Gateway Theory in a Dynamic Context:
The Effect of Past Substance Use on Current Demand

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A dissertation submitted to the faculty of the University of North Carolina at Chapel Hill in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Economics.

Chapel Hill
2012

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Abstract

WILLIAM B. GRIDER: Gateway Theory in a Dynamic Context:
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(Under the direction of Donna Gilleskie.)

This study investigates the idea that prior consumption of one drug leads to current consumption of another - the so-called gateway theory. The research implements a discrete factor random effects model to jointly estimate current cigarette, alcohol, and marijuana consumption as a function of endogenous prior drug consumption behavior and current prices. The model controls for endogenous arrest incidence and sample attrition by modeling those events. I develop reduced form demand models for the consumption of the three drugs by a cohort of adolescents transitioning into young adulthood, and first estimate the model equations independently of one another, treating endogenous prior behavior as being exogenous. I then implement controls for unobservable heterogeneity, allowing errors to be correlated across the equations in a jointly estimated model that enables an analysis of the effects of changes in prior drug consumption and policy variables on consequent drug use. Results largely support the idea that drinking and tobacco smoking serve as gateways into marijuana consumption, but also indicate that controlling for unobservables weakens the estimated impact of prior drug use on current drug consumption. While the results support the idea that gateway effects exist, they also indicate that ignoring unobservable factors produces upwardly biased estimates of the magnitude of these effects.

Acknowledgments

This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. I would like to thank my advisor, Donna Gilleskie, for helping me see this through to completion despite numerous setbacks along the way. I would also like to thank the other members of my committee: Frank Sloan, Helen Tauchen, Sally Stearns, Clement Joubert, and Sandra Campo, for their invaluable assistance during the process of writing this dissertation. I thank participants in the Applied Microeconomics Student Workshop, as well as Brian McManus specifically, for helpful comments. I thank Sara Markowitz, Rosalie Pacula, and Jeffrey DeSimone for providing marijuana price data from the DEA, and Mireille Jacobson for providing marijuana price data from *High Times* magazine. All errors are my own.

Finally, I would like to thank my family for their unwavering support. In particular, I thank my wife, Christine, and my children, Will and Sadie June. I also thank my father and late mother, without whom this effort would not have been possible.

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Chapter 1

Introduction

This study follows an initially adolescent cohort over eight years to investigate the gateway theory of drug use by examining the joint consumption of cigarettes, alcohol, and marijuana. The gateway theory hypothesizes that prior consumption of one drug leads to current consumption of another, and can be applied to various combinations of drug consumption. The drugs most commonly viewed as gateways are tobacco, alcohol, and marijuana. A version studied here posits that tobacco and alcohol consumption increase the probability of subsequently using marijuana and then progressively more powerful drugs (Martin, 2003). Various disciplines examine various versions of the theory, using methods ranging from straightforward correlation analyses to investigations of whether changes in the price of a gateway drug affect future consumption of a harder drug. This research is closer to the latter studies, examining the theory from an economics perspective and using a discrete factor random effects model to jointly estimate current cigarette, alcohol, and marijuana consumption as a function of prior drug consumption behavior and current prices. The model directly controls for arrest activity and sample attrition by jointly modeling those events as well. I postulate reduced form demand models for the consumption of the three drugs by a cohort of adolescents transitioning into young adulthood, and then jointly estimate a dynamic model that controls for unobservable heterogeneity and allows for an analysis of the effects of changes in prior drug consumption and policy variables on observed behavior. Though the model is reduced form, I account for unobservables directly by modeling endogenous past drug use and arrest behavior. The current study does not allow an analysis of the idea that these drugs, and marijuana in particular, serve as gateways into the use of drugs such as cocaine or heroin, but the results largely support the idea that drinking and tobacco smoking serve as gateways into marijuana consumption. Gateway effects are significantly lessened, however, after the implementation of controls for unobservable heterogeneity.

Empirical evidence shows the first use of cigarettes, alcohol, and illicit drugs almost universally occurs, and consequent consumption patterns typically emerge, during adolescence (Kandel, 1975 and 1978). The gateway theory suggests that drugs that are legal for adults, i.e., alcohol and tobacco, lead to marijuana use, and that partially decriminalized drugs like marijuana lead to consumption of harder drugs. Supporters of the theory acknowledge that observed use patterns are consistent with the idea that youth simply experiment with the most accessible, or cheapest, drugs first, but this acknowledgment does not preclude a causal effect resulting from consumption of the gateway drug (Beenstock and Rahav, 2002). For economists, the validity of the theory rests on an assessment of the demand relationship between drugs, and the relevant economic literature typically examines the theory by evaluating whether particular drugs are intertemporal economic complements or substitutes; evidence for complementarity supports the theory; evidence for substitutability calls the theory into question (Pacula, 1995).[1] Assessing all potential avenues for gateway demand relationships between drugs is not the goal of this study, which will focus on potential gateways that lead, on net, from tobacco and alcohol use into marijuana consumption, but the policy implications of any gateway effects are clear. Policies that successfully discourage adolescent consumption of a gateway drug also discourage the consumption of the complementary harder drug (Golub and Johnson, 2001 and 2002); however, if adolescents simply substitute one drug for another, then policies designed to discourage youthful use of, say, alcohol, might have the unintended consequence of encouraging adolescent marijuana use (Pacula, 1995 and 1998b).

Though the empirical literature studying gateway effects largely supports a causal role for the gateway drugs (Golub and Johnson, 2001), questions still exist about attributing direct causality to use of those drugs as opposed to other, potentially unobservable factors. A number of studies produce findings that are not consistent with the theory,[2] and Morral, McCaffrey, and Paddock (2002) develop a model showing that observed cannabis and hard drug usage patterns are consistent with alternative explanations to marijuana serving as the gateway to those other drugs. The gateway literature in economics largely focuses on the effects of gateway drug prices, especially lagged prices - e.g., Pacula (1997) - on harder drug consumption. Few researchers implement controls for unobservables, but those that do find that gateway effects are lessened after the implementation of such controls (Pudney, 2003; Van Ours,

[1] This view is not universal, however. Saffer and Chaloupka (1999), for instance, argue that substitution implies the existence of a gateway. Certainly, multiple and competing effects could be in play, but examining all such effects is beyond the scope of this study.

[2] See, for example, Model (1993); Thies and Register (1993); Goel and Morey (1995); Chaloupka and Laixuthai (1997); DiNardo and Lemieux (2001); Saffer and Chaloupka (1999); Pudney (2003); and Van Ours (2003).

2003; Bretteville-Jensen, Melberg, and Jones, 2008). This study adds to the existing literature by implementing robust controls for unobservable heterogeneity, controlling for current prices, and modeling endogenous prior drug consumption along with endogenous arrest incidence and endogenous sample attrition. The main premise of the gateway theory is that prior consumption of one drug directly affects current demand of a harder drug, so I model current consumption as directly dependent upon past consumption, allowing the implementation of experiments that speak directly to the study of gateway effects. Federal, state, and local authorities spend billions of dollars a year not only to suppress drug use but also to deal with the often costly consequences of such use. To the extent that existing policies result from the idea that a gateway effect drives drug use progression, this study will provide valuable evidence on the validity of those assumptions and the efficacy of the resulting policy approaches.

Much of the existing economic work investigating gateway theory suffers from various limitations. Typically, studies use data that cover only one, or sometimes two, time periods, face a shortage of information on illegal drug prices, and/or model consumption of each drug separately. While this study cannot perfectly circumvent all of these problems, I follow 1997 National Longitudinal Survey of Youth (NLSY97) respondents over eight years and develop a methodology for incorporating non-pecuniary prices for marijuana to estimate the joint consumption of three drugs - tobacco, alcohol, and cannabis. The findings presented below generally support the idea that past cigarette smoking and drinking lead to current marijuana use. The empirical approach, however, permits the implementation of robust controls for both permanent and time-varying unobservable factors like risk preferences, social learning, peer effects, or even budget constraints. These factors constitute avenues through which prior drug consumption might impact current use, and offer an alternative explanation to those posited by proponents of the gateway theory about the causal processes underlying adolescent drug progression (Zickler, 2006). Though the purpose of this work is not to directly examine the different avenues through which latent information impacts drug consumption, and I cannot empirically determine the relative importance of any particular unobservable variable (e.g., peer effects) in explaining the results, the introduction of controls for unobservable heterogeneity significantly reduces predicted gateway effects when compared to results from a model without such controls.

This study does not address the idea that marijuana provides a starting point that leads to consumption of more powerful drugs such as cocaine and heroin due to a lack of specific data on consumption of harder drugs in the NLSY97. Instead, and unlike prior work, I model the joint consumption of cigarettes, alcohol, and marijuana, accounting for unobservables while empirically estimating current drug consumption as a function of three endogenous determinants of self-reported drug use: prior use, arrest incidence, and attrition from the NLSY97 sample. The next chapter provides a more detailed review of the relevant literature. Chapter 3 outlines a theoretical model of how adolescents make drug use decisions; Chapter 4 presents more detailed information about the NLSY97 data and the analytic sample in particular; and Chapter 5 outlines the empirical model. Chapter 6 presents results, and Chapter 7 concludes.

Chapter 2

Background

2.1 Gateway Theory

Gateway theory has no single definition. Across academic disciplines, the theory generally posits that a dose-response relationship exists between a drug user and less powerful "gateway" drugs, with the risk of progression to a more powerful drug increasing with use of the gateway drug (Bretteville-Jensen, Melberg, and Jones, 2008).[1] My work in particular builds off of empirical studies finding that cigarettes and alcohol serve as gateways into the use of illegal drugs,[2] as well as other work (generally not supportive of gateway effects) that investigates the demand relationship between alcohol and marijuana.[3] The theory originates in the fields of psychology and sociology, with early work based on data from various epidemiological studies of U.S. youth and young adults in the late 1960's and early 1970's. Kandel (1978) lists the general empirical observations underlying the theory:[4]

> 1. The period for risk of initiation into illicit drug use is over by the mid 20's.
> 2. A high proportion of youths who have tried marijuana will eventually go on to experiment with other illicit drugs.
> 3. Later age of onset is associated with lesser involvement and greater probability of stopping.
> 4. Clear-cut developmental steps and sequences define drug behavior, so that use of one of the legal drugs almost always precedes use of illegal drugs.

The concept of one drug serving as a gateway to use of another first arose from commonly observed

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1 See Lindsay and Rainey (1997) for a brief summary of potential causal mechanisms proposed over time to explain gateway effects from perspectives outside of economics.

2 See Kandel (1975); Fishburne, Abelson, and Cisin (1980); Kandel and Maloff (1983); Yamaguchi and Kandel (1984a); Ellickson and Hays (1991); Smith (1992); Abdel-Ghany and Wang (2003); Riala et al. (2004); Farrelly et al. (2001); Williams et al. (2001); Sen, Agarwal, and Hofler (2002); and Williams and Mahmoudi (2004).

3 This work includes Thies and Register (1993); Pacula (1995 and 1998b); Chaloupka and Laixuthai (1997); DeSimone (1998); Saffer and Chaloupka (1999); DiNardo and Lemieux (2001); and Cameron and Williams (2001).

4 Kandel actually lists six "patterns of involvement," but the last two apply specifically to heroin addiction.

progression patterns among youth and young adult consumers of drugs. Drug initiation for both licit and illicit substances almost always occurs during adolescence and follows a consistent consumption pattern. This pattern sees adolescents move first from nonuse into tobacco and/or alcohol consumption, then into marijuana use, and finally into the consumption of harder drugs (Kandel, 1975 and 1978). The early literature largely ignores a role for unobservable factors as an explanation for individual drug consumption patterns, and the general pattern is so consistent that teasing evidence for any alternative explanations to gateway effects out of existing data presents a great challenge due to the sheer numbers involved (Kleiman, 1992). When looking at users of harder drugs, for instance, nearly all use a gateway drug first - a 1994 report (CASA, 1994) shows that prior to using cocaine, nearly 90 percent of cocaine consumers had experience using each of the drugs under study here (i.e., cigarettes, alcohol, and marijuana). Pacula (1995) summarizes general avenues for studying the causal mechanisms underlying gateway theory as follows:

1. The standard economics approach, focusing on preferences and costs and using a rational addiction framework (Becker and Murphy, 1988) where individuals rationally assess costs, including both pecuniary and non-pecuniary prices as well as potential health consequences. Individuals initiate into a new drug once the (discounted) expected marginal benefit of consumption exceeds the corresponding expected marginal cost.

2. Social interaction and learning theories that emphasize the importance of peer and family effects and social settings, where individual surroundings are the major determinant of drug demand. Particular social environments result in individuals trying certain drugs; in an alternative environment, no such experimentation would occur.

3. Information theories that relate to social interaction and offer that drug consumption decisions are initially determined by the individual's environment. Preferences are subsequently updated, imperfectly, based on personal experience through experimentation, such that new drug experimentation occurs based on inaccurate assessments of the potential risks involved.

4. Development theories postulating that preferences for drug consumption change over time and are highly correlated with risk preferences and life experiences. Drug experimentation is a stage of adolescent psychosocial development where the degree of experimenation is determined by individual surroundings as well as tastes for risk.

Investigating the relative importance of these potential causal mechanisms, however, is difficult with existing data. Data on many of the theorized causal mechanisms are often unobservable to the researcher. Few if any datasets offer the information necessary to study drug consumption, prices, individual preferences, risk aversion, and budgetary considerations while simultaneously assessing the importance of peers, parents, family, school, church, and other factors associated with an adolescent's social and developmental environment. A few studies attempt to control for unobserved heterogeneity that affects observed drug demand, finding that such controls mitigate apparent gateway effects (Pudney, 2003; Van Ours, 2003; Bretteville-Jensen, Melberg, and Jones, 2008). With so many potentially important variables unobservable, however, the researchers cannot speak to the relative importance of any particular latent factor. In much of the gateway literature, though, the mechanisms underlying any relationship are less important than the fact that a relationship exists, with the prevalence of observed consumption patterns and the timing of drug initiations serving as proof of a causal relationship (O'Donnell and Clayton, 1982). While not supporting the latter concept of causation, this study builds off of the idea by estimating a reduced form demand model of current drug use as a function of previous drug consumption behavior, allowing prior behavior to affect current drug consumption directly.

Despite an existing literature generally supportive of the gateway concept, an alternative view argues that while intake of a gateway drug predicts progression to more powerful substances, such consumption does not directly cause progression to a more powerful drug (Baumrind, 1983; Beenstock and Rahav, 2002). Under this alternative argument, unobservables are often the most important factors underlying observed substance use patterns - factors that influence both the probability of using the gateway drug and the subsequent drug. While not always finding strong evidence for the role of unobservables, economists typically approach the subject with the idea that they play some role.

A standard definition of gateway effects in the field of economics is simply the existence of a complimentary demand relationship between two drugs during or across time periods (Pacula, 1998a; Kenkel, Mathios, and Pacula, 2001), where the use of one drug serves as the gateway to use of the other.[5] Economic models of this demand relationship often build off of the single good rational addiction framework of Becker and Murphy (1988).[6] Gateway-related habit formation models incorporate consumption of more than one good, such that a reinforcing effect allows a stock of drug consumption to build up across goods, or drugs. With respect to gateway theory, initiation occurs when the expected marginal benefit of

[5] A positive sign for a coefficient estimate of the effect of the past price of a gateway drug on current consumption of a harder drug is usually viewed as contradicting the concept of a gateway since it implies that increasing the price of the gateway drug decreases consumption of the gateway drug (by assumption) but increases consumption of the harder drug. The idea of a gateway implies that a decrease in use of the gateway drug would cause a similar reduction in consumption of the harder substance.

[6] The Becker and Murphy model itself builds off of prior work by Stigler and Becker (1977) and Iannaccone (1984 and 1986). See Becker, Grossman, and Murphy (1994) for an application of the model.

consumption exceeds the expected marginal cost (Pacula, 1997); an increasing tolerance to a lesser drug decreases the marginal utility of consumption for that drug, increasing the likelihood that initiation into a new, more powerful drug occurs due to altered expectations. Below, along the lines of Orphanides and Zervos (1995) and Lochner (2007), I will incorporate an element of updating into the cost side of this type of model, where a "bad outcome" resulting from drug use in one period, such as an arrest, affects decisions to consume drugs going forward.[7]

Economists investigating the subject typically look for evidence contradicting gateway effect, such as findings that gateway drugs and other drugs are intertemporal substitutes in consumption or that gateway effects are dampened after instituting controls for unobservable heterogeneity. Indeed, unobservable, individual-level heterogeneity might account for the fact that many users never make the progression beyond the gateway drug. In 2008, Monitoring the Future (MTF) survey data (Johnston et al., 2009) indicate that while 71.9 percent of high school seniors report drinking alcohol at some point in their life and 54.7 percent report having been drunk at least once, only 42.6 percent report ever using marijuana. Further, only 24.9 percent of those seniors record consumption of an illegal drug (beyond alcohol) other than marijuana. With only 47.4 percent of seniors reporting ever using any illicit drug (including marijuana), such numbers are not necessarily supportive of the idea that use of a gateway drug *causes* use of the harder drug.

Indeed, Morral, McCaffrey, and Paddock (2002) find that gateway effects disappear after controlling for an individual's propensity to use drugs, which they theorize to be correlated both with the opportunity to use drugs and the probability of use given such an opportunity. Using a common factor model based on parameter estimates generated from National Household Survey of Drug Abuse (NHSDA) data, they impose a condition of no causal gateway effects and demonstrate that they can match observed data use patterns among U.S. youth - patterns that are typically ascribed to gateway effects. Indeed, one alternative (and straightforward) explanation for the initial use of the gateway drugs is wider availability, and MTF data show that in 2008, over 92 percent of high school seniors state that alcohol is "fairly" or "very" easy to get and that nearly 84 percent state the same for marijuana (Johnston et al., 2009).

[7] Though Orphanides and Zervos (1995) focus on the negative effects of becoming addicted to the good being consumed, I build off of their model in earlier work to derive a Bayesian updating mechanism where the possibility of experiencing a punishment (legal or otherwise) resulting from positive consumption factors into the adolescent drug use decision (Grider, 2005). Pacula (1998b) and Kenkel, Mathios, and Pacula (2001) also describe models incorporating a legal risk of using a drug, where the risks are drug-specific. Lochner examines how actual arrests impact an individual's appraisal of the likelihood of a future arrest if committing a specific crime.

The highest corresponding percentage for any other drug (amphetamines) in the MTF data is less than 48 percent, potentially lending credence to the argument that availability differentials partially explain observed drug usage. The availability hypothesis is just a theory, however, since the model does not allow the authors to quantify the effects of specific unobservable factors. While this same limitation affects this study, my contribution below is a study that jointly models consumption of three drugs, includes robust, dynamic controls for unobservables, and finds that such controls significantly reduce the magnitude of estimated gateway effects but do not eliminate them completely.

2.2 Literature Review

Non-economic Studies

I categorize the relevant literature on adolescent substance use into economic and non-economic studies, since economists do not perform much research on the subject prior to the 1990s. As such, the literature is dominated by work from researchers in other academic disciplines such as psychology, psychiatry, sociology, and public health. Table 2.1 provides an incomplete synopsis of these studies, which are largely supportive of the gateway concept. The earliest studies tend to base findings on correlations and sequencing analyses; only later do researchers turn to statistical modeling. The idea of the gateway emerged with the seminal work of researchers such as Single, Kandel, and Faust (1974); Kandel (1975); and Jessor and Jessor (1975). Simply stated, the theory asserts that hard drug experimentation results directly from earlier consumption of drugs like tobacco and alcohol (Kandel, 1975; Pacula, 1995). Early work supporting gateway theory tends to offer evidence based on use correlations between drugs (Kleiman, 1992), and data bear out that drug use follows a remarkably consistent pattern. Smoking and drinking precede marijuana use, which precedes use of more powerful drugs. The early literature attempts to establish that the gateway effect results from a direct causal process based on the strength of observed use correlations, usually without attempting to establish the plausibility of a direct causal process.[8] Later studies theorize how causation might operate.[9] Researchers often posit that cau-

[8] Kandel (1978) and Clausen (1978) review these studies, and Kandel and Maloff (1983) provide a more detailed summary of early research.

[9] Since this research is based in the field of economics, a complete review of the vast literature on gateway theory that exists in various fields of natural science or other social sciences is beyond the scope of the study. See Kandel and Jessor (2002); Agrawal et al. (2004); Ellgren, Spano, and Hurd (2007); Choo, Roh, and Robinson (2008); and Maldonado-Molina and Lanza (2010) for reviews.

Table 2.1: Non-Economic Studies Related to Gateway Theory

Researchers (Year)	Findings
Sinnett, Wampler, and Harvey (1972)	Heroin use accurately predicts prior use of gateway drugs, but marijuana use does not accurately predict progression.
Single, Kandel, and Faust (1974)	Guttman scaling shows that hard drug progression begins with alcohol and tobacco use, which begets marijuana consumption.
Jessor and Jessor (1975)	Bivariate analysis shows that the onset of drinking in adolescence may lead to marijuana use.
Kandel (1975)	Sequencing analysis indicates alcohol and tobacco lead to marijuana use and marijuana use leads to harder drug consumption.
Adler and Kandel (1981)	Sequencing models show French, Israeli youth use alcohol, tobacco before illegal drug use onset, though cultural differences exist.
Huba, Wingard, and Bentler (1981)	Guttman scaling indicates that alcohol, marijuana use "causally interconnected."
O'Donnell and Clayton (1982)	Bivariate analysis of marijuana and heroin associations concludes that marijuana use is a cause of heroin use.
Donovan and Jessor (1983)	Guttman scaling shows problem drinking more highly associated with progression than marijuana use.
Yamaguchi and Kandel (1984a)	Hazard models show that alcohol, tobacco influence progression to marijuana and marijuana strongly influences later hard drug use.
Yamaguchi and Kandel (1984b)	Guttman scaling shows alcohol, cigarettes precede marijuana use and all three precede use of hard drugs.
Welte and Barnes (1985)	Guttman scaling shows that alcohol is the gateway to all other drugs.
Ellickson and Hays (1991)	Saturated regression models show exposure to smoking environment predicts higher incidence of drinking and heavy drinking later.
Ellickson, Hays, and Bell (1992)	Longitudinal scalogram analysis shows use of alcohol, tobacco plays a key role in transition to hard drug use.
White (1992)	Regression analysis shows that drug use patterns are the strongest predictors of eventual drug problems.
Fergusson and Horwood (2000)	Hazard model analysis of New Zealand youth shows cannabis, especially by amount used, leads use of more powerful drugs.
Morral, McCaffrey, and Paddock (2002)	Drug use propensity influences likelihood of both marijuana and other drug use, explaining initial findings of a gateway effect.
Agrawal et al. (2004)	Some evidence of causality in genetic factors that relate cannabis use with subsequent harder drug consumption.
Riala et al. (2004)	Logit models show Finnish teen alcohol, tobacco use as significant predictors of later substance abuse problems.
Degenhardt et al. (2010)	Gateway associations vary across countries, and this variation belies differences in drug sequencing across countries.

sation flows from the physical act of using one of the gateway drugs (O'Donnell and Clayton, 1982). Two potential types of causal mechanisms - physiological and sociological - are most often cited (Lindsay and Rainey, 1997; Pacula et al., 2000; Pudney, 2003; Degenhardt et al., 2010). Lindsay and Rainey describe psychosocial-based explanations for causation: Learning-based models offer that use of a gateway drug like tobacco imparts learning on the user on a number of levels (legal, health, etc.), fostering an increased likelihood of trying other drugs; alternatively, social and behavioral models emphasize the potential gateway effects of social influences such as peer effects and social pressures. Two 1997 studies document marijuana's effects on neurochemical processes (Rodriguez-de-Fonseca et al.; Tanda, Pontieri, and Di Chiara), and additional physiological evidence from Ellgren, Spano, and Hurd (2007) indicates that marijuana use may alter the brain's opioid receptors in a way that promotes later use of opiates. Agrawal et al. (2004) find a role for genetics in a study of twins. For the vast majority of this work, the authors view their findings as being supportive of the gateway theory.

Economic Studies

Kleiman (1992), DeSimone (1998), and Pacula (1998b) generalize two avenues based in economic theory through which a gateway effect might operate.[10] The first avenue reflects a reduced uneasiness or enhanced curiosity about trying progressively more powerful drugs after initiation into, and subsequent use of, the gateway drug, where the initial experimentation proves enjoyable and increases utility. Consumption of the gateway drug thereby alters the expected marginal benefit-marginal cost comparison for initial use of other drugs, increasing the probability of subsequent initiations.[11] The second pathway describes a rational addiction process, where the "high" from using a lower tier substance gradually diminishes and the user subsequently experiments with more powerful substances in order to replicate the lost effect on overall utility.

The relevant literature in economics can be divided into three categories: use of one substance in isolation, single-period use of two or more substances, and multi-period use of more than one drug. Kenkel, Mathios, and Pacula (2001) provide a concise outline of single-substance studies, which are contextually relevant in describing the economics of substance use and abuse. Table 2.2 summarizes the

[10] I define economic studies as those done by economists and/or presenting results based off of an economic model of drug demand.

[11] As noted above, my study introduces factors that influence expected costs in the form of legal risks, as also envisioned in Pacula (1998b) and Kenkel, Mathios, and Pacula (2001).

Table 2.2: Single Period Economic Studies Related to Gateway Theory

Researchers (Year)	Findings
Supporting Gateway Theory	***Drugs are complements in consumption***
Moore and Cook (1995)	Drinking patterns developed in adolescence persist into adulthood, mainly from habit formation instead of unobserved variables.
Chaloupka et al. (1999)	Cigarette prices are inversely related to marijuana use, both for initial use as well as the amount of consumption by current users.
Dee (1999)	Increasing the legal drinking age from 18 to 21 decreased youth smoking; higher tobacco taxes associate with less teenage drinking.
Farrelly et al. (2001)	An increase in the tax on cigarettes induces a decrease in the level of marijuana use among current users.
Williams et al. (2001)	College students use alcohol and marijuana in complement.
Williams and Mahmoudi (2004)	Alcohol prices and fines for a higher blood alcohol content than legally allowed (for driving) are both negatively related to marijuana use.
Corman et al. (2004)	Higher cocaine prices decrease illegal drug use among pregnant women. Marijuana is by far the drug most prevalently used by these women.
Not Supporting Gateway Theory	***Drugs are substitutes in consumption***
Model (1993)	Marijuana decriminalization is correlated with a "significant reduction" in hospital emergency room visits related to harder drugs.
Goel and Morey (1995)	Cigarettes and liquor are substitutes, though both appear to be complementary to leisure.
Chaloupka and Laixuthai (1997)	Marijuana possession decriminalization by some states in the 1980's led to decreased drinking among youth compared to criminalized states.
DiNardo and Lemieux (2001)	The standardization of the legal drinking age in the U.S. (to 21 in 1984)* led to an increase in marijuana consumption.
Bask and Melkersson (2004)	Swedish data show alcohol and cigarettes to be complements in consumption.
Other Findings	***Mixed results about drug complementarity/substitutability***
Grossman and Chaloupka (1998)	Cocaine is a complement to marijuana but a substitute for alcohol.
Decker and Schwartz (2000)	Alcohol prices are inversely related to smoking participation, but cigarette prices are positively correlated with alcohol use.
Cameron and Williams (2001)	Marijuana substitutes for alcohol but is complementary to cigarette use; drinking and tobacco smoking are complementary.
DeSimone and Farrelly (2003)	Higher cocaine prices depress marijuana demand, but youthful marijuana use is unaffected by cocaine prices.
Dave (2008)	Cocaine and heroin are complements in consumption.

* Standardization increased the legal drinking age in most states (Toomey, Rosenfeld, and Wagenaar, 1996).

single-period, multi-drug studies. Many of these studies explicitly look for cross-price elasticity estimates and do not claim to be studying gateway theory. Among those that claim gateway implications, findings of contemporaneous complementarity are typically viewed as supportive of the gateway hypothesis, since such complementarity does not contradict the observed gateway progression in the sense that if use of one drug leads to use of another, research results should indicate that consumption of those drugs exhibits complementarity. The concensus view, however, is that complementarity must be intertemporal to support the existence of gateway effects (Pacula, 1995), since use of data from a single time period does not provide robust evidence about a theory of drug use progression over time.

A commonly accepted methodology for investigating gateways in economics estimates intertemporal cross-price elasticities based on individual-level demand equations (Pacula, 1998b; Kenkel, Mathios, and Pacula, 2001). Finding data on illegal drug prices is difficult, so non-pecuniary prices such as the severity of drug laws are often used instead. (I will do the same below.) Researchers often assume that results support the gateway theory if a change in a policy, law, or monetary price affects future drug use in a manner consistent with the theory; findings that these factors do not influence future consumption, or influence future use in a way that is inconsistent with gateway effects, are seen as contradicting the theory. A summary of the relevant multi-period economic literature appears in Table 2.3.[12]

The studies listed in Table 2.3 utilize a variety of methods to examine the use of multiple drugs and contain conflicting findings, sometimes even within a single study. Thies and Register (1993), for example, study the effects of marijuana decriminalization on alcohol, marijuana, and cocaine consumption using Monitoring the Future (MTF) data, finding that results using the 1984 sample differ from those using the 1988 sample. Decriminalization significantly increases cocaine consumption in 1984 but not 1988, and substantially decreases heavy drinking in 1988 but not in 1984. They take their results to be unsupportive of the gateway idea that marijuana use leads to "compulsive" use of harder drugs, but that continued decriminalized status for marijuana correlates with more occasions of consuming six or more drinks in a single sitting. In the context of the Degenhardt et al. (2010) study mentioned above, though,

[12] The categorization of findings in Tables 2.2 and 2.3 is based on my own understanding of the literature, and does not necessarily reflect the referenced investigators' views on gateway theory. An example of a conflict between my categorization and the cited authors' description of their findings in Table 2.3 is the Saffer and Chaloupka (1999) study. The authors see complementarity as simply "suggesting that drug users prefer to use various drugs together rather than to substitute one for the other."

Table 2.3: Multiple Period Economic Studies Related to Gateway Theory

Researchers (Year)	Findings
Supporting Gateway Theory	***Drugs are complements in consumption***
DeSimone (1998)	Prior marijuana participation increases the chances of current cocaine use, and alcohol and marijuana are complements.
Pacula (1998b)	Higher alcohol prices in the past lead to lower current marijuana use, though previous period marijuana prices have no effect on current alcohol consumption.
Beenstock and Rahav (2002)	A natural experiment among Israelis aged 18 to 40 indicates a "causal effect" of smoking on later marijuana use, but no such effect for marijuana use on harder drug usage.
Sen, Agarwal, and Hofler (2002)	Smoking and drinking are gateways to marijuana, and vice versa.
Abdel-Ghany and Wang (2003)	Cigarettes are a gateway to marijuana, though alcohol is not.
Bretteville-Jensen, Melburg, and Jones (2008)	Multivariate probit model shows that gateway effects remain considerable, though reduced, after controlling for unobserved heterogeneity.
Not Supporting Gateway Theory	***Drugs are substitutes in consumption***
Thies and Register (1993)	Marijuana decriminalization may have increased hard drug experimentation and is correlated with heavy drinking episodes, but did not cause strong gateway effects.
Saffer and Chaloupka (1999)	Alcohol and marijuana are substitutes, though alcohol, marijuana, cocaine, and heroin are all complements with the noted exception. Marijuana decriminalization results provide weak, but not meaningful, evidence for complementarity between marijuana and alcohol.
Pudney (2003)	Controlling for associations between problem behaviors, gateway effects from models that do not control for these correlations are significantly lessened.
Van Ours (2003)	Sensitivity analysis shows that any gateway from marijuana to hard drugs is limited and that evidence of complementarity reflects unobserved heterogeneity.

one might see such results as definitively contradicting the gateway concept since they indicate variation in effects over time and are therefore not consistent with a straightforward causal process.

