, 2009, 2011, and 2012 National Youth Tobacco Survey data on high school students ages 14-18, along with federal cigarette tax rates (in dollars), electronic cigarette sales (in $100 million units), and electronic cigarette advertising (in $1 million units). Additional controls include year fixed effects and interactions between a change in time trend terms and each propensity to smoke group. Full results are given in Table A3.3. ** [*] denotes statistical significance at the 1% [5%] level.

Adding the change in e-cigarette sales interaction terms, the second specification finds statistically significant 2.0 and 2.4 percentage point reductions in current smoking in the middle and high propensity groups for every $100 million increase in e-cigarette sales. The corresponding low propensity group response is a statistically insignificant 0.04 percentage point increase in smoking. Overall, the e-cigarette sales coefficients are indicative of harm reduction in the middle and high propensity to smoke groups, in response to increased e-cigarette availability.

Limiting consideration to the change in smoking rates from 2009 to 2011, and 2011 to 2012, the next two specifications remove the tax-change variable from consideration.[95] E-cigarette sales again indicate reductions in current smoking in the middle and high propensity groups—statistically significant decreases of 3.1 and 4.0 percentage points, respectively, for every $100 million increase in e-cigarette sales—but a small statistically insignificant response in the low propensity group. Interacting propensity groups with e-cigarette advertising instead of sales also indicates statistically significant reductions in current smoking in the middle and high propensity to smoke groups, with a small and statistically insignificant effect in the low propensity group. A $1 million increase in advertising is associated with 0.6 and 0.8 percentage point reductions in current smoking, for the middle and high groups, respectively.

Table 3.3 presents two sets of specification checks for the 2009 to 2012 regressions. The first, adding controls that interact a change in time trend with the quantile mean of every variable used to predict propensity to smoke, continues to show a statistically significant drop in current smoking in the middle and high (but not low) propensity to smoke groups. The second specification check adjusts the method of estimating propensity to smoke, omitting indicators of how likely a respondent is to smoke a cigarette if their best friend offered it, to address the possibility that this may be endogenous to trends in current smoking among youths. These regressions continue to find statistically significant reductions in current smoking in the high

[95] Recall footnote 92: the Act establishing this tax was signed just before the 2009 data collection began, but the tax itself went into effect just short of halfway through the interview period. Lacking data on each respondent's survey date, I cannot distinguish interviews conducted before versus after the tax change. As every interview took place after the law was signed and over half of the survey administration period is after it went into effect, I treat all 2009 surveys as post-tax. While one might question the idea that teens anticipated this tax increase in the two months before it went into effect, news coverage of the tax was common in this period, as it was to be the largest federal cigarette tax increase to date. Thus, the tax change may have been quite salient in those months.

propensity to smoke groups, but not in the middle or low groups.

Table 3.3: Specification Checks for 2009 to 2012 Change in Current Smoking Analyses, Coefficients/(Standard Error)

	Δ Current Smoker			
Specification Check:	Table 3.2 Specification + Controls for ΔTime trend ˙ Independent Variables from Propensity Regression		Alternative Propensity to Smoke Estimation	
	(1)	(2)	(3)	(4)
Low Pr(Smoker) ˙ ΔE-cig. sales (in $100m)	-0.0020		-0.0052	
	(0.0050)		(0.0062)	
Mid Pr(Smoker) ˙ ΔE-cig. sales (in $100m)	-0.0336**		0.0000	
	(0.0074)		(0.0096)	
High Pr(Smoker) ˙ ΔE-cig. sales (in $100m)	-0.0457**		-0.0202*	
	(0.0082)		(0.0090)	
Low Pr(Smoker) ˙ ΔE-cig. ads (in $1m)		-0.0004		-0.0010
		(0.0010)		(0.0012)
Mid Pr(Smoker) ˙ ΔE-cig. ads (in $1m)		-0.0067**		0.0000
		(0.0015)		(0.0019)
High Pr(Smoker) ˙ ΔE-cig. ads (in $1m)		-0.0091**		-0.0040*
		(0.0016)		(0.0018)
Constant	0.0138	0.0138	-0.0034	-0.0034
	(0.0497)	(0.0497)	(0.0107)	(0.0107)
N	200	200	198	198
Adjusted R-square	0.330	0.330	0.085	0.085
Mean (Δ Current Smoking)	-0.009	-0.009	-0.014	-0.014

Notes: These specification checks adjust the regressions in column 3 and 4 of Table 3.2. Observations are year-specific quantiles of predicted propensity to be a current smoker in the absence of e-cigarettes, estimated using logistic regression analysis of current smoking in the 2006 data and applying the resulting equation to later years' data. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. In estimating propensity to smoke, the first specification uses the ages 14 to 18 current smoker regression in Appendix Table A3.1, while the second uses the Appendix Table A3.2 regression (which omits controls for how likely the respondent would be to smoke a cigarette if a friend offered it). Regressions use 2009, 2011, and 2012 National Youth Tobacco Survey data on high school students ages 14-18, along with federal cigarette tax rates (in dollars), electronic cigarette sales (in $100 million units), and electronic cigarette advertising (in $1 million units). Controls not mentioned above include year fixed effects and the change in time-trend interacted with each propensity group. The first two specification checks also include quantile means of fixed effects for respondent sex, year of age, grade, race, and ethnicity, as well as how often the respondent sees actors using tobacco on TV or in movies and whether he or she would smoke a cigarette if a friend offered it, lives with someone who smokes, lives with someone who uses smokeless tobacco, and believes smoking makes people look cool/fit in. Full results are given in Table A3.5. ** [*] denotes statistical significance at the 1% [5%] level.

Table 3.4: OLS analysis of Change in Electronic Cigarette Use, Coefficients/(Standard Error)

	ΔEver-use of Electronic Cigarettes, Ages 14-18	
	Baseline Propensity to Smoke Estimation	Alternative Propensity to Smoke Estimation
Low Pr(Smoker) ˙ ΔE-cig. ads (in $1m)	0.0019*	0.0015*
	(0.0009)	(0.0008)
Mid Pr(Smoker) ˙ ΔE-cig. ads (in $1m)	0.0096**	0.0096**
	(0.0013)	(0.0012)
High Pr(Smoker) ˙ ΔE-cig. ads (in $1m)	0.0172**	0.0085**
	(0.0013)	(0.0011)
Low Pr(Smoker) ˙ Year = 2011	0.0027	0.0194
	(0.0120)	(0.0104)
Mid Pr(Smoker) ˙ Year = 2011	0.0398*	0.0002
	(0.0177)	(0.0163)
High Pr(Smoker) ˙ Year = 2011	0.1168**	0.0532**
	(0.0172)	(0.0153)
Year = 2011	-0.0079	-0.0078
	(0.0130)	(0.0111)
Constant	0.0117	0.0207**
	(0.0092)	(0.0079)
N	200	198
Adjusted R-square	0.735	0.474
Mean (ΔE-cigarette use)	-0.009	-0.014

Notes: Observations are year-specific quantiles of predicted propensity to be a current smoker in the absence of e-cigarettes. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. Both specifications use propensity to smoke estimates derived via logistic regression analysis of current smoking in the 2006 data, applying the resulting equation to later years' data. The first specification uses the ages 14 to 18 current smoker regression in Appendix Table A3.1, while the second uses the Table A3.2 regression. Data include 2011 and 2012 National Youth Tobacco Survey data on high school students ages 14-18, along with electronic cigarette advertising levels for 2010 and 2011 (in $1 million units). All controls are listed. ** [*] denotes statistical significance at the 1% [5%] level.

To understand the magnitude of these effects relative to changes in e-cigarette use, Table 3.4 presents the change in ever-use of e-cigarettes regressions, the first using the baseline approach to estimating propensity to smoke, and the second employing the alternative approach applied in Table 3.3's specification check.[96] In both regressions, all propensity groups show a statistically significant increase in e-cigarette use in response to greater e-cigarette advertising.

[96] As neither harm reduction nor gateway effects necessitate continued e-cigarette use, focusing on the change in ever-use of e-cigarettes (instead of current use) is appropriate.

Both regressions show statistically significant increases in ever-use of e-cigarettes of 0.2 and 1.0 percentage points for every additional $1 million of advertising in the low and middle propensity groups, respectively. The high propensity response ranges from 1.7 percentage points in the baseline specification to 0.9 in the adjusted version.

Table 3.5 applies these results to relate the current smoking response to changes in e-cigarette use. Results for the low and middle propensity groups vary markedly depending on how propensity to smoke is estimated. Yet no ratios based on statistically significant coefficients are indicative of increased smoking in response to greater e-cigarette use. For the high propensity group, the baseline and adjusted methods of estimating propensity to smoke yield coefficient ratios of -0.46 and -0.47, respectively. These findings point to e-cigarettes acting as a means of harm reduction in high propensity to smoke cohorts, with current smoking dropping almost one percentage point for every two percentage point increase in e-cigarette use.

Table 3.5: Relationship between Changes in Cigarette Smoking and Electronic Cigarette Use

A. Coefficients using Baseline Propensity Specification			
Propensity to be a Current Smoker	Δ Current Smoking	Δ E-cigarette Use	Ratio
Low	0.0002	0.0019*	0.11
Middle	-0.0063**	0.0096**	-0.66
High	-0.0079**	0.0172**	-0.46
B. Coefficients using Adjusted Propensity Specification			
Propensity to be a Current Smoker	Δ Current Smoking	Δ E-cigarette Use	Ratio
Low	-0.0010	0.0015*	-0.67
Middle	0.0000	0.0096**	0.00
High	-0.0040*	0.0085**	-0.47

Note: Coefficients presented here are each propensity group's e-cigarette advertising interaction term, taken from the ages 14 to 18 change in current smoking regression (last column of Table 3.2 for baseline and in Table 3.3 for adjusted) and from the corresponding change in e-cigarette use regression in Table 3.4. Note that the baseline specification uses the age 14 to 18 current smoker regression in Appendix Table A3.1 to estimate propensity to smoke, while the adjusted specification uses the Table A3.2 specification. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. See the notes to Tables 3.2, 3.3, and 3.4 for more on the specific regressions. ** [*] denotes statistical significance at the 1% [5%] level.