DeSimone (1998), using the 1979 National Longitudinal Survey of Youth (NLSY79), finds support for gateway effects in that prior marijuana use significantly increases the current probability of using cocaine. Also employing the NLSY79, Pacula (1998b) finds evidence consistent with gateways in that prior smoking and drinking increase the probability an individual currently smokes marijuana. More recently, Beenstock and Rahav (2002) use Israeli data to show that cohorts growing up in periods of cheaper cigarette prices are more likely to smoke and to start smoking at a younger age than cohorts growing up in times when cigarette prices are relatively higher. Both Beenstock and Rahav and Abdel-Ghany and Wang (2003) determine that prior smoking significantly increases the likelihood an individual currently consumes marijuana.[13]

Other work calls the gateway theory into question, however. Utilizing the National Household Surveys on Drug Abuse (NHSDA), Saffer and Chaloupka (1999) state that although statistically significant results show that alcohol, marijuana, cocaine, and heroin are generally complements in consumption, these results actually argue against gateway theory due to the small magnitude of the effects. Pudney (2003), using a 1998 sample of British youth with retrospective information, employs a discrete time random effects methodology to control for unobservables, finding in behavioral simulations that gateway effects are substantially lessened when implementing these controls. Work by Van Ours (2003) uses data from Amsterdam to similarly suggest the importance of unobserved heterogeneity among drug users. The author uses a bivariate duration model to control for unobservables, reporting that marijuana causally serves as a "stepping-stone" into cocaine use but that the relationship is highly correlated with unobservable heterogeneity that leads individuals to consume both drugs. Finally, Bretteville-Jensen, Melberg, and Jones (2008) work with a unique survey taken from a questionnaire mailed to young adults in Oslo to determine that unobserved heterogeneity accounts for some of the gateway effect they find, but that the estimated gateway remains substantial even after taking these unobservable factors into account.

Particularly relevant for this research are the studies by Pacula (1995 and 1998b), Van Ours (2003), and Bretteville-Jensen, Melberg, and Jones (2008). The Pacula (1998b) study inspires this work in terms of the basic framework employed,[14] as does Van Ours in terms of implementing controls for unobserved heterogeneity and observing more than two time periods of drug behavior. Finally, Bretteville-Jensen, Melberg, and Jones estimate a multinomial probit model to control for unobservable heterogeneity, providing an impetus for measuring smoking and drinking behavior in a multinomial framework. Specifically, using two panels of the NLSY79 cohort, Pacula (1998b) estimates the effects of lagged beer prices (in the form of beer taxes) on current period marijuana consumption and vice versa, with System to Retrieve Information from Drug Evidence (STRIDE) data from the Drug Enforcement Administration (DEA) providing marijuana price information. She finds that alcohol and marijuana use are complementary among the adolescent group under study, with higher beer taxes significantly de-

[13] Alcohol use, however, does not affect the likelihood of marijuana use in the Abdel-Ghany and Wang (2003) study. The authors use the 1999 National School-based Youth Risk Behavior Survey (YRBS).

[14] While this research uses the NLSY97 instead of the NLSY79, my related replication study of Pacula (1998b) uses the NLSY79 and constructs similar price proxy measures to the ones used in the earlier work (Grider, 2005).

creasing both the likelihood of marijuana use and the amount used conditional on positive consumption. These findings support the gateway idea, but the relevance of her work does not end there.

Also relevant to the current research is the inclusion of non-monetary prices such as whether or not possession of small amounts of marijuana is decriminalized in the respondent's state of residence. While use of this decriminalization variable remains standard in such studies, Pacula's (1998b) results lead her to theorize that because of potential differences in perception of enforcement and actual enforcement, variation in enforcement of a law may be more important than its existence.[15] Consistent with initial evidence that marijuana consumption does not correlate with the standard definition for whether or not a state has decriminalized marijuana (Johnston, O'Malley, and Bachman, 1981; Murphy, 1986; Single, 1989; Thies and Register, 1993), Pacula, Chriqui, and King (2003) later find that the standard definition lacks consistency, with four of the states normally defined as being decriminalized classifying a first-time offense as a criminal offense. Seven states not normally defined to be decriminalized states, however, classify first-time offenses to be non-criminal. The research below makes use of these insights by defining decriminalized states as those in which a first-time possession offense is not classified as a misdemeanor or felony. I also control for non-pecuniary marijuana prices, though I do so while jointly estimating the effect of prior drug use on current drug consumption instead of following most authors of related economic studies, which focus on how drug consumption varies with changes in either pecuniary or non-pecuniary prices.

The Bretteville-Jensen, Melberg, and Jones (2008) and Van Ours (2003) studies both use geographically consistent panels (from Oslo and Amsterdam, respectively) and implement controls for unobservable, individual-level heterogeneity.[16] Bretteville-Jensen, Melberg, and Jones condition on use of a defined gateway drug (or drugs), and subsequently estimate a multinomial equation capturing cannabis, amphetamine, and cocaine consumption.[17] Van Ours combines elements of a mixed proportional hazard model (to adjust initiation by age) and a bivariate duration model (to specify a joint density function for

[15] MacCoun et al. (2009) find evidence that individuals do not accurately assess whether or not marijuana is decriminalized in their state.

[16] Bretteville-Jensen, Melberg, and Jones use a single round of survey questionnaires sent out to 21-30 year olds in Oslo, Norway. The response rate to the questionnaires is 50 percent. Van Ours employs four rounds of a survey of Amsterdam residents aged 26-47 who are both of Dutch nationality and born in The Netherlands.

[17] Marijuana use likelihood is conditioned on prior alcohol use. The probability of amphetamine consumption is conditioned on prior consumption of both alcohol and cannabis. Finally, cocaine use likelihood is conditioned on prior consumption of alcohol, marijuana, and amphetamine.

duration of nonuse for both cocaine and cannabis) to allow unobservables to impact cocaine starting rates. Both studies find that meaningful gateway effects persist after the implementation of controls for unobservables. Bretteville-Jensen, Melberg, and Jones state that the gateway is lessened "substantially" but remains "considerable," however, while Van Ours concludes that while a potential gateway exists, the connection between cocaine and marijuana consumption mainly lies with unobserved individual characteristics.

Indeed, unobservable factors may explain the observation by Abdel-Ghany and Wang (2003) that "the earlier an individual starts using these gateway substances (cigarettes and alcohol), the more likely such an individual is to progress to smoking marijuana." That statement suggests the presence of bias in findings that do not control for individual-level variables that influence both early experimentation with gateway drugs as well as any eventual progression into harder drugs. As outlined above, much work has been done to study the gateway theory within an economics framework, where things like prices matter and are often controlled for despite the difficulties involved with obtaining price information. This study will combine standard price controls in the form of taxes for beer and cigarettes with new controls for marijuana prices to circumvent the dearth of monetary price information. In addition, while the research described in the preceding paragraphs implements controls for unobservables, I attempt to implement such controls more robustly than prior work, estimating a system of equations defining basic drug demand as depending on prior drug use, controlling for criminal activity and sample attrition, and following the survey cohort over a longer period of time than previous gateway-related work. I outline the theory behind this study in the next chapter.

Chapter 3

Theoretical Framework

This model provides a framework for assessing gateway theory by analyzing the degree to which prior consumption of tobacco, alcohol, and marijuana impact current demand for those drugs by adolescents and young adults. The model accounts for observable and unobservable factors that simultaneously affect consumption of each of the three subtances under study. Solving a dynamic optimization problem suggests that the derived demand function is a direct function of prior consumption of all three drugs. As noted above, the approach offers the advantages of estimating demand equations for individual drugs that directly control for prior drug use in addition to current prices, instead of using a standard approach that instruments for prior consumption with prior-period prices. I implement controls for unobservable heterogeneity, and unlike prior studies that do the same, I follow a diverse, representative cohort of individuals over many (eight) time periods. Finally, I also model arrests and sample attrition, behaviors that directly relate to drug use and reported use. The timing of the model is as follows:

1. An adolescent/young adult enters each period with a history of tobacco, alcohol, and marijuana use, inclusive of the option of complete abstention from all drugs, and the experience of any legal repercussions resulting from positive past consumption.

2. The individual chooses a drug consumption combination from a set of eight possible choices, ranging from complete abstention to consumption of all three drugs.

3. The individual potentially experiences legal repercussions from positive drug consumption.

4. At the end of the period, the individual's history of drug use and consequent legal repercussions is updated based on the decisions and events during the period. The individual enters the next period and repeats the process.

3.1 Demand for Multiple Drugs

While the empirical work in this study analyzes three drugs, the underlying economic model can be extended to incorporate any number of drugs. For instance, while this study attempts to examine potential gateway effects surrounding the joint consumption of cigarettes, alcohol, and marijuana, a study using different data might examine the gateway from marijuana to cocaine. From a theoretical standpoint, the gateway theory can be examined using a rational addiction framework[1] that analyzes the effect an accumulated addictive stock of (prior) gateway drug consumption has on current consumption of a more powerful drug. In practice, prior consumption is typically proxied for using prior prices or taxes (Pacula, 1997; Kenkel, Mathios, and Pacula, 2001), and, as outlined above, the assessment of gateway effects depends on the sign of the coefficient estimate on those price variables. In this chapter, however, I specify a system of equations that allows prior consumption to enter the demand equation directly along with current-period drug prices and observable and unobservable covariates.[2]

During each period t, individuals choose to either abstain from or consume three potentially addictive drugs. For each period t, drug consumption choices can be represented by a vector of dichotomous indicators $\mathbf{d_t^j} \in \{0,1\}$ defining consumption of drug j, $j = 1,2,3$, where j corresponds to cigarettes, alcohol, and marijuana, respectively. I denote eight possible consumption combinations for the three drugs by $\mathbf{d_t} = d$, $d = 0,\ldots,7$. An alternative formulation of $\mathbf{d_t^j}$ could employ a multinomial indicator to distinguish between no use and "light" and "heavy" consumption of the drug, a count variable, or even a continuous variable to capture use amounts. Definitions of light and heavy use might vary by drug or the age of the consumer. I posit that empirically, it is important to define, conditional on positive consumption of a particular drug, how much of that drug the individual consumes. As outlined in the Becker and Murphy (1988) model of rational addiction, an addictive stock of accumulated past use builds up over time when an individual uses a drug, increasing current and future consumption amounts of that good. Whereas non-addictive good consumption only affects current utility through current consumption, the stock of past addictive good use affects current utility through the addiction mechanisms

1 See Stigler and Becker (1977); Iannaccone (1984 and 1986); Becker and Murphy (1988); and Becker, Grossman, and Murphy (1994).

2 The theoretical formulation is used to motivate specification of an estimable empirical model below. Using the estimated model, the effect of the addictive stock of past use will be examined by establishing a baseline of simulated behavior against which results from alternative behaviors and policy experiments will be compared.

of habit, tolerance, and withdrawal.[3] While much of the rational addiction literature focuses on consumption decisions as they relate to one good, the concept of rational addiction in a gateway theory context allows the addictive stock to accumulate additively across drugs. Over time, as individuals' consumption of, and tolerance to, gateway drugs increases, so does the likelihood that these individuals initiate into consumption of harder drugs (Pacula, 1997; Kenkel, Mathios, and Pacula, 2001).[4] Thus, an appropriate methodology for examining gateway effects is to estimate current consumption as a function of the stock of past consumption while controlling for current prices.

To capture the stock of past addictive good use, any number of variables might be included in a vector of state variables Ω_t, which consists of observable factors that evolve over time and influence current period drug consumption decisions. Time-varying drug consumption stock variables in Ω_t are defined entering *t*. For example, one might define a series of variables that represents the addictive drug consumption stock $S_t^{j,k}$, where *j* continues to identify the drug and *k* enumerates the variable that defines a particular type of addictive stock, $k \in [0, 1, \ldots, K]$. Stock variables include experience (cumulative amount of time spent as a user), duration (consecutive amount of time in consumption spent by current users), and variables that describe prior consumption (e.g., use indicators and conditional consumption amounts). The empirical work below will employ information about whether or not a person has ever (separately) used cigarettes, alcohol, and marijuana entering period *t*, whether the person is a current a user of each drug entering *t* (i.e., did he use in period *t-1*?), and a conditional indicator for whether or not positive use of a particular drug at *t-1* represents the individual's first ever use of that drug. Note that the drug use state variables, while defined with a *t* subscript, apply to variables defined *entering* the period, such that their values represent the state of the world at the end of the prior period.

The representation of the drug use state variables in this model flexibly accommodates consumption measured in time, amount, or by some other definition. Additional state variables include a vector

[3] Because this is a model of drug use by adolescents and young adults, the model presented here largely ignores health effects that might also impact current period utility. While an argument can be made for directly including health consequences in a theoretical model of addictive good consumption over the life cycle, I do not directly incorporate health here since the study only covers the initiation stages of drug use. Instead, this model captures health effects via the idiosyncratic error term, through a potential health shock like the type that might be experienced with an overdose. Given that initiation into any drug almost universally occurs by the mid-20's (Kandel, 1978), long-term health effects should be, or at least be perceived to be, minimal for most adolescents and young adults (Lindsay and Rainey, 1997). To the extent such effects do occur, it seems likely that they might either go unnoticed or simply not affect the consumption decision significantly.

[4] One way to think about this process is that for an adolescent who drinks alcohol, is becoming accustomed to drinking, and is therefore building up an alcohol tolerance, the expected marginal benefit of consumption of a harder drug may increase, such that initiation into a new, harder drug becomes more likely.

of observable personal characteristics ($\mathbf{X_t}$) and a vector of local factors ($\mathbf{Z_t}$) that might affect an individual's ongoing drug consumption. Examples of variables in $\mathbf{Z_t}$ include factors like unemployment (Ruhm, 1995), which correlate with income, or regional arrest rates,[5] which reflect law enforcement priorities and the degree to which laws are enforced. Finally, a vector of variables $C_t^{j,l}, l \in [0, 1, \ldots, L]$ describes drug-related arrests and captures an individual's history of trouble with the police with respect to drug-related crime (along L dimensions) entering each period. It also includes current-period drug use.[6] In the empirical model below, the variables defined in $C_t^{j,l}$ are analogous to the drug use state variables, including an indictor for ever having experienced any arrest (not necessarily a drug-related one) entering *t*. Additional variables indicate whether the person was arrested during *t-1* and whether or not a period *t-1* arrest was the respondent's first arrest. The theoretical model, however, limits itself to drug-related arrests, and might include other variables such as a cumulative number of drug-related arrests or an individual's history, in terms of calendar time, of such interactions with law enforcement for drug-related criminal activity.

The importance of these potential legal consequences lies in an information updating process that affects future drug consumption decisions. Prior to initiation into use of a particular drug, the consequences of use are difficult to gauge.[7] After initiation, consumption that results in favorable experiences and no ill consequences will tend to affect future consumption decisions positively (Pacula et al., 2000), while unfavorable experiences along any dimension (e.g., an arrest resulting from positive consumption) will tend to decrease future consumption. The rational addiction literature is based on the idea of a drug consumer rationally assessing expected benefits and costs of substance use (Becker and Murphy, 1988), but does not focus much attention on the cost side of the decision to use a drug. Orphanides and Zervos (1995) outline a model of substance use behavior that describes the costs of becoming addicted to the drug. I build off of that idea in this model, explicitly modeling costs, in the form of drug-related arrests, under the assumption that an arrest (or lack thereof) impacts future drug consumption through an updating process that affects individual assessments of the likelihood of a future arrest. Prior work supports the idea that the experience of an arrest alters an adolescent's perception of the likelihood of

[5] See Pacula (1995 and 1998b) and Caulkins, Feichtenger, and Tragler (2001).

[6] $C_t^{j,l}$ includes whether or not the person consumes drugs *this* period, since a drug user's current utility from drug consumption undoubtedly depends upon whether or not they have recently been arrested or are currently in jail.

[7] In fact, in the theoretical model, one might also incorporate other potential consequences of drug consumption, such as school-related or parental punishments, that potentially impact an adolescent's decision to use drugs.

being arrested in the future (Lochner, 2007). As mentioned above, Pacula (1998b) and Kenkel, Mathios, and Pacula (2001) describe models incorporating a drug-specific legal risk of using a drug, but do not specify the risk, nor directly control for it empirically in the manner I do here.

Entering period t, then, the information available to the individual as he makes the period t drug consumption decision, d_t^j, is summarized by the following vector of state variables:

$$\Omega_t = \left\{ S_t^{j,k}, \mathbf{X_t}, \mathbf{Z_t}, C_t^{j,l} \right\} \tag{3.1}$$

The drug consumption stock variables represented by $S_t^{j,k}$ take on positive values for current and past users but zeroes for non-initiates. Using the variables described above (experience entering period *t-1* and use during the period) as examples, four drug-specific alternatives describe a person's state of drug use entering period t (as noted above, leaving a total of 12 such states):

1. Complete abstainer (never used before *t-1* and did not use in *t-1*)
2. Former user (used before *t-1* but not in *t-1*)
3. New user (initiated in *t-1*)
4. Continuing user (used before *t-1* and continued to use during *t-1*)

On the basis of these four potential states of drug use entering period t, a similar construct for four states of drug-related criminal history, observables, and, as described in the next section, factors that are unobservable to the researcher, the individual makes the period t consumption decision.

3.2 Gateways - Current Consumption as a Function of Past Use

At t, adolescents make drug consumption decisions that maximize expected lifetime utility, which depends on drug consumption, consumption of a standard composite good (G_t) with price normalized to one, and two types of taste shifting factors that alter the consumption utility of both drugs and the composite good. In the case of S_t, $\mathbf{X_t}$, $\mathbf{Z_t}$, and C_t,[8] these taste shifters are observable. The other type of taste shifters includes unobservables in the form of permanent (μ), time-varying (ν_t), and idiosyncratic (ε_t) heterogeneity. Not all unobservable factors can be definitively classified into these three categories of latent variables, but an example of a drug-related shock captured by ε_t could include a sudden health problem, such as one resulting from an overdose. Alternatively, an example of permanent heterogeneity affecting drug use decisions between individuals would be innate, time-invariant differentials in risk

[8] For notational simplicity, I suppress the superscripts on $S_t^{j,k}$ and $C_t^{j,l}$.

preferences, where one person is a risk taker and therefore more likely to experiment with drugs than a relatively more risk averse individual. Examples of time-varying heterogeneity could include influences like peer or parental effects that vary both between individuals as well as over time for a particular individual.[9] (Of course, risk aversion might vary over time as well.) Thus, utility depends on current consumption decisions, but shifts based on unobservables, the amount of the stock of accumulated drug use, police detected criminal activity, and observables in $\mathbf{X_t}$ and $\mathbf{Z_t}$. Defining β as a discount factor, expected lifetime utility can be written as

$$E_t\left[\sum_{t=0}^{T}\beta^t U_t\left(G_t,\mathbf{d_t};S_t,\mathbf{X_t},\mathbf{Z_t},C_t,\mu,\nu_t,\varepsilon_t\right)\right] \tag{3.2}$$

where $E_t[\cdot]$ is the expectations operator. Note that current utility directly depends on past drug consumption in this rational addiction-type framework through S_t. Current utility also depends directly on whether or not one experiences a legal consequence from use of a drug during the current period. In terms of model timing, the drug consumption decision is made during t and any legal trouble from that decision occurs in the same period, presumably while one is still "high" on the drug(s) being used. Those drug use decisions, any legal consequences that result, and other factors that the researcher either can or cannot observe factor into next period's decisions. Defining a drug price vector $\mathbf{P_t}=[P_t^1,P_t^2,P_t^3]$ for each drug,[10] expected lifetime utility is maximized subject to a lifetime budget constraint,

$$\sum_{t=0}^{T}R^tY_t=\sum_{t=0}^{T}(G_t+\mathbf{P_t}\bullet\mathbf{d_t}) \tag{3.3}$$

where Y_t is period t income and $R_t=\frac{1}{(1+r)^{t-1}}$ is a discount factor that depends on the market interest rate r. Recalling that $\Omega_t=\left\{S_t^{j,k},\mathbf{X_t},\mathbf{Z_t},C_t^{j,l}\right\}$ and allowing realized drug-related consequences to be defined

[9] It is well accepted that unobservable factors such as family influences (Altonji, Cattan, and Ware, 2010) and peer effects (Kandel, 1985) play a role in youthful substance use decisions. In the context of gateway effects, however, determining the relative importance of any particular latent factor is empirically difficult and not a goal of the current study. Like prior work that controls for unobservable heterogeneity, then, this study makes no explicit claim about the exact avenue through which unobservable variables affect drug consumption, just that they play a role.

[10] C_t might be thought of as a price, since a previous drug-related arrest would tend to increase the full price (pecuniary plus non-pecuniary) of future drug consumption (Grossman, Chaloupka, and Shim, 2002). For instance, a second drug arrest results in harsher penalties than a first arrest. As such, C_t can be modeled as a function of prior and/or current drug use, since the history and levels of current use both impact a drug user's likelihood of current arrest. For current use, a DWI arrest necessitates use of a drug (usually alcohol) in close proximity to the time of arrest. In terms of looking at the potential impact of past drug use on an arrest, a 1999 study of DWI probationers reveals that nearly 70 percent had used drugs at some point in the past and that 17 percent report drug use in the month prior to their arrest (Maruschak, 1999).

as $c \in [0, C]$, where C is the maximum possible drug-related punishment, the period t lifetime value of choosing a combination of drug consumption d is defined as:

$$V_d^c(\Omega_t, \varepsilon_t) = U_t\left(G_t, \mathbf{d_t}; \Omega_t, \mu, \nu_t, \varepsilon_t^d\right) + \beta\left[\sum_{c'=0}^{C} Prob(c_{t+1} = c)\, V^{c'}(\Omega_{t+1})\right] \tag{3.4}$$

where the maximum expected lifetime utility value is:

$$V^c(\Omega_t) = E_{t-1}\left\{max_{d\in[0,7]}\left[V_d^c(\Omega_t, \varepsilon_t)\right]\right\} \forall t \tag{3.5}$$

At the end of each period, the individual's drug use and criminal histories evolve based on the drug use decisions made during the period and the resulting legal consequences of those decisions, if any. An arrest for possession of marijuana in one period, for instance, should affect the decision to use marijuana in the following period.[11] The evolution of both histories reflects the drug consumption choices made during the period. The drug consumption stock variables provide the main parameters of interest in this model, in the form of testable hypotheses regarding the importance of a joint stock of drug consumption capital, controlling for current prices and arrest history as well as the impact of these factors on current drug demand. Specifically, the empirical model described below will conduct various policy experiments that compare the effects of theoretical "full enforcement" policies[12] versus the effects of changes in pecuniary and non-pecuniary prices. Discussion will focus on potential gateways from cigarettes and alcohol use into marijuana consumption, as well as the relationship between marijuana consumption and demand for cigarettes and alcohol. Before discussing the empirical model based on the framework presented above, though, in the next chapter I describe the data used to estimate the model.

[11] As mentioned above, Lochner (2007) uses NLSY97 data to study an individual's assessment of the likelihood of arrest upon commission of a crime. He finds that the experience of an arrest increases the individual's belief about the probability of being arrested upon the commission of a future crime, while the commission of a crime without an arrest causes the individual to reduce his or her assessment of that probability.

[12] I define a full enforcement policy to be a hypothetical policy that succeeds in completely eliminating a certain type of drug consumption. An example is a hypothetical policy that successfully stops all drinking before the legal drinking age of 21. While not realistic, this type of empirical experiment offers a straightforward mechanism for examining gateway effects.

Chapter 4

Data

4.1 The 1997 National Longitudinal Survey of Youth

To investigate joint substance use decisions as described above, this research employs the first eight annual waves of the 1997 National Longitudinal Survey of Youth (NLSY97)[1]. The NLSY97 is a continuing national probability sample of 8,984 young men and women aged 12 to 18 as of December 31, 1996 (who were thus born from 1980 to 1986), and is designed to gather data on these individuals as they move out of school and into the labor force and adult life. The survey oversamples (non-military) Hispanics, blacks, and economically disadvantaged non-black/non-Hispanic youth.[2] As described in Chapter 5, attrition is modeled, so the only survey members dropped from the analysis are the 600 who were either not interviewed in the second round or had missing first round drug use data (and for whom I therefore could not construct a full set of initial conditions), and the 84 dropped for other reasons related to missing data. Not including obvious, logical imputations,[3] there are 366 individuals for whom I impute drug consumption information or drug use history for at least one survey period, using responses to questions from subsequent or previous survey years. Most of these imputations are for conditional consumption amounts, and therefore minimally impact this study. For more details on the imputation procedures and sample attrition, see Appendix A.

Each year, the NLSY97 asks respondents about their consumption of cigarettes, alcohol, and marijuana. For each substance, questions include indicators for ever having used the drug, use in the past year, and frequency of use. For all three drugs, a consumption question asks how many days of con-

[1] The NLSY97 is collected for the U.S. Department of Labor by the Center for Human Resource Research at The Ohio State University.

[2] For a more detailed summary of the survey methodology, see the *NLSY97 User's Guide*.

[3] For example, when a respondent has missing smoking data for one survey period but in every other period reports that he or she has never smoked a cigarette, I assign that respondent to be a nonsmoker with no smoking history.

sumption occurred during the past 30; for cigarettes and alcohol, an additional question asks how many cigarettes or drinks are typically consumed on days that consumption occurs. The survey collects much less in the way of drug use information from respondents than surveys like the Monitoring the Future (MTF) Study or the National Survey on Drug Use and Health (NSDUH), but, unlike these surveys, advantageously provides a mechanism with which to access detailed levels of geographic data (i.e., county) about where a respondent lives. Additionally, the NLSY97 proves more useful to this study than would the NLSY79 because of the yearly, as opposed to periodic, frequency of drug use inquiries.

Questions arise with surveys like the NLSY97 about the validity/reliability of self-reported drug use questions (Pacula, 1995). One response to such questions might simply be that datasets such as the ones referenced, which all rely on self-reported drug use information, provide the best available source for the information being sought and that researchers just have to make do. A lot of work has been done to assess the validity of these types of self-reported data, though, and the general concensus is that self-reports serve as valid sources of drug use information.[4] Using data from the NSDUH, for instance, Harrison et al. (2007) conduct a study comparing self-reported drug use to urine tests and/or hair samples, noting the caveat that the drug tests themselves are not completely accurate. They find that 85 percent of tobacco use responses match the results of the drug tests. For marijuana, nearly 90 percent of responses match. One advantage of the NLSY97 is that respondents who provide false information (or refuse to answer) one year may act differently during the following round and provide information that allows the researcher to either correct misreported information or fill in missing information. Additionally, the NLSY97 provides data on whether or not someone the respondent knows is present while the interview is being conducted, providing a mechanism to control for misreports. This study will control for whether or not a parent is present for the interview.

In addition to the information about respondent drug use, I make use of self-reported arrest data in the NLSY97 to directly model the impact on future drug consumption of any legal consequences resulting from prior drug use. A respondent reports whether or not he or she has been arrested since the prior interview date, and if formally charged, the reason for the arrest. Though detail on whether an arrest is drug-related is available, I model the experience of any arrest in the empirical work because of the sparsity of incidence for drug-related criminal activity in the data, as well as the fact that data on the reason for an arrest is only available in the event of a formal charge. Only 5.29 percent of respondents

[4] Pacula (1995) and Harrison et al. (2007) provide discussions.

report an arrest during any period of the study, and it is well-established that general criminal activity is correlated with drug use. For instance, in 2004, a survey of state and federal correctional facilities indicates that around 30 percent of prisoners were under the influence of drugs (excluding alcohol) when committing the crime for which they were in prison. Thus, while I argue above that it is appropriate to model drug-related arrests, data constraints justify the use of any reported arrest instead of any drug-related arrest in the empirical work. Importantly, I lack detailed information about non-charged arrests. Alternatively, one could justify the empirical approach by mirroring the arguments of Pacula (1995 and 1998b) regarding the use of property crime rates instead of drug-related crime rates to control for marijuana price variation.

4.2 Excise Taxes and Marijuana Laws

I accumulate price and related information for alcohol and tobacco from various editions of the *Brewer's Almanac, The Tax Burden on Tobacco*, and *The Book of the States*, plus state-level marijuana information from various sources.[5] State-level beer and cigarette tax rates, which proxy for prices for those goods, come from the *Brewer's Almanac* and *The Tax Burden on Tobacco*, respectively. Beer taxes are those levied per case of 24 12-ounce cans or bottles; cigarette taxes are those levied per 20-cigarette package. I deflate nominal tax rates to reflect real taxes. The use of taxes to represent prices is standard in the substance use literature (Cook and Moore, 2000; Carpenter and Cook, 2008), since they are measured more accurately than retail prices and can flexibly deal with policy changes, e.g., changes in tax rates (Pacula, 1995). In addition, prior work indicates that excise taxes on cigarettes[6] and alcohol[7] are almost entirely passed on to the consumer, even when the consumer is underage.[8]

One might argue that using alcohol excise taxes to proxy for the price of alcohol is inappropriate

[5] In particular, I gather information from the website of the National Organization for the Reform of Marijuana Laws (NORML, 2012), documents such as ImpacTeen's *Illicit Drug Policies: Selected Laws from the 50 States* (2002), various versions of Bruce Margolin's *The Margolin Guide* (in current form and under its former name, *The Hempster's Guide to State and Federal Marijuana Laws*), and a research paper on marijuana decriminalization by Pacula, Chriqui, and King (2003).

[6] See Becker, Grossman, and Murphy (1994); Sung, Hu, and Keeler (1994); Barnett, Keeler, and Hu (1995); Keeler et al. (1996); and Chaloupka et al. (2000).

[7] See Cook and Tauchen (1982); Grossman, Coate, and Arluck (1987); and Pacula (1995).

[8] See Grossman, Coate, and Arluck (1987); Coate and Grossman (1988); Laixuthai and Chaloupka (1993); and Chaloupka, Grossman, and Saffer (2002).

when most underage drinkers do not purchase their own alcohol, especially at very young ages (CAMY, 2006). Even so, the price of alcohol has been shown to affect underage drinking rates (Chaloupka, Grossman, and Saffer, 2002), and higher excise taxes feed through to drinkers in the form of higher prices. For example, Grossman, Coate, and Arluck (1987) and Coate and Grossman (1988) find that higher beer prices decrease beer consumption for those aged 16 to 21, and also reduce the percentage who drink frequently at those ages. In addition, Laixuthai and Chaloupka (1993) conclude that both overall youthful drinking incidence and heavy drinking incidence decline in frequency as beer taxes rise. Though the premise is less controversial, evidence also shows that higher cigarette taxes discourage youthful smoking (Chaloupka et al., 1999).