Conclusion

Examining adolescents grouped by their propensity to be current smokers, this analysis finds support for the harm reduction hypothesis, with responses to e-cigarette advertising indicating that a one percentage point increase in ever-use of e-cigarettes is associated with a .5 percentage point reduction in current smoking in the high propensity to smoke group. In 2012, this cohort comprised about 10 percent of high school students ages 14 to 18, but accounted for between 34 and 82 percent of current smokers in that age group, depending on how propensity to smoke is specified. Thus, while this group may not represent most 14 to 18 year olds, it is a key cohort in which one would want to see substitution of e-cigarette use for smoking. The high propensity group's share of dual use is approximately proportionate to its share of smokers (36 and 87 percent, depending on the specification). Ascertaining whether dual use is harm reducing or increasing depends on whether it facilitates or delays cessation over time. Given that ever-use of e-cigarettes doubled from 2011 to 2012 among high school students ages 14 to 18, such an analysis will likely require more years of data to account for a lag between smokers' e-cigarette take-up and smoking cessation.

This paper has several limitations. First and foremost, limited years of data on e-cigarette use by respondents and e-cigarette advertising expenditure prevent regressions from accounting for a more granular trend in e-cigarette use or the current smoking response to advertising. I hope to rectify this as further data become available. Next, as the e-cigarette market is quite young and evolving quickly, it is not clear how the estimated relationships will look at equilibrium. If the observed response among teens is partially a response to the controversy around e-cigarettes, their behavior may change as that controversy abates and the product becomes less novel. Third, these regressions assume that the non-linear trends in e-cigarette sales and advertising are not correlated with another non-linear trend affecting adolescent smoking or e-cigarette use. Note that, if this assumption is violated, it only biases the coefficient ratios upward if the confounding factor biases the advertising coefficient upward for smoking and/or downward for e-cigarette use. Once geocoded data on e-cigarette use becomes available, future research might address this concern by using variation in the existence and timing of state-specific policies as a source of identifying variation (e.g., bans on selling e-cigarettes to minors, state taxes, etc.). A fourth limitation has to do with marketing: gateway effects from e-cigarettes may be responsive to marketing, especially if the products increase nicotine dependence among marginal smokers. The lack of evidence for gateway effects in this analysis does not rule out such effects in a different marketing context. The use of candy-like flavors in e-cigarettes is one area worth closer consideration here, as such flavors seem particularly palatable to youths and may raise the propensity for nicotine dependence (and eventual smoking) over longer periods of use than can be considered with the current data. Finally, this analysis limits itself to considering the potential costs and benefits of e-cigarettes in terms of their relationship to cigarette smoking. It does not address complementarities with other risky behaviors (e.g., alcohol consumption) or potential long run health effects from the products themselves. As data on such consequences becomes available, they will clarify the full costs and benefits of these products.

This paper offers several key contributions. Using 2006 data to generate predicted propensities to smoke helps address selection in the decision to use e-cigarettes, a key problem that has prevented existing research from distinguishing causal effects of e-cigarettes on smoking. It also opens the door to analyses of how e-cigarettes influence smoking in different subgroups, along with the size of these groups and consequent population-level implications. Identifying off of variation in e-cigarette sales and advertising in an era strongly influenced by non-market events further supports the estimation of causal effects. And the analyses' specific findings provide the first evidence identifying e-cigarettes as a means of harm reduction among teens who are otherwise likely to smoke.

Still, from a policy perspective, this evidence of harm reduction is not a straightforward guide to regulation. Even beyond the lingering question of dual-use, the market had not reached equilibrium by 2012, meaning that my findings may not represent teen behavior in a context where e-cigarettes are less novel, less controversial, or marketed differently. For example, if higher propensity to smoke youths are attracted to controversial or risky behaviors, sanctioning e-cigarettes and reducing the associated controversy might reduce use in this cohort but increase use among more risk-averse teens, potentially resulting in gateway effects and a decline in harm reduction.

Assuming that e-cigarettes are indeed less risky to one's health than traditional cigarettes, these results indicate that the optimal policies would limit the appeal of e-cigarettes to those with a lower propensity to smoke, but attract those most likely to smoke cigarettes. This suggests a tax structure that keeps the price of e-cigarettes favorable relative to that of traditional cigarettes, alongside restrictions on marketing likely to appeal to more general or risk-averse youth populations. It may also support the FDA's proposed ban on sales to those under age 18, since this seems unlikely to deter those who would violate such a ban in order to smoke cigarettes, but may deter those unlikely to smoke. Further research clarifying why different subgroups use cigarettes and/or e-cigarettes may be key to guiding such regulations.

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Appendix

Table A1.1: Missing Observations in Baseline Data

	Number with Missing (Total Sample Size: 3099)
Baseline Context & Characteristics	
Enrolled in School	8
Race	15
Peer Pressure to Try Cigarettes	86
Peer Pressure to Drink Alcohol	89
Peer Pressure to Try Marijuana or Other Drugs	87
Peer Pressure to Work Hard in School	86
Peer Pressure to Commit a Crime or Violence	81
Neighborhood Ranking: Crime and Violence	55
Neighborhood Ranking: Parental Supervision	80
Perceived Parental Supervision	244
Family Income at Respondent Age 0-5 as % of Federal Poverty Guideline	119
There is no one R would go to for help with a personal or emotional problem	57
Childhood Shocks	
Death of Non-Family Member R was Close	46
Victim of a Violent Crime	63
Either Shock	64
Ever Used Substance	
Full Serving of Alcohol	12
Binge Drink	211
Downers	1

Notes: Except for adverse events, data described here come from the respondent's first Young Adult Interview between 2002 and 2010. Childhood shocks are those occurring up to the respondent's last child (non-young-adult) interview, but not after. Only variables with missing observations at the point in question are listed.

Table A1.2: Does peer pressure drive the relationship between adverse events and substance use?						
	1st Binge Drinking		1st Use of Marijuana		1st other Illegal Drug	
	(1)		(2)		(3)	
Parameter Estimates	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*	OR/(t-stat)	*AME˙100*
Δ Shock since last interview	1.404 (1.33)	*2.9%*	1.437 (1.39)	*4.7%*	1.987 (1.51)	*2.0%*
Lag: ΔShock since last interview	1.903 (1.30)	*5.5%*	0.974 (-0.05)	*-0.3%*	4.501** (2.01)	*4.5%*
Δ Peer Pressure	1.085 (0.47)	*0.7%*	4.305*** (6.12)	*18.9%*	1.803 (1.17)	*1.8%*
Δ Shock since last interview ˙ Δ Peer pressure	2.176* (1.88)	*6.6%*	0.920 (-0.11)	*-1.1%*		
Lag: ΔShock since last interview ˙ Lag: Δ Peer pressure					48.028*** (3.36)	*11.5%*
N	2962		2732		3327	
Mean(Δ Behavior)	0.082		0.167		0.031	
Shock Effects						
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	*2.9%* (1.33)		*4.7%* (1.39)		*2.0%* (1.51)	
Long-run ($\beta_{\text{Lag}(\Delta\text{Shock})} - \beta_{\Delta\text{Shock}}$)	*2.6%* (0.69)		*-5.0%* (-0.75)		*2.4%* (1.29)	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates (as AMEs). Additional controls are described in the note to Table 3. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

Table A1.3: Differential First-Use-Responses to Adverse Events by Family Income in Childhood

	1st Cigarette		1st Binge Drinking		1st Marijuana		1st Illegal Drug	
	(1)		(2)		(3)		(4)	
Parameter Estimates	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*	OR/(t-stat)	*AME·100*
Δ Shock since last interview	2.558*** (3.17)	*11.6%*	1.525 (1.48)	*3.6%*	1.547 (1.49)	*5.6%*	1.932 (1.31)	*2.0%*
Lag: ΔShock since last interview	4.519*** (2.93)	*18.7%*	2.056 (1.20)	*6.1%*	1.004 (0.01)	*0.1%*	6.821*** (2.61)	*5.9%*
Δ Shock since last interview · Family income > 75th percentile	1.168 (0.29)	*1.9%*	0.775 (-0.43)	*-2.2%*	0.790 (-0.45)	*-3.0%*	1.465 (0.48)	*1.2%*
Lag: ΔShock since last interview · Family income >75th percentile	0.657 (-0.32)	*-5.2%*	0.757 (-0.28)	*-2.4%*	1.076 (0.06)	*0.9%*	3.396 (1.09)	*3.7%*
Δ Shock since last interview · Missing family income	1.155 (0.16)	*1.8%*	0.492 (-0.90)	*-6.1%*	0.387 (-1.34)	*-12.3%*	0.509 (-0.73)	*-2.1%*
Lag: ΔShock since last interview · Missing family income	1.274 (0.14)	*3.0%*	0.292 (-1.38)	*-10.5%*	0.342 (-1.21)	*-13.9%*	0.085** (-2.48)	*-7.5%*
N	2411		2962		2732		3327	
Mean(Δ Behavior)	0.160		0.082		0.167		0.031	
Shock Effects								
Recent ($\beta_{\Delta\text{Shock since last interview}}$)	*11.6%**** (3.17)		*3.6%* (1.48)		*5.6%* (1.49)		*2.0%* (1.31)	
Added recent shock effect if family income >75th percentile	*1.9%* (0.29)		*-2.2%* (-0.43)		*-3.0%* (-0.45)		*1.2%* (0.48)	
Long-run ($\beta_{Lag(\Delta Shock)}$ - $\beta_{\Delta Shock}$)	*7.0%* (1.32)		*2.5%* (0.54)		*-5.6%* (-0.76)		*3.9%*** (2.05)	
Added long-run shock effect for family income >75th percentile	*-7.1%* (-0.48)		*-0.2%* (-0.03)		*4.0%* (0.28)		*2.6 %* (0.94)	

The parameter estimates section gives OR (odds ratio) and AME (average marginal effect) estimates from the regression, with AMEs presented in percentage-points. The shock effects section presents the short and long run effects of adverse events implied by parameter estimates (as AMEs). Additional controls are described in the note to Table 3. Standard errors are clustered by the unit of survey randomization: mother's 1979-household. *** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

Table A1.4: OLS First Differences for Substance Dosage, Coefficient/(t-statistic)			
	Cigarette Packs / Day in Past 30 Days	Drinks / Day-Drank in Past 30 (Capped at 20/day)	Number of Sex Partners in Past 12 Months
Independent Variables	(1)	(2)	(3)
Δ Shock since last interview	0.046*	0.151	0.240***
	(1.67)	(0.74)	(2.60)
Lag: ΔShock since last interview	0.062	0.099	0.230
	(1.59)	(0.23)	(1.61)
Δ Peer Pressure			
Δ Peer pressure to try Cigarettes	0.074**		
	(2.51)		
Δ Peer pressure to drink Alcohol		0.717***	
		(4.73)	
SES Control			
Mother graduated college	-0.047**	0.244	-0.136*
	(-2.01)	(1.51)	(-1.73)
N	3425	3099	2997
R-squared	0.079	0.148	0.178
Mean(Δ Behavior)	0.071	0.638	0.413

Additional controls are described in the note to Table 1.3. Specification 3 uses the 18-or-older sibling-age indicator as in the non-binge-drinking regressions, and lacks a behavior-specific peer pressure variable. Standard errors are clustered by the unit of survey randomization: mother's 1979-household.
*** [**] (*) denotes statistical significance at the 1% [5%] (10%) level.

Table A2.1: Explaining Smoking Initiation and Cessation, 1950-1980
Odds ratio (t-statistic)

	Initiation, Ages 14-22		Cessation, Ages 14-46	
Independent Variable	**Men**	**Women**	**Men**	**Women**
Cig-Ad$_t$ $ * > HS Grad	0.9835*	0.9733*	1.0214*	0.9874
	(-2.85)	(-4.43)	(2.04)	(-0.72)
Yes Tar-Nic. * >HS Grad	0.8755*	0.8762*	0.9727	1.1569
	(-2.20)	(-2.06)	(-0.28)	(1.07)
Public Health Info * >HS Grad	0.9928	1.2231*	0.7294*	1.0383
	(-0.10)	(2.56)	(-2.72)	(0.24)
Cigarette Tax Rate$_t$ * >HS Grad	1.0026	0.9902**	1.0137*	1.0012
	(0.44)	(-1.95)	(2.40)	(0.14)
Education Groups (ref: H.S. Graduates)				
< 8 years of School	0.9685	0.7441*	0.7257*	0.6229*
	(-0.24)	(-2.00)	(-2.68)	(-2.88)
Did Not Graduate High School	1.6008*	1.5429*	0.7144*	0.5811*
	(7.84)	(7.88)	(-5.24)	(-6.71)
Completed Some College	0.9280	3.7923*	0.2661*	1.3609
	(-0.13)	(2.66)	(-1.99)	(0.29)
College Graduate +	0.5765	2.7404*	0.3789	2.0432
	(-0.97)	(2.01)	(-1.46)	(0.68)
N	81,492	119,962	212,738	181,303
Pseudo-R^2	0.046	0.040	0.051	0.064

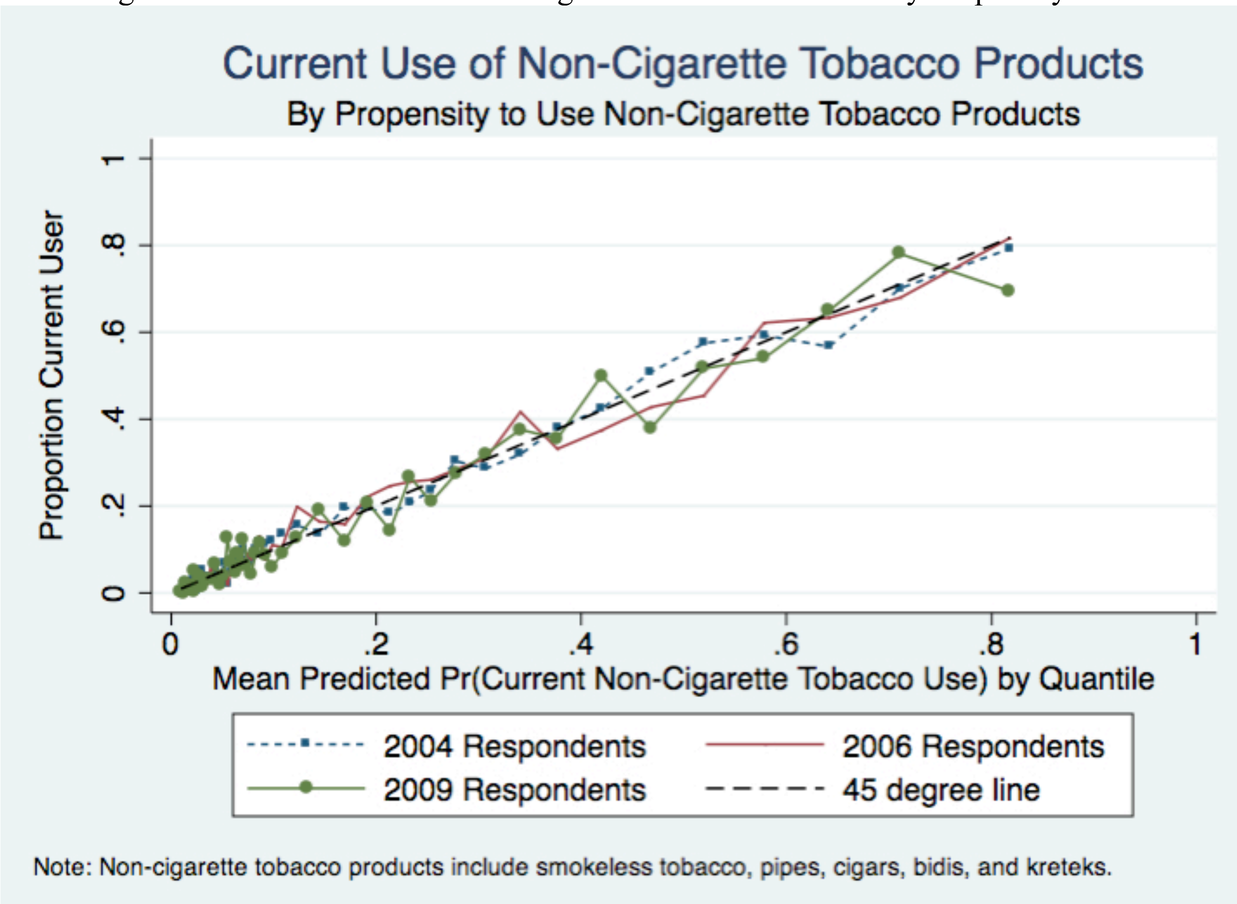
Note: These are the same regression presented in Table 3, with results here given as odds ratios. All regressions include fixed effects for year, race, ethnicity (including a missing ethnicity indicator), Census region, urbanicity, Veteran status (by war), ever draft eligible, survey year, and decade of birth. Cigarette advertising expenditure is in hundred-millions of 2010 dollars. Tax rates are in 2010 cents. The sample for the cessation analyses is individuals who smoked in the prior year (i.e., potential quitters). ** (*) denotes statistical significance at the 10% (5%) level.

Table A2.2: Brand Choice Analyses
Multinomial Logistic Regressions (Base=Straights)
[Relative Risk Ratio (t-statistic)]

	Men	**Women**
Hi-Fi		
< 8 years of School	0.4152*	0.3917*
	(-445.43)	(-329.45)
Did Not Graduate High School	0.5098*	0.3799*
	(-583.26)	(-586.35)
Completed Some College	1.2504*	1.1805*
	(163.18)	(63.24)
College Graduate +	3.1557*	1.3618*
	(642.46)	(88.33)
Lo-Fi		
< 8 years of School	0.9314*	0.5683*
	(-44.41)	(-207.47)
Did Not Graduate High School	0.6999*	0.5650*
	(-344.65)	(-352.51)
Completed Some College	0.8698*	0.9118*
	(-105.36)	(-35.03)
College Graduate +	1.6649*	1.0102*
	(287.41)	(2.88)
N	5,817	5,633
Pseudo-R^2	0.067	0.058

Notes: The sample in each case is smokers ages 25-64. In addition to the variables listed above and a fourth-order polynomial in age, the following controls are included in the above regressions, all as dummy variables: income of household breadwinner (by NHIS grouping), survey year, census region, urbanicity, Veteran status, Hispanic ethnicity, non-white race, and ever married, and an indicator for missing marital status.
** (*) denotes statistical significance at the 10% (5%) level.

Figure A3.1: Current Use of Non-Cigarette Tobacco Products by Propensity to Use



Notes: Data come from the 2004, 2006, and 2009 NYTS surveys, with survey weights used throughout. Applying Table A3.1 coefficients to these data yields the predicted propensity to have used a non-cigarette tobacco product in the past 30 days. Predicted propensities are divided into 50 quantiles. Each quantile's observed mean for this behavior is plotted against its mean predicted value.