For this study, I investigated numerous avenues for obtaining actual prices for marijuana, ranging from System to Retrieve Information from Drug Evidence (STRIDE) data gathered during undercover purchase operations by the Drug Enforcement Administration (DEA) to reader-reported data from *High Times* magazine.[9] These efforts ultimately proved unsuccessful due to a sparsity of price observations in comparison to the national sample underlying the NLSY97 data. Similar to other studies, however, this work takes advantage of the fact that prices for illegal drug consumption are not exclusively monetary. When assessing the expected (marginal) utility and cost of consuming an illicit good, an adolescent must factor in the potential that such consumption (or even possession of the good) might result in punishment of some sort, including an arrest.[10] To control for the possibility of arrest, and/or to mitigate a general lack of available drug price data, Pacula (1995 and 1998b) includes a control variable that measures the ratio of property crimes committed in a geographic region to the population-weighted number of law enforcement officers in the area. She argues, per Kleiman and Smith (1990), that enforcement risk increases monetary illegal drug prices by increasing the legal risk for sellers of drugs, and increases non-pecuniary prices by increasing the legal risk for possessors of drugs. Thus, following Pacula and Corman et al. (2004), one effort to control for the price of marijuana in this study is the inclusion of

[9] I obtained DEA price data via three different mechanisms. First, I requested and obtained actual transaction data directly from the DEA. Second, Jeff Desimone, Sara Markowitz, and Rosalie Pacula generously provided me with another set of price information that they created from DEA data. Price information from *High Times* was collected and generously provided to me by Mireille Jacobson. Unfortunately, none of these data proved to be usable for the purposes of this study.

[10] Potential health effects might also be considered a price that factors into drug consumption decisions, but as discussed above, I do not attempt to control for such effects since this is a model of drug demand by adolescents and young adults (Lindsay and Rainey, 1997).

controls for (county-level) drug arrest rates[11] from the Uniform Crime Reporting (UCR) Program.[12]

In addition to county-level drug arrest rates and the relatively standard use of state-level beer and cigarette taxes as proxies for the prices of those drugs, an innovative approach used by this study to control for the price of marijuana is the accumulation of information outlining six categories of state-level marijuana laws. I use these measures to capture the legal permissiveness of the geographic environment within which adolescent marijuana consumption decisions are made. In addition to the oft-used measure of marijuana decriminalization,[13] I also employ other measures of state-level attitudes toward marijuana use. These measures include indicators for mandatory minimum sentencing for low-level marijuana possession, the existence of legalized marijuana for medical use (i.e., the use of marijuana for medical purposes is not a crime in the state), the existence of separate statutes penalizing drugged drivers (as opposed to generic laws covering all types of driving under the influence), and whether or not tax stamp legislation has been enacted in the state.[14] With insufficient price information available over the course of the study to construct stable marijuana pricing information, these variables proxy for the price of marijuana along a number of dimensions in each state rather than solely relying on a potentially problematic measure of decriminalization by state. I construct this state-level information using data from the NORML website combined with various searches of the internet to verify the date a change in one of the laws goes into effect. Table 4.1 demonstrates that in addition to providing pertinent information, these marijuana descriptors contain sufficient variation to be useful in the empirical model, especially when combined with the movement of individuals across states that occurs during the study

[11] While Pacula argues for the use of property crime rates instead of drug crime rates because of the relatively high incidence of crimes committed under the influence of drugs. I employ drug arrest rates since I directly model the likelihood of arrest for any crime, as detailed above.

[12] The Federal Bureau of Investigation (FBI) assembles the UCR data, which do not contain data for all counties in all states, and do not contain any data for some states during the 1997-2004 timeframe. Florida and Illinois, for instance, do not participate in the program. In cases where no county-level data exist, I use state-level arrest rates.

[13] Prior to 2003, there was one commonly used decriminalization measure for which 12 states currently classify as decriminalized (Kleiman, 1992; Pacula, 1995; NORML, 2012). The measure used here is a newer definition referenced above and outlined by Pacula, Chriqui, and King (2003), and is based on the fact that the actual criminal penalties for possessing or selling marijuana are not universally harsher in non-decriminalized states than in states commonly classified as decriminalized (Thies and Register, 1993). The states meeting this more recent decriminalization definition do not change over the course of this study.

[14] Tax stamp laws make possession of illegal drugs taxable, requiring a tax stamp as proof that the tax has been paid. Thus, if a person is found to be in a possesstion of an illegal drug in a state with a tax stamp law and cannot produce a valid stamp demonstrating payment of the applicable tax, he or she potentially faces additional monetary penalties or even tax evasion charges above and beyond the punishment that would have been associated with the drug possession alone (NORML, 2012).

Table 4.1: Percent of Analysis Sample Affected by State Marijuana Policies

Variable/Year (N=8,300)	1997	1998	1999	2000	2001	2002	2003	2004
Marijuana Decriminalized	25.90	25.98	25.87	26.04	26.26	26.17	25.93	26.21
Mandatory Minimum Drug Sentencing	61.19	61.22	54.51	54.55	51.79	51.27	32.01	32.37
Medical Marijuana Legal	14.47	17.76	17.99	20.43	20.34	20.05	19.98	22.16
Tax Stamp Law Enacted	34.82	28.04	31.23	31.57	31.98	32.05	32.33	32.16
Drugged Driving Law Enacted	7.71	7.83	11.76	11.80	11.67	11.95	20.17	21.84

NLSY97 Data: 1997-2004 Survey Rounds.

time frame. A state-level breakdown of these laws appears in Appendix B.[15]

4.3 Sample Statistics

I restrict the research sample to respondents who participate in at least the first two periods of the survey and for whom dependent variable information is either available or can be constructed for each study period. These restrictions result in losses of 600 and 28 survey members, respectively. Additionally, I drop 56 respondents due to missing or inconsistent covariate information. Since I model attrition, all other respondents (8,300) are included in the estimated model. The next three tables describe the excluded sample and sample attrition, providing a comparison of baseline characteristics for the analysis sample versus those respondents completely excluded from the analysis, then analyzing sample attrition over time and by respondent age. Table 4.2 reports basic demographic characteristics for the included and excluded samples as well as baseline measures of drug use. Looking at the means for the demographic variables, the samples appear to be similar, but the drug use, arrest, age, and education differences between the included and excluded samples are mostly statistically significant. In addition, the differences for whites and blacks are marginally significant, with t-statistics over 1.75. Not much can be done about these differences for the initial period itself, since the model being estimated requires two consecutive periods of data from the outset of the survey. One thing to note about the large differences in magnitude for amounts of (cigarette) smoking and drinking between the analysis sample and those excluded from the analysis is that the amounts shown reflect consumption over a 30-day time period.

[15] Looking subjectively at the price controls outlined above and their use in prior work, the use of alcohol and cigarette taxes seems to adequately capture variation in the prices of those drugs, while the use of decriminalization measures and crime rates to proxy for marijuana prices has been called into question. While the combination of measures used here to control for variation in marijuana prices is admittedly imperfect, they are consistent with what has been done in prior work and reflect my best effort to implement such controls in the absence of actual price data.

Table 4.2: Analysis Sample versus Excluded Sample at Baseline

Variable	Analysis Sample	Excluded Sample	Difference*
	Mean *(std. deviation)*	Mean *(std. deviation)*	*Significant at α=.05
Age at Baseline	14.33 *(1.48)*	14.70 *(1.52)*	-0.37*
Highest Grade Completed at Baseline	7.70 *(1.56)*	8.00 *(1.63)*	-0.30*
Male	0.51 *(0.50)*	0.52 *(0.50)*	-0.01
White	0.49 *(0.50)*	0.52 *(0.50)*	-0.03
Black	0.26 *(0.44)*	0.23 *(0.42)*	0.03
Hispanic	0.21 *(0.41)*	0.21 *(0.41)*	0.00
Ever Smoked	0.39 *(0.49)*	0.44 *(0.50)*	-0.05*
Current Smoker	0.21 *(0.40)*	0.26 *(0.44)*	-0.05*
Past 30 Day Use (Smokes - current smokers)	100.03 *(169.18)*	140.80 *(204.97)*	-40.77*
Ever Drank	0.43 *(0.49)*	0.47 *(0.50)*	-0.04*
Current Drinker	0.23 *(0.42)*	0.27 *(0.45)*	-0.04*
Past 30 Day Use (Drinks - current drinkers)	18.95 *(49.45)*	27.65 *(68.87)*	-8.70*
Ever Smoked Pot	0.20 *(0.40)*	0.25 *(0.44)*	-0.05*
Current Pot Smoker	0.10 *(0.31)*	0.14 *(0.35)*	-0.04*
Past 30 Day Pot Use (Days - current users)	7.68 *(9.43)*	7.57 *(9.28)*	0.11
Ever Arrested	0.08 *(0.27)*	0.13 *(0.34)*	-0.05*
Arrested Past Year	0.04 *(0.20)*	0.07 *(0.26)*	-0.03*
State Cigarette Tax (per pack of 20) - 2004 $	0.49 *(0.37)*	-	
State Beer Tax (per case of 24 cans) - 2004 $	0.28 *(0.25)*	-	
N	8,300	684**	

** 600 respondents have insufficient year two interview data; 28 have no drug use information. 56 are dropped due to missing covariate information. See Appendix A.

NLSY97 Data: 1997-2004 Survey Rounds.

For smoking, therefore, the difference amounts to two extra packs of cigarettes a month for the excluded group, on average, and the difference for drinking amounts to less than a drink per day.[16] Regardless, not much can be done about these differences, but they do provide preliminary evidence that modeling sample attrition over time is appropriate. I deal with these issues over time through the model, which includes equations for sample attrition and arrest, and categorizes substance use into light and heavy use for alcohol and tobacco. I estimate current drug use as a function of prior use, defining ever having used a substance and prior period use as state variables to be tracked over time.[17] The methodology seeks to minimize the potential for sample bias along the dimensions of the basic descriptive statistics shown below, but I can do little about the baseline differences that exist between these samples.

[16] Of course, the differences lie outside the bounds of how I define heavy use of these drugs (see below). For example, the differences could reflect two extra episodes of binge drinking for those dropped from the sample completely.

[17] An indicator for initiation during the preceding period is also tracked over time, but cannot be defined accurately at baseline. A state variable for actual consumption amounts, to distinguish, for example, between someone who drinks moderately but regularly and someone who has an occasional beer, might also be employed but is not used in this research.

Table 4.3: Sample Attrition over Time

Variable*	Never	Ever	1998	1994	2000	2001	2002	2003
Number (N)	6,269	2,031	423	363	382	253	266	344
Percent (%)	(75.5%)	(24.5%)	(5.1%)	(4.4%)	(4.6%)	(3.0%)	(3.2%)	(4.1%)
Male	3,064	1,179	238	203	223	149	145	221
	(48.9%)	(58.1%)	(56.3%)	(55.9%)	(58.4%)	(58.9%)	(54.5%)	(64.2%)
Female	3,205	852	185	160	159	104	121	123
	(51.1%)	(41.9%)	(43.7%)	(44.1%)	(41.6%)	(41.1%)	(45.5%)	(35.6%)
White	3,101	954	191	174	166	123	128	172
	(49.5%)	(47.0%)	(45.2%)	(47.9%)	(43.5%)	(48.6%)	(48.1%)	(50.0%)
Black	1,625	553	122	94	125	63	55	94
	(25.9%)	(27.2%)	(28.8%)	(25.9%)	(32.7%)	(24.9%)	(20.7%)	(27.3%)
Hispanic	1,306	451	98	79	80	58	70	66
	(20.8%)	(22.2%)	(23.2%)	(21.8%)	(20.9%)	(22.9%)	(26.3%)	(19.2%)
Baseline Smoker	1,241	465	162	129	154	99	102	139
	(19.8%)	(22.9%)	(38.3%)	(35.5%)	(40.3%)	(39.1%)	(38.3%)	(40.4%)
Baseline Drinker	1,447	503	195	170	192	132	140	185
	(23.1%)	(24.8%)	(46.1%)	(46.8%)	(50.3%)	(52.2%)	(52.6%)	(53.8%)
Baseline Pot Smoker	615	254	93	73	76	42	49	54
	(9.8%)	(12.5%)	(22.0%)	(20.1%)	(19.9%)	(16.6%)	(18.4%)	(15.7%)
Arrested at Baseline	245	112	39	37	33	21	12	41
	(3.9%)	(5.5%)	(9.2%)	(10.2%)	(8.6%)	(8.3%)	(4.5%)	(11.9%)
Last Age Observed	21.89	18.62	16.56	17.51	18.38	19.19	20.06	21.08

* Operationally, attrition is defined a year forward, but here captures the last year a survey is completed.

NLSY97 Data: 1997-2004 Survey Rounds.

To see definitively why it is appropriate to directly model sample attrition, Table 4.3 demonstrates that the likelihood a respondent ever leaves the sample varies significantly, both across survey periods and by observable characteristics. Thus, simply taking attrition as given may bias the empirical results. Ignoring the excluded sample shown in Table 4.2, men are more likely to leave the NLSY97 sample then women in all survey periods, and African-Americans and Hispanics are more likely to exit than Whites in most survey periods. Males make up 51 percent of the 8,300 total respondents who appear in this study, but account for over 54 percent of attrition each survey period and sometimes account for substantially more than that (e.g., over 64 percent in 2003). Similarly, while Blacks make up 26 percent of the analysis sample and Hispanics 21 percent, yearly attrition figures indicate that the percentage of Blacks among those leaving the sample is less than 26 percent in only two survey periods. For Hispanics, this percentage is similarly less than 21 percent in only two survey years. Table 4.3 also shows that attrition varies based on the behaviors of interest to this study - drug use and arrests. The chance of attrition at any point in the survey is much higher among those who report drug use at baseline, as well as among those who report having been arrested at some point in the year prior to the baseline NLSY97 interview. In some years, the percentage of those who leave the sample and report these behaviors at baseline (i.e., drug use and/or an arrest within the past year) is double the percentage of respondents reporting those behaviors at baseline in the full analysis sample. In addition, though not shown in the table, disproportionate arrest and incarceration rates for Blacks compared to Whites across the U.S. are well known, providing another reason to model sample attrition. Also reflecting nationwide statistics is the fact that males far outnumber females in the sample in terms of attrition-related factors such as being arrested and serving time in jail.[18] Therefore, attrition bias with respect to men, particularly Black and Hispanic men, may affect results and their interpretation, indicating that modeling attrition is appropriate.[19]

Table 4.4 shows attrition by age. Attrition peaks at ages 18 and 19 (the mean age is 18.62, with a median age of 19), and these ages are consistent across the demographic and behavioral groupings shown. Definitionally, I construct the attrition variable to indicate that respondents leave the sample the year after they last complete the NLSY97 survey. Thus, attrition at age 19 means that the respondent last completes a survey at age 18, a typical age for high school graduation. Interestingly, for drug use, a significant jump occurs in the percentage of those who report drug use at baseline and subsequently leave the sample upon or after reaching the age of 17, suggesting that sample attrition correlates with drug use. More evidence for such a correlation is provided by looking at current use (as reported during the last interview) among those who leave the sample. Though less variation exists for current use percentages by attrition age, similar patterns emerge.

For a preliminary idea of how drug use varies by age as well as by how I define drug use in the empirical work below, Table 4.5 provides an assessment of demographic characteristics and use rates across drugs in the analysis sample, at various ages. I define current use each year for each respondent as use within the 30 days preceding the interview. As noted above, these definitions are standard in the literature. Table 4.5 also distinguishes between light and heavy drinking. This distinction also appears in the empirical work described below. According to a review of the literature of adolescent smoking by

[18] In 2011, 87.3 percent of jail inmates and over 92 percent of state and federal prison inmates were male (Maguire, 2012a and 2012b).

[19] The percentage of males in the analysis sample who are black falls from 25.4 percent at baseline to 24.4 percent in 2004; for Hispanic men, the percentage falls from 21.3 to 20.5.

Table 4.4: Sample Attrition by Age

Variable	Age 15	Age 16	Age 17	Age 18	Age 19	Age 20	Age 21	Age 22	Age 23
Number (N)*	105	164	291	396	384	264	155	152	72
% of Total	5.2%	8.1%	14.3%	19.5%	18.9%	13.0%	7.6%	7.5%	3.5%
Male	61	87	177	215	236	148	91	91	42
(% of Group)	(58.1%)	(53.1%)	(60.8%)	(54.3%)	(61.5%)	(56.1%)	(58.7%)	(59.9%)	(58.3%)
Female	44	77	114	181	148	116	64	61	30
(% of Group)	(41.9%)	(47.0%)	(39.2%)	(45.7%)	(38.5%)	(43.9%)	(41.3%)	(40.1%)	(41.7%)
White	47	71	145	177	167	139	76	70	40
(% of Group)	(44.8%)	(43.3%)	(49.8%)	(44.7%)	(43.5%)	(52.7%)	(49.0%)	(46.1%)	(55.6%)
Black	26	49	78	114	104	68	43	43	15
(% of Group)	(24.8%)	(29.9%)	(26.8%)	(28.8%)	(27.1%)	(25.8%)	(27.7%)	(28.3%)	(20.8%)
Hispanic	27	41	60	91	94	49	31	33	13
(% of Group)	(25.7%)	(25.0%)	(20.6%)	(23.0%)	(24.5%)	(18.6%)	(20.0%)	(21.7%)	(18.1%)
Baseline smoker	21	44	80	134	165	111	61	64	27
(% of Total)	(20.0%)	(26.8%)	(27.5%)	(33.8%)	(43.0%)	(42.1%)	(39.4%)	(42.1%)	(37.5%)
Current smoker	30	60	110	147	172	112	56	58	26
(% of Total)	(28.6%)	(36.6%)	(37.8%)	(37.1%)	(44.8%)	(42.4%)	(36.1%)	(38.2%)	(36.1%)
Baseline drinker	23	43	103	161	211	135	75	94	46
(% of Total)	(21.9%)	(26.2%)	(35.4%)	(40.7%)	(55.0%)	(51.1%)	(48.4%)	(61.8%)	(63.9%)
Current drinker	35	65	135	190	211	142	91	87	41
(% of Total)	(33.3%)	(39.6%)	(46.4%)	(48.0%)	(55.0%)	(53.8%)	(58.7%)	(57.2%)	(56.9%)
Baseline marijuana	13	27	58	93	109	72	32	29	16
(% of Total)	(12.4%)	(16.5%)	(19.9%)	(23.5%)	(28.4%)	(27.3%)	(20.7%)	(19.1%)	(22.2%)
Current marijuana	25	40	81	102	112	68	30	28	17
(% of Total)	(23.8%)	(24.4%)	(27.8%)	(25.8%)	(29.2%)	(25.8%)	(19.4%)	(18.4%)	(23.6%)
Baseline arrest	8	15	29	34	39	27	12	20	5
(% of Total)	(7.6%)	(9.2%)	(10.0%)	(8.6%)	(10.2%)	(10.2%)	(7.7%)	(13.2%)	(6.9%)
Current arrest	9	14	34	36	29	25	7	19	7
(% of Total)	(8.6%)	(8.5%)	(11.7%)	(9.1%)	(7.6%)	(9.5%)	(4.5%)	(12.5%)	(9.7%)

* 40 people leave the sample at age 14; even fewer at ages 13, 24, and 25. Attrition is defined a year forward.

NLSY97 Data: 1997-2004 Survey Rounds.

Table 4.5: Drug Use Rates by Gender at Different Ages

Age 14 (N=1,694)	Light Smoking	Heavy Smoking	Light Drinking	Heavy Drinking	Pot Use
Male	0.1333	0.0678	0.2039	0.0362	0.1149
Female	0.1387	0.0690	0.2250	0.0423	0.1125
Smokers	0.6652	0.3348	0.4564	0.1359	0.3948
Drinkers	0.2918	0.1857	0.8454	0.1546	0.3356
Pot smokers	0.3650	0.3445	0.5347	0.2134	1.0000
Age 17					
(N=6,418)					
Male	0.1237	0.2110	0.2649	0.1813	0.2787
Female	0.1164	0.1974	0.3123	0.1129	0.2228
Smokers	0.3702	0.6298	0.3795	0.3136	0.4951
Drinkers	0.1859	0.3299	0.6612	0.3388	0.4364
Pot smokers	0.1938	0.4454	0.3979	0.3591	1.0000
Age 18					
(N=7,602)					
Male	0.1209	0.2624	0.2723	0.2520	0.3070
Female	0.1101	0.2289	0.3530	0.1436	0.2369
Smokers	0.3198	0.6802	0.3743	0.3619	0.4920
Drinkers	0.1656	0.3554	0.6120	0.3880	0.4186
Pot smokers	0.1717	0.4814	0.3652	0.4202	1.0000
Age 20					
(N=6,897)					
Male	0.1081	0.3143	0.2999	0.3096	0.2841
Female	0.0798	0.2561	0.3664	0.1763	0.2262
Smokers	0.2477	0.7523	0.3572	0.4096	0.4410
Drinkers	0.1342	0.3702	0.5792	0.4208	0.3684
Pot smokers	0.1519	0.5034	0.3680	0.4642	1.0000
Age 21					
(N=5,297)					
Male	0.0989	0.3193	0.3071	0.3931	0.2709
Female	0.0824	0.2664	0.4290	0.2094	0.1960
Smokers	0.2365	0.7635	0.3557	0.4749	0.3966
Drinkers	0.1171	0.3586	0.5508	0.4492	0.3098
Pot smokers	0.1514	0.5004	0.3296	0.5595	1.0000

NLSY97 Data: 1997-2004 Survey Rounds.

Rubinstein et al. (2003), definitions of light and heavy smoking vary widely within that literature.[20] I make the distinction using a slight modification of the definition of Resnicow et al. (1999), who define light smoking for an adolescent as consumption of 10 cigarettes (half a pack) or less during the last 30 days. I define light smoking as consumption of less than one pack of 20 cigarettes during the past 30 days, and heavy smoking as consumption of a pack or more. For drinking, I distinguish heavy versus light based off of a review of the relevant literature that specifically attempts to arrive at an operational definition for binge drinking (having at least five drinks in a single drinking episode) in adolescence (Parada et al., 2011). Parada et al. define heavy drinking as binge drinking at least once during the previous two weeks; I translate that definition to at least two such episodes in the past 30 days.

Table 4.5 demonstrates that at age 14, most cigarette smokers are light smokers and most drinkers are light drinkers, and the same holds true when comparing use across those substances (most smokers who drink are light drinkers and most drinkers who smoke are not heavy smokers). Nearly 40 percent of tobacco smokers use marijuana, while only 33.6 percent of drinkers do so. Among marijuana users, over a third are heavy smokers while a little over a fifth drink heavily, so one takeaway is that the table potentially provides some evidence of gateways from cigarette and alcohol consumption into marijuana use, with the cigarette gateway appearing to be slightly stronger. One interesting finding is that females are more likely to smoke cigarettes and drink alcohol than males at this young age.[21]

Table 4.5 further shows that from age 17 forward, most smokers of cigarettes smoke them heavily, as do most drinkers who smoke. Among consumers of alcohol, though the ratio of light to heavy drinking decreases as age increases, the majority of drinkers continue to drink lightly through age 21 and beyond. Once respondents reach the legal drinking age of 21, a majority of cigarette smokers who drink consume at least five drinks on at least two separate occasions in the 30 days preceding their most recent NLSY97 interview. Looking at marijuana consumers, those who smoke cigarettes tend to smoke heavily at all ages in the study above age 14, and the majority of those who drink do so heavily once they reach the age of 18. Further evidence of gateways, not explicitly shown in Table 4.5, is demonstrated by the fact that 25.2 percent of pot smokers do not report drinking at age 14 but, by age 21, only 11.1 percent report abstaining from alcohol during the 30 days preceding the NLSY97 interview. This percentage

[20] Sargent, Mott, and Stevens (1988) define light smoking as less than one cigarette per day.

[21] This finding is consistent with other recent research (CAMY, 2010).

of non-drinkers who smoke pot consistently decreases as the survey members age. This is consistent across the age range when current marijuana use rate increases prior to age 18 as well as after the use rate begins to decline after respondents reach age 18. To summarize, over time, fewer and fewer marijuana users do not drink. This finding might have gateway implications, but drawing conclusions about gateway effects from a table of means is problematic. I now describe a more robust method of looking for gateway effects related to joint consumption of these drugs.

Chapter 5

Empirical Model

5.1 Empirical Specification - Drug Use

Based on the theoretical framework described in Chapter 3, a young person chooses each period whether or not to consume cigarettes, alcohol, and marijuana in each of eight observed survey periods. Assuming that the additive error component is extreme value distributed yields multinomial logit choice probabilities defined as:

$$Prob(\mathbf{d_t} = d) = \frac{e^{\bar{V}_d(\Omega_t)}}{\sum_{d'=0}^{7} e^{\bar{V}_{d'}(\Omega_t)}} \tag{5.1}$$

where $\bar{V}_d(\Omega_t) = V_d(\Omega_t, \varepsilon_t) - \varepsilon_t^d$. The function $\bar{V}_d(\Omega_t)$ can be approximated with a n^{th} order Taylor series expansion of its arguments such that the log odds of falling within a particular use category are:

$$ln\left[\frac{Pr(\mathbf{d_t} = d)}{Pr(\mathbf{d_t} = 0)}\right] = a_{d0} + \alpha_{d1}S_t + \alpha_{d2}\mathbf{P_t} + \alpha_{d3}\mathbf{X_t} + \alpha_{d4}\mathbf{Z_t} + \xi_t^d, d = 0, \ldots, 7 \tag{5.2}$$

To estimate the effect of past drug use on current drug consumption, however, instead of estimating a multinomial logit with eight potential observed outcomes for the dependent variable, I estimate dynamic demand equations that separate $\mathbf{d_t}$ out into three equations, one for each drug.[1] I estimate the three equations jointly, allowing the errors to be correlated across equations and preserving the essence of the model outlined above, since the joint correlation of the drug demand equations captures the key aspects of the multinomial drug consumption variable defined in the theoretical model. For cigarettes and alcohol, I expand consumption beyond simple participation to include levels of consumption. I

[1] While I still divide alcohol and cigarette consumption into multinomial categories for heavy, light, and zero consumption, this approach minimizes potential problems relating to the Independence of Irrelevant Alternatives assumption underlying multinomial logit models in terms of the consistency of covariates across dependent variable outcomes (McFadden, Train, and Tye, 1976).

define drug use as individuals i's consumption of the three different drugs, $j = 1,2,3,$ during period t. The counter $j = 1$corresponds to cigarette consumption, which is categorized into three categories, where:

$$d_{it}^1 = \begin{cases} 0, & \text{no use during the past month} \\ 1, & \text{light use (nonzero but less than 20 cigarettes during the past month)} \\ 2, & \text{heavy use (20 or more cigarettes during the past month)} \end{cases}$$

The counter $j = 2$ defines alcohol consumption, similarly categorized:

$$d_{it}^2 = \begin{cases} 0, & \text{no use during the past month} \\ 1, & \text{light use (nonzero but 5 + drinks in one day no more than once during the past month)} \\ 2, & \text{heavy use (5+ drinks in one day at least twice during the past month)} \end{cases}$$

Lastly, the counter $j = 3$ corresponds to marijuana consumption, which is categorized dichotomously, yielding:

$$d_{it}^3 = \begin{cases} 0, & \text{no use during the past month} \\ 1, & \text{any use during the past month} \end{cases}$$

For marijuana, $d = 1$ if use occurs. I do not model conditional consumption amounts because of the lack of data on consumption frequency in the data beyond days of use. Users and former users of each drug face similar choices but have different histories of drug use. The key explanatory variables for the analysis are the state variables capturing the existing consumption stock of researcher-defined gateway drug (i.e., cigarettes and alcohol) consumption in relation to current period, researcher-defined harder drug (marijuana) consumption. The price variables will also play a major role in simulating results from experiments below that allow an analysis of potential gateway effects.[2] As detailed previously, I control for observable individual- and area-level factors in $\mathbf{X_t}$ and $\mathbf{Z_t}$, respectively, and for non-idiosyncratic unobservables. In addition to prior drug consumption and current prices, the explanatory variables in the longitudinal drug use and arrest equations contain interactions of prior period drug consumption indicators to control for differential impacts of the joint consumption of multiple drugs. Finally, similar to Gilleskie and Strumpf (2005), I also include interactions of prior drug consumption with current prices to control for differential price elasticities among different sample groups, e.g., between drinkers, former drinkers, and total abstainers.

[2] As described above, I do not have actual price data for these drugs, and will proxy for prices using state-level excise taxes for beer and cigarettes, and with state-level marijuana laws. Therefore, any reference to prices below implies a reference to the variables proxying for those prices.

5.2 Empirical Specification - Arrests

In addition to the evolution of drug consumption stock at the end of the period, legal consequences (i.e., the reporting of at least one arrest) that impact future drug consumption choices also evolve. I model the probability of (any) self-reported arrest as a function of current period drug consumption, the accumulated stock of previous drug consumption, observable covariates, and price proxies, excluding cigarette taxes for purposes of identification. The probability of experiencing legal consequences resulting from drug use, in log odds, then, is

$$ln\left[\frac{Pr\left(c_t^j = c\right)}{Pr(c_t^j = 0)}\right] = \gamma_{c0} + \boldsymbol{\gamma_{c1}}\mathbf{d_t} + \gamma_{c2}S_t + \boldsymbol{\gamma_{c3}}\mathbf{P_t} + \gamma_{c4}\mathbf{X_t} + \gamma_{c5}\mathbf{Z_t} + \xi_t^c,\ c = 0, 1 \tag{5.3}$$

Equation 5.3 is identified by the exclusion of cigarette taxes from $\mathbf{P_t}$, and also from the corresponding tax/use interactions. Identification of arrest likelihood also provides identification for the drug use equations, where the current period price variables are potentially endogenous. Statistical justification for this exclusion restriction arises from estimations (results not shown) showing that cigarette prices affect drug use but not the likelihood of arrest. The variables are jointly insignificant when included in the (stand alone) arrest equation (Equation 5.3), but jointly significant in each of the drug use equations when these equations are estimated independently. The same statement applies for the tax variables themselves, with a likelihood ratio test of the joint signficance of the cigarette tax variables (including interactions) in the arrest equation yielding a Chi square statistic with a corresponding *p*-value of 0.3622.[3] Ignoring the consumption/price interactions produces a statistic with a *p*-value of 0.3108.[4] In terms of theoretical justification for excluding the cigarette taxes but not taxes on alcohol from this equation, the alcohol tax variables are arguably correlated with an equation of arrests because many respondents in the NLSY97 data spend a good deal of the study period below the nationally standardized legal consumption age of 21. For instance, a combined analysis across years of the analysis sample indicates that over 75 percent of interviews are conducted when the respondent is less than 21 years of age. Though an argument can be made that the legal consequences of underage drinking are usually minimal, excluding these variables from the legal consequence equation while drinking is clearly illegal for three

[3] The corresponding *p*-value for the test of all beer tax variables, including tax/use interactions, is a marginally statistically significant 0.1012. Testing both cigarette and beer taxes yields a *p*-value of 0.1790.

[4] The corresponding *p*-value for the test of beer taxes is a statistically significant 0.0136. Testing both cigarette and beer taxes (exclusive of interactions) yields a *p*-value of 0.0286.

quarters of the sample does not seem to be justified. The argument for excluding cigarette taxes, however, stands on better ground. Whereas the legal consequences of underage cigarette consumption are also arguably minimal, over 60 percent of the NLSY97 interviews used in this study reference behavior in periods where the respondent is aged 18 or older.[5] Thus, smoking a cigarette is not illegal for the majority of respondents, whereas alcohol consumption is.[6]

Sample Attrition

Because drug use decisions and any legal problems resulting from those decisions are correlated with the likelihood a respondent stops participating in the NLSY97, I jointly model attrition at the end of each period in the empirical model as a function of observable information and unobservable heterogeneity. Based on the analysis of attrition shown above, people in the tails of the drug use distribution are more likely to leave the sample. As a result, the analysis sample includes fewer and fewer such people over time, potentially biasing estimates of gateway effects. Modeling sample attrition and controlling for the (non-random) characteristics of those who remain in the sample assures that I estimate the correct coefficients on the drug use and legal consequence variables. Non-random characteristics that might be correlated with attrition include observables, such as the drug decision variables of interest and the endogenous state variables, as well as unobservables such as peer effects and other factors. Information on how I impute data to keep some respondents in the sample who would otherwise be dropped for reasons other than attrition appears in Appendix A.

5.3 Estimation Method

Discrete Factor Random Effects

The equations representing demand for drugs, the probability of legal consequences, and attrition from the sample are jointly estimated using the Discrete Factor Random Effects (DFRE) technique.

[5] For actual interviews (where those who leave the sample are excluded), the percentage of respondents 18 years old or older at the interview date is 67.6; when counting the attrition subsample, this percentage is 61.7. Alternatively, less than 25 percent of interviews occur when the respondent is of legal drinking age under either counting method.

[6] In North Carolina, for instance, smoking under the age of 18 is a Class 2 misdemeanor, potentially punishable by community service and a $1,000 fine (http://www.ncdps.gov/Index2.cfm?a=000003,000005,000996,000378). In practical terms, however, evidence does not indicate that these types of laws substantially deter teenage smoking (Wakefield and Giovino, 2003), and there are questions about how stringently such laws are enforced.

The joint estimation process allows the errors to be correlated across the equations such that correlation exists between drug use, legal consequences, and attrition. As outlined by Heckman and Singer (1984), the estimation technique attempts to control for factors that the researcher cannot observe, impact each estimated equation, and produce biased results when not controlled for in the estimation process. By employing this method, the unobservables are broken down into the three types of errors outlined above: a permanent component (μ) for which respondents receive one random draw that sticks with them forever, a time-varying component (ν_t) for which a draw is received each period, and an idiosyncratic component (ε_t) that captures random shocks. The distribution of each error type is estimated across all equations e_j, and for each logit equation outcome o, the effect of the heterogeneity in each equation is also estimated.[7] Allowing the error terms to be nonlinear allows a combined error term for all equations to be defined as:

$$\xi_{et}^{o} = \mu_{e}^{o} + \nu_{et}^{o} + \varepsilon_{et}^{o} \tag{5.4}$$

The permanent and time-varying components are assumed to be correlated across equations while the idiosyncratic component is not since it is random. Fixed effects estimation provides an alternative methodology to control for hidden factors over time. The inclusion of fixed effects controls for permanent unobservable heterogeneity that impacts the drug consumption decision for each individual. Fixed effects, however, *only* control for permanent unobservables, whereas the DFRE method offers the additional flexibility to assess the effect of time-varying unobservable factors that are important in the context of drug use. Questions might be raised about how well DFRE estimation handles time-varying latent variables, but as opposed to fixed effects, the DFRE approach does not subsume the effects on drug use of observable variables that do not vary over time. Further, while I am unable to make statements about the relative importance of the particular avenues through which unobservables affect drug use behavior while using this method, one goal of the study is simply to demonstrate that unobservables play a significant role. Thus, to the extent that the model fails to adequately capture time-varying heterogeneity that affects drug use decisions, results are biased against finding a role for unobservables, thereby strengthening the evidence behind any findings that latent factors matter. A final advantage of the DFRE technique over fixed effects estimation is that it introduces a limited number of additional parameters into the estimation process in comparison to a model with, for example, state- or county-level

[7] Note that this empirical model consists entirely of logit and multinomial logit equations.

controls.[8]

Initial Conditions

At the point that an individual in the sample is first observed, she may have a history of previous drug use and/or arrests. Because the empirical equations are dynamic (i.e., depend on past behavior), and retrospective data on drug use is not comprehensive, I must model the initially observed state variables as a function of the common unobservables that not only influence prior behavior but are also correlated with current behavior. I first establish initial conditions for drug use and arrests along three period-specific dimensions, or states. For drug use at baseline and for each drug *j* of alcohol, cigarettes, and marijuana, I specify three state variables that define whether or not each individual *i* has ever used the drug entering the period, whether or not the individual uses the drug during the period, and finally whether or not an observation of positive use for drug *j* during the period is the first ever use of that drug. The latter variable is defined based on the first two, requiring the estimation of two baseline equations for each drug. I estimate six drug use equations, then, to establish initial conditions from which to trace a set of 12 possible drug consumption behavioral states. For arrests, I similarly estimate two equations to establish whether the individual has ever been arrested entering *t*, whether she is arrested during the period, and whether an arrest at *t* is the individual's first ever arrest. Defining initial conditions therefore requires the estimation of eight total equations.

The first set of four initial condition logit equations makes use of survey questions about whether or not the respondent has ever used each of the three drugs under study, or has ever been arrested, as of the baseline survey period. For cigarette use, I estimate ever having smoked as a function of respondent, parental, and peer characteristics based on questions asked at baseline. This estimation establishes initial conditions on which to base later period estimations.[9] I also include exclusion restrictions in the form of the value of the beer tax, cigarette tax, and marijuana policy variables in the state the respondent

[8] Another potential estimation strategy is the use of a hazard model along the lines of Van Ours (2003) or Sen, Agarwal, and Hofler (2002), but the imprecision of drug initiation information makes the use of such a model with the NLSY97 difficult. (Sen, Agarwal, and Hofler use the NLSY97 data with such a model, but they only employ the first year of survey data, inclusive of retrospective information about the age of first use for each drug.) Measures of initiation into consumption of an individual drug are measured at approximately one year intervals, making it impossible to distinguish which drug is first consumed if the first use of two different drugs occurs in the same year (or, in the case of retrospective responses, at the same age). Thus, the use of a hazard model does not seem appropriate for the purposes of this study, which investigates the gateway theory by following individuals over eight survey periods/years and jointly modeling demand for three drugs.

[9] As noted in the literature review above, the effects of parents and peers on youthful substance use and abuse is well established in the substance use literature. See Kandel (1996) and Allen et al. (2003) for reviews.

currently lives in at the time the respondent was 12 years old.[10] The NLSY97 questions used to define respondent, parental, and peer characteristics are either only asked at baseline or only asked again a number of years later. The variables include parental education information, the respondent's combined Armed Services Vocational Aptitude Battery (ASVAB) test score (the ASVAB test is administered as part of the survey), the respondent's religious affiliation, the respondent's answer to how many of his or her peers smoke, drink, and use other (illegal) drugs, and questions about whether or not the respondent has experienced events such as his or her residence being broken into or being involved in a fight at school. While the peer effect variables are potentially endogenous and correlated with later behavior, they provide valuable information underlying baseline substance use. This information is only observed at baseline, so the model makes use of it both here and in the next set of initial condition equations, despite the potential endogeneity problems.[11] Identification flows from the fact that the age 12 price variables are correlated only through their effect on ever having used each of the three drugs or ever having been arrested. Using cigarette consumption as an example of how the first four equations are estimated (the first of 13 total equations), we have

$$ln\left[\frac{Pr\left(d_{i1}^{1}(ever)=1\right)}{Pr(d_{i1}^{1}(ever)=0)}\right] = \gamma_0 + \gamma_1 P_{AGE12} + \gamma_2 X_1 + \mu_1 + \varepsilon_{11},\, d_{i1}^{1}(ever) = 0,1 \qquad (5.5)$$

Equations for any prior alcohol and marijuana consumption and ever having been arrested are estimated similarly. The final four initial condition logit equations estimate baseline (1997) drug use and arrest likelihood as functions of the drug use histories represented by equation 5.5, inclusive of drug use interactions. In addition to respondent characteristics and the baseline characteristics included in

[10] These age twelve measures are not correct for some respondents who are older than 12 during the first survey period and have moved across states since the time they were 12. Because such moves predate the survey period, the survey does not allow a robust check on the following, but I assume that most respondents have either not moved since age 12 or moved within the state, and that those who have moved across state lines have done so randomly, such that results are not biased.

[11] There is evidence that the peer effect variables are not correlated across all equations. As a test, I run separate longitudinal linear probability models (not reported) of cigarette smoking, drinking, and marijuana use for the post-baseline periods. These regressors are the same ones used in the longitudinal drug use equations outlined below (e.g., for marijuana use, the equation is exactly the one estimated as part of the empirical model, and for drinking and cigarette smoking the equations only differ from the actual empirical model in that drug use is defined to be binary). I then separately regress the residuals on each of the baseline peer effect variables to assess the potential endogeneity problem. (There are three separate tests run for each drug, such that in each regression, there is one independent variable and a constant.) For the cigarette smoking residuals, only the coefficient estimate for the variable describing the percentage of peers who smoke cigarettes is statistically significant. For drinking, the peer cigarette and peer drug use coefficients are statistically significant, but the peer drinking coefficient is not. For marijuana, the peer drug use percentage estimate significantly affects the marijuana residuals and the peer smoking coefficient is marginally significant. The peer drinking percentage, however, does not significantly impact the marijuana residuals.

X_1 above, I also include baseline drug price variables as regressors such that these (current) prices are available and serve to distinguish the second four baseline equations with respect to the first four. More endogeneity issues exist from the inclusion of current prices, and since the equations make use of the other initial condition equation outcomes for drug use and arrest entering the initial period. Because of the timing of the information available in the survey with respect to baseline, however, I choose not to ignore this information here since the information about pre-baseline behavior that enters these equations is identified and establishes the variables upon which the simulated outcomes described in the next chapter are based. Including this information in the estimation process allows simulated drug use behavior in the results presented below to be consistent at baseline in terms of any consumption entering baseline versus subsequently measured consumption; not including the information produces inconsistencies in the mix of users and abstainers, as well as in the identification of first-time users. Since baseline drug use is included in the baseline arrest likelihood equation, then, and since all but a handful of respondents are younger than age 18 at baseline, I exclude the cigarette and beer tax variables from the arrest likelihood equation. I do this despite arguments against this particular exclusion restriction in the sense that these variables impact behavior that is illegal for nearly the entire sample at baseline. In the first year of the survey, however, only 6.16 percent of the analysis sample report ever having been arrested, and only 4.30 percent report an arrest during the past year. Moreover, a likelihood ratio test of this exclusion restriction when estimating the equations separately yields a Chi square statistic with a corresponding p-value of 0.8428, indicating that including the tax variables does not substantially improve the fit of the baseline arrest equation itself.[12] In terms of the comparable initial condition equations describing behavior at baseline, then, I again estimate each of the four equations similarly. Continuing to use the cigarette equation as an example, for the fifth initial condition equation we have:

$$ln\left[\frac{Pr\left(d_{i1}^{1}=1\right)}{Pr(d_{i1}^{1}=0)}\right]=\gamma_0+\gamma_1 P_1+\gamma_2 X_1+\gamma_3 S_1+\mu_5+\varepsilon_{51},\, d_{i1}^{1}=0,1 \tag{5.6}$$

In Equation 5.6, S_1consists of indicators for ever having used each drug, interactions for such use, and ever having been arrested entering the baseline period. As stated above, the use of current period prices identify this equation with respect to Equation 5.5 since their only effect on ever having used one of the drugs is through their effect on baseline use. Additionally, this is the only equation (or set of equa-

[12] A test for adding cigarette taxes, but not beer taxes, to the equation produces a test statistic with a p-value of 0.5846. The converse test for adding only beer taxes yields a statistic with a p-value of 0.9695.

tions, since Equation 5.6 is one example of four similarly estimated equations), in which interactions for consumption of the drugs under study at any point in the past appear. Similar to the longitudinal equations described above, the baseline drug use equations are identified in the joint estimation process by the exclusion of tax variables from the arrest likelihood equation.

Thus, many young people enter the research sample with a history of drug use and key variables in the model include lagged consumption stock and arrest information. As a result, behavior during the baseline survey period cannot be modeled, necessitating the estimation of equations defining initial conditions. I estimate eight initial condition equations for drug use and arrests jointly with five longitudinal equations (Equations 5.2 and 5.3 above) describing each period's drug use decisions, resulting arrests, and sample attrition. After establishing baseline conditions from which initiation into a drug or a first arrest can be identified, I estimate equations for each drug, arrests, and attrition as a function of the state variables, a slate of interactions of these state variables, current prices, vectors of individual-specific (X_t) and contextual (Z_t) covariates, and unobservables.

Identification

The empirical model recovers causal effects of prior drug consumption on current drug use as long as the equations are identified statistically. As described above, after establishing the initial conditions with which respondents enter the NLSY97 survey, specific identification for drug use in the model derives from the use of drug price proxies and the exclusion of cigarette tax proxies from the equation describing the legal consequences of drug use. General identification surrounding the inclusion of current drug prices themselves (nonlinearly for alcohol and tobacco) comes from the fact that these prices, while endogenous to the drug use decision, vary geographically as well as over time for the eight survey periods, and the fact that survey respondents also move across states over the study period.[13] I model current drug consumption as a function of current prices, lagged behavior, and interactions of cigarette and alcohol prices with lagged behavior. The model, based on the identification provided by the exclusion restriction in the arrest equation, generates unbiased parameter estimates of the effect of lagged drug consumption behavior on current drug use. Except for the inclusion of variables describing past

[13] Among the 6,269 sample members who complete all eight NLSY surveys, 496, or 7.91 percent, live in a different state in year eight than the one in which they lived at baseline. That establishes a minimum for such movement, since it ignores anyone who moved between states at some point in the interim and moved back before 2004.

drug use, the drug demand equations are reduced form in the sense that the model does not include controls for variables such as schooling, work, or marriage.[14] As a result, any feedback these activities might have on drug use will not be explained. Finally, while the parameter estimates I generate are unbiased, I cannot interpret them in relation to gateway effects since the model is jointly estimated. Instead, and as I now describe, I conduct a series of experiments with which to test gateway theory.

[14] Only three members of the analysis sample are married at baseline. By 2004, 12.47 percent report being married.

Chapter 6

Results

This chapter reports the results of the estimated empirical model outlined above. I analyze how observed behavior changes as a result of various gateway-related policy experiments, comparing a set of baseline outcomes to simulated outcomes resulting from changes in prices, the likelihood that drug consumption occurs, and the probability that drug consumption results in an arrest. The experiments attempt to get unbiased estimates of so-called gateway effects by estimating the joint demand of cigarettes, alcohol, and marijuana as a function of prior consumption of those drugs, controlling for the effects of consequent arrests, current prices, unobservables, and sample attrition. At the end of the chapter, I discuss what the results of the experiments imply with respect to gateway theory.

6.1 Model Metrics

I estimate three drug use equations (one each for cigarettes, alcohol, and marijuana), an equation for arrests, and an equation for sample attrition jointly with eight initial condition equations for drug use (history and baseline consumption) and arrests (history and baseline incidence). I estimate these equations simultaneously in order to incorporate unobservable permanent and time-varying heterogeneity. I detail model fit in Table 6.1 by comparing actual outcomes from the NLSY97 data with the predictions from the model across all seven post-baseline survey periods. The table demonstrates that the model, when updating iteratively over time and integrating out over unobservables, adequately matches actual outcomes as measured over the same yearly metric in which the data are collected. Since the estimation process will tend to smooth point estimates over time, dispersion exists in how well the model fits the data in any particular survey period, particularly when a relatively sharp change occurs in drug use behavior or arrest incidence. Nonetheless, the table demonstrates that the model performs adequately

Table 6.1: Model Fit over All Post-Baseline Periods

Variable (N = 8,300)	Actual Value	Predicted Value
Smoking (Yes/No)	0.3473	0.3485
Smoking (Categorical)		
No Smoking	0.6527	0.6515
Light Smoking	0.1105	0.1111
Heavy Smoking	0.2368	0.2374
Drinking (Yes/No)	0.5261	0.5281
Drinking (Categorical)		
No Drinking	0.4739	0.4719
Light Drinking	0.3225	0.3220
Heavy Drinking	0.2036	0.2062
Marijuana (Yes/No)	0.2370	0.2322
Arrest (Yes/No)	0.0624	0.0619

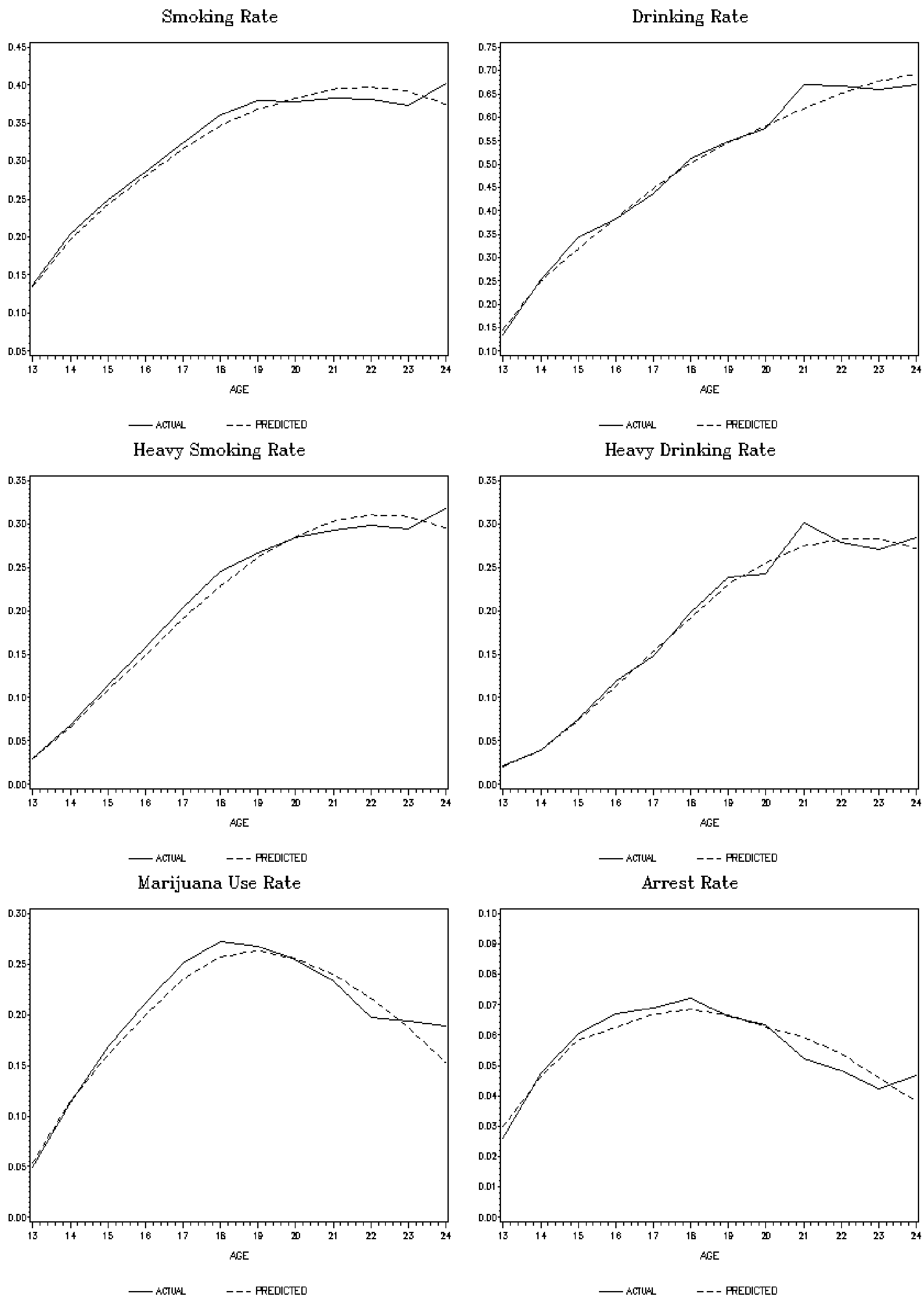
NLSY97 Data: 1997-2004 Survey Rounds.

when looking at predicted outcomes over the entire survey period.

Figure 6.1 shows that the model predictions also adequately match the actual values of the dependent variables when summarized by respondent age, a more appropriate comparison metric than survey period. These results do not imply the modeling process is perfect. For an example in the figure of how the model misses a sharp change in behavior, for instance, see how the point estimates for the drinking variables compare to the jumps in actual drinking behavior at age 21.[1] Also, one might wonder under what conditions a researcher would reject a null hypothesis that the model fits the data well in this type of exercise. The goal in fitting the data, however, is not to conduct such a statistical test, but rather to demonstrate that the results shown in the simulations that follow are based on a model that accounts for unobservable heterogeneity but still accurately predicts the observed behavior in the underlying data. For instance, the largest predicted differential shown in Table 6.1 is 0.48 percentage points for incidence of marijuana use. This difference tranlates to 243 individuals over seven years, or an average prediction error of around 35 respondents per year. The smallest number of actual marijuana consumers in any given sample year is 1,314, in the final year of the data used in this study (2004). The model predicts

[1] I drop respondents aged 12 and 25 from the graphs below; insufficient observations exist at these ages to make accurate predictions.

Figure 6.1: Model Fit by Age



1,311 cannabis users for that period.

Tables 6.2 and 6.3 report the parameter estimates generated by the empirical model for the effects of the price and drug consumption stock variables, along with consumption stock/price interactions, on current drug consumption.[2] Table 6.2 reports results estimating the equations independently, without controlling for unobservable individual-level heterogeneity, treating the endogenous state variables as exogenous, and updating iteratively over time. Table 6.3 shows results from the preferred model, which accounts for endogeneity by integrating out over the unobserved heterogeneity during the iterative updating process. This model controls for unobservables using five permanent and three time-varying mass points, a choice that testing indicates maximizes the likelihood function subject to the condition that the distribution of the resulting estimates remains sufficiently smooth.[3] Standard errors are bootstrapped, and statistical significance is then assessed with a t-test.

The many feedbacks in the model do not allow a direct assessment of gateway effects by examining these parameter estimates, nor do they allow for an interpretation of the meaning of the parameter estimates themselves in terms of the estimated effect of an independent variable on a dependent variable. Given the evidence above that the preferred model does a good job of matching observed behavior from the NLSY97, I investigate gateway effects below by introducing gateway-inspired experiments into the jointly estimated model. I conduct these experiments by imposing the conditions of the experiment and simulating behavior forward to see how results change versus the preferred model that controls for unobservable heterogeneity, as well as in comparison to what would be predicted if unobservables were ignored (i.e., estimating the equations independently). I conduct two types of experiments, one type investigating cost effects in terms of changing prices (taxes) and arrest likelihood. Another type of experiment, mentioned briefly above, hypothesizes what might happen under a hypothetical full enforcement policy that successfully (and completely) enforces a legal prohibition against consumption of a particular drug. The next two sections describe the results of these efforts.

[2] Not all coefficient estimates or equations appear here; full model results appear in Appendices C and D.

[3] To arrive at the numbers of mass points employed here, I estimate each equation separately (i.e., with one permanent and one time-varying mass point). I then add mass points and re-estimate the model. So long as the likelihood function improves measurably and the associated probability weights for all of those mass points are not close to zero, the model with the additional mass point is preferred to the model without it, in the sense that there is a sufficiently large underlying distribution to support the additional mass point and improve the fit of the model. The optimal choice of mass points, then, is the combination that produces the highest likelihood function subject to the condition that any associated probability weight is sufficiently non-zero. All combinations of permanent and time-varying mass points up to and including seven were tested before settling on five and three mass points, respectively.

Table 6.2: Parameter Estimates Controlling for Unobservables

Variable	**Smoking**		**Drinking**		**Marijuana**
Coefficient *(Standard Error)**	Light vs. None	Heavy vs. None	Light vs. None	Heavy vs. None	Any vs. None
Has smoked entering t?	0.37 *(0.10)**	1.51 *(0.09)**	0.12 *(0.04)**	0.44 *(0.06)**	0.41 *(0.06)**
Smoker $(t-1)$	1.06 *(0.38)**	2.83 *(0.33)**	0.18 *(0.28)*	0.32 *(0.30)*	0.55 *(0.34)*
First time smoker $(t-1)$	-0.22 *(0.29)**	-1.42 *(0.22)**	0.21 *(0.23)*	0.12 *(0.32)*	0.02 *(0.12)*
Has drank entering t?	0.28 *(0.08)**	0.24 *(0.08)**	0.31 *(0.04)**	1.01 *(0.09)**	0.62 *(0.06)**
Drinker $(t-1)$	0.22 *(0.31)**	-0.70 *(0.27)**	1.12 *(0.21)**	1.97 *(0.25)**	0.35 *(0.24)*
First time drinker $(t-1)$	-0.09 *(0.13)*	0.16 *(0.11)*	-0.48 *(0.08)**	-1.10 *(0.13)**	-0.26 *(0.09)**
Has smoked pot entering t?	0.10 *(0.12)*	0.51 *(0.09)**	0.01 *(0.06)*	0.23 *(0.08)**	0.73 *(0.06)**
Pot smoker $(t-1)$	0.42 *(0.59)*	-0.32 *(0.44)*	0.56 *(0.35)*	0.78 *(0.46)*	1.69 *(0.40)**
First time pot smoker $(t-1)$	0.11 *(0.46)*	-0.23 *(0.36)*	0.01 *(0.25)*	-0.19 *(0.31)*	-0.53 *(0.11)**
Cigarette Tax	-0.11 *(0.63)*	-1.04 *(0.79)*	0.48 *(0.36)*	1.10 *(0.50)**	0.65 *(0.53)*
Cigarette Tax Squared	-0.29 *(0.59)*	0.80 *(0.60)*	-0.42 *(0.32)*	-0.90 *(0.39)**	-0.68 *(0.52)*
Beer Tax	-0.06 *(0.57)*	-0.80 *(0.70)*	-0.42 *(0.62)*	-0.01 *(0.72)*	0.07 *(0.66)*
Beer Tax Squared	-0.29 *(0.58)*	1.01 *(0.64)*	0.20 *(0.65)*	-0.43 *(0.73)*	-0.52 *(0.66)*
Smoker*Cig Tax	-0.01 *(0.67)*	-0.79 *(0.71)*	0.36 *(0.63)*	0.13 *(0.64)*	-0.19 *(0.45)*
Smoker*Cig Tax Squared	0.54 *(0.71)*	1.16 *(0.58)*	-0.43 *(0.53)*	-0.25 *(0.54)*	0.03 *(0.53)*
Smoker*Beer Tax	0.05 *(0.60)*	-1.46 *(0.58)**	-0.08 *(0.73)*	0.83 *(0.68)*	-0.19 *(0.66)*
Smoker*Beer Tax Squared	-0.67 *(0.70)*	1.88 *(0.71)**	0.12 *(0.79)*	-0.94 *(0.78)*	0.35 *(0.85)*
Drinker*Cig Tax	0.62 *(0.76)*	1.15 *(0.46)**	-0.21 *(0.51)*	-0.66 *(0.45)*	0.12 *(0.55)*
Drinker*Cig Tax Squared	-0.48 *(0.69)*	-1.15 *(0.45)**	0.35 *(0.45)*	0.78 *(0.39)**	-0.18 *(0.51)*
Drinker*Beer Tax	0.46 *(0.70)*	1.31 *(0.54)**	0.57 *(0.63)*	-1.05 *(0.64)*	0.06 *(0.72)*
Drinker*Beer Tax Squared	-0.97 *(0.80)*	-1.23 *(0.68)**	-0.59 *(0.70)*	1.07 *(0.89)*	0.30 *(0.93)*
Pot Smoker*Cig Tax	-0.85 *(0.69)*	0.27 *(0.69)*	-0.15 *(0.72)*	-0.34 *(0.58)*	-0.37 *(0.60)*
Pot Smoker*Cig Tax Squared	0.79 *(0.62)*	-0.40 *(0.63)*	0.29 *(0.65)*	0.45 *(0.52)*	0.63 *(0.60)*
Pot Smoker*Beer Tax	-0.09 *(0.67)*	-0.17 *(0.65)*	-0.04 *(0.77)*	0.22 *(0.75)*	0.23 *(0.77)*
Pot Smoker*Beer Tax Squared	0.95 *(0.71)*	0.08 *(0.76)*	0.26 *(0.69)*	0.24 *(0.88)*	-0.02 *(0.67)*
Legal Medical Marijuana	-0.02 *(0.07)*	-0.41 *(0.08)**	0.08 *(0.04)**	0.02 *(0.06)*	0.19 *(0.06)**
Decriminalized Marijuana	-0.01 *(0.06)*	0.03 *(0.08)*	0.06 *(0.03)**	-0.04 *(0.05)*	0.06 *(0.04)*
Mandatory Minimum Sentence	0.04 *(0.05)*	0.08 *(0.04)*	0.01 *(0.04)*	0.03 *(0.05)*	-0.03 *(0.04)*
Drugged Driving Law	0.04 *(0.08)*	-0.05 *(0.09)*	0.03 *(0.05)*	-0.05 *(0.07)*	-0.03 *(0.08)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Table 6.3: Parameter Estimates Not Controlling for Unobservables