Table A3.1: Logistic Analysis of Smoking Behavior pre-Electronic Cigarettes, Odds Ratio/(t-statistic)

	Ever tried cigarettes	Ever used non-cigarette tobacco products	Used non-cigarette tobacco products in past 30 days	Smoked 100+ cigarettes	Current smoker	Current smoker, under age 18
Year of age = 15	1.525**	1.282*	1.341	1.666*	2.027**	2.008**
	(3.86)	(2.25)	(1.90)	(2.09)	(2.67)	(2.58)
Year of age = 16	2.683**	1.745**	1.615**	2.456**	3.148**	3.103**
	(7.37)	(4.16)	(2.67)	(3.23)	(3.84)	(3.70)
Year of age = 17	2.858**	1.980**	1.800**	4.020**	4.509**	4.552**
	(6.63)	(4.29)	(2.85)	(4.50)	(4.50)	(4.40)
Year of age = 18	3.329**	2.249**	1.872**	5.681**	8.133**	
	(6.49)	(4.31)	(2.60)	(5.10)	(5.62)	
Grade 10	0.891	1.061	1.143	1.528*	1.402	1.399
	(-1.24)	(0.63)	(1.16)	(2.53)	(1.89)	(1.81)
Grade 11	1.018	1.160	1.109	1.559*	1.459	1.544
	(0.15)	(1.20)	(0.69)	(2.09)	(1.64)	(1.82)
Grade 12	1.240	1.228	1.107	1.335	1.268	1.212
	(1.43)	(1.34)	(0.53)	(1.14)	(0.85)	(0.64)
Female	1.070	0.382**	0.310**	0.839	0.882	0.874
	(1.28)	(-17.63)	(-16.26)	(-1.91)	(-1.22)	(-1.17)
Missing Obs.: Gender	0.826	0.384**	0.420*	0.704	0.699	0.640
	(-0.71)	(-3.42)	(-2.25)	(-0.70)	(-0.60)	(-0.67)
Hispanic	1.495**	1.047	1.063	0.536**	0.557**	0.574**
	(5.45)	(0.63)	(0.66)	(-4.52)	(-3.66)	(-3.20)
Hispanic & White	0.784	0.762	0.760	1.102	0.826	0.898
	(-1.67)	(-1.91)	(-1.62)	(0.42)	(-0.77)	(-0.40)
Missing Obs.: Ethnicity	1.963*	1.339	1.139	1.116	1.366	1.265
	(2.48)	(1.25)	(0.45)	(0.27)	(0.70)	(0.49)
Race: White & another race	1.191	1.627**	1.760**	1.874*	2.367**	2.430**
	(1.15)	(3.30)	(3.05)	(2.46)	(3.06)	(2.84)

Table A3.1 (Continued)

Race: Black	1.438**	0.980	1.118	0.326**	0.374**	0.374**
	(5.89)	(-0.31)	(1.27)	(-8.08)	(-6.20)	(-5.65)
Race: Asian	0.621**	0.403**	0.432**	0.773	0.874	0.995
	(-4.05)	(-7.32)	(-4.81)	(-1.17)	(-0.49)	(-0.02)
Race: Native Hawaiian or Pacific	1.057	0.856	0.998	1.392	1.454	1.338
Islander	(0.35)	(-0.98)	(-0.01)	(1.29)	(1.21)	(0.79)
Race: American Indian or Alaska	1.128	0.893	0.852	0.880	0.803	0.859
Native	(0.86)	(-0.81)	(-0.94)	(-0.51)	(-0.81)	(-0.52)
Missing Obs.: Race & ethnicity	1.144	0.920	1.523	0.932	0.989	0.558
	(0.35)	(-0.23)	(1.03)	(-0.12)	(-0.01)	(-0.58)
Lives with someone who smokes	2.455**	1.302**	1.104	2.094**	2.196**	2.338**
cigarettes	(16.43)	(4.72)	(1.38)	(8.07)	(7.69)	(7.44)
Lives with someone who uses	1.527**	2.066**	2.361**	1.190	1.027	0.936
smokeless tobacco	(4.24)	(7.93)	(8.67)	(1.26)	(0.18)	(-0.40)
Missing: Lives with someone who	1.267	1.018	1.551	2.946*	2.669	4.363
smokes cigarettes	(0.62)	(0.06)	(1.30)	(2.11)	(1.35)	(1.86)
Missing: Lives with someone who	0.940	1.322	1.224	1.314	1.546	1.247
uses smokeless tobacco	(-0.16)	(0.77)	(0.58)	(0.51)	(0.68)	(0.32)
How often see actors using	1.331	1.152	1.069	0.997	0.735	0.888
tobacco: Missing	(0.95)	(0.43)	(0.19)	(-0.01)	(-0.58)	(-0.21)
How often see actors using	1.312	1.559	1.695	0.930	0.678	0.600
tobacco: Do not watch TV/movies	(1.13)	(1.83)	(1.73)	(-0.17)	(-0.82)	(-0.99)
How often see actors using	0.908	0.938	1.125	0.673	0.593	0.617
tobacco: Rarely	(-0.58)	(-0.33)	(0.47)	(-1.08)	(-1.21)	(-1.02)
How often see actors using	1.064	1.378	1.379	0.761	0.739	0.680
tobacco: Sometimes	(0.41)	(1.76)	(1.41)	(-0.81)	(-0.77)	(-0.90)
How often see actors using	1.410*	1.659**	1.702*	1.015	0.850	0.787
tobacco: Most of the time	(2.24)	(2.78)	(2.35)	(0.04)	(-0.42)	(-0.57)
Smoking makes people look	2.136*	1.391	1.779	0.877	1.037	0.800
cool/fit in? - Missing	(2.05)	(1.15)	(1.69)	(-0.32)	(0.08)	(-0.43)
Smoking make people look	1.233	1.634**	1.704**	0.545**	0.416**	0.364**
cool/fit in? - Definitely yes	(0.97)	(2.83)	(3.42)	(-2.97)	(-4.07)	(-4.19)

Table A3.1 (Continued)

Smoking make people look cool/fit in? - Probably yes	1.380*	1.247*	1.535**	0.579**	0.482**	0.452**
	(2.50)	(2.06)	(3.92)	(-3.23)	(-4.04)	(-3.88)
Smoking make people look cool/fit in? - Probably not	1.233*	1.198*	1.469**	0.865	0.833	0.721*
	(2.26)	(2.18)	(4.20)	(-1.22)	(-1.42)	(-2.31)
Smoke cigarette if friend offered it? - Missing	3.557**	3.842**	3.779**	11.279**	25.431**	23.860**
	(3.45)	(3.72)	(3.36)	(4.17)	(4.81)	(4.34)
Smoke cigarette if friend offered it? - Definitely yes	122.647**	24.877**	22.233**	213.898**	672.856**	846.186**
	(19.13)	(27.93)	(28.45)	(34.12)	(29.06)	(25.98)
Smoke cigarette if friend offered it? - Probably yes	38.963**	12.171**	11.369**	31.843**	86.475**	99.666**
	(29.15)	(29.12)	(24.62)	(23.54)	(20.92)	(18.37)
Smoke cigarette if friend offered it? - Probably not	5.355**	4.123**	4.053**	4.487**	6.885**	6.586**
	(26.04)	(21.71)	(14.66)	(9.32)	(7.98)	(6.47)
Constant	0.075**	0.110**	0.027**	0.004**	0.001**	0.001**
	(-14.59)	(-10.96)	(-13.82)	(-13.28)	(-12.84)	(-11.74)
N	13203	13092	13228	13327	13332	11778
Pseudo R-square	0.3258	0.2673	0.2848	0.4799	0.5522	0.5627
Mean(Dependent Variable)	0.477	0.344	0.152	0.103	0.088	0.079

Notes: Regressions use survey-weighted 2006 National Youth Tobacco Survey data on high school students aged 14 to 18, unless otherwise noted. "Non-cigarette tobacco products" refer to chewing tobacco, snuff, dip, cigars, pipes, bidis, and kreteks. All controls are listed. **[*] denote statistical significance at the 1%[5%] level.

Table A3.2: Adjusted Specification for Propensity to be a Current Smoker, Odds Ratio/(t-statistic)	
	Current Smoker
Year of age = 15	0.9890**
	(0.2499)
Year of age = 16	1.4886**
	(0.2731)
Year of age = 17	1.7918**
	(0.2966)
Year of age = 18	2.1851**
	(0.3188)
Grade 10	0.1740
	(0.1468)
Grade 11	0.0539
	(0.1839)
Grade 12	-0.0427
	(0.2165)
Female	0.0402
	(0.0767)
Missing Obs.: Gender	-0.5385
	(0.4860)
Hispanic	-0.5771**
	(0.1242)
Hispanic & White	0.2764
	(0.2063)
Missing Obs.: Ethnicity	0.2751
	(0.3266)
Race: White & another race	0.7569**
	(0.2406)
Race: Black	-1.3327**
	(0.1221)
Race: Asian	-0.7868**
	(0.2087)
Race: Native Hawaiian or Pacific Islander	-0.0325
	(0.2467)
Race: American Indian or Alaska Native	-0.3136
	(0.2368)
Missing Obs.: Race & ethnicity	-0.4433
	(0.5289)
Lives with someone who Smokes Cigarettes	1.1857**
	(0.0780)
Lives with someone who uses smokeless tobacco	0.3480** (0.1069)
Missing: Lives with someone who smokes cigarettes	0.6890 (0.4207)

Table A3.2 (Continued)

Missing: Lives with someone who uses smokeless tobacco	0.5433 (0.4160)
How often see actors using tobacco: Missing	0.1975 (0.4071)
How often see actors using tobacco: Do not watch TV/movies	0.3851 (0.3315)
How often see actors using tobacco: Rarely	-0.4189 (0.3050)
How often see actors using tobacco: Sometimes	-0.1578 (0.2758)
How often see actors using tobacco: Most of the time	0.2412 (0.2734)
Smoking makes people look cool/fit in? - Missing	0.9555* (0.3784)
Smoking make people look cool/fit in? - Definitely yes	1.3972** (0.1675)
Smoking make people look cool/fit in? - Probably yes	0.9451** (0.1304)
Smoking make people look cool/fit in? - Probably not	1.1818** (0.0955)
Constant	-4.4836** (0.3676)
N	13332
Adjusted R-square	0.1527
Mean(Current Smoking)	0.088

Notes: Data are from the 2006 National Youth Tobacco Survey on high school students aged 14 to 18. The regression applies survey weights. All controls are listed. **[*] denote statistical significance at the 1%[5%] level.