Variable	**Smoking**		**Drinking**		**Marijuana**
Coefficient *(Standard Error)**	Light vs. None	Heavy vs. None	Light vs. None	Heavy vs. None	Any vs. None
Has smoked entering t?	0.35 *(0.05)**	1.77 *(0.10)**	0.15 *(0.05)**	0.61 *(0.09)**	0.33 *(0.05)**
Smoker $(t-1)$	1.38 *(0.19)**	2.68 *(0.18)**	0.22 *(0.13)*	0.64 *(0.20)**	0.40 *(0.14)**
First time smoker $(t-1)$	-0.26 *(0.10)**	-1.13 *(0.11)**	0.28 *(0.08)**	0.22 *(0.11)**	0.15 *(0.06)**
Has drank entering t?	0.38 *(0.07)**	0.77 *(0.09)**	0.44 *(0.05)**	1.33 *(0.10)**	0.73 *(0.06)**
Drinker $(t-1)$	0.06 *(0.13)*	-0.33 *(0.19)*	1.59 *(0.11)**	2.55 *(0.16)**	0.32 *(0.13)**
First time drinker $(t-1)$	-0.02 *(0.07)*	-0.15 *(0.10)*	-0.70 *(0.07)**	-1.23 *(0.10)**	-0.23 *(0.06)**
Has smoked pot entering t?	0.10 *(0.06)*	0.65 *(0.09)**	-0.02 *(0.05)*	0.14 *(0.09)*	0.82 *(0.05)**
Pot smoker $(t-1)$	0.36 *(0.19)*	0.56 *(0.24)**	0.75 *(0.17)**	1.08 *(0.18)**	1.99 *(0.14)**
First time pot smoker $(t-1)$	0.13 *(0.08)*	-0.30 *(0.09)**	-0.01 *(0.08)*	-0.10 *(0.12)*	-0.56 *(0.05)**
Cigarette Tax	-0.29 *(0.33)*	-1.82 *(0.47)**	0.83 *(0.26)**	1.50 *(0.48)**	0.73 *(0.37)**
Cigarette Tax Squared	-0.12 *(0.33)*	1.41 *(0.41)**	-0.66 *(0.23)**	-1.05 *(0.39)**	-0.66 *(0.32)**
Beer Tax	-0.26 *(0.40)*	-1.46 *(0.70)**	-0.41 *(0.45)*	0.31 *(0.58)*	0.19 *(0.42)*
Beer Tax Squared	-0.19 *(0.49)*	1.70 *(0.72)**	0.07 *(0.55)*	-1.42 *(0.64)**	-0.79 *(0.47)*
Smoker*Cig Tax	-0.11 *(0.34)*	-0.32 *(0.51)*	0.51 *(0.35)*	-0.01 *(0.54)*	-0.06 *(0.43)*
Smoker*Cig Tax Squared	0.63 *(0.31)**	0.65 *(0.42)*	-0.69 *(0.32)**	-0.51 *(0.47)*	-0.20 *(0.37)*
Smoker*Beer Tax	-0.38 *(0.53)*	-1.77 *(0.56)**	0.15 *(0.56)*	1.06 *(0.53)**	-0.22 *(0.47)*
Smoker*Beer Tax Squared	1.46 *(0.64)**	2.77 *(0.69)**	-0.25 *(0.65)*	-1.44 *(0.67)**	0.43 *(0.54)*
Drinker*Cig Tax	1.00 *(0.42)**	1.81 *(0.48)**	-0.33 *(0.28)*	-0.77 *(0.47)*	0.14 *(0.42)*
Drinker*Cig Tax Squared	-0.85 *(0.38)**	-1.48 *(0.41)**	0.55 *(0.25)**	1.09 *(0.43)**	0.21 *(0.34)*
Drinker*Beer Tax	1.02 *(0.47)**	2.00 *(0.54)**	0.64 *(0.55)*	-1.20 *(0.69)*	0.12 *(0.48)*
Drinker*Beer Tax Squared	-1.58 *(0.59)**	-1.76 *(0.63)**	-0.74 *(0.67)*	1.30 *(0.84)*	0.30 *(0.60)*
Pot Smoker*Cig Tax	-1.08 *(0.50)**	0.22 *(0.56)*	-0.31 *(0.43)*	-0.36 *(0.56)*	-0.47 *(0.35)*
Pot Smoker*Cig Tax Squared	0.91 *(0.42)**	-0.53 *(0.47)*	0.48 *(0.37)*	0.48 *(0.49)*	0.76 *(0.32)**
Pot Smoker*Beer Tax	-0.06 *(0.73)*	0.06 *(0.62)*	-0.15 *(0.69)*	-0.01 *(0.52)*	0.17 *(0.52)*
Pot Smoker*Beer Tax Squared	0.81 *(0.90)*	-0.44 *(0.69)*	0.49 *(0.77)*	0.79 *(0.70)*	0.05 *(0.62)*
Legal Medical Marijuana	-0.11 *(0.06)*	-0.67 *(0.09)**	0.10 *(0.04)**	0.08 *(0.07)*	0.25 *(0.04)**
Decriminalized Marijuana	0.04 *(0.05)*	0.17 *(0.08)**	0.07 *(0.04)*	-0.05 *(0.07)*	0.07 *(0.03)**
Mandatory Minimum Sentence	0.05 *(0.04)*	0.09 *(0.06)*	0.03 *(0.03)*	0.06 *(0.06)*	-0.04 *(0.04)*
Drugged Driving Law	0.02 *(0.06)*	-0.04 *(0.09)*	0.04 *(0.04)*	-0.06 *(0.08)*	-0.04 *(0.05)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

6.2 Full Enforcement Effects on Current Use

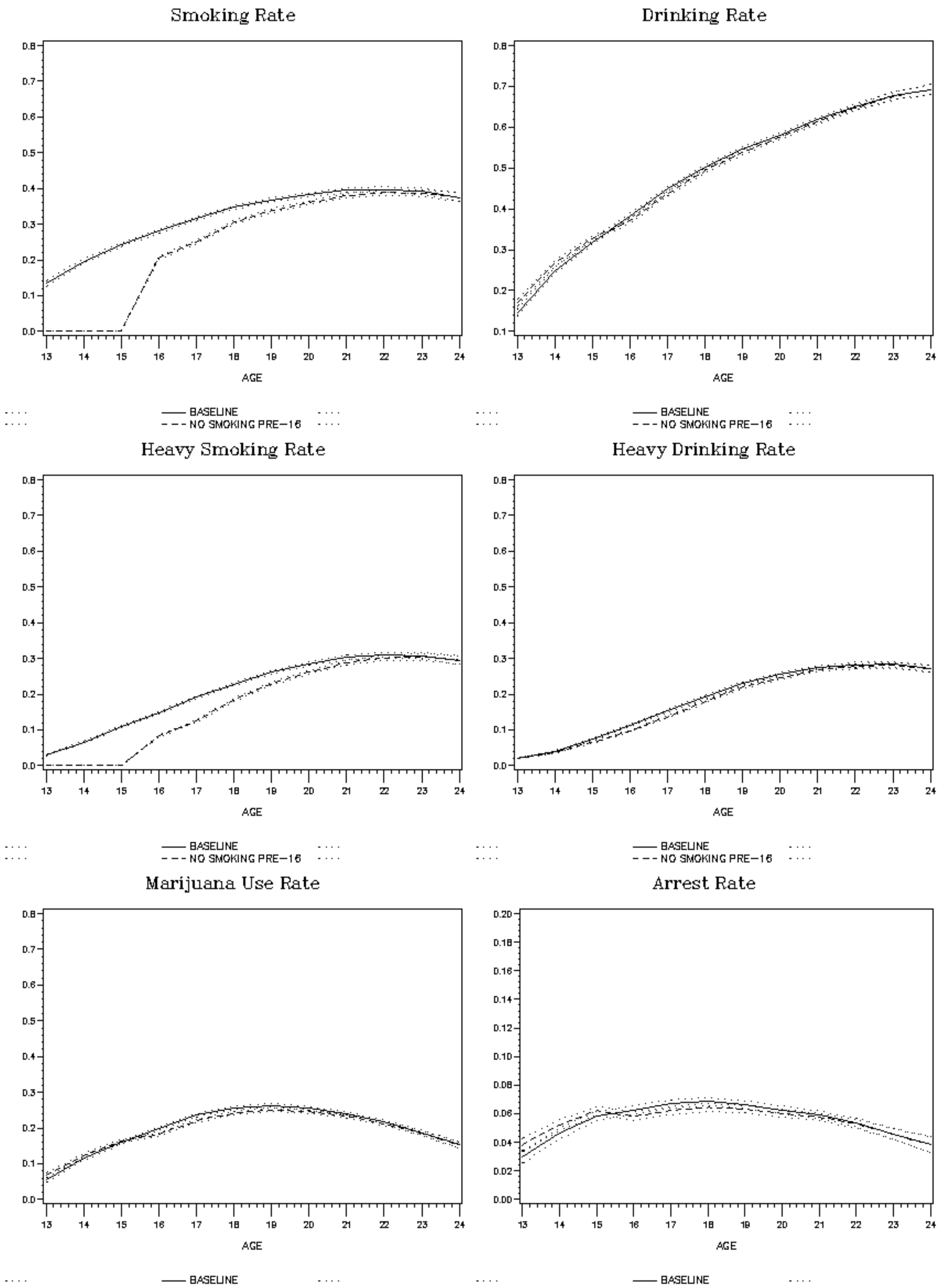
The most basic policy prescription relating to the gateway effect is that if one can prevent initiation into the gateway drugs, subsequent consumption of harder drugs becomes less likely. While acknowledging that the implementation of a policy completely preventing use of any particular drug is not feasible, the dynamic model I estimate allows me to impose a such condition and simulate resulting respondent behavior. This experiment provides a clean test for gateway effects by imposing a condition whereby no one consumes a particular drug before reaching a certain age. Though not feasible in the real world, these experimental simulations allow me to predict how successful enforcement of actual tobacco and alcohol policies might impact marijuana consumption among youth and young adults.

I present the results of these simulations below,[4] illustrating point estimates and 95 percent confidence intervals that compare outcomes under the new, simulated policy scenario with the estimated outcomes from the preferred model discussed in the previous section. This preferred model is the jointly estimated model incorporating five permanent and three time-varying points of support to account for unobservable heterogeneity. For the remainder of this chapter, I refer to this model as a baseline model, against which I depict experimental outcomes. The first two simulations impose conditions on the model such that no respondent smokes cigarettes until the ages of 16 and 18, respectively.

Figure 6.2 demonstrates predicted effects on respondent outcomes under a theoretical scenario where a full enforcement policy successfully eliminates all cigarette smoking prior to the age of 16. Results shown in the figure predict that such a policy delays the onset of smoking behavior but does not ultimately succeed in reducing use of any of the three drugs under study. Though small in magnitude, I forecast statistically significant reductions in heavy drinking, marijuana use, and arrests over the middle of the age distribution, but by the time respondents reach age 22, experimental outcomes converge back to those observed in the baseline model. Predictions also indicate that cigarette smoking rates fall dramatically over much of the age distribution, but that forecasts of smoking and heavy smoking incidence are not statistically different from the baseline case for the oldest respondents in the sample. My forecast of a 29.56 percent heavy smoking rate remains somewhat higher, however, than the predicted rate of 28.20 percent obtained when conducting this experiment by estimating the equations independently

[4] Scaling for all figures below is uniform with the exception of arrests, which I scale differently because of the rarity of arrest events in comparison to observations of drug consumption.

Figure 6.2: No One Smokes Before Age 16



and ignoring unobservables (results not shown in the figure). A difference in the predicted declines for rates of heavy drinking, marijuana use, and arrests also exists between these alternative estimation procedures. The age of 16, however, is not far removed from the actual first use of tobacco for most of these respondents, since the median age of initiation into cigarettes for the sample is 15 (the average age is 14.9). What about a scenario where a policy successfully prevents tobacco use prior to the age at which such consumption becomes legal in the U.S. - the age of 18?

Figure 6.3 shows that a full enforcement policy with respect to cigarette consumption before the age of 18 is predicted to be marginally more successful in discouraging both cigarette and marijuana smoking by individuals in their twenties than the previous such policy. The forecast of eventual convergenge to the baseline predictions remains, but estimated reductions in age 24 rates of cigarette smoking incidence (from 37.50 to 35.87 percent) and heavy cigarette smoking (from 29.56 to 28.20 percent) are statistically significant. I also forecast a reduction in the rate of marijuana consumption at age 24 (from 15.26 to 14.63 percent), but the reduction is not statistically significant. The predicted rate of heavy alcohol use at age 24 declines from 27.14 to 26.15 percent, and these forecasts each lie near the boundaries of the 95 percent confidence intervals for the comparable point estimates at that age. Ignoring unobservable heterogeneity completely, however, predicted heavy drinking and marijuana consumption rates (25.22 percent and 13.10 percent, respectively) from this experiment lie over a percentage point lower than those forecast by the baseline model.[5] I now turn to analyzing hypothetical full enforcement effects from the prevention of youthful drinking and marijuana smoking habits.

Figure 6.4 illustrates predicted outcomes after the imposition of a full enforcement policy that successfully stops young people from drinking until the age of 21, the minimum legal drinking age in the U.S. After experimentally imposing this policy, I forecast that age 24 marijuana smoking rates decrease from 15.26 to 11.70 percent, versus 10.95 percent when ignoring unobservables, and this reduction is statistically significant. Thus, the experiment predicts that completely preventing underage drinking reduces subsequent rates of marijuana use. Plus, forecasted rates of marijuana use never undergo the substantial increase from ages 13 to 19 predicted under the baseline scenario and observed in the actual data. With this particular experiment, predicted marijuana consumption rates top out at 15.57 percent at

[5] The influence of unobservables on substance use behavior can be seen more dramatically when looking at predicted cigarette smoking rates from this experiment, which drop to 28.15 percent for smoking incidence and 19.43 percent for heavier use when I estimate the equations in the model independently, versus 35.87 and 28.20 percent, respectively, when I estimate the equations jointly.

Figure 6.3: No One Smokes Before Age 18

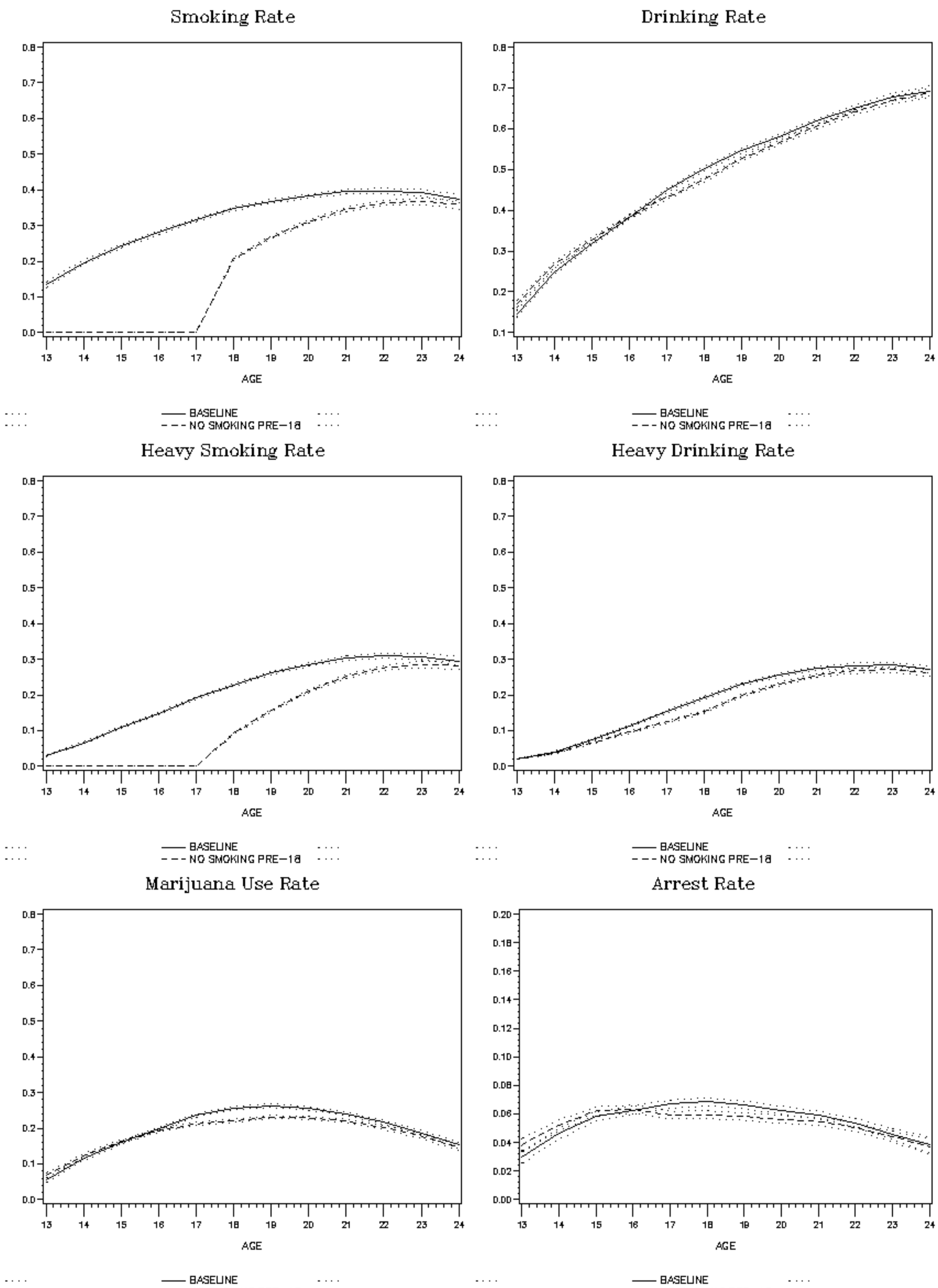
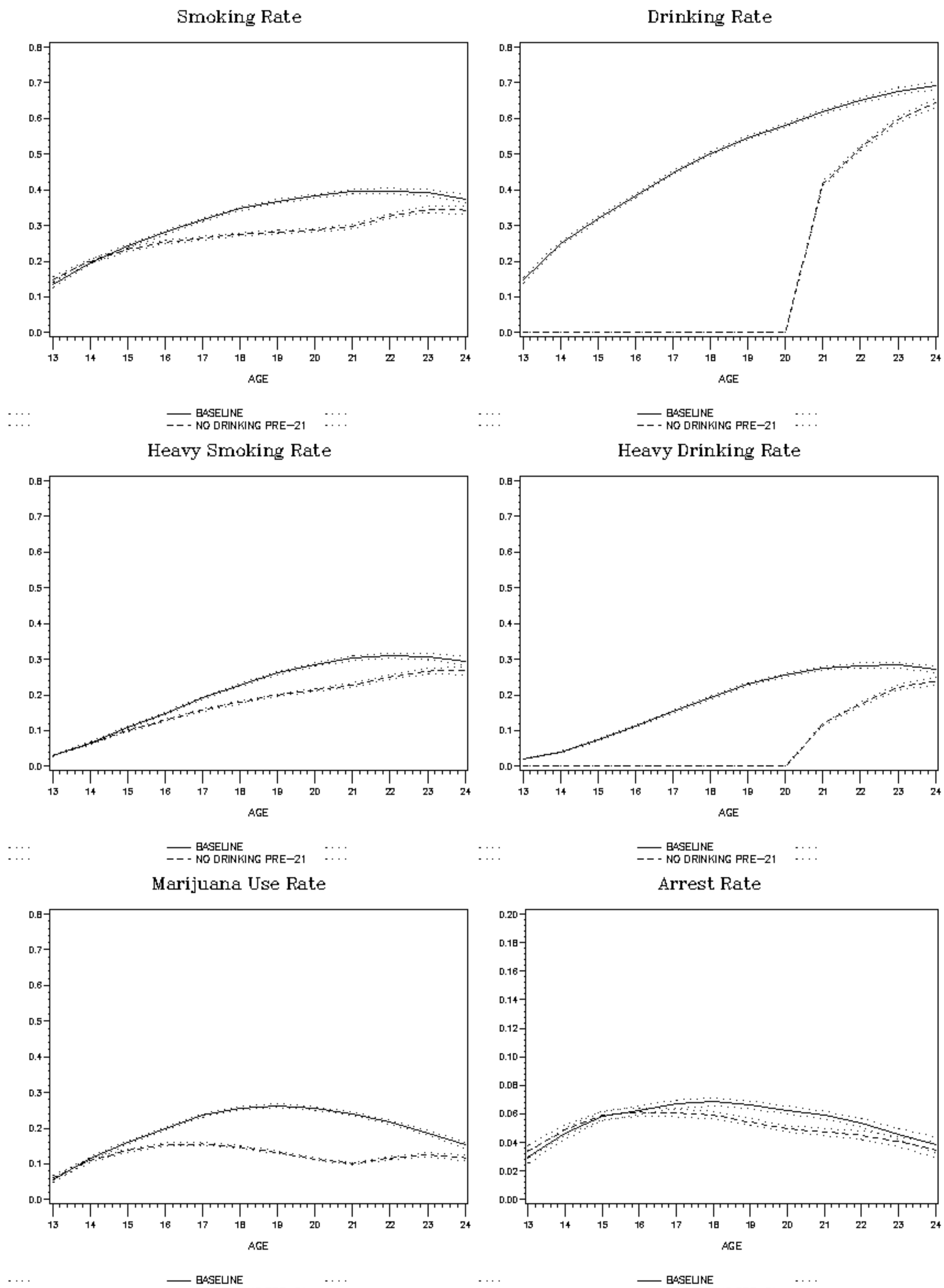


Figure 6.4: No One Drinks Before Age 21



age 17; otherwise, the maximum rate is 26.35 percent at age 19. Predicted age 24 cigarette smoking rates also lessen significantly under this experiment versus the baseline model, from 37.50 to 34.33 percent for any smoking and from 29.56 to 26.77 percent for heavy smoking. Since no one initiates alcohol consumption until the age of 21 in this scenario, age 24 drinking rates understandably decrease as well (by 4.88 percentage points for drinking and by 3.34 percentage points for heavy drinking), though these reductions remain smaller in magnitude than the predicted declines of 7.94 and 7.24 percentage points, respectively, from a model ignoring unobservables. I now turn to a full enforcement policy with respect to marijuana consumption before the age of 21.

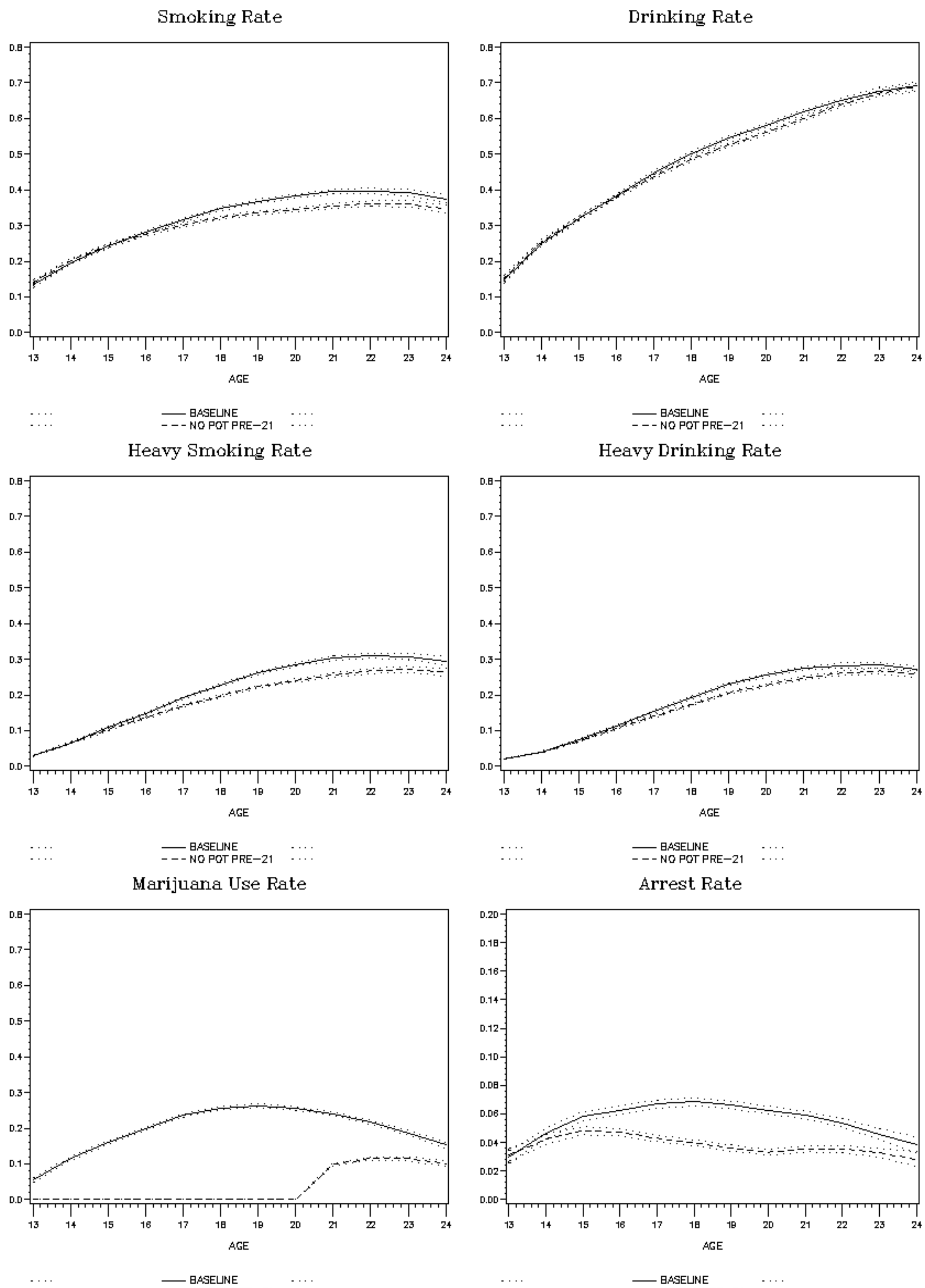
Figure 6.5 shows that a policy preventing all marijuana consumption before the age of 21 significantly reduces predicted rates of cigarette smoking incidence, heavy smoking, and heavy drinking. I forecast drops in age 24 heavy smoking rates (from 29.56 to 26.34 percent) and heavy drinking rates (from 27.14 to 25.88 percent) that are both large and statistically meaningful.[6] Given the imposition of zero consumption prior to the age of 21, predicted marijuana consumption also declines significantly, to 10.12 percent. Similar to the results from the scenarios presented above, this predicted reduction is smaller in magnitude than the predicted reduction (the forecast is a 9.43 percent use rate) when estimating the equations in the model independently. Thus, these and the previous results presented in this section provide clear evidence that unobservable heterogeneity plays a role in explaining observed gateway effects. In the next section, I turn to policy experiments related to drug prices and arrests in order to further investigate this finding.

6.3 Price and Arrest Effects on Current Use

The next set of gateway-related policy experiments examines the effects of changes in the costs of drug consumption in the form of excise taxes on cigarettes and beer, the legality of marijuana consumption for medical purposes, and the likelihood that illegal drug consumption results in an arrest. These simulations are more realistic than the scenarios presented above, since policymakers have some control over these types of mechanisms. The first three experiments investigating taxes and the legalization of

[6] Predicted rates for cigarette smoking remain much higher than predictions from a model ignoring unobservables, which predicts a reduction in heavy smoking rates to 21.13 percent; heavy drinking is predicted to be 24.28 percent when ignoring unobservables.

Figure 6.5: No One Smokes Marijuana Before Age 21



medical marijuana are similar to how some prior studies of gateway theory approach the subject.[7] The final simulation exercise examines differences in the likelihood a drug user who is committing a crime by using a particular drug is arrested.

Figure 6.6 depicts predicted outcomes from a doubling cigarette taxes in the jointly estimated model.[8] Higher cigarette taxes have minimal effects on predicted drinking incidence and heavy drinking rates. I forecast that drinking incidence decreases slightly. At the end of the age range, the estimated decline becomes marginally statistically significant. Estimates of heavy drinking increase slightly after age 18, though the point estimates are not statistically different from one another. Predicted marijuana use is slightly higher with the imposition of the tax increase, but only barely. Predicted own price effects are also minimal but not in the expected direction since estimated cigarette smoking rates increase slightly under this tax increase scenario. Thus, while the experiment provides some evidence that higher cigarette taxes discourage drinking incidence, the forecasted impacts on heavy drinking and, especially, marijuana consumption, are slightly positive but small.

Figure 6.7 shows the results forecast from a doubling of beer taxes across all time periods, a potential policy to prevent progression into marijuana use under the assumption that drinking is a gateway to marijuana (LaChance, 1988).[9] A doubling of beer taxes slightly decreases the predicted rate of age 24 marijuana use, which is 15.26 percent under the baseline case and 15.05 percent after the theoretical tax increase. Ignoring unobservables, however, the predicted consumption rate increases to 15.55 percent. Also, while use rates decline slightly, but not statistically significantly, over the middle of the age range, these declines disappear after age 22. Forecasts for cigarette smoking are largely unaffected. Forecasted heavy smoking rates are slightly higher but lie inside the baseline case confidence interval.[10] Finally, impacts on forecasts of arrests in previous simulations have not been yet discussed because the earlier

[7] See Thies and Register (1993) and Pacula (1998b) for examples.

[8] The average state-level cigarette tax per pack of 20 cigarettes across all time periods for respondents in the survey is \$0.49, so doubling the tax would take the average to \$0.98 a pack. The median tax per pack is lower (\$0.41), such that doubling the tax would increase the median to \$0.82 per pack.

[9] The average state-level beer tax per case of 24 cans or bottles across all time periods for respondents in the survey is \$0.28. Doubling the tax would take the average to \$0.56 a case. The median tax per case is lower (\$0.21), such that doubling the tax would increase the median to \$0.42 per case.

[10] Unlike the previous experiment, the predicted own price effects of higher beer taxes are in the expected direction, particularly for heavy drinking. Similar to the results for cigarette taxes, ignoring unobservables results in higher predicted drinking rates under this experiment than when unobservables are accounted for in the model, again indicating a relationship between unobservable factors and the price elasticity of drug demand.