Table A3.3: Change in Current Cigarette Smoking, Coefficients/(Standard Error)				
	Δ Current Smoker, Ages 14 to 18			
Years considered:	2004-2012		2009-2012	
Specification:	Baseline	ΔE-cig sales	ΔE-cig sales	ΔE-cig ad spending
Low Pr(Smoker) · ΔCig. tax (in dollars)	0.0044	0.0056		
	(0.0245)	(0.0254)		
Mid Pr(Smoker) · ΔCig. tax (in dollars)	-0.0299	-0.0846*		
	(0.0363)	(0.0377)		
High Pr(Smoker) · ΔCig. tax (in dollars)	-0.1046**	-0.1683**		
	(0.0352)	(0.0366)		
Low Pr(Smoker) · ΔTime trend	-0.0016	-0.0019	-0.0029	-0.0026
	(0.0036)	(0.0043)	(0.0075)	(0.0063)
Mid Pr(Smoker) · ΔTime trend	-0.0084	0.0055	0.0264*	0.0161
	(0.0053)	(0.0063)	(0.0112)	(0.0094)
High Pr(Smoker) · ΔTime trend	-0.0092	0.0071	0.0376**	0.0245**
	(0.0051)	(0.0061)	(0.0108)	(0.0091)
Low Pr(Smoker) · ΔE-cig. sales (in $100m)		0.0004	0.0010	
		(0.0037)	(0.0049)	
Mid Pr(Smoker) · ΔE-cig. sales (in $100m)		-0.0203**	-0.0313**	
		(0.0055)	(0.0073)	
High Pr(Smoker) · ΔE-cig. sales (in $100m)		-0.0237**	-0.0397**	
		(0.0054)	(0.0071)	
Low Pr(Smoker) · ΔE-cig. ads (in $1m)				0.0002
				(0.0010)
Mid Pr(Smoker) · ΔE-cig. ads (in $1m)				-0.0063**
				(0.0015)
High Pr(Smoker) · ΔE-cig. ads (in $1m)				-0.0079**
				(0.0014)
Year = 2009	-0.0009	0.0051		
	(0.0116)	(0.0118)		
Year = 2011	0.0036	0.0104	-0.0013	-0.0013
	(0.0058)	(0.0071)	(0.0123)	(0.0123)
Year = 2012	-0.0132*	0.0031		
	(0.0064)	(0.0125)		
Constant	0.0003	-0.0057	0.0003	0.0003
	(0.0069)	(0.0077)	(0.0087)	(0.0087)
N	400	400	200	200
Adjusted R-square	0.130	0.208	0.262	0.262
Mean (Δ Current Smoking)	-0.010	-0.010	-0.009	-0.009

Notes: Observations are year-specific quantiles of predicted propensity to be a current smoker absent e-cigarettes, estimated by logistic analysis of current smoking in 2006 (see Table A3.1), with the resulting equation applied to each year's data. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. Regressions use NYTS data on high school students ages 14-18, federal cigarette taxes (in dollars), e-cigarette sales (in $100 million units), and e-cigarette advertising (in $1 million units). All controls are listed ** [*] denotes statistical significance at the 1% [5%] level.

Table A3.4: Change in Current Cigarette Smoking, Coefficients/(Standard Error)

	Δ Current Smoker, Ages 14 to 17			
Years considered:	2004-2012		2009-2012	
Specification:	Baseline	ΔE-cig sales	ΔE-cig sales	ΔE-cig ad spending
Low Pr(Smoker) ˙ ΔCig. tax (in dollars)	0.0095	0.0109		
	(0.0273)	(0.0275)		
Mid Pr(Smoker) ˙ ΔCig. tax (in dollars)	-0.0278	-0.0696		
	(0.0409)	(0.0413)		
High Pr(Smoker) ˙ ΔCig. tax (in dollars)	-0.1609**	-0.2587**		
	(0.0409)	(0.0413)		
Low Pr(Smoker) ˙ ΔTime trend	-0.0019	-0.0022	-0.0033	-0.0030
	(0.0040)	(0.0046)	(0.0071)	(0.0060)
Mid Pr(Smoker) ˙ ΔTime trend	-0.0077	0.0029	0.0149	0.0077
	(0.0060)	(0.0069)	(0.0107)	(0.0089)
High Pr(Smoker) ˙ ΔTime trend	-0.0037	0.0212**	0.0862**	0.0629**
	(0.0060)	(0.0069)	(0.0107)	(0.0089)
Low Pr(Smoker) ˙ ΔE-cig. sales (in $100m)		0.0005	0.0011	
		(0.0040)	(0.0047)	
Mid Pr(Smoker) ˙ ΔE-cig. sales (in $100m)		-0.0155*	-0.0219**	
		(0.0061)	(0.0070)	
High Pr(Smoker) ˙ ΔE-cig. sales (in $100m)		-0.0364**	-0.0705**	
		(0.0061)	(0.0070)	
Low Pr(Smoker) ˙ ΔE-cig. ads (in $1m)				0.0002
				(0.0009)
Mid Pr(Smoker) ˙ ΔE-cig. ads (in $1m)				-0.0044**
				(0.0014)
High Pr(Smoker) ˙ ΔE-cig. ads (in $1m)				-0.0141**
				(0.0014)
Year = 2009	0.0016	0.0083		
	(0.0129)	(0.0127)		
Year = 2011	0.0093	0.0169*	-0.0007	-0.0007
	(0.0066)	(0.0078)	(0.0116)	(0.0116)
Year = 2012	-0.0133	0.0050		
	(0.0072)	(0.0135)		
Constant	-0.0022	-0.0089	0.0004	0.0004
	(0.0077)	(0.0083)	(0.0082)	(0.0082)
N	320	320	160	160
Adjusted R-square	0.157	0.276	0.498	0.498
Mean (Δ Current Smoking)	-0.009	-0.009	-0.008	-0.008

Notes: Observations are year-specific quantiles of predicted propensity to be a current smoker absent e-cigarettes, estimated by logistic analysis of current smoking in 2006 (see Table A3.1), with the resulting equation applied to each year's data. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. Regressions use NYTS data on high school students ages 14-17, federal cigarette taxes (in dollars), e-cigarette sales (in $100 million units), and e-cigarette advertising (in $1 million units). All controls are listed ** [*] denotes statistical significance at the 1% [5%] level.

Table A3.5: Specification Checks for 2009 to 2012 Change in Current Smoking Analyses, Coefficients/(Standard Error)

	Δ Current Smoker			
Years considered:	Table 3.2 Specification + Controls for ΔTime trend · Independent Variables from Propensity Regression		Alternative Propensity to Smoke Estimation	
	(1)	(2)	(3)	(4)
Low Pr(Smoker) · ΔTime trend	-0.0003	-0.0010	0.0017	-0.0000
	(0.0110)	(0.0101)	(0.0094)	(0.0079)
Mid Pr(Smoker) · ΔTime trend	0.0514*	0.0403	0.0016	0.0016
	(0.0257)	(0.0250)	(0.0148)	(0.0124)
High Pr(Smoker) · ΔTime trend	0.0397	0.0247	-0.0063	-0.0130
	(0.0295)	(0.0290)	(0.0138)	(0.0115)
Low Pr(Smoker) · ΔE-cig. sales (in $100m)	-0.0020		-0.0052	
	(0.0050)		(0.0062)	
Mid Pr(Smoker) · ΔE-cig. sales (in $100m)	-0.0336**		0.0000	
	(0.0074)		(0.0096)	
High Pr(Smoker) · ΔE-cig. sales (in $100m)	-0.0457**		-0.0202*	
	(0.0082)		(0.0090)	
Low Pr(Smoker) · ΔE-cig. ads (in $1m)		-0.0004		-0.0010
		(0.0010)		(0.0012)
Mid Pr(Smoker) · ΔE-cig. ads (in $1m)		-0.0067**		0.0000
		(0.0015)		(0.0019)
High Pr(Smoker) · ΔE-cig. ads (in $1m)		-0.0091**		-0.0040*
		(0.0016)		(0.0018)
Year = 2011	-0.0058	-0.0058	0.0022	0.0022
	(0.0500)	(0.0500)	(0.0152)	(0.0152)
ΔTime trend · Age 15	-0.0203	-0.0203		
	(0.0220)	(0.0220)		
ΔTime trend · Age 16	-0.0311	-0.0311		
	(0.0300)	(0.0300)		
ΔTime trend · Age 17	-0.0648	-0.0648		
	(0.0389)	(0.0389)		
ΔTime trend · Age 18	-0.0713	-0.0713		
	(0.0444)	(0.0444)		
ΔTime trend · Grade 10	0.0200	0.0200		
	(0.0238)	(0.0238)		
ΔTime trend · Grade 11	0.0475	0.0475		
	(0.0301)	(0.0301)		
ΔTime trend · Grade 12	0.0439	0.0439		
	(0.0382)	(0.0382)		
ΔTime trend · Female	-0.0204	-0.0204		
	(0.0125)	(0.0125)		

Table A3.5 (Continued)

ΔTime trend · Missing Obs: Gender	-0.1088	-0.1088
	(0.1846)	(0.1846)
ΔTime trend · Hispanic	-0.0128	-0.0128
	(0.0308)	(0.0308)
ΔTime trend · White & Hispanic	0.0366	0.0366
	(0.0519)	(0.0519)
ΔTime trend · White & Other Race	0.0530	0.0530
	(0.0755)	(0.0755)
ΔTime trend · Race: Black	0.0087	0.0087
	(0.0226)	(0.0226)
ΔTime trend · Race: Asian	0.0782	0.0782
	(0.0617)	(0.0617)
ΔTime trend · Race: Native Hawaiian or Pacific Islander	0.1226	0.1226
	(0.1159)	(0.1159)
ΔTime trend · Race: American Indian or Alaska Native	-0.2536**	-0.2536**
	(0.0883)	(0.0883)
ΔTime trend · Missing Obs: Hispanic	0.3344	0.3344
	(0.2028)	(0.2028)
ΔTime trend · Missing Obs.: Race & ethnicity	0.2572	0.2572
	(0.2630)	(0.2630)
ΔTime trend · Lives with someone who smokes cigarettes	0.0074	0.0074
	(0.0131)	(0.0131)
ΔTime trend · Lives with someone who uses smokeless tobacco	0.0363	0.0363
	(0.0406)	(0.0406)
ΔTime trend · Missing: Lives with someone who smokes cigarettes	-0.0205	-0.0205
	(0.1233)	(0.1233)
ΔTime trend · How often see actors using tobacco: Missing	0.1753	0.1753
	(0.1496)	(0.1496)
ΔTime trend · How often see actors using tobacco: Do not watch TV/movie	-0.1193	-0.1193
	(0.1035)	(0.1035)
ΔTime trend · How often see actors using tobacco: Rarely	0.0162	0.0162
	(0.0489)	(0.0489)
ΔTime trend · How often see actors using tobacco: Sometimes	0.0222	0.0222
	(0.0440)	(0.0440)
ΔTime trend · How often see actors using tobacco: Most of the time	-0.0032	-0.0032
	(0.0454)	(0.0454)
ΔTime trend · Smoking makes people look cool/fit in? - Missing	0.0771	0.0771
	(0.0670)	(0.0670)
ΔTime trend · Smoking make people look cool/fit in? - Definitely yes	0.1436*	0.1436*
	(0.0717)	(0.0717)
ΔTime trend · Smoking make people look cool/fit in? - Probably yes	-0.0326	-0.0326
	(0.0497)	(0.0497)
ΔTime trend · Smoking make people look cool/fit in? - Probably not	-0.0703*	-0.0703*
	(0.0309)	(0.0309)

Table A3.5 (Continued)

ΔTime trend · Smoke cigarette if friend offered it? - Missing	-0.6382*	-0.6382*		
	(0.3026)	(0.3026)		
ΔTime trend · Smoke cigarette if friend offered it? - Definitely yes	0.0182	0.0182		
	(0.0301)	(0.0301)		
ΔTime trend · Smoke cigarette if friend offered it? - Probably yes	-0.0122	-0.0122		
	(0.0266)	(0.0266)		
ΔTime trend · Smoke cigarette if friend offered it? - Probably not	0.0117	0.0117		
	(0.0116)	(0.0116)		
Constant	0.0138	0.0138	-0.0034	-0.0034
	(0.0497)	(0.0497)	(0.0107)	(0.0107)
N	200	200	198	198
Adjusted R-square	0.330	0.330	0.085	0.085
Mean(Δ Current Smoking)	-0.009	-0.009	-0.014	-0.014

Notes: These specification checks adjust the regressions in column 3 and 4 of Table 3.2. Observations are year-specific quantiles of predicted propensity to be a current smoker in the absence of e-cigarettes, estimated using logistic regression analysis of current smoking in the 2006 data and applying the resulting equation to later years' data. Low, Mid, and High propensity to smoke groups indicate whether the quantile covers the following percentiles of predicted propensity to be a current smoker: (20, 80], (80, 90], and (90,100], respectively. In estimating propensity to smoke, the first specification uses the current smoker regression in Appendix Table A3.1, while the second uses the Appendix Table A3.2 regression (which omits controls for how likely the respondent would be to smoke a cigarette if a friend offered it). Regressions use 2009, 2011, and 2012 National Youth Tobacco Survey data on high school students ages 14-18, along with federal cigarette tax rates (in dollars), electronic cigarette sales (in $100 million units), and electronic cigarette advertising (in $1 million units). All controls are listed. ** [*] denotes statistical significance at the 1% [5%] level.

Data Appendix

I. Paper I Data

All data come from the 1979 National Longitudinal Survey of Youth (NLSY), using data on either female respondents to the original cohort or their children. As the analysis is longitudinal covering 2002 to 2010, I use cross-survey weights for respondents who are “in any or all” of those years (http://www.nlsinfo.org/weights/childya). Most variables are coded exactly as in the data or fully described sufficiently in the text. However, the adverse event variables and a few controls are somewhat more complicated. These are listed in below, along with a detailed description of how each was constructed.

<u>Adverse Event Variables</u>

Shock Since Last Interview – The key independent variable in these analyses is $Shock_{it}$, a binary indicator for whether either adverse event (crime victimization or the death of a non-family member the respondent felt close to) occurred since the prior interview (i.e., after interview t-1 and before interview t). $Shock_{it}$ is coded as non-missing if and only if both the crime and death data are non-missing:

$Shock_{it}$ = 0 if crime-since-last-interview=0 & non-family-death-since-last-interview=0

$Shock_{it}$ = 1 if crime-since-last-interview=1 & non-family-death-since-last-interview=1

$Shock_{it}$ = 1 if crime-since-last-interview=1 & non-family-death-since-last-interview=0

$Shock_{it}$ = 1 if crime-since-last-interview=0 & non-family-death-since-last-interview=1

$Shock_{it}$ = 0 if t is a childhood survey (i.e., pre-young adult survey eligibility)[97]

[97] Childhood shocks are coded and controlled for separately.

Crime Victim Since Last Interview – Crime victimization questions include both “ever” and “since last interview” interpretations. Respondents are also asked their age at victimization; and those who indicate multiple victimizations are asked their age at first victimization and at the most recent victimization. This analysis requires a “since-last-interview” interpretation. Thus, the age-at-victimization data are used to determine when the respondent was victimized relative to each survey, based on the following rules:

1) If the respondent’s age-at-victimization is strictly greater than their interview-age at the survey prior to that in which they reported having been victimized, the crime victimization is considered as since-last-interview for the reporting-year.
2) If the respondent’s age-at-victimization is an age they were not interviewed at (e.g., they were interviewed at ages 14, 16, and 18, and report a crime having occurred at age 15), crime-since-last-interview is assigned to the first survey after that age (here, the age-16 survey).
3) If the respondent’s age-at-victimization is an age they were interviewed at, but not the age of the year in which the crime was reported, crime-since-last interview is assigned probabilistically. For clarity, assume a respondent who was interviewed at ages 14, 16, and 18 reports having been victimized at age 14 in his age-18 survey. Using the month-of-birth and interview-month data, I calculate the number of months the respondent had been age-14 as of his age-14 survey.
    a. If the respondent had been the victimization age for 7 or more months at the interview date, the crime is assigned as having occurred in the period prior to that interview.

b. If the respondent had been the victimization age for 6 or fewer months at the interview date, the crime is assigned as having occurred in the period just after that interview.

4) For interviews prior to the respondent's first young adult survey (young adult survey eligibility begins the year a respondent turns 15), crime-since-last-interview is coded as 0. Childhood crime victimization is coded and controlled for separately.

Death Since Last Interview Variables – Data on deaths of someone the respondent felt close to is recorded beginning in 2006, and always by the respondent's relationship to the specific person. Retrospective questions ask about the date of death (year-month combinations) and, if month was not provided, the respondent's age when the death occurred. First, I explain how death-since-last-interview is calculated, and then list the relationship-types that correspond to non-family deaths, immediate-family deaths, and extended-family deaths.

Before being asked about specific deaths, respondents are asked if anyone they felt close to died since they were age 10. If they answer yes, a series of more detailed questions are asked (e.g., their relationship to that person, the date of that person's death, the respondent's age at that person's death). Death since last interview is assigned based first based on date of death, as follows:

1) If the date of death falls strictly between two interviews (i.e., after interview A and before the next interview, B) or is reported at interview B and occurred in that same month and year, death since last interview is assigned to interview B.
2) Say a death is reported as having occurred in the same month and year as a prior interview. If, at the prior interview, the respondent stated that no one close to them had

died as of that interview, the death is considered to have occurred after that interview and before the subsequent one.

a. If the date of death is equal to a prior interview's date, and the respondent does not state "no deaths" at that interview, I cannot assign the event as having occurred directly before or after that interview. In this case, the since-last-interview assignment is omitted. This may tend to bias my results towards zero, as some individuals marked as not having a death-since-last-interview will have.

If the date of death is missing and the respondent's age at the death is provided, proceed as follows:

3) If the respondent's age when the death occurred falls strictly between two interview ages (i.e., older than at interview A and younger than at interview B), death since last interview is assigned to interview B.
4) If the respondent's age when the death occurred equals his/her age at the interview when that death is reported, the "death since last interview" is assigned to the reporting-interview.
5) If the respondent's age when the death occurred equals his/her age at a prior interview the death, death-since-last interview is assigned probabilistically. For clarity, assume a respondent who was interviewed at ages 14, 16, and 18 reports, in his age-18 survey, that a friend died when he was 14. Using the month-of-birth and interview-month data, I calculate the number of months the respondent had been age-14 as of his age-14 survey.

    a. If the respondent had been 14—the age at friend's death—for 7 or more months at the interview date, the death is assigned as having occurred in the period prior to that interview.

b. If the respondent had been 14 for 6 or fewer months at the interview date, the crime is assigned as having occurred in the period just after that interview.

6) For interviews prior to the respondent's first young adult survey (young adult survey eligibility begins the year a respondent turns 15), non-family-deaths-since-last-interview are coded as 0. Childhood non-family deaths are coded and controlled for separately.

Relationship-categories

Non-Family Relationships: teacher, friend, other non-relative adult, other non-relative child.

Immediate Family Relationships: parent, stepparent, sibling, child, spouse/partner

Extended Family Relationships: grandparent, great grandparent, step-grandparent, aunt, uncle, great-aunt, great-uncle, cousin, niece, nephew, other relative

Childhood Adverse Events

Childhood Shock – This variable is a binary indicator that the respondent was either the victim of a violent crime or experienced the death of a non-family-member she was close to before her last childhood interview (where "childhood interview" refers to the NLSY child surveys, administered up until the year a respondent turned 15, when young adult surveys began). It is defined as follows:

Childhood Shock = 0 if Childhood Crime Victim=0 & Non-Family Death in Childhood=0

Childhood Shock = 1 if Childhood Crime Victim=1 or Non-Family Death in Childhood=1

Childhood Crime Victim – Childhood crime victimization refers to having been a crime victim before one's last childhood interview (i.e., the last interview before the year a respondent turned 15). It is assigned as follows:

1) If age at victimization is strictly less that the individual's age at her last childhood survey (i.e., crime since last interview would apply to a childhood survey year), that respondent is coded as having been a childhood crime victim.