Figure 6.6: Cigarette Tax Doubled

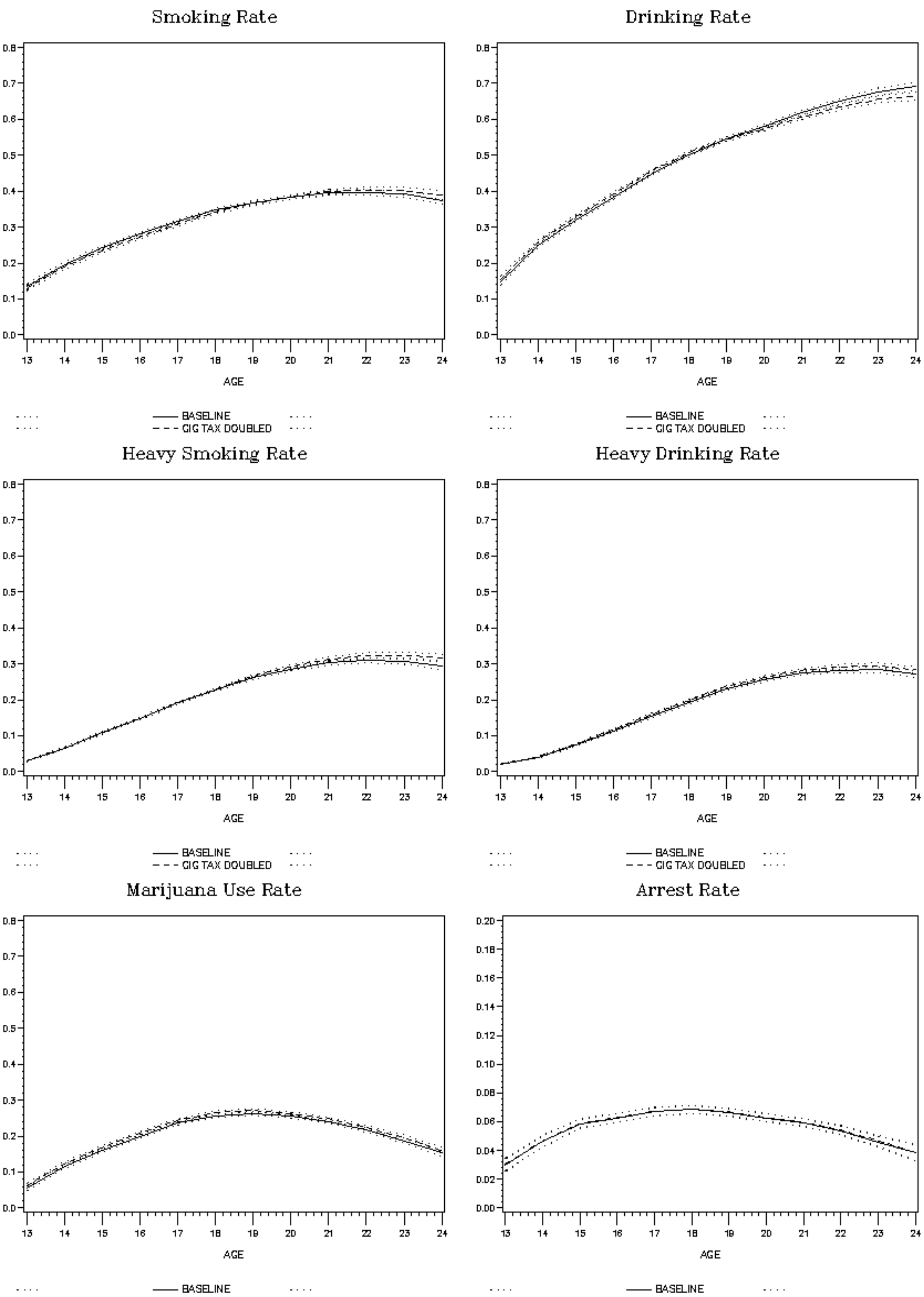
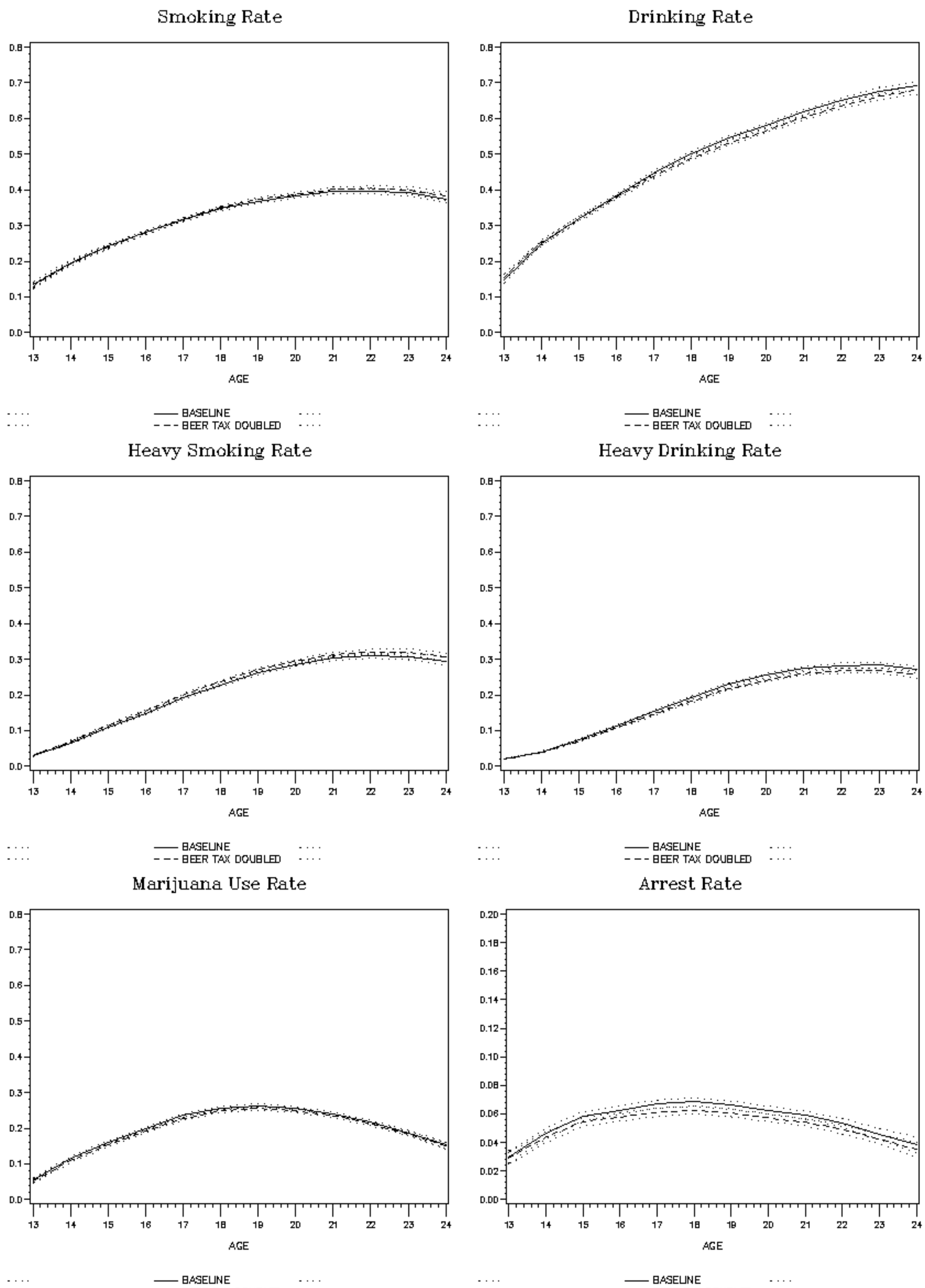


Figure 6.7: Beer Tax Doubled



experiments either explicitly change a legally prohibited behavior or, with higher cigarette taxes, do not produce significant impacts. Here, I predict that a doubling of beer taxes produces statistically significant reductions in arrest rates until age 23. This result is consistent with evidence that alcohol is a contributing factor for many arrests not directly tied to alcohol consumption (Greenfield, 1998).

The next experiment I conduct is timely, given ongoing debates about marijuana legalization and a recent increase in the number of states legalizing marijuana for medical and even recreational uses. Without price data for marijuana with which to conduct a simulation similar to the previous two experiments, this simulation makes use of the variable for whether or not a state has legalized medical marijuana. I examine how the model predicts behavior to change if marijuana is legalized for medical use in all states (i.e., lowering the price of marijuana), versus the counterfactual case where medical marijuana is illegal in all states.[11] Figure 6.8 presents the results of this experiment. The predictions indicate that legalized medical marijuana results in statistically significant reductions in age 24 smoking rates (from 38.43 to 33.72 percent for smoking incidence and from 30.69 to 25.25 percent for heavy smoking). These reductions are substantially overstated ignoring unobservables, with results from a model estimating the equations independently predicting cigarette use rates of 30.56 percent and heavy smoking rates of 20.89 percent. Heavy drinking rates remain almost entirely unchanged; age 24 drinking incidence increases by 1.88 percentage points. Predicted marijuana consumption rates increase by a statistically meaningful 3.37 percentage points at age 24 under this simulation exercise (ignoring unobservables, the predicted rate is 3.43 percentage points higher). While such an increase is not trivial, these results potentially indicate that legalization of marijuana for medical purposes might not lead to skyrocketing marijuana consumption rates, and are consistent with the findings of a recent study of use rates in states where medical marijuana has been legalized (Harper, Strumpf, and Kaufman, 2012).

My concluding experiment examines differential arrest likelihoods with respect to illegal drug consumption. Figure 6.9 depicts a hypothetical test of how drug consumption responds when no one is arrested for any reason versus the case where any illegal drug use results in an arrest.[12] This scenario

[11] While one might presume that the use of medical marijuana is highly regulated, since possession requires a prescription and thereby makes it theoretically difficult to obtain for recreational use through state-regulated programs, the experience of states that have legalized medical marijuana suggests that such laws tend to make marijuana more accessible generally (Salomonsen-Sautel et al., 2012).

[12] In practical terms, because of how arrests are defined in the model, defining all drug use to be legal simply means that no one is ever arrested for any crime. This definition does not mean that cigarettes and alcohol become illicit drugs, but that any underage consumption of those goods results in an arrest.

Figure 6.8: Medical Marijuana - Legal Versus Illegal

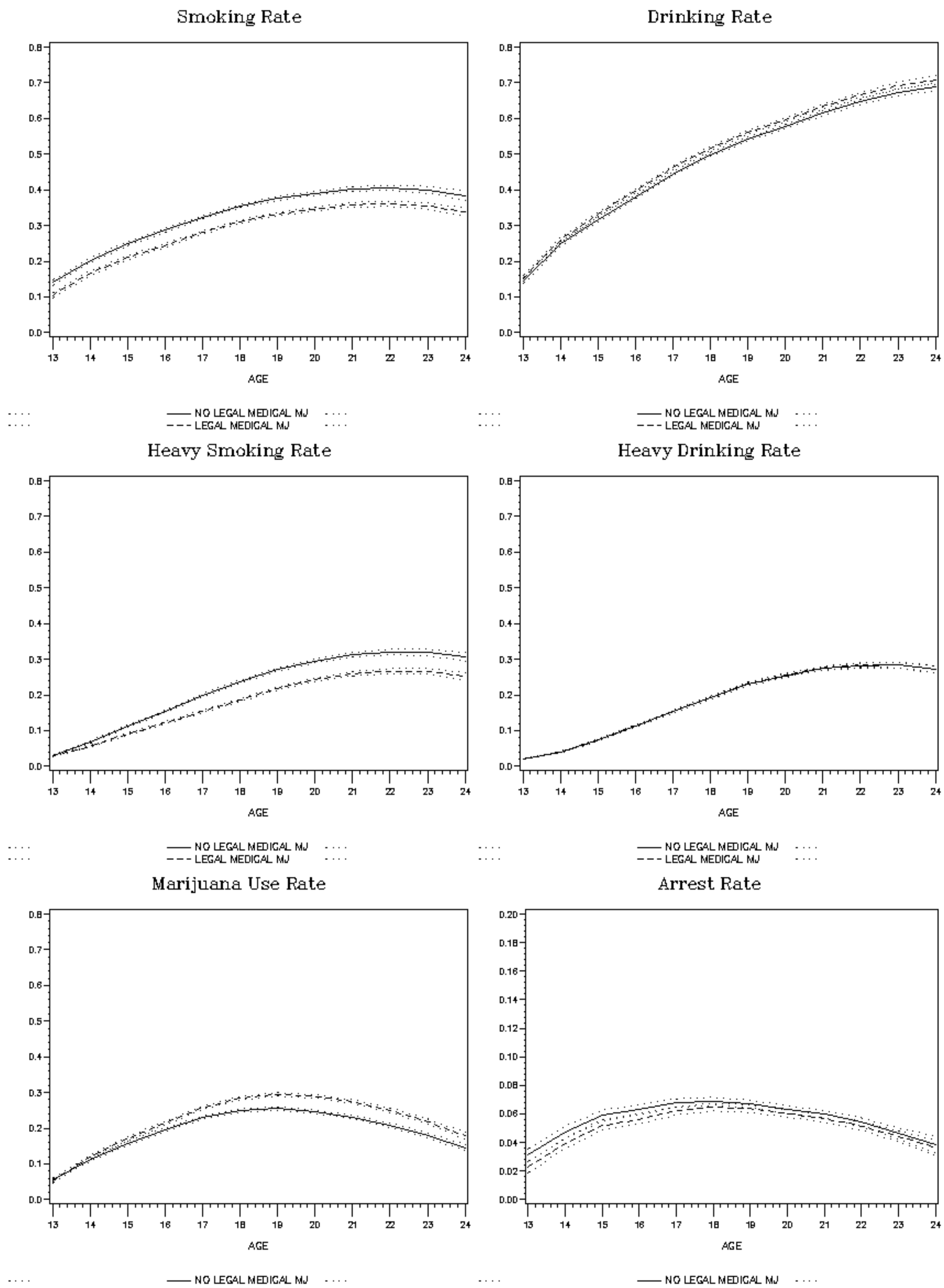
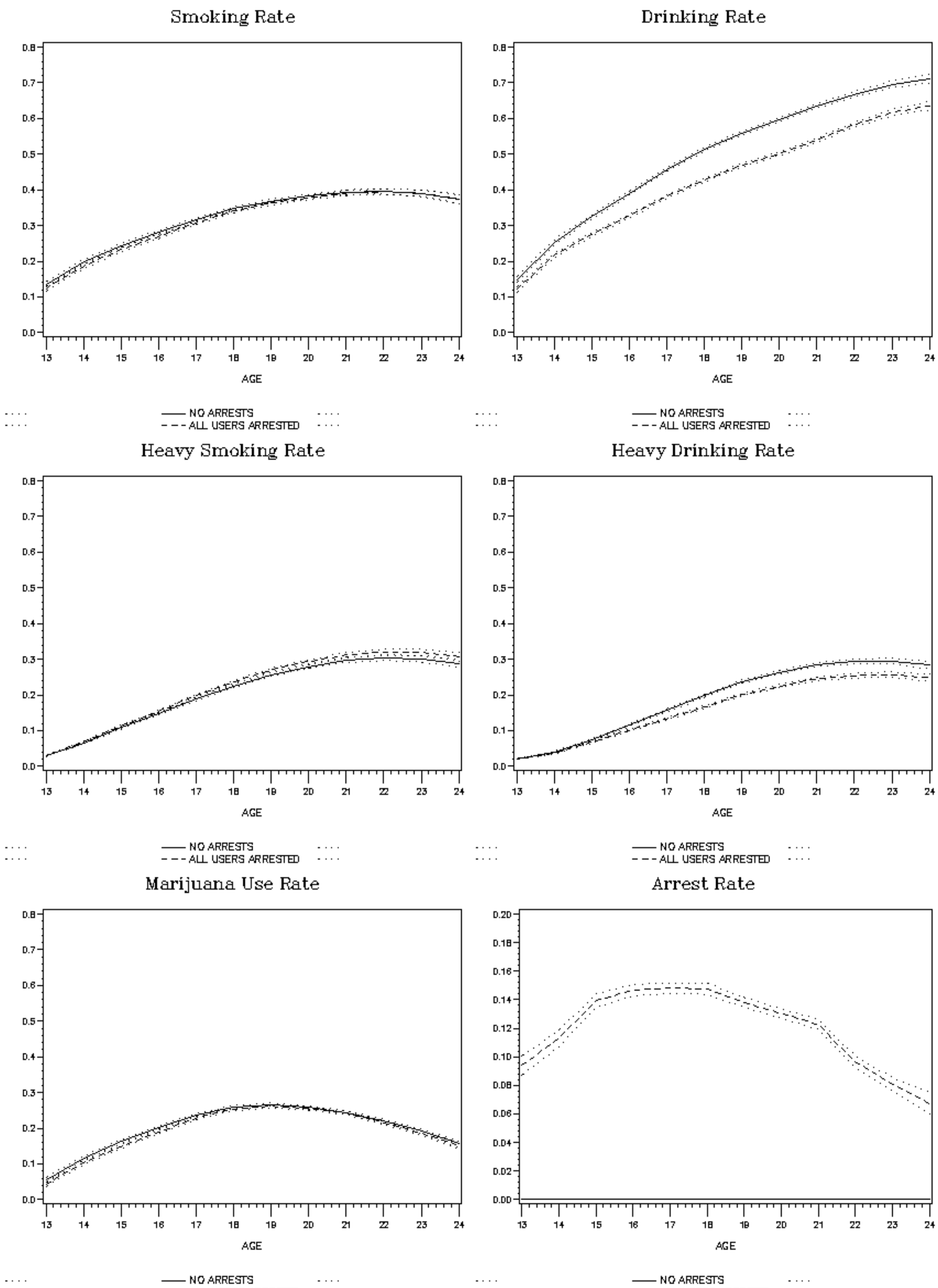


Figure 6.9: All Illegal Drug Users Arrested Versus No Arrests



presents another useful thought experiment that, while not realistic, potentially offers insight regarding the presence of gateway effects. Estimates of drinking incidence at age 24 decrease significantly, from 71.15 to 63.62 percent, as do forecasts for heavy drinking (from 28.30 to 24.78 percent).[13] Predicted age 24 marijuana use declines slightly, from 15.60 percent to 15.19 percent. Ignoring unobservables, however, the predicted marijuana use rate jumps to 18.30 percent at age 24, and the predicted heavy drinking rate increases to 29.80 percent, again demonstrating the importance of controlling for latent factors.[14]

6.4 Discussion

Above, I conduct a number of simulations with the potential to offer insights about whether tobacco and alcohol consumption lead to subsequent marijuana consumption. I describe these results, in relation to gateway theory, under a standard assumption based on average drug initiation ages that there exists a natural ordering to drug use progression.[15] Based on predicted outcomes from the experiments imposed, the full enforcement experiments provide consistent evidence that tobacco and alcohol serve as gateways into marijuana use. The exercises also demonstrate that unobservables matter in drug consumption decisions. Controlling for unobservables significantly lessens the magnitude of most of the predicted gateway effects shown, though evidence for a gateway from alcohol to marijuana is robust.

As shown in Figure 6.2, the hypothetical outcomes from imposing the condition that no one smokes a cigarette before the age of 16 predict that preventing younger adolescents from smoking is ultimately

[13] One concern might be that most of the arrests in the NLSY97 data are drinking offenses. While alcohol was undoubtedly a contributing factor for many of the arrests recorded, other charges underlie most of the arrests. For instance, in the 2000 survey, respondents indicate that charges were filed for 359 of the 551 reported arrests, and some arrests involve multiple charges. Respondents were charged with 69 major traffic offenses, and non-specified offenses account for 71 charges. Thus, most of the charged offenses do not seem to be drunken driving charges, which I cannot directly identify in the data since there is no further delineation for a driving-related charge beyond "major traffic offense." Some of the non-charged offenses probably relate to drinking, perhaps even directly, but it seems unlikely that many minor drinking offenses result in actual arrests, and in any case I cannot identify such arrests in the data. According to Puzzanchera (2009), only 6.97 percent of juvenile arrests in 2008 were for alcohol offenses not related to drunken driving, while drunken driving accounted for less than one percent of those arrests.

[14] Predicted smoking rates for incidence are basically unchanged at around 37.5 percent, and heavy smoking slightly increases from 28.80 to 30.69 percent. If unobservable heterogeneity is ignored, however, these predictions differ more significantly - estimates when all illegal drug consumption results in an arrest are 44.27 percent for incidence and 34.40 percent for heavier smoking.

[15] In the NLSY97 data employed here, for instance, the average age of first drug use, for those who report using the respective drug, is 14.9 for cigarettes, 15.3 for alcohol, and 16.4 for marijuana. The ordering of these estimates is consistent with what is described in the relevant literature.

not successful in cutting rates of alcohol and marijuana use after individuals reach their early twenties. Conversely, forecasting the effects of universal abstention from cigarettes before 16 when estimating the model equations independently - ignoring the role of latent heterogeneity in drug use decisions - yields forecasts of slightly reduced drinking. The heavy drinking rate is predicted to be 1.14 percentage points lower when not controlling for unobservables but is unchanged when adding those controls, and the rate of marijuana smoking is unchanged when controlling for latent factors but half of a percentage point lower when not. The experimental results shown in Figure 6.3 come from a repetition of the previous exercise that imposes abstention from cigarette consumption until the age of 18, when such consumption currently becomes legal in the U.S. Under this scenario, I predict that the proportion of heavy drinkers decreases by nearly two percentage points when estimating the equations in the model independently, and forecast that the proportion of marijuana users declines by over two percentage points. Jointly estimating the model cuts these predicted consumption rate declines by half in the case of heavy drinking and by around 75 percent for marijuana. Both of these experiments provide some evidence supporting gateways from cigarettes to alcohol and marijuana, but a weaker one than the estimated gateway effect obtained from a model ignoring unobservables. As such, these exercises do not provide convincing evidence supporting cigarettes as a gateway drug. Controlling for unobservables in this context causes small predicted gateway effects to nearly disappear.

The predicted outcomes shown in Figure 6.4, where I dictate that no alcohol consumption occurs before age 21, the legal drinking age in the U.S., provide the strongest evidence among these experiments in support of a gateway from alcohol to marijuana. Versus the baseline model, the predicted rate of marijuana use declines by 3.57 percentage points when underage drinking is theoretically eliminated. Controlling for unobservables weakens the strength of this predicted gateway effect, but only by 17 percent (instead of the 3.57 percentage point decrease, the forecast for the rate of marijuana consumption falls by 4.32 percentage points when ignoring latent variables). Thus, while a joint estimation process controlling for unobservables reduces the predicted gateway impacts, these results demonstrate the essence of a gateway effect. More evidence of complementarity between alcohol and marijuana demand appears in Figure 6.5, which shows a final full enforcement experiment, the prevention of all marijuana use before the age of 21.[16]

[16] Predicted rates of heavy drinking fall by 1.26 percentage points, though this decrease is less than half of the reduction predicted when ignoring unobservable factors.

The second set of experiments I conduct examines various costs and potential costs associated with drug consumption in the context of the empirical model. I impose higher excise taxes on cigarettes and beer, compare differences in predicted outcomes when medical marijuana is legal in all states versus when it is not legal in any state, and examine the effects on drug use of differential arrest policies with respect to drug consumption. The first two exercises impose higher excise taxes. As noted previously, a logical construct of gateway theory is the idea that preventing gateway drug consumption correspondingly discourages subsequent harder drug consumption. If cigarettes serve as a gateway into alcohol and marijuana use, then, increases in cigarette taxes should discourage the consumption of all three drugs; similarly, increases in alcohol taxes should discourage marijuana use if alcohol serves as a gateway into cannabis.

Figure 6.6 describes the estimates obtained when imposing a doubling of cigarette taxes on the model. Predicted rates shown for all drug consumption except alcohol incidence increase, indicating some extent of complementarity in demand between these drugs, but it is difficult to know what to make of these findings beyond that. Turning to the imposition of a doubling of the beer tax in Figure 6.7, results do not provide much support for gateway effects. Forecasts for age 24 marijuana consumption decline, but barely (by only 0.21 percentage points; ignoring unobservables, the predicted rate increases by 0.29 percentage points). Both of the tax experiments however, yield more evidence for the importance of unobservables. Predicted heavy drinking rates barely fall (by 0.22 percentage points) when beer taxes increase and the equations in the model are estimated independently, but fall by 1.37 percentage points when estimating the equations jointly.[17]

Figure 6.6 also demonstrates little evidence of predicted own price effects for cigarette taxes on cigarette consumption. Models without the controls for interactions of current taxes and prior period drug use (results not reported) produce similar results (i.e., that higher cigarette taxes have little impact on predicted cigarette consumption). The graphs may simply indicate that these taxes matter less as respondents get older and cigarette consumption habits become ingrained. When summarized by survey year, for instance, estimates of both smoking incidence and heavy smoking rates are lower under the increased tax scenario until the seventh survey period, when the average age of those surveyed is 20.85 and the median age is 21. Interestingly, estimating the model without controls for unobservable

[17] Similarly, I forecast that heavy smoking rates increase by 3.13 percentage points in the cigarette tax experiment when ignoring unobservables, but only increase by 1.25 percentage points when controlling for factors hidden to the researcher.

heterogeneity predicts even higher cigarette smoking rates (by a little more than a percentage point each for both incidence and heavy smoking), indicating that unobservable factors matter and seem to be important determinants of how both own and cross prices affect drug demand.

In Figure 6.8, the imposition of universally legal medical marijuana in the U.S. is predicted to increase marijuana consumption rates by 2.7 to 2.8 percentage points versus both the counterfactual case where medical marijuana use is illegal in all 50 states (shown in Figure 6.8) and the baseline results (not shown). Estimated heavy drinking rates are unchanged in the legal versus illegal exercise, but decline by nearly a percentage point in comparison to the baseline results. Predicted smoking rates for both incidence and heavy smoking decline against both the baseline metric and the scenario completely banning medical marijuana. Unlike the earlier marijuana experiment, these predictions do not support complementarity in the use of these three drugs.

Figure 6.9 reflects predictions from a comparison of experimental scenarios where all drug use is either universally legal (i.e., less costly) or always results in an arrest when consumption is illegal (i.e., is more costly). The drinking results of this experiment, though smaller in magnitude, resemble the predicted outcomes from the beer tax experiment as discussed in the previous section, again potentially demonstrating the well-documented link between alcohol and many types of crime (Greenfield, 1998). Above, when drinking becomes more expensive, predicted heavy alcohol consumpion rates decrease and forecasted arrests do the same; here, when (illegal) drug use leads to an arrest (i.e., drug use becomes more expensive), predicted alcohol consumption similarly decreases. Estimated heavy smoking rates do the opposite, increasing when the cost of drug use increases. The most interesting result of this experiment, however, is that when all illegal drug consumption results in an arrest, predicted rates of age 24 marijuana use decline by half a percentage point when controlling for unobservable factors, but are unexpectedly predicted to increase in the absence of such controls.

To summarize, the first set of full enforcement experiments provide evidence consistent with prior efforts to examine gateway effects while controlling for unobservables. Simulated outcomes based on imposed conditions provide evidence that cigarettes and especially alcohol serve as gateways into marijuana use, but the results also show that ignoring unobservable heterogeneity overstates the magnitude of these predicted effects. In the case of cigarettes, predicted gateway effects are overstated substantially. The latter set of experiments that investigates the costs of drug consumption is less supportive of gateway effects. While these experiments are more feasible than the full enforcement experiments, they imperfectly measure costs in comparison to actual price data. Without such data, then, it seems appropriate to place less weight on the latter results and instead emphasize the evidence presented by the full enforcement experiments, which are not feasible but provide a cleaner test for gateway effects. These predicted results consistently support a gateway effect, but a lessened one in comparison to results that do not account for unobservable heterogeneity.

Chapter 7

Conclusion

The results presented above support the idea that alcohol consumption serves as a gateway into marijuana use, a previously studied potential path for such a gateway in the economics literature. When conducting experiments that impose conditions designed to test the gateway theory, the introduction of controls for unobservable heterogeneity reduces the gateway effects predicted by a model estimated without such controls. Predictions with the model equations estimated independently (ignoring unobservable heterogeneity) indicate that cigarettes and alcohol lead to subsequent marijuana consumption. While the jointly estimated model that controls for unobservables produces the same predictions, the magnitude of the predicted gateway effects decrease, and in the case of cigarettes decrease significantly. The methodology underlying the controls for unobservables makes a significant contribution to the existing literature, and the results are consistent with prior work in the economics literature that implements controls for latent heterogeneity between individual drug consumers. While the research methodology does not allow me to assess the avenues through which either gateways or unobservables operate, one possibility is that the results support a viewpoint summarized by Kleiman (1998) in the sense that they do not support the idea of a "strong" gateway effect, where use of a gateway drug causes a physiological change in the user that leads to consumption of a harder drug. Instead, they perhaps support a more plausible idea of causation where the use of a gateway drug opens up various avenues for the subsequent use of other drugs - avenues that remain unknown to an individual who has never used the gateway drug.

Though this research outlines a structural model, the empirical model is reduced form. Nonetheless, the work differs from existing gateway research by treating past behavior as being endogenous to current consumption and modeling it. I also examine the role of unobservables. The results clearly demonstrate that unobservables play a role, though they do not allow me to comment on the degree to which particular latent factors affect drug consumption. Despite using respondent assessments of peer drug use as controls in equations establishing the model's initial conditions, for instance, the many feedback effects in the jointly estimated system of equations do not allow for an interpretation of the corresponding parameter estimates. Additionally, I cannot comment on the effects of events such as a respondent breaking up with a boyfriend/girlfriend or dealing with a parental divorce. Instead, the model allows such occurrences to influence future behavior through the treatment of unobservables. The impacts of peers, parents, and other latent influences is subsumed into the overall impact of hidden factors in gateway-related experiments. Presumably, these time-varying factors do not homogeneously encourage, or conversely, discourage, drug consumption, so the inability to assess the degree of importance for particular latent variables, especially variables that change over time, is a major limitation of the study. Yet, one reason I develop a simple demand model for drug consumption, omitting marriage, schooling, and other endogenous information that reflects respondent decisions, is to show that unobservable information significantly influences substance use behavior. The resulting work, demonstrating that unobservables affect drug consumption after directly controlling for the role of past consumption, reflects an important contribution to the gateway literature.

Unobservable characteristics explain some of the estimated gateway effects, but not all of them. With better controls for drug prices, estimated gateway effects may be lessened still, but in the absence of that price information it remains difficult to dismiss gateway effects entirely. Thus, avenues for future work include continuing efforts to construct adequate drug prices and/or develop better methods of controlling for drug prices. The tax and marijuana policy variables I employ are endogenous to youthful drug consumption decisions, so the development of better price measures is an important task. Further, since I include a number of potentially endogenous variables in the empirical work described above, an important extension of this work is to develop an even simpler model of drug demand with which to investigate the role of unobservables. Another potential expansion of the research is the inclusion of the use of harder drugs such as cocaine or heroin in the empirical work, a task not addressed here because of the limited amount of relevant information contained in the NLSY97 data. The more recent gateway literature tends to focus on marijuana as a gateway drug into the use of more powerful substances, so investigating that subject is a natural extension of this study.

In the end, I view this study as employing valid methods with which to conclude that gateway effects persist after controlling for unobservables. While not questioning prior work that reaches the same conclusion (or an alternative conclusion), or criticizing earlier work implementing such controls, my research concludes that latent factors matter after controlling directly for prior drug consumption. Instead of simply implementing methods to control for unobservable heterogeneity, the above results indicate that cigarette and alcohol consumption serve as a gateways into marijuana use while modeling endogenous prior drug consumption. Controlling for unobservable heterogeneity reduces the gateway effects predicted by a model estimated without such controls, but does not eliminate them completely. Findings are strongest when conducting experiments that impose total abstinence for particular gateway drugs before particular ages are reached. The gateway theory implies that use of gateway drugs causes later use of more powerful substances, such that if the gateway theory is true, preventing gateway drug consumption also discourages the use of the harder drugs. These findings support the latter idea, even after controlling for unobservables, but also clearly demonstrate that unobservable heterogeneity explains a significant portion of predicted gateway effects.

Appendix A

Data Imputation

A.1 Drug Use

This section describes data imputation procedures and sample attrition in more detail. Even including obvious, logical imputations,[1] there are only 333 unique individuals for whom I impute drug consumption amounts during at least one survey period. The imputation procedure employs responses to questions from subsequent or previous survey years. I impute responses for 80 of these individuals in more than one period, leaving a total of 413 such imputations. I initially identify smoking behavior off of consumption amounts, defining missing information to indicate non-use, but adjusting those definitions should subsequent answers refute this assumption. The key point is that these imputations do not have the potential to bias this analysis to the degree they might if the analysis employed continuous consumption measures.

At baseline, four questions for cigarettes, five questions for alcohol, and three questions for marijuana define drug use.[2] Questions about ever having used and the age of first use are asked for each drug.[3] For cigarettes and alcohol, two other questions for users of each drug first ask on how many separate days the respondent smoked or drank during the past 30 days, and then ask how many cigarettes or drinks the user consumed, on average, on those days. For marijuana, the user is only asked about days of use during the past 30 days; no information on average daily consumption is available. One additional question for drinking asks on how many days the respondent drank five or more drinks during the past thirty days, reflecting information that I later use to distinguish light versus heavy drinking. First, however, I define a baseline indicator for ever having used each substance by taking a positive response to any of the questions as indicating use and dropping

[1] For example, when a respondent has missing smoking data for one survey period but in every other period reports that he or she has never smoked a cigarette, that respondent is assigned to be a nonsmoker with no smoking history.

[2] I do not make use of questions for alcohol and marijuana that inquire about use before school or work.

[3] The age of first use question is not asked in all years, but this does not affect the analysis.

those with missing consumption amounts from the analysis.[4] Even if the number of respondents (333) for whom drug consumption amounts are imputed seems high, I argue that the imputation procedure is sufficiently accurate because no one in the final sample has missing drug consumption data at baseline.

For those with missing data in a subsequent period, I use a straightforward hot deck procedure to impute all missing drug consumption variables except conditional consumption amounts, which I reiterate are not central to the definition of the variables used in the statistical analysis. I define current consumption indicators with the conditional consumption amount questions,[5] initially defining non-response that does not result from attrition as indicative of no consumption. Though these responses are only updated if data from a subsequent period conflict with this initial definition, the imputation for conditional consumption amounts probably corrects for most falsely defined zeroes. For instance, if consumption data are missing during a period but the respondent has reported cigarette consumption in both the prior and subsequent periods, I assume that the respondent also smoked during the period for which data are missing. (If non-missing conditional consumption amounts exist for the periods previous and subsequent to a period in which consumption amounts are missing, I take the average of use during the two surrounding periods; current consumption indicators are subsequently updated to reflect these imputations.) If the information is still missing at that point, I define consumption for the period during which it is missing to be zero for those who can be logically inferred to have either never used the drug or only tried it once or twice. Finally, for a remaining handful of observations where consumption during period t is zero, consumption during $t+1$ is missing, and consumption during $t+2$ is positive and indicates that regular consumption has started, the default zero consumption amount during period $t+1$ is changed to indicate consumption of one cigarette, drink, or day of marijuana use. As stated above, any bias introduced by the imputation procedures should be minimal because conditional consumption amounts affected by the imputation are only used to distinguish between light and heavy cigarette and alcohol use, because the imputations are straightforward and likely to be accurate,

[4] The baseline numbers are as follows. For tobacco, 31 respondents do not answer the question about ever having smoked a cigarette, but only 20 do not answer the days of use question and even fewer (10) do not answer the average daily conditional consumption question. Thus, only the latter 10 are immediately dropped from the analysis. For drinking, 18 respondents are dropped from an initial 40 with no conditional consumption information, and for marijuana, 10 individuals are dropped from a potential 41 respondents without such data.

[5] For marijuana, remember that consumption amounts are measured by days of use during the previous 30 days.

and because only 333 respondents are impacted by the procedure.

A.2 Geography

Defining missing geography data (based on county of residence) is perhaps more problematic than the procedure described above in terms of the accuracy of the imputation, but I similarly argue that the results presented above are not biased by the geographical imputation procedure. This procedure simply defines someone with missing county of residence information for any survey period to be living in the same county they lived in at the previous survey. The procedure affects 324 survey observations, but only 184 respondents. Unlike the drug use imputations, where the argument against bias is based on the presumed accuracy of the procedure, any inaccuracies from this procedure should be random. Similar to the drug use imputation procedure, this imputation procedure should also leave the statistical estimates unbiased.

Appendix B

State-Level Marijuana Laws

Table B.1 describes the underlying distribution of state-level marijuana laws employed in the models estimated above. Some of these laws change during the course of the survey, usually in the sense that they are enacted.[1] The decriminalization measure is constant across the study period, but the other variables change as follows. Mandatory minimum sentencing laws exclusively decrease over the period, with New York (1999); Mississippi and North Dakota (2001); New Mexico (2002); and Colorado, Kansas, Maine, Michigan, Missouri, and Texas (2003) ending mandatory sentencing. The trend for medical marijuana has been similarly been in one direction, with Oregon and Washington (1998); Maine (1999); Colorado, Hawaii, and Nevada (2000); and Montana and Vermont (2004) changing their laws to allow marijuana to be used for medical purposes over the study period. Drugged driving laws increase in number, with Iowa (1998); Illinois and Nevada (1999); Rhode Island (2000); Michigan and Pennsylvania (2003); and Wisconsin (2004) enacting these laws. Finally, tax stamp laws are taken off the books in Wisconsin (1998) and Arizona (1999), and briefly disappear in Massachusetts and North Carolina in 1998 in response to court rulings. The Massachusetts and North Carolina laws are soon amended to comply with the court rulings, and consequently go back into effect.

[1] For instance, Colorado's medical marijuana statute legalizing the use of medical marijuana takes effect on June 1, 2001.

Table B.1: State-Level Marijuana Law Indicators

State[1]	Decriminalized Marijuana	Mandatory Minimum Sentencing	Legalized Medical Marijauana	Drugged Driving Law	Tax Stamp Law
Alabama	-	YES	-	-	YES
Alaska	-	-	YES	-	-
Arizona	-	-	-	YES	YES*
Arkansas	-	-	-	-	-
California	-	-	YES	-	-
Colorado	YES	YES*	YES*	-	-
Connecticut	YES	YES	-	-	YES
Delaware	-	-	-	-	-
District of Columbia	-	YES	-	YES	-
Florida	-	YES	-	-	-
Georgia	-	YES	-	YES	YES
Hawaii	-	-	YES*	-	-
Idaho	-	-	-	-	-
Illinois	-	-	-	YES*	-
Indiana	-	-	-	YES	YES
Iowa	-	YES	-	YES*	YES
Kansas	-	YES*	-	-	YES
Kentucky	-	-	-	-	YES
Louisiana	YES	YES	-	-	YES
Maine	YES	YES*	YES*	-	-
Maryland	-	YES	YES	-	-
Massachusetts	YES	YES	-	-	YES*
Michigan	-	YES*	-	YES*	-
Minnesota	YES	-	-	-	YES
Mississippi	YES	YES*	-	-	-

1 I cannot report state-level respondent numbers per my data use agreement with the Bureau of Labor Statistics.

* Indicates that the state changes this law at some point during the study period.

Table B.1 (Continued): State-Level Marijuana Law Indicators

State[1]	Decriminalized Marijuana	Mandatory Minimum Sentencing	Legalized Medical Marijuana	Drugged Driving Law	Tax Stamp Law
Missouri	-	YES	-	-	-
Montana	-	YES	YES*	-	-
Nebraska	YES	YES	-	-	YES
Nevada	YES	-	YES*	YES*	YES
New Hampshire	-	-	-	-	-
New Jersey	YES	YES	-	-	-
New Mexico	-	YES*	-	-	-
New York	YES	YES*	-	-	-
North Carolina	-	-	-	-	YES*
North Dakota	-	YES*	-	-	-
Ohio	YES	YES	-	-	-
Oklahoma	-	YES	-	-	YES
Oregon	YES	YES	YES*	-	-
Pennsylvania	-	-	-	YES*	-
Rhode Island	-	YES	-	YES*	YES
South Carolina	-	YES	-	-	YES
South Dakota	-	YES	-	-	-
Tennessee	-	-	-	-	-
Texas	-	YES*	-	-	YES
Utah	-	-	-	YES	YES
Vermont	YES	-	YES*	-	-
Virginia	-	YES	-	-	-
Washington	-	YES	YES*	-	-
West Virginia	YES	YES	-	-	-
Wisconsin	YES	-	-	YES*	YES*
Wyoming	-	YES	-	-	-

1 I cannot report state-level respondent numbers per my data use agreement with the Bureau of Labor Statistics.

* Indicates that the state changes this law at some point during the study period.

Appendix C

Full Model Results - Controlling for Unobservables

Whereas similar tables in the text above report parameter estimates for selected state variables (for drug use and prices) from the longitudinal drug use equations in the 13 equation model estimated as part of this research, Tables C.1 through C.5 report full results from all equations in the model when controlling for unobservables using five permanent and three time-varying mass points. The choice of mass points is based on testing that determined that these numbers maximize the likelihood function subject to the condition that the distribution of the resulting estimates remains sufficiently smooth. The standard errors shown are bootstrapped.

Table C.1: Parameter Estimates - Initial Conditions Entering Baseline

Variable **Coefficient *(Standard Error)******	**Ever Smoked** **Pre-1997?**	**Ever Drank** **Pre-1997?**	**Ever Smoked** **Pot Pre-1997?**	**Ever Arrested** **Pre-1997?**
Age	0.53 *(0.08)**	0.44 *(0.09)**	0.87 *(0.16)**	0.50 *(0.13)**
Age squared	-0.03 *(0.01)**	0.03 *(0.02)**	-0.01 *(0.03)*	-0.05 *(0.02)**
Female	-0.18 *(0.08)**	-0.21 *(0.07)**	-0.62 *(0.12)**	-0.89 *(0.11)**
Black	-1.62 *(0.12)**	-0.88 *(0.11)**	-0.48 *(0.16)**	0.15 *(0.13)*
Hispanic	-0.67 *(0.13)**	-0.50 *(0.11)**	-0.13 *(0.19)*	-0.12 *(0.18)*
Other race	-0.71 *(0.25)**	-0.44 *(0.23)*	-0.19 *(0.39)*	-0.25 *(0.30)*
ASVAB	-0.01 *(0.01)**	0.01 *(0.01)**	0.01 *(0.01)*	-0.01 *(0.01)**
ASVAB missing	-0.20 *(0.11)*	-0.40 *(0.11)**	-0.51 *(0.16)**	0.05 *(0.13)*
Catholic	-0.41 *(0.11)**	-0.07 *(0.11)*	-0.82 *(0.20)**	-0.17 *(0.16)*
Protestant	-0.34 *(0.10)**	-0.45 *(0.10)**	-0.95 *(0.18)**	-0.32 *(0.14)**
Other religion	-0.56 *(0.19)**	-1.08 *(0.21)**	-0.97 *(0.31)**	-0.04 *(0.26)*
% of peers who smoke	0.83 *(0.16)**	-0.47 *(0.15)**	-0.72 *(0.25)**	0.88 *(0.24)**
% of peers who drink	-0.04 *(0.19)*	1.08 *(0.19)**	0.44 *(0.27)*	0.37 *(0.23)*
% of peers who do drugs	1.97 *(0.17)**	1.92 *(0.16)**	5.61 *(0.28)**	0.53 *(0.22)**
Ever been in a fight at school?	0.45 *(0.11)**	0.41 *(0.12)**	0.89 *(0.17)**	0.48 *(0.12)**
Ever threatened to be hurt at school?	0.34 *(0.11)**	0.15 *(0.09)*	0.48 *(0.15)**	0.49 *(0.12)**
Experience a breakin before age 12?	0.24 *(0.09)**	0.33 *(0.10)**	0.33 *(0.17)**	-0.02 *(0.12)*
Witness a shooting before age 12?	0.54 *(0.12)**	0.60 *(0.12)**	0.74 *(0.21)**	0.28 *(0.12)**
Bullied before age 12?	0.34 *(0.10)**	0.35 *(0.10)**	0.27 *(0.16)*	0.13 *(0.12)*
Number of siblings	-0.02 *(0.01)*	-0.02 *(0.01)*	-0.02 *(0.02)*	-0.01 *(0.01)*
Parent present at interview	-0.09 *(0.08)*	-0.19 *(0.07)**	-0.02 *(0.14)*	-0.04 *(0.13)*
Father's education	-0.02 *(0.02)*	-0.01 *(0.01)*	0.03 *(0.02)*	-0.02 *(0.02)*
Father's education missing	-0.13 *(0.11)*	-0.24 *(0.09)**	0.08 *(0.15)*	0.02 *(0.12)*
Mother's education	-0.02 *(0.02)*	0.01 *(0.01)*	-0.01 *(0.03)*	-0.04 *(0.02)**
Mother's education missing	0.16 *(0.14)*	-0.05 *(0.14)*	0.54 *(0.21)**	0.02 *(0.12)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.1 (Continued): Parameter Estimates - Initial Conditions Entering Baseline

Variable **Coefficient** ***(Standard Error)****	**Ever Smoked** **Pre-1997?**	**Ever Drank** **Pre-1997?**	**Ever Smoked** **Pot Pre-1997?**	**Ever Arrested** **Pre-1997?**
Cigarette tax - age 12	-0.85 *(0.31)**	-0.12 *(0.25)*	-0.20 *(0.55)*	-0.04 *(0.51)*
Beer tax - age 12	0.03 *(0.24)*	-0.21 *(0.20)*	-0.52 *(0.54)*	-0.65 *(0.41)*
Marijuana decriminalized - age 12	0.22 *(0.10)**	0.02 *(0.08)*	0.06 *(0.15)*	-0.24 *(0.14)*
Mandatory minimum - age 12	-0.14 *(0.09)*	0.12 *(0.09)*	-0.27 *(0.13)**	-0.21 *(0.11)**
Legal medical marijuana - age 12	-0.38 *(0.16)**	0.04 *(0.15)*	-0.32 *(0.30)*	-0.37 *(0.29)*
Drugged driving law - age 12	-0.11 *(0.20)*	0.01 *(0.18)*	0.01 *(0.30)*	0.61 *(0.24)**
Tax stamp law - age 12	0.25 *(0.08)**	0.12 *(0.08)*	0.04 *(0.15)*	0.16 *(0.12)*
# of residences prior to age 12	0.10 *(0.03)**	-0.01 *(0.04)*	0.11 *(0.04)**	0.11 *(0.03)**
Candidness during interview	-0.20 *(0.09)**	0.07 *(0.08)*	-0.22 *(0.14)*	-0.22 *(0.11)**
Constant	3.12 *(0.31)**	1.31 *(0.31)**	-0.09 *(0.63)*	-0.87 *(0.37)**
Permanent Support Point 1		Normalized to zero.		
Permanent Support Point 2	-1.66 *(0.17)**	-0.98 *(0.16)**	-1.88 *(0.28)**	-0.70 *(0.19)**
Permanent Support Point 3	-0.36 *(0.21)*	-0.50 *(0.20)**	-0.56 *(0.28)**	-0.65 *(0.20)**
Permanent Support Point 4	-2.37 *(0.17)**	-1.94 *(0.14)**	-3.01 *(0.24)**	-1.86 *(0.22)**
Permanent Support Point 5	-0.91 *(0.20)**	-1.46 *(0.21)**	-1.51 *(0.29)**	-0.76 *(0.18)**
Time-Varying Support Point 1		Normalized to zero.		
Time-Varying Support Point 2	-1.89 *(0.16)**	-1.45 *(0.14)**	-3.60 *(0.22)**	-0.92 *(0.18)**
Time-Varying Support Point 3	-4.71 *(0.18)**	-4.13 *(0.14)**	-7.26 *(0.31)**	-2.00 *(0.16)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.2: Parameter Estimates - Initial Conditions at Baseline

Variable **Coefficient *(Standard Error)******	**Smoker in 1997?**	**Drinker in 1997?**	**Pot Smoker in 1997?**	**Arrested in 1997?**
Age	0.36 *(0.04)**	0.39 *(0.03)**	0.42 *(0.06)**	0.03 *(0.06)*
Female	0.17 *(0.11)*	-0.20 *(0.08)**	-0.42 *(0.14)**	-0.32 *(0.13)**
Black	-1.37 *(0.14)**	-1.28 *(0.12)**	-0.87 *(0.19)**	-0.23 *(0.16)*
Hispanic	-0.68 *(0.16)**	-0.39 *(0.14)**	-0.34 *(0.18)*	-0.35 *(0.18)*
Other race	-0.53 *(0.28)*	-1.08 *(0.28)**	-0.64 *(0.39)*	-0.61 *(0.47)*
ASVAB	-0.01 *(0.01)**	0.01 *(0.01)*	-0.01 *(0.01)**	-0.01 *(0.01)**
ASVAB missing	-0.12 *(0.13)*	-0.21 *(0.11)**	-0.27 *(0.20)*	0.21 *(0.17)*
Catholic	-0.29 *(0.16)*	-0.07 *(0.16)*	-0.52 *(0.22)**	-0.05 *(0.17)*
Protestant	-0.67 *(0.15)**	-0.43 *(0.13)**	-0.97 *(0.17)**	-0.61 *(0.18)**
Other religion	-1.18 *(0.22)**	-0.79 *(0.17)**	-1.34 *(0.30)**	-0.59 *(0.39)*
% of peers who smoke	1.18 *(0.22)**	0.07 *(0.18)*	-0.18 *(0.27)*	0.24 *(0.29)*
% of peers who drink	0.45 *(0.21)**	1.94 *(0.20)**	1.06 *(0.26)**	0.26 *(0.27)*
% of peers who do drugs	2.15 *(0.26)**	1.76 *(0.19)**	5.47 *(0.34)**	1.82 *(0.34)**
Ever been in fight at school?	0.78 *(0.14)**	0.51 *(0.12)**	0.85 *(0.18)**	0.82 *(0.15)**
Ever threatened to be hurt at school?	0.53 *(0.12)**	0.35 *(0.09)**	0.56 *(0.18)**	0.22 *(0.15)*
Experience a breakin before age 12?	0.20 *(0.13)*	0.10 *(0.12)*	0.22 *(0.17)*	0.07 *(0.18)*
Witness a shooting before age 12?	0.76 *(0.14)**	0.68 *(0.14)**	0.91 *(0.20)**	0.30 *(0.16)*
Bullied before age 12?	0.09 *(0.11)*	0.07 *(0.09)*	0.29 *(0.16)*	0.11 *(0.17)*
Number of siblings	-0.03 *(0.01)**	-0.03 *(0.01)**	-0.02 *(0.02)*	-0.03 *(0.02)**
Parent present at interview	-0.06 *(0.09)*	-0.11 *(0.09)*	-0.03 *(0.15)*	0.11 *(0.11)*
Father's education	0.01 *(0.02)*	0.04 *(0.01)**	0.06 *(0.02)**	-0.04 *(0.03)*
Father's education missing	-0.23 *(0.12)*	0.04 *(0.11)*	-0.08 *(0.16)*	0.31 *(0.17)*
Mother's education	0.01 *(0.02)*	-0.01 *(0.02)*	-0.03 *(0.02)*	-0.01 *(0.03)*
Mother's education missing	0.24 *(0.18)*	-0.25 *(0.17)**	0.38 *(0.27)*	0.04 *(0.25)*
Candidness during interview	-0.19 *(0.09)*	-0.04 *(0.09)*	-0.29 *(0.14)**	0.04 *(0.15)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.2 (Continued): Parameter Estimates - Initial Conditions at Baseline

Variable **Coefficient** ***(Standard Error)****	**Smoker in 1997?**	**Drinker in 1997?**	**Pot Smoker in 1997?**	**Arrested in 1997?**
Cigarette tax	-0.42 *(0.33)*	0.11 *(0.32)*	0.50 *(0.44)*	
Beer tax	-0.32 *(0.32)*	0.60 *(0.29)**	-0.45 *(0.42)*	
Marijuana decriminalized	-0.05 *(0.11)*	0.03 *(0.11)*	0.16 *(0.14)*	-0.20 *(0.14)*
Mandatory sentencing	-0.07 *(0.11)*	-0.01 *(0.10)*	-0.31 *(0.16)*	-0.23 *(0.15)*
Legal medical marijuana	-0.52 *(0.16)**	0.04 *(0.14)*	-0.11 *(0.25)*	-0.36 *(0.25)*
Drugged driving law	-0.41 *(0.18)**	-0.45 *(0.16)**	-0.15 *(0.24)*	-0.49 *(0.28)*
Tax stamp law	0.06 *(0.12)*	-0.08 *(0.12)*	0.10 *(0.17)*	-0.06 *(0.14)*
County-level arrest rate	-0.03 *(0.01)**	0.01 *(0.01)*	-0.05 *(0.02)**	-0.01 *(0.02)*
Smoker at baseline?				0.17 *(0.18)*
Drinker at baseline?				-0.09 *(0.16)*
Pot smoker at baseline?				-0.26 *(0.22)*
Ever smoked entering baseline?	-0.01 *(0.03)*	-0.60 *(0.15)**	-0.16 *(0.23)*	-0.53 *(0.26)**
Ever drank entering baseline?	-0.93 *(0.15)**	0.03 *(0.13)*	-0.62 *(0.22)**	-0.30 *(0.22)*
Ever smoked pot entering baseline?	-0.59 *(0.41)*	-1.13 *(0.47)**	1.22 *(0.42)**	-0.63 *(0.51)*
Ever arrested entering baseline?	-0.31 *(0.14)**	-0.58 *(0.14)**	-0.70 *(0.21)**	2.64 *(0.15)**
Smoked*Drank (ever)	0.19 *(0.20)*	-0.15 *(0.18)*	-0.18 *(0.30)*	-0.10 *(0.29)*
Smoked*Smoked pot (ever)	-0.55 *(0.47)*	-0.07 *(0.48)*	-2.33 *(0.49)**	0.35 *(0.57)*
Drank*Smoked pot (ever)	-0.42 *(0.49)*	0.38 *(0.46)*	-1.22 *(0.48)**	1.04 *(0.61)*
Smoked*Drank*Smoked pot (ever)	0.77 *(0.53)*	0.02 *(0.48)*	1.45 *(0.53)**	-1.19 *(0.61)**
Candidness during interview	-0.19 *(0.09)*	-0.04 *(0.09)*	-0.29 *(0.14)**	0.04 *(0.15)*
Constant	2.65 *(0.45)**	1.16 *(0.41)**	1.14 *(0.65)*	-0.97 *(0.63)*
Permanent Support Point 1		Normalized to zero.		
Permanent Support Point 2	-1.99 *(0.22)**	-0.83 *(0.17)**	-1.61 *(0.28)**	-0.09 *(0.29)*
Permanent Support Point 3	-0.57 *(0.24)**	-0.38 *(0.21)*	-0.36 *(0.28)*	-0.38 *(0.33)*
Permanent Support Point 4	-3.09 *(0.20)**	-2.28 *(0.17)**	-3.22 *(0.28)**	-0.98 *(0.32)**
Permanent Support Point 5	-0.86 *(0.19)**	-1.40 *(0.22)**	-1.57 *(0.26)**	-0.49 *(0.36)*
Time-Varying Support Point 1		Normalized to zero.		
Time-Varying Support Point 2	-2.59 *(0.18)**	-2.29 *(0.16)**	-4.06 *(0.25)**	-1.36 *(0.32)**
Time-Varying Support Point 3	-5.67 *(0.29)**	-5.02 *(0.21)**	-7.87 *(0.44)**	-2.61 *(0.49)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.3: Parameter Estimates - Longitudinal Drug Use

Variable **Coefficient *(Standard Error)***	**Light vs. No** **Smoking**	**Heavy vs. No** **Smoking**	**Light vs. No** **Drinking**	**Heavy vs. No** **Drinking**	**Any vs. No** **Pot Smoking**
Age	0.04 *(0.04)*	0.72 *(0.07)**	0.07 *(0.03)**	0.68 *(0.06)**	0.06 *(0.04)*
Age squared	-0.01 *(0.01)**	-0.04 *(0.01)**	0.01 *(0.01)**	-0.03 *(0.01)**	-0.02 *(0.01)**
Female	-0.14 *(0.04)**	-0.25 *(0.06)**	0.01 *(0.03)*	-1.18 *(0.06)**	-0.27 *(0.03)**
Black	-0.51 *(0.06)**	-1.36 *(0.10)**	-0.84 *(0.05)**	-2.12 *(0.09)**	-0.04 *(0.05)*
Hispanic	-0.09 *(0.06)*	-1.27 *(0.10)**	-0.41 *(0.04)**	-0.74 *(0.07)**	-0.32 *(0.05)**
Other race	-0.03 *(0.12)*	-0.40 *(0.20)**	-0.45 *(0.08)**	-1.14 *(0.16)**	-0.24 *(0.09)**
Cigarette tax	-0.29 *(0.32)*	-1.79 *(0.65)**	0.79 *(0.26)**	1.45 *(0.55)**	0.73 *(0.34)**
Cigarette tax squared	-0.16 *(0.30)*	1.31 *(0.52)**	-0.65 *(0.22)**	-1.05 *(0.45)**	-0.69 *(0.33)**
Beer tax	-0.15 *(0.47)*	-1.35 *(0.54)**	-0.38 *(0.36)*	0.22 *(0.52)*	0.22 *(0.65)*
Beer tax squared	-0.32 *(0.57)*	1.57 *(0.70)**	0.04 *(0.39)*	-1.32 *(0.60)**	-0.84 *(0.76)*
Decriminalized marijuana	0.04 *(0.05)*	0.19 *(0.07)**	0.06 *(0.04)*	-0.07 *(0.07)*	0.07 *(0.04)*
Mandatory minimum sentence	0.05 *(0.04)*	0.09 *(0.05)*	0.02 *(0.03)*	0.05 *(0.06)*	-0.04 *(0.03)*
Legal medical marijuana	-0.10 *(0.06)*	-0.63 *(0.08)**	0.10 *(0.04)**	0.03 *(0.07)*	0.24 *(0.04)**
Drugged driving law	0.02 *(0.06)*	-0.02 *(0.11)*	0.04 *(0.05)*	-0.06 *(0.08)*	-0.04 *(0.05)*
Tax stamp law	0.08 *(0.05)*	0.03 *(0.08)*	0.06 *(0.04)*	0.09 *(0.06)*	-0.01 *(0.04)*
County-level arrest rate	-0.02 *(0.01)**	-0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*	0.01 *(0.01)*
Candidness during interview	0.13 *(0.04)**	-0.02 *(0.05)*	0.35 *(0.04)**	0.37 *(0.05)**	0.10 *(0.04)**
Time	-0.83 *(0.15)**	-0.86 *(0.19)**	-0.96 *(0.11)**	-0.87 *(0.16)**	-0.84 *(0.12)**
Time Squared	0.18 *(0.04)**	0.17 *(0.05)**	0.23 *(0.03)**	0.19 *(0.04)**	0.19 *(0.03)**
Time Cubed	-0.01 *(0.01)**	-0.01 *(0.01)**	-0.02 *(0.01)**	-0.01 *(0.01)**	-0.01 *(0.01)**
County-level unemployment rate	1.55 *(0.54)**	-0.65 *(0.73)*	-2.70 *(0.62)**	-3.20 *(0.98)**	-3.53 *(0.85)**
Has driver's license?	-0.03 *(0.05)*	-0.17 *(0.07)**	0.43 *(0.04)**	0.53 *(0.06)**	0.07 *(0.04)*
Parent present at interview	-0.01 *(0.06)*	0.01 *(0.09)*	-0.11 *(0.05)*	-0.12 *(0.08)*	-0.03 *(0.05)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.3 (Continued): Parameter Estimates - Longitudinal Drug Use

Variable **Coefficient *(Standard Error)******	**Light vs. No Smoking**	**Heavy vs. No Smoking**	**Light vs. No Drinking**	**Heavy vs. No Drinking**	**Any vs. No Pot Smoking**
Smoker $(t-1)$	1.39 *(0.17)**	2.64 *(0.19)**	0.27 *(0.13)**	0.63 *(0.17)**	0.40 *(0.14)**
Drinker $(t-1)$	0.09 *(0.14)*	-0.33 *(0.19)*	1.61 *(0.13)**	2.56 *(0.17)**	0.32 *(0.13)**
Pot smoker $(t-1)$	0.41 *(0.19)**	0.57 *(0.22)**	0.78 *(0.16)**	1.14 *(0.19)**	1.99 *(0.14)**
Arrested during $(t-1)$?	0.03 *(0.13)*	-0.04 *(0.12)*	-0.39 *(0.10)**	-0.25 *(0.13)*	0.19 *(0.07)**
Smoker*Drinker $(t-1)$	0.15 *(0.11)*	-0.01 *(0.13)*	-0.31 *(0.09)**	-0.37 *(0.12)**	-0.21 *(0.08)**
Smoker*Pot Smoker $(t-1)$	-0.32 *(0.20)*	-0.40 *(0.20)*	-0.04 *(0.13)*	-0.14 *(0.21)*	-0.26 *(0.12)**
Drinker*Pot Smoker $(t-1)$	-0.06 *(0.14)*	0.01 *(0.19)*	-0.35 *(0.12)**	-0.34 *(0.17)**	0.26 *(0.11)**
Smoker*Drinker*Pot Smoker $(t-1)$	0.29 *(0.23)*	0.38 *(0.25)*	0.02 *(0.17)*	0.01 *(0.24)*	0.11 *(0.14)*
First time smoker $(t-1)$	-0.24 *(0.08)**	-1.08 *(0.11)**	0.27 *(0.08)**	0.22 *(0.12)*	0.15 *(0.06)**
First time drinker $(t-1)$	-0.03 *(0.07)*	-0.19 *(0.10)*	-0.71 *(0.07)**	-1.24 *(0.10)**	-0.23 *(0.06)**
First time pot smoker $(t-1)$	0.11 *(0.08)*	-0.29 *(0.10)**	-0.03 *(0.08)*	-0.13 *(0.10)*	-0.56 *(0.05)**
First arrested at $(t-1)$?	0.08 *(0.16)*	-0.07 *(0.15)*	0.58 *(0.13)**	0.80 *(0.16)**	-0.02 *(0.09)*
Has smoked entering t?	0.42 *(0.06)**	1.97 *(0.12)**	0.20 *(0.05)**	0.75 *(0.09)**	0.33 *(0.05)**
Has drank entering t?	0.39 *(0.06)**	0.85 *(0.11)**	0.46 *(0.05)**	1.39 *(0.10)**	0.73 *(0.06)**
Has smoked pot entering t?	0.12 *(0.06)**	0.66 *(0.10)**	0.01 *(0.04)*	0.23 *(0.07)**	0.82 *(0.05)**
Has ever been arrested entering t?	-0.20 *(0.07)**	0.20 *(0.09)**	-0.41 *(0.05)**	-0.59 *(0.09)**	-0.08 *(0.04)*
Smoker$(t-1)$*cigarette tax(t)	-0.07 *(0.43)*	-0.34 *(0.52)*	0.47 *(0.33)*	0.02 *(0.50)*	-0.06 *(0.43)*
Smoker$(t-1)$*cigarette tax squared(t)	0.58 *(0.40)*	0.61 *(0.45)*	-0.66 *(0.29)**	-0.49 *(0.43)*	-0.20 *(0.37)*
Smoker$(t-1)$*beer tax(t)	-0.34 *(0.55)*	-1.61 *(0.45)**	0.04 *(0.51)*	0.93 *(0.50)*	-0.22 *(0.47)*
Smoker$(t-1)$*beer tax squared(t)	1.41 *(0.65)**	2.56 *(0.56)**	-0.12 *(0.61)*	-1.18 *(0.64)**	0.43 *(0.54)*
Drinker$(t-1)$*cigarette tax(t)	0.99 *(0.46)**	1.80 *(0.48)**	-0.33 *(0.36)*	-0.78 *(0.52)*	0.14 *(0.42)*
Drinker$(t-1)$*cigarette tax squared(t)	-0.80 *(0.41)*	-1.38 *(0.42)**	0.55 *(0.30)*	1.10 *(0.46)**	0.21 *(0.34)*
Drinker$(t-1)$*beer tax(t)	0.94 *(0.62)*	1.96 *(0.58)**	0.58 *(0.53)*	-1.26 *(0.57)**	0.12 *(0.48)*
Drinker$(t-1)$*beer tax squared(t)	-1.47 *(0.77)*	-1.68 *(0.65)**	-0.67 *(0.63)*	1.43 *(0.65)**	0.30 *(0.60)*
Pot smoker$(t-1)$*cigarette tax(t)	-1.05 *(0.54)*	0.31 *(0.58)*	-0.29 *(0.44)*	-0.41 *(0.55)*	-0.47 *(0.35)*
Pot smoker$(t-1)$*cig tax squared(t)	0.92 *(0.47)*	-0.55 *(0.49)*	0.46 *(0.40)*	0.52 *(0.50)*	0.76 *(0.32)**
Pot smoker$(t-1)$*beer tax(t)	0.05 *(0.53)*	0.19 *(0.56)*	-0.07 *(0.61)*	0.19 *(0.58)*	0.17 *(0.52)*
Pot smoker$(t-1)$*beer tax squared(t)	0.66 *(0.67)*	-0.58 *(0.66)*	0.35 *(0.72)*	0.38 *(0.71)*	0.05 *(0.62)*
Constant	1.69 *(0.24)**	0.69 *(0.33)**	2.73 *(0.22)**	1.55 *(0.34)**	0.56 *(0.17)**
Permanent Support Point 1			Normalized to zero.		
Permanent Support Point 2	-0.72 *(0.12)**	-4.84 *(0.15)**	0.53 *(0.09)**	0.42 *(0.15)**	-0.33 *(0.07)**
Permanent Support Point 3	0.13 *(0.13)*	-2.12 *(0.15)**	-0.37 *(0.12)**	-2.57 *(0.17)**	-0.06 *(0.09)*
Permanent Support Point 4	-1.86 *(0.12)**	-7.97 *(0.54)**	-0.93 *(0.10)**	-3.88 *(0.18)**	-0.07 *(0.06)*
Permanent Support Point 5	0.03 *(0.13)*	0.63 *(0.16)**	-0.67 *(0.09)**	-3.04 *(0.12)**	-1.40 *(0.10)**
Time-Varying Support Point 1			Normalized to zero.		
Time-Varying Support Point 2	-5.01 *(0.18)**	-6.70 *(0.23)**	-5.49 *(0.20)**	-8.53 *(0.28)**	-2.11 *(0.08)**
Time-Varying Support Point 3	-2.23 *(0.10)**	-3.59 *(0.17)**	-2.88 *(0.16)**	-4.94 *(0.21)**	-3.58 *(0.10)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.4: Parameter Estimates - Longitudinal Arrest and Attrition

Variable	**Any vs. No Arrests**	**Sample Attrition**
Coefficient *(Standard Error)**		
Age	0.05 *(0.05)*	0.50 *(0.06)**
Age Squared	-0.01 *(0.01)**	-0.02 *(0.01)**
Female	-0.90 *(0.04)**	-0.27 *(0.05)**
Black	0.22 *(0.06)**	-0.06 *(0.07)*
Hispanic	0.01 *(0.06)*	0.02 *(0.06)*
Other race	-0.13 *(0.11)*	-0.04 *(0.11)*
Beer Tax	-0.61 *(0.51)*	
Beer Tax Squared	0.36 *(0.61)*	
Marijuana decriminalized	-0.11 *(0.05)**	
Mandatory minimum sentence law	0.06 *(0.05)*	
Legal medical marijuana	-0.06 *(0.06)*	
Drugged driving law	0.19 *(0.06)**	
Tax stamp law	0.05 *(0.04)*	
County-level arrest rate	0.01 *(0.01)*	
Respondent candid during interview?	-0.11 *(0.04)**	-0.38 *(0.05)**
Time	-0.45 *(0.14)**	0.25 *(0.21)*
Time Squared	0.10 *(0.04)**	-0.23 *(0.07)**
Time Cubed	-0.01 *(0.01)**	0.03 *(0.01)**
County-level unemployment rate	2.25 *(0.78)**	
Has driver's license?	-0.29 *(0.05)**	
Parent present at interview	-0.05 *(0.07)*	-0.18 *(0.06)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses.