2) If age at victimization equals the respondent's age at her last childhood survey, she is coded as having been a childhood crime victim if and only if she had been that age for 7 or more months at interview (based on her month of birth and month of interview). Otherwise, that crime victimization is assign as having occurred between her last childhood and first young adult interviews.

Non-Family Death in Childhood – Deaths occurring in childhood refer to those that happened before one's last childhood interview (i.e., the last interview before the year a respondent turned 15). This control is used for non-family deaths only. It is assigned as follows:

1) If date at death precedes the date of the last childhood survey or age-at-death precedes the respondent's age at his last childhood survey, the respondent is coded as having experienced the death of someone close to them during their childhood.

   a. Note here again, that if date-at-death equals the date at the respondent's last childhood interview, I have no basis for assigning the death as having occurred directly before or after that interview. In this case, the individual is not noted as having experienced a death during their childhood. This may bias certain results towards zero, as some individuals marked as not having experienced a death in childhood will have.

2) If age-at-death equals the respondent's age at his last childhood survey, he is coded as having experienced a death in childhood if and only if he had been that age for 7 or more

months at interview (based on month of birth and month of interview). Otherwise, that death is assigned as having occurred between his last childhood and first young adult interviews (i.e., a death since last interview).

Non-Adverse-Event Variables

Early Childhood Family Income – Original cohort data on total net family income in the past calendar year and on the poverty level cutoff for the families' size are used to calculate percent of the poverty level in any given year. This is merged with data on the age each child-respondent turned in a given year. Respondents are assigned the earliest non-zero observation for family's percent of the poverty level that is observed for ages 0 to 5.[98]

Days Exercised per Week – This variable uses data from a question posed in the 2008 and 2010 young adult survey: "During a typical week (7 days), how many times on average do you do the following kinds of activities for more than 15 minutes during your free time? Strenuous exercise where your heart beats rapidly such as running, jogging, basketball, cheerleading, vigorous cycling, rollerblading, soccer, martial arts, aerobics, etc." The recorded data groups responses exceeding "1 time per week" in sets of two. The exercise variable used in this paper recodes these responses to represent the average days exercised per week implied by each NLSY-code, such that a one unit increase reflects one additional day of exercise, as follows:

Respondent Answer	NLSY Code	Days Exercised per Week
0 times per week	1	0
1 time per week	2	1
2 or 3 times per week	3	2.5
4 or 5 times per week	4	4.5
6 or 7 times per week	5	6.5

[98] Earliest non-zero observations are used instead of averages or earliest non-missing observations because careful consideration of coded values suggests that some zeros may represent miscoded missing variables.

Mother's Education – Mother's education is defined based on mother's highest-grade-completed as reported in the child data at the last childhood survey. Thus, graduating high school is defined as having completed 12th grade, some college as 1 to 3 years of college, and graduating college as 4 or more years of college. For respondents missing mother's education observations at their last childhood survey, the most recent earlier survey is use (moving back in time until a non-missing observation is identified). For six cases, this procedure still leaves a missing observation. In these cases, I go to the original cohort data. Two of these mothers list their education as in the lowest education grouping (fewer than 12 years) at or after their child's first young adult interview, and are thus assigned that level. One gives her education only in the year of the respondent's first young adult interview, and is assigned that value. For the remaining three, the only observed education levels are recorded for the 1980s or early 1990s. They are assigned the education level that corresponds to the latest of these observations.

Sibling Age – Sibling age data comes from age data on respondent's siblings who were also respondents to the NLSY (i.e., same mother). Siblings are matched based on sibling identifiers included in the child data.

Neighborhood rankings (crime and violence; parent supervision) – Respondents are asked to rank the degree to which specific issues are a problem in their neighborhood, given the options: "big problem," "somewhat of a problem," "not a problem". All ranks are coded as 0 (not a problem), 1 (somewhat of a problem), and 2 (big problem). The "crime and violence" and "too many parents who don't supervise their children" ranks are used in the regression analyses.

Sports/team variables – The corresponding survey questions all refer to school, either by asking about behaviors between school and dinner, or asking about teams/clubs at school. Thus, for respondents who left this question blank and were not enrolled in school as of their interview, missing observations are coded as zeros.

II. Paper II Data

NHIS Smoking Histories

The NHIS allows for a variety of smoking statuses at interview, with slight variations between the years. In the 1987 survey, respondents who indicated that they had smoked 100 cigarettes in their lifetime could identify as a former occasional smoker, a former regular smoker, a current smoker, a smoker whose current status is unknown, or "unknown." The 1979 options differ slightly, as they replace the two former-smoker options with a single "former smoker" response, and add an "occasional smoker" option. For our purposes, a current or past "smoker" includes only those who smoked regularly, as the questions about age at initiation and cessation were only asked of these respondents. The unknown current status smokers were also asked for initiation ages, and thus are included in initiation analyses but not cessation analyses.

For current or past regular smokers, we define smoking histories by deducing an initiation year from the respondent's indicated age at which he or she began smoking regularly, and a cessation year from either the amount of time since he or she quit smoking (1978-1980 surveys) or the age at which he or she quit (1987 survey). Individuals are considered smokers in all years between initiation and cessation, or through the survey date if they smoke at interview.

Due to differences in the survey questions, the process to generate smoking histories varies slightly between the 1978-1980 survey respondents and the 1987 respondents. As the

1978-1980 surveys do not list year of birth, we use age-at-interview, age-at-initiation, and interview timing (deduced from interview year, quarter, and week) to deduce year of smoking initiation. The year assigned as a respondent's year of initiation is whichever year the respondent spent the most days at their listed initiation-age. For example, a respondent who began smoking at age 15 and was born in the first half of 1950 has a start year of 1965, whereas one born in the second half of 1950 would be assigned a start year of 1966. Year of cessation is assigned in the same manner, such that a respondent who quit at age 20 and was born in the first half of 1950 would have a cessation-year of 1970, and 1971 if born in the second half of the year.

Based on this approach, 47 respondents have a cessation year that precedes their initiation year. However, as age-at-initiation allows for two distinct years in which the individual could have started smoking, 14 of these contradictions could be due to assigning the respondent to the wrong initiation year. These are corrected such that the initiation year is moved one year earlier, to coincide with the cessation year. We drop the 33 remaining respondents whose initiation and cessation years conflict. One respondent to the 1987 survey gave a birth year and age at cessation that implied a quit age after the survey. We omit this individual from the analyses.

Smoking histories for respondents to the 1987 NHIS are based on year and month of birth; reported age started smoking regularly; age stopped smoking the last time (asked of former smokers); and years a regular smoker. The survey asks respondents the age when they began smoking regularly. If age of initiation is missing but the respondent is a smoker when interviewed, we can use the number of years they smoked cigarettes regularly along with age at interview to deduce age at initiation: If the individual is not a current smoker and did not give an age at initiation, we cannot deduce the year they began smoking.

If a birth month is not listed, we assume the respondent was born in the first half of the year. If a birth year is not listed, we define birth year probabilistically based on interview date and age at interview. That is, respondents interviewed in the first half of the year are assumed to not have had a birthday yet that year, and respondents interviewed in the second half of the year are assumed to have passed their birthdate.

Table DA1 details missing initiation and cessation year data for each survey, limiting consideration to respondents who meet our inclusion criteria. Among current, former, and missing information smokers, only 0.8 percent are missing their initiation-year. Among former smokers, only 3.3 percent are missing their cessation-year.

Table DA1: Missing Smoking Initiation and Cessation Years,
Sample limited to respondents ages 25 to 64 at interview who completed their own survey (i.e., not completed by a proxy)

	Survey Year			
	1978	1979	1980	1987
Current Smokers				
N	2898	5866	2483	4685
# Missing Year-Start	17	52	14	19
Former Smokers				
N	1597	3326	1416	3298
# Missing Year-Start	14	24	8	16
# Missing Year-Stop	122	120	43	33
Smoker, Current Status Unknown				
N	27	63	27	29
# Missing Year Start	13	19	10	11

Other Information

Education is defined based on a question about “the highest grade or year” the respondent completed in a “regular school”—“one which advanced a person towards an elementary or high school diploma or a college, university or professional school degree.”

The 1978-1980 NHIS codes income data in groups. To consider the most-disaggregated form of these data, we specify income as a series of dummy variables for each group.

Brand Choice Assignment to Filtration Categories

Figure DA1 shows the top 10 brands smoked for men and women in 1978-80. Rather than estimate the actual brand smoked, we focus on categories of filtration.

Figure DA1: Brand-Shares of Smokers by Education, for Top 10 Brands
(a) Men

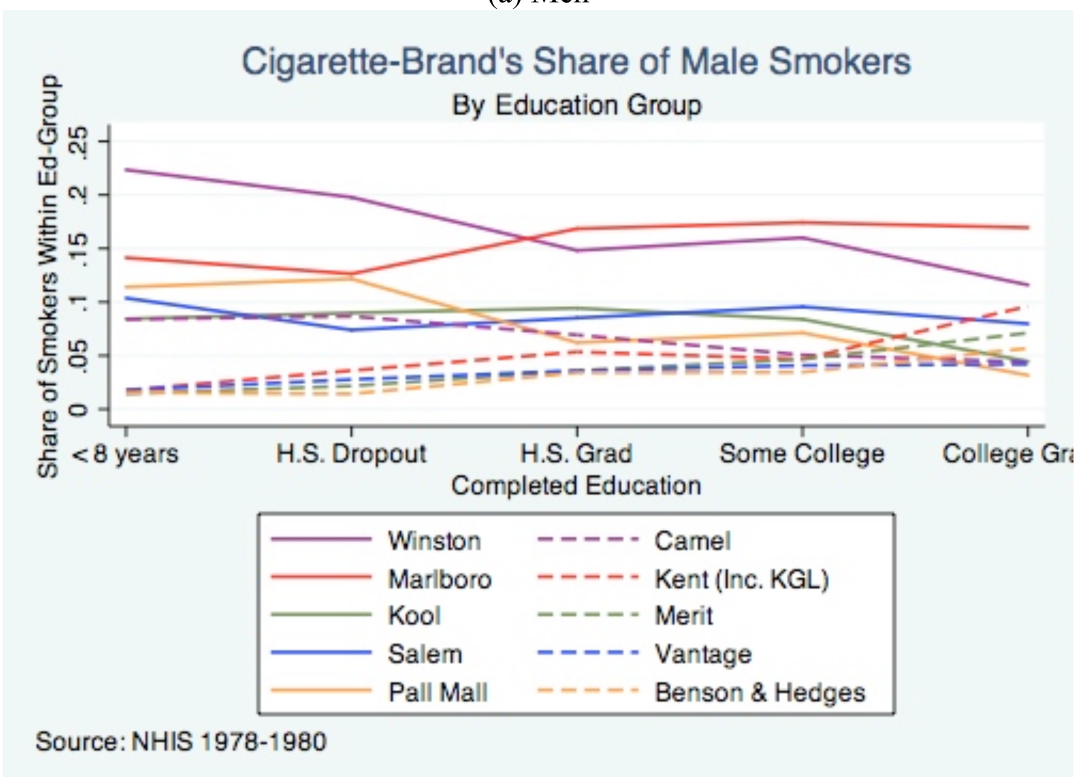
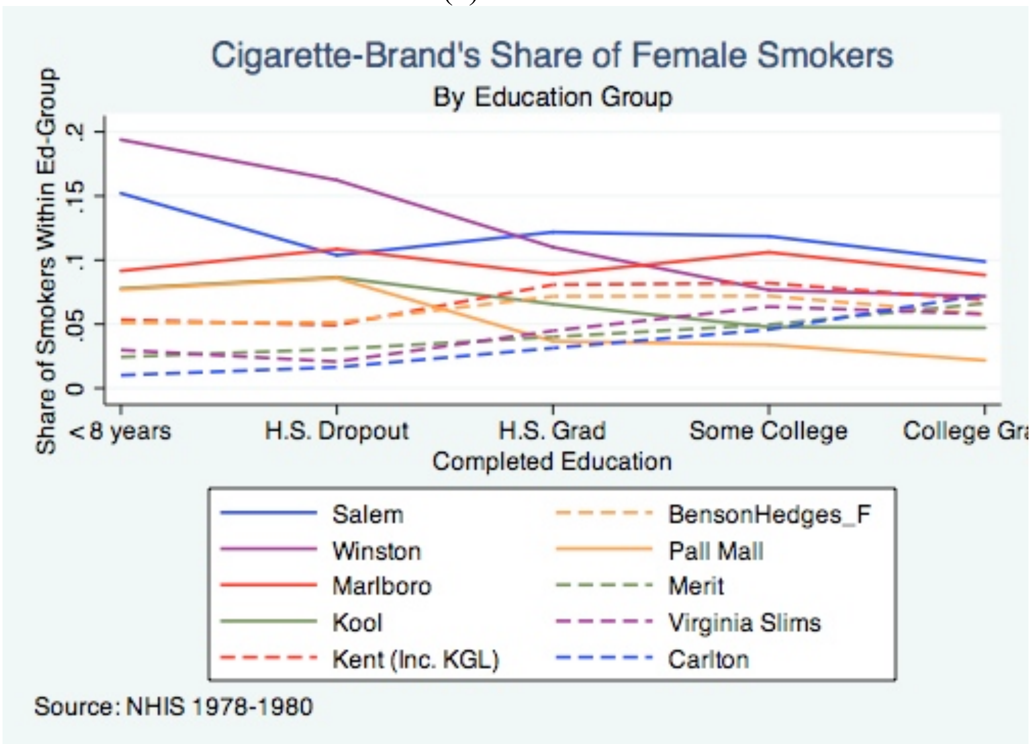


Figure DA1 (Continued)
(b) Women



Cigarette brands listed by respondents to the 1978-1980 NHIS smoking supplements were assigned as high filtration (Hi-Fi), low-filtration (Lo-Fi) or unfiltered (Straights) based on categorization in industry documents from the late 1970s and early 1980s. Any cigarette categorized as a particularly high filtration version of Hi-Fi (e.g., Super Hi-Fi, Ultra Hi-Fi, etc.) is considered a Hi-Fi for the purposes of this analysis, as the small number of cigarettes in these subgroups would amount to more of a brand-specific identifier than a subgroup classification. Note that, for comparability of brands over time, we do not include 1987 brands in this section.

Table DA2 lists cigarettes and filtration-type by the source providing that categorization. Note that the NHIS brand data include not only cigarette name, but length, filtration status, and package type. The categorization below omits the latter three categories. Hi-Fi cigarettes are identified by whether the parent brand includes exclusively high-filtration cigarettes ("Hi-Fi

Brand") or multiple types ("Hi-Fi Brand Extension"). Any cigarette name that an NHIS respondent paired with an "unfiltered cigarette" description is recoded as a Straight (i.e., unfiltered cigarette) for that respondent, regardless of the type indicated below. Cigarettes not listed below but named by at least one 1978-1980 NHIS respondent meeting our inclusion restrictions are coded as unfiltered.

Table DA2: Categorization of Cigarettes by Filtration

Filtration Category	Source	Cigarette Name in NHIS Data
High Filtration		
Hi-Fi Brand	Lorillard 1975	American Lights, Carlton, Doral, Galaxy, Kent, Kent Golden Lights, Lark, Merit, Multifilter, Now, Parliament, Silva Thins, Tareyton, Tempo, True, Vantage
Hi-Fi Brand	Lorillard 1978	Decade, Real
Hi-Fi Brand	Philip Morris 1980	Triumph, Cambridge
Hi-Fi Line Extension	Lorillard 1975	Kool Milds, Kool Milds 100, Kool Milds (unknown), L & M Lights, Lucky Ten, Lucky 100's, Marlboro Lights, Pall Mall Extra Lights, Raleigh Lights, Raleigh Lights 100, Salem Lights, Salem Lights (unknown), Winston Lights, Winston Lights 100
Hi-Fi Line Extensions	Brown & Williamson 1976	Arctic Lights, Iceberg 100's, Newport Lights, Salem Long Lights, Salem Long Lights 100, Salem Ultra (100)
Hi-Fi Line Extensions	Brown & Williamson (n.d.)	Virginia Slim Lights
Hi-Fi Line Extensions	Lorillard 1978	Benson & Hedges Lights, Camel Lights, Kool Super Lights, Kool Super Lights (unknown), Old Gold Light, Old Gold Lights, Viceroy Rich Lights, Viceroy Extra Milds,
Hi-Fi Line Extensions	Philip Morris 1980	Salem Ultra, Camel Long Lights, Pall Mall Lights, Kool International
Low Filtration		
Lo-Fi	Lorillard 1975	Alpine, Belair, Benson & Hedges, Camel, Chesterfield, DuMaurier, Eve, Kool, Lucky Strike, L&M, Marlboro, Max, Montclair, More, Newport, Old Gold, Raleigh, Pall Mall, Philip Morris International, Salem, Saratoga, Spring, Tall, Viceroy, Virginia Slims, Winston

Cigarette Advertising Data

Cigarette advertising expenditure for 1970 and from 1975 onward is available in *The Federal Trade Commission Cigarette Report for 2007 and 2008*. That publication provides two sets of data on overall cigarette advertising expenditure, one for 1970 and from 1975-onward, and a second for 1962 to 1974. These two sets differ slightly in their 1970 values, with the latter data presented as more complete. Thus, we use the ratio of the two 1970 expenditures in the two series to adjust the 1963-1974 data so that it matches the more recent data. This provides our cigarette advertising data for 1963 through 1980.

Kellner (1973, p. 234) lists advertising expenditures for 1950-1970 drawn from issues of *Advertising Age*. We take ratios of the 1963-1970 FTC advertising figures to Kellner's corresponding data to generate an adjustment factor, which we apply to Kellner's data on advertising between 1950-1962 and to the subsequent data presented below.

Nicholls (1951) provides cigarette advertising expenditure statistics for 1939 to 1949 (p. 160) as well as an index of cigarette advertising expenditure from 1925 to 1931 (p. 82). Borden (1942) provides advertising data for 1929 through 1939 (p. 229). Using the overlap in 1939, we adjust the Borden data to match the Nicholls data. This gives us an advertising series from 1929-1949.

The Nicholls cigarette advertising expenditure index for 1925 to 1931 is the foundation for our 1925-1928 expenditure estimates. To translate this into dollar values, we use Keller's estimate of advertising expenditure in 1925 and our revised series from Borden for 1929-31. The average of these adjustment factors is used to impute spending in 1926-28 from the index data.

The entire 1925-2008 data series is adjusted by CPI to real 2010 dollars.

Cigarette Tax Rate Data

We construct a time-series of cigarette tax rates for 1925-2011 from the 2011 volume of *The Tax Burden on Tobacco: Historical Compilation*. For 1950-2011, this data provides the federal tax rate as well as a weighted average of the state tax rates for all taxing states. For every year in this period, at least 41 states are taxing cigarettes (47 or more for 1960 onwards). Thus, we define the average annual cigarette tax as the sum of these rates.

The Tax Burden on Tobacco provides federal cigarette tax rates dating back farther (through 1864), but not state rates. To estimate state rates for 1921-1950, we divide net state tobacco taxes collected by the number of cigarette-packs sold in that year (measured as the number of cigarettes divided by 20). This gives us a preliminary estimate for the average state tax rate. Taking the ratio of this estimate for 1950 to the 1950 weighted average state tax rate in the *Tax Burden* gives us an adjustment factor to bring these estimates in line with the observed rates. We adjust the data from 1921 through 1949 using this method and add the state tax rate to the national rate.

Taxes are expressed in cents and are adjusted to 2010 dollars using the CPI.

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