N (arrest) = 50,584. N (attrition) = 44,315.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.4 (Continued): Parameter Estimates - Longitudinal Arrest and Attrition

Variable **Coefficient** ***(Standard Error)****	**Any vs. No Arrests**	**Sample Attrition**
Smoker during t?	0.38 *(0.12)**	-0.28 *(0.11)**
Drinker during t?	0.19 *(0.14)*	-0.12 *(0.10)*
Pot smoker during t?	1.49 *(0.13)**	-0.30 *(0.16)*
Arrested during $(t-1)$?	0.88 *(0.06)**	0.31 *(0.10)**
Smoker*Drinker (t)	-0.38 *(0.12)**	-0.16 *(0.12)*
Smoker*Pot Smoker (t)	-0.65 *(0.16)**	0.49 *(0.21)**
Drinker*Pot Smoker (t)	-0.85 *(0.16)**	-0.09 *(0.20)*
Smoker*Drinker*Pot Smoker (t)	0.63 *(0.20)**	-0.22 *(0.27)*
First time smoker during t?	0.14 *(0.10)*	0.30 *(0.12)*
First time drinker during t?	0.16 *(0.11)*	0.10 *(0.12)*
First time pot smoker during t?	0.18 *(0.08)**	0.14 *(0.11)*
First arrested during $t-1$?	0.28 *(0.08)**	-0.54 *(0.18)**
Has smoked entering $t/t+1$?	0.23 *(0.07)**	0.05 *(0.08)*
Has drank entering $t/t+1$?	0.14 *(0.08)*	0.08 *(0.07)*
Has smoked pot entering $t/t+1$?	0.46 *(0.06)**	0.06 *(0.08)*
Has ever been arrested entering $t/t+1$?	0.75 *(0.06)**	0.12 *(0.06)*
Smoker(t)*beer tax(t)	0.45 *(0.45)*	
Smoker(t)*beer tax squared(t)	-0.78 *(0.52)*	
Drinker(t)*beer tax(t)	0.72 *(0.69)*	
Drinker(t)*beer tax squared(t)	-0.70 *(0.84)*	
Pot smoker(t)*beer tax(t)	-1.43 *(0.46)**	
Pot smoker(t)*beer tax squared(t)	1.56 *(0.57)**	
Constant	-2.03 *(0.31)**	-3.64 *(0.34)**
Permanent Support Point 1	Normalized to zero.	
Permanent Support Point 2	-0.31 *(0.09)**	-0.71 *(0.18)**
Permanent Support Point 3	-0.51 *(0.10)**	-0.13 *(0.14)*
Permanent Support Point 4	-1.55 *(0.16)**	-0.70 *(0.17)**
Permanent Support Point 5	-0.23 *(0.10)**	0.09 *(0.15)*
Time-Varying Support Point 1	Normalized to zero.	
Time-Varying Support Point 2	-0.58 *(0.19)**	-0.30 *(0.21)*
Time-Varying Support Point 3	-0.51 *(0.14)**	-0.20 *(0.16)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses.

N (arrest) = 50,584. N (attrition) = 44,315.

NLSY97 Data: 1997-2004 Survey Rounds.

Table C.5: Unobserved Heterogeneity Information

Point of Support	**Probability Weight**	**Coefficient *(Standard Error)****
Permanent Support Point 1	0.18	Normalized to zero.
Permanent Support Point 2	0.18	0.01 *(0.09)*
Permanent Support Point 3	0.15	-0.15 *(0.11)*
Permanent Support Point 4	0.15	-0.16 *(0.08)**
Permanent Support Point 5	0.34	0.67 *(0.08)**
Time-Varying Support Point 1	0.16	Normalized to zero.
Time-Varying Support Point 2	0.61	1.35 *(0.05)**
Time-Varying Support Point 3	0.24	0.40 *(0.07)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Appendix D

Full Model Results - Ignoring Unobservables

Whereas similar tables in the text above report parameter estimates for selected state variables (for drug use and prices) from the longitudinal drug use equations in the 13 equation model estimated as part of this research, Tables D.1 through D.4 report full results from all equations in the model when estimating the equations in the model independently. This estimation method does not control for unobservable individual-level heterogeneity, and consequently treats the endogenous state variables as being exogenous. The standard errors shown are bootstrapped.

Table D.1: Parameter Estimates - Initial Conditions Entering Baseline

Variable **Coefficient *(Standard Error)******	**Ever Smoked** **Pre-1997?**	**Ever Drank** **Pre-1997?**	**Ever Smoked** **Pot Pre-1997?**	**Ever Arrested** **Pre-1997?**
Age	0.35 *(0.20)*	0.34 *(0.22)*	0.66 *(0.66)*	0.52 *(0.75)*
Age squared	-0.02 *(0.03)*	0.01 *(0.04)*	-0.04 *(0.12)*	-0.05 *(0.14)*
Female	-0.21 *(0.10)**	-0.20 *(0.10)**	-0.41 *(0.29)**	-0.89 *(0.65)*
Black	-1.22 *(0.34)**	-0.73 *(0.30)**	-0.63 *(0.71)*	-0.07 *(0.83)*
Hispanic	-0.54 *(0.43)*	-0.41 *(0.27)*	-0.19 *(0.82)*	-0.22 *(0.67)*
Other race	-0.41 *(0.67)*	-0.27 *(0.81)*	-0.04 *(0.70)*	-0.21 *(1.07)*
ASVAB	-0.01 *(0.01)*	0.01 *(0.01)*	0.01 *(0.01)*	-0.01 *(0.01)*
ASVAB missing	-0.03 *(0.18)*	-0.19 *(0.13)*	-0.17 *(0.64)*	0.10 *(0.79)*
Catholic	-0.31 *(0.42)*	-0.05 *(0.44)*	-0.49 *(0.55)*	-0.20 *(0.65)*
Protestant	-0.26 *(0.34)*	-0.31 *(0.33)*	-0.49 *(0.62)*	-0.32 *(0.55)*
Other religion	-0.34 *(0.82)*	-0.73 *(0.60)*	-0.46 *(0.78)*	-0.04 *(0.64)*
% of peers who smoke	0.68 *(0.67)*	-0.20 *(0.60)*	-0.24 *(0.73)*	0.84 *(0.68)*
% of peers who drink	0.03 *(0.54)*	0.78 *(0.76)*	0.36 *(0.71)*	0.42 *(0.73)*
% of peers who do drugs	1.40 *(0.64)**	1.43 *(0.66)**	2.84 *(0.67)**	0.54 *(0.62)*
Ever been in a fight at school?	0.39 *(0.30)*	0.35 *(0.24)*	0.51 *(0.55)*	0.51 *(0.67)*
Ever threatened to be hurt at school?	0.20 *(0.23)*	0.05 *(0.23)*	0.17 *(0.48)*	0.42 *(0.54)*
Experience a breakin before age 12?	0.14 *(0.28)*	0.20 *(0.17)*	0.11 *(0.70)*	-0.01 *(0.81)*
Witness a shooting before age 12?	0.47 *(0.27)*	0.50 *(0.38)*	0.55 *(0.52)*	0.38 *(0.78)*
Bullied before age 12?	0.21 *(0.18)*	0.23 *(0.14)*	0.08 *(0.54)*	0.08 *(0.71)*
Number of siblings	-0.01 *(0.02)*	-0.01 *(0.01)*	-0.01 *(0.02)*	-0.01 *(0.04)*
Parent present at interview	-0.14 *(0.09)*	-0.20 *(0.08)**	-0.06 *(0.37)*	-0.08 *(0.82)*
Father's education	-0.03 *(0.02)*	-0.02 *(0.02)*	0.01 *(0.03)*	-0.03 *(0.04)*
Father's education missing	-0.01 *(0.31)*	-0.10 *(0.23)*	0.15 *(0.54)*	0.04 *(0.85)*
Mother's education	-0.01 *(0.03)*	0.01 *(0.02)*	0.01 *(0.03)*	-0.03 *(0.05)*
Mother's education missing	0.08 *(0.72)*	-0.05 *(0.55)*	0.26 *(0.77)*	0.65 *(0.76)*
Cigarette tax - age 12	-0.60 *(0.90)*	-0.12 *(0.71)*	-0.28 *(0.45)*	-0.08 *(0.56)*
Beer tax - age 12	-0.04 *(0.69)*	-0.17 *(0.97)*	-0.32 *(0.80)*	-0.60 *(0.78)*
Marijuana decriminalized - age 12	0.13 *(0.12)*	-0.01 *(0.20)*	0.06 *(0.37)*	-0.22 *(0.94)*
Mandatory minimum - age 12	-0.07 *(0.09)*	0.09 *(0.10)*	-0.12 *(0.38)*	-0.18 *(0.67)*
Legal medical marijuana - age 12	-0.20 *(0.58)*	0.08 *(0.64)*	-0.03 *(0.57)*	-0.29 *(0.92)*
Drugged driving law - age 12	-0.01 *(0.55)*	0.08 *(0.94)*	0.16 *(0.63)*	0.61 *(0.85)*
Tax stamp law - age 12	0.18 *(0.15)*	0.09 *(0.17)*	0.01 *(0.44)*	0.15 *(0.65)*
# of residences prior to age 12	0.12 *(0.03)**	0.04 *(0.02)**	0.12 *(0.03)**	0.13 *(0.06)**
Candidness during interview	-0.13 *(0.12)*	0.04 *(0.12)*	-0.12 *(0.44)*	-0.22 *(0.69)*
Constant	-0.77 *(0.89)*	-1.79 *(0.58)**	-3.62 *(0.37)**	-2.86 *(0.52)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.2: Parameter Estimates - Initial Conditions at Baseline

Variable **Coefficient *(Standard Error)****	**Smoker in 1997?**	**Drinker in 1997?**	**Pot Smoker in 1997?**	**Arrested in 1997?**
Age	-0.01 *(0.09)*	0.05 *(0.08)*	-0.09 *(0.11)*	-0.11 *(0.13)*
Female	0.27 *(0.24)*	-0.04 *(0.20)*	-0.09 *(0.78)*	-0.26 *(0.69)*
Black	-0.69 *(0.51)*	-0.60 *(0.53)*	-0.16 *(0.82)*	0.13 *(0.79)*
Hispanic	-0.40 *(0.64)*	-0.14 *(0.61)*	-0.04 *(0.74)*	-0.18 *(0.68)*
Other race	-0.09 *(0.76)*	-0.63 *(0.71)*	-0.03 *(0.73)*	-0.40 *(0.49)*
ASVAB	-0.01 *(0.01)**	0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*
ASVAB missing	0.15 *(0.57)*	0.01 *(0.70)*	0.07 *(0.84)*	0.30 *(0.70)*
Catholic	-0.08 *(0.48)*	0.16 *(0.55)*	-0.07 *(0.68)*	0.06 *(0.65)*
Protestant	-0.27 *(0.46)*	-0.08 *(0.46)*	-0.24 *(0.57)*	-0.41 *(0.62)*
Other religion	-0.49 *(0.74)*	-0.26 *(0.88)*	-0.32 *(0.72)*	-0.22 *(0.40)*
% of peers who smoke	1.08 *(0.83)*	0.16 *(0.56)*	0.07 *(0.70)*	0.17 *(0.52)*
% of peers who drink	0.08 *(0.81)*	1.37 *(0.67)**	0.48 *(0.67)*	-0.01 *(0.73)*
% of peers who do drugs	0.16 *(0.71)*	0.06 *(0.62)*	1.56 *(0.72)**	0.87 *(0.80)*
Ever been in fight at school?	0.40 *(0.52)*	0.18 *(0.43)*	0.22 *(0.78)*	0.59 *(0.84)*
Ever threatened to be hurt at school?	0.22 *(0.50)*	0.08 *(0.32)*	0.09 *(0.64)*	0.06 *(0.91)*
Experience a breakin before age 12?	0.01 *(0.84)*	-0.06 *(0.60)*	-0.02 *(0.87)*	-0.01 *(0.87)*
Witness a shooting before age 12?	0.35 *(0.70)*	0.27 *(0.63)*	0.28 *(0.76)*	0.06 *(0.93)*
Bullied before age 12?	-0.08 *(0.61)*	-0.10 *(0.62)*	-0.02 *(0.81)*	0.05 *(0.78)*
Number of siblings	-0.01 *(0.02)*	-0.01 *(0.02)*	-0.01 *(0.04)*	-0.03 *(0.04)*
Parent present at interview	-0.05 *(0.34)*	-0.10 *(0.11)*	-0.04 *(0.69)*	0.12 *(1.01)*
Father's education	0.01 *(0.02)*	0.02 *(0.02)*	0.03 *(0.04)*	-0.05 *(0.05)*
Father's education missing	-0.08 *(0.50)*	0.10 *(0.65)*	0.04 *(0.70)*	0.34 *(0.64)*
Mother's education	0.01 *(0.03)*	0.01 *(0.02)*	-0.01 *(0.04)*	0.01 *(0.05)*
Mother's education missing	0.01 *(0.78)*	-0.35 *(0.78)*	0.01 *(0.88)*	-0.04 *(0.94)*

* - Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.2 (Continued): Parameter Estimates - Initial Conditions at Baseline

Variable **Coefficient *(Standard Error)****	**Smoker in 1997?**	**Drinker in 1997?**	**Pot Smoker in 1997?**	**Arrested in 1997?**
Cigarette tax	-0.12 (0.68)	0.28 (0.75)	0.62 (0.89)	
Beer tax	-0.20 (0.71)	0.51 (0.75)	-0.26 (0.63)	
Marijuana decriminalized	-0.09 (0.59)	-0.01 (0.45)	0.06 (0.63)	-0.21 (0.55)
Mandatory sentencing	0.02 (0.35)	0.03 (0.33)	-0.13 (0.74)	-0.20 (0.55)
Legal medical marijuana	-0.41 (0.72)	0.08 (0.70)	-0.06 (0.92)	-0.33 (0.94)
Drugged driving law	-0.35 (0.63)	-0.34 (0.69)	-0.08 (0.79)	-0.39 (0.94)
Tax stamp law	0.04 (0.27)	-0.06 (0.34)	0.09 (0.68)	-0.07 (0.74)
County-level arrest rate	-0.02 (0.03)	0.01 (0.02)	-0.03 (0.05)	-0.01 (0.05)
Smoker at baseline?				0.58 (0.84)
Drinker at baseline?				0.35 (0.88)
Pot smoker at baseline?				0.33 (0.83)
Ever smoked entering baseline?	1.48 (0.64)*	0.82 (0.48)	1.56 (0.77)*	0.04 (0.68)
Ever drank entering baseline?	0.55 (0.61)	1.27 (0.47)*	1.05 (0.67)	0.30 (0.71)
Ever smoked pot entering baseline?	1.49 (0.67)*	0.84 (0.68)	3.15 (0.65)*	0.08 (0.60)
Ever arrested entering baseline?	0.30 (0.55)	-0.01 (0.48)	0.09 (0.70)	2.83 (0.54)*
Smoked*Drank (ever)	-0.09 (0.56)	-0.37 (0.69)	-0.39 (0.49)	-0.30 (0.55)
Smoked*Smoked pot (ever)	-0.64 (0.68)	-0.14 (0.90)	-1.77 (0.63)*	0.35 (0.77)
Drank*Smoked pot (ever)	-0.74 (0.63)	-0.03 (0.73)	-1.15 (0.62)*	0.85 (0.78)
Smoked*Drank*Smoked pot (ever)	0.79 (0.74)	0.16 (0.77)	1.14 (0.76)	-1.12 (0.82)
Candidness during interview	-0.09 (0.32)	0.02 (0.32)	-0.12 (0.65)	0.08 (0.59)
Constant	-2.46 (0.36)*	-3.33 (0.50)*	-4.59 (0.63)*	-3.32 (0.56)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=8,300.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.3: Parameter Estimates - Longitudinal Drug Use

Variable **Coefficient *(Standard Error)****	**Light vs. No** **Smoking**	**Heavy vs. No** **Smoking**	**Light vs. No** **Drinking**	**Heavy vs. No** **Drinking**	**Any vs. No** **Pot Smoking**
Age	-0.08 *(0.12)*	0.27 *(0.17)*	-0.02 *(0.07)*	0.22 *(0.13)*	-0.03 *(0.17)*
Age Squared	0.01 *(0.01)*	-0.02 *(0.01)*	0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*
Female	-0.12 *(0.03)**	-0.12 *(0.03)**	0.12 *(0.02)**	-0.69 *(0.03)**	-0.21 *(0.03)**
Black	-0.24 *(0.08)**	-0.67 *(0.07)**	-0.42 *(0.04)**	-1.08 *(0.07)**	0.01 *(0.04)*
Hispanic	0.10 *(0.07)*	-0.72 *(0.07)**	-0.23 *(0.04)**	-0.33 *(0.05)**	-0.23 *(0.05)**
Other race	0.07 *(0.43)*	0.01 *(0.36)*	-0.22 *(0.16)*	-0.57 *(0.33)*	-0.15 *(0.10)*
Cigarette Tax	-0.11 *(0.63)*	-1.04 *(0.79)*	0.48 *(0.36)*	1.10 *(0.50)**	0.65 *(0.53)*
Cigarette Tax Squared	-0.29 *(0.59)*	0.80 *(0.60)*	-0.42 *(0.32)*	-0.90 *(0.39)**	-0.68 *(0.52)*
Beer Tax	-0.06 *(0.57)*	-0.80 *(0.70)*	-0.42 *(0.62)*	-0.01 *(0.72)*	0.07 *(0.66)*
Beer Tax Squared	-0.29 *(0.58)*	1.01 *(0.64)*	0.20 *(0.65)*	-0.43 *(0.73)*	-0.52 *(0.66)*
Decriminalized Marijuana	-0.01 *(0.06)*	0.03 *(0.08)*	0.06 *(0.03)**	-0.04 *(0.05)*	0.06 *(0.04)*
Mandatory Sentencing	0.04 *(0.05)*	0.08 *(0.04)*	0.01 *(0.04)*	0.03 *(0.05)*	-0.03 *(0.04)*
Legal Medical Marijuana	-0.02 *(0.07)*	-0.41 *(0.08)**	0.08 *(0.04)**	0.02 *(0.06)*	0.19 *(0.06)**
Drugged Driving Law	0.04 *(0.08)*	-0.05 *(0.09)*	0.03 *(0.05)*	-0.05 *(0.07)*	-0.03 *(0.08)*
Tax stamp law	0.07 *(0.06)*	-0.02 *(0.06)*	0.04 *(0.03)*	0.04 *(0.05)*	0.01 *(0.04)*
County-level arrest rate	-0.01 *(0.01)*	0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*	0.01 *(0.01)*
Candidness during interview	0.07 *(0.06)*	-0.11 *(0.04)**	0.22 *(0.04)**	0.22 *(0.05)**	0.07 *(0.06)*
Time	-0.70 *(0.45)*	-0.42 *(0.43)*	-0.75 *(0.39)*	-0.49 *(0.40)*	-0.70 *(0.53)*
Time Squared	0.15 *(0.13)*	0.08 *(0.12)*	0.18 *(0.11)*	0.10 *(0.11)*	0.15 *(0.14)*
Time Cubed	-0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*	-0.01 *(0.01)*
County-level unemployment rate	1.08 *(1.00)*	0.77 *(0.84)*	-1.97 *(0.60)**	-1.60 *(0.81)**	-2.88 *(0.84)**
Has driver's license?	-0.02 *(0.06)*	-0.26 *(0.06)**	0.29 *(0.04)**	0.40 *(0.06)**	0.06 *(0.06)*
Parent present at interview	-0.01 *(0.06)*	0.04 *(0.06)*	-0.08 *(0.04)**	-0.05 *(0.06)*	-0.02 *(0.07)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.3 (Continued): Parameter Estimates - Longitudinal Drug Use

Variable **Coefficient *(Standard Error)******	**Light vs. No** **Smoking**	**Heavy vs. No** **Smoking**	**Light vs. No** **Drinking**	**Heavy vs. No** **Drinking**	**Any vs. No** **Pot Smoking**
Smoker $(t-1)$	1.06 *(0.38)**	2.83 *(0.33)**	0.18 *(0.28)*	0.32 *(0.30)*	0.55 *(0.34)*
Drinker $(t-1)$	0.22 *(0.31)*	-0.70 *(0.27)**	1.12 *(0.21)**	1.97 *(0.25)**	0.35 *(0.24)*
Pot smoker $(t-1)$	0.42 *(0.59)*	-0.32 *(0.44)*	0.56 *(0.35)*	0.78 *(0.46)*	1.69 *(0.40)**
Arrested during $(t-1)$?	0.10 *(0.48)*	0.11 *(0.27)*	-0.27 *(0.56)*	-0.06 *(0.45)*	0.20 *(0.51)*
Smoker*Drinker $(t-1)$	0.09 *(0.35)*	0.28 *(0.41)*	-0.29 *(0.23)**	-0.14 *(0.36)*	-0.18 *(0.31)*
Smoker*Pot Smoker $(t-1)$	-0.43 *(0.51)*	-0.37 *(0.38)*	-0.12 *(0.44)*	-0.24 *(0.49)*	-0.30 *(0.33)*
Drinker*Pot Smoker $(t-1)$	-0.10 *(0.54)*	0.02 *(0.40)*	-0.32 *(0.41)*	-0.06 *(0.53)*	0.21 *(0.34)*
Smoker*Drinker*Pot Smoker $(t-1)$	0.25 *(0.60)*	0.20 *(0.47)*	0.06 *(0.61)*	-0.03 *(0.56)*	0.12 *(0.43)*
First time smoker $(t-1)$	-0.22 *(0.29)*	-1.42 *(0.22)**	0.21 *(0.23)*	0.12 *(0.32)*	0.02 *(0.12)*
First time drinker $(t-1)$	-0.09 *(0.13)*	0.16 *(0.11)*	-0.48 *(0.08)**	-1.10 *(0.13)**	-0.26 *(0.09)**
First time pot smoker $(t-1)$	0.11 *(0.46)*	-0.23 *(0.36)*	0.01 *(0.25)*	-0.19 *(0.31)*	-0.53 *(0.11)**
First arrested at $(t-1)$?	0.02 *(0.69)*	-0.18 *(0.39)*	0.39 *(0.74)*	0.34 *(0.70)*	-0.07 *(0.75)*
Has smoked entering t?	0.37 *(0.10)**	1.51 *(0.09)**	0.12 *(0.04)**	0.44 *(0.06)**	0.41 *(0.06)**
Has drank entering t?	0.28 *(0.08)**	0.24 *(0.08)**	0.31 *(0.04)**	1.01 *(0.09)**	0.62 *(0.06)**
Has smoked pot entering t?	0.10 *(0.12)*	0.51 *(0.09)**	0.01 *(0.06)*	0.23 *(0.08)**	0.73 *(0.06)**
Has ever been arrested entering t?	-0.07 *(0.19)*	0.45 *(0.10)**	-0.21 *(0.08)**	-0.03 *(0.07)*	0.04 *(0.06)*
Smoker*Cig Tax	-0.01 *(0.67)*	-0.79 *(0.71)*	0.36 *(0.63)*	0.13 *(0.64)*	-0.19 *(0.45)*
Smoker*Cig Tax Squared	0.54 *(0.71)*	1.16 *(0.58)*	-0.43 *(0.53)*	-0.25 *(0.54)*	0.03 *(0.53)*
Smoker*Beer Tax	0.05 *(0.60)*	-1.46 *(0.58)**	-0.08 *(0.73)*	0.83 *(0.68)*	-0.19 *(0.66)*
Smoker*Beer Tax Squared	-0.67 *(0.70)*	1.88 *(0.71)**	0.12 *(0.79)*	-0.94 *(0.78)*	0.35 *(0.85)*
Drinker*Cig Tax	0.62 *(0.76)*	1.15 *(0.46)**	-0.21 *(0.51)*	-0.66 *(0.45)*	0.12 *(0.55)*
Drinker*Cig Tax Squared	-0.48 *(0.69)*	-1.15 *(0.45)**	0.35 *(0.45)*	0.78 *(0.39)*	0.18 *(0.51)*
Drinker*Beer Tax	0.46 *(0.70)*	1.31 *(0.54)**	0.57 *(0.63)*	-1.05 *(0.64)*	0.06 *(0.72)*
Drinker*Beer Tax Squared	-0.97 *(0.80)*	-1.23 *(0.68)**	-0.59 *(0.70)*	1.07 *(0.89)*	0.30 *(1.93)*
Pot Smoker*Cig Tax	-0.85 *(0.69)*	0.27 *(0.69)*	-0.15 *(0.72)*	-0.34 *(0.58)*	-0.37 *(0.60)*
Pot Smoker*Cig Tax Squared	0.79 *(0.62)*	-0.40 *(0.63)*	0.29 *(0.65)*	0.45 *(0.52)*	0.63 *(0.60)*
Pot Smoker*Beer Tax	-0.09 *(0.67)*	-0.17 *(0.65)*	-0.04 *(0.77)*	0.22 *(0.75)*	0.23 *(0.77)*
Pot Smoker*Beer Tax Squared	0.95 *(0.71)*	0.08 *(0.76)*	0.26 *(0.69)*	0.24 *(0.88)*	-0.02 *(0.67)*
Constant	-1.13 *(0.57)**	-3.12 *(0.65)**	-0.63 *(0.51)*	-3.41 *(0.48)**	-1.52 *(0.55)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses. N=50,584.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.4: Parameter Estimates - Longitudinal Arrest and Attrition

Variable	**Any vs. No Police Contact**	**Sample Attrition**
Coefficient *(Standard Error)**		
Age	-0.01 *(0.26)*	0.46 *(0.18)**
Age Squared	-0.01 *(0.02)*	-0.02 *(0.01)*
Female	-0.88 *(0.06)**	-0.27 *(0.05)**
Black	0.30 *(0.22)*	0.01 *(0.16)*
Hispanic	0.05 *(0.09)*	0.06 *(0.13)*
Other race	-0.07 *(0.38)*	0.01 *(0.80)*
Beer Tax	-0.63 *(0.63)*	
Beer Tax Squared	0.45 *(0.71)*	
Marijuana decriminalized	-0.11 *(0.05)**	
Mandatory minimum sentence law	0.06 *(0.17)*	
Legal medical marijuana	-0.07 *(0.24)*	
Drugged driving law	0.18 *(0.30)*	
Tax stamp law	0.05 *(0.23)*	
County-level arrest rate	0.01 *(0.01)*	
Respondent candid during interview?	-0.13 *(0.07)*	-0.40 *(0.08)**
Time	-0.40 *(0.65)*	0.26 *(0.37)*
Time Squared	0.09 *(0.18)*	-0.23 *(0.11)**
Time Cubed	-0.01 *(0.01)*	0.03 *(0.01)**
County-level unemployment rate	2.42 *(0.69)**	
Has driver's license?	-0.30 *(0.14)**	
Parent present at interview	-0.05 *(0.15)*	-0.17 *(0.22)*

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses.

N (arrest) = 50,584. N (attrition) = 44,315.

NLSY97 Data: 1997-2004 Survey Rounds.

Table D.4 (Continued): Parameter Estimates - Longitudinal Arrest and Attrition

Variable	**Any vs. No Police Contact**	**Sample Attrition**
Coefficient *(Standard Error)**		
Smoker during t?	0.77 *(0.54)*	0.02 *(0.68)*
Drinker during t?	0.32 *(0.38)*	-0.13 *(0.44)*
Pot smoker during t?	1.59 *(0.36)**	-0.23 *(0.79)*
Arrested during $(t-1)$?	0.91 *(0.32)**	0.34 *(0.50)*
Smoker*Drinker (t)	-0.37 *(0.65)**	-0.12 *(0.84)*
Smoker*Pot Smoker (t)	-0.74 *(0.47)**	0.42 *(0.74)*
Drinker*Pot Smoker (t)	-0.82 *(0.68)**	-0.14 *(0.83)*
Smoker*Drinker*Pot Smoker (t)	0.70 *(0.42)**	-0.14 *(0.90)*
First time smoker during t?	0.21 *(0.57)*	0.19 *(0.69)*
First time drinker during t?	0.24 *(0.55)*	0.18 *(0.72)*
First time pot smoker during t?	0.31 *(0.53)*	0.18 *(0.77)*
First arrested during $t-1$?	0.22 *(0.39)*	-0.57 *(0.66)*
Has smoked entering $t/t+1$?	0.30 *(0.28)*	0.14 *(0.31)*
Has drank entering $t/t+1$?	0.16 *(0.39)*	0.06 *(0.62)*
Has smoked pot entering $t/t+1$?	0.50 *(0.31)*	0.08 *(0.28)*
Has ever been arrested entering $t/t+1$?	0.87 *(0.13)**	0.18 *(0.31)*
Smoker(t)*beer tax(t)	0.33 *(0.87)*	
Smoker(t)*beer tax squared(t)	-0.63 *(0.82)*	
Drinker(t)*beer tax(t)	0.74 *(0.76)*	
Drinker(t)*beer tax squared(t)	-0.71 *(0.86)*	
Pot smoker(t)*beer tax(t)	-1.38 *(0.65)**	
Pot smoker(t)*beer tax squared(t)	1.45 *(0.70)**	
Constant	-3.24 *(0.41)**	-4.25 *(0.41)**

* Significant at 95 percent confidence level. Bootstrapped standard errors are in parentheses.

N (arrest) = 50,584. N (attrition) = 44,315.

NLSY97 Data: 1997-2004 Survey Rounds.

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