Essays in the Economics of Crime and Discrimination

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ABSTRACT

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This dissertation studies marginalized populations in the United States and Western countries, with a broad focus on how legal and social institutions affect individual economic outcomes and wellbeing. The first chapter examines the impacts of incarceration on criminal defendants in Houston, Texas, documenting patterns of worsening criminality, diminished earnings and social detachment after exposure to the prison system. The second chapter develops a framework to consider the interplay between discrimination and concealment of minority status in the context of sexual orientation and shows empirical evidence from the United States on the large magnitudes of concealment costs. The third chapter considers the role of legal recognition of unions in shaping the labor market activity and childbearing decisions of same-sex couples in Sweden, implicitly providing insight into some of the constraints imposed on same-sex couples by widespread exclusion from the institution of marriage throughout the world. Together these essays highlight how public institutions and social systems influence lifecycle outcomes in the population, particularly among minority and other vulnerable groups.

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Introduction

The United States and other Western countries stand out as global leaders in development and human rights (United Nations Development Programme (2014)). Despite this fact, many groups in these societies continue to be marginalized with limited access to the opportunities and institutions that make these countries exceptional. This dissertation focuses on two vulnerable populations in this context: criminal defendants and sexual minorities. My goal in studying these specific groups is to document how legal and social systems shape individuals' economic behavior and wellbeing.

The annual correctional population in United States currently stands at roughly 7 million people, and combined annual federal, state and local justice-related spending amounts to nearly $260 billion per year. In spite of the wide reach and non-trivial costs, limited research provides causal evidence to evaluate whether current policy is in fact welfare enhancing. In "The Criminal and Labor Market Impacts of Incarceration," I help address this gap in the literature through investigating the pre- and post-release impacts of incarceration on defendants using newly collected data from Harris County, Texas. I find that incarceration generates modest incapacitation effects, which are offset in the long-run by an increased likelihood of defendants reoffending after being released. I also show that incarceration reduces post-release employment and wages, increases take-up of food stamps, decreases likelihood of marriage and increases the likelihood of divorce. Together the estimates suggest the use of incarceration for marginal defendants generates substantial welfare loses.

The research design relies on exogenous variation in the extensive and intensive margins of incarceration stemming from the random assignment of defendants to courtrooms. The fact that the courts exhibit multidimensional and non-monotonic sentencing patterns in this context complicates the estimation of treatment effects. Because standard techniques

can result in bias, I develop a new data-driven estimation procedure: first, I allow the impact of court assignment to vary according to observable defendant characteristics, and second, I simultaneously instrument for all endogenous sentencing outcomes. To deal with the introduction of many, weak instruments, I employ the least absolute shrinkage and selection operator (LASSO) to efficiently reduce dimensionality.

The second research agenda I explore in my dissertation examines issues related to discrimination and sexual orientation, with emphasis on how life-cycle outcomes for sexual minorities are shaped by legal and social systems. In "Discrimination with Concealable Characteristics: Evidence and Application to Sexual Orientation in the United States," I study trait concealment in economic models of discrimination using sexual orientation in the United States as a working example. Updating the classic models to allow for concealment provides a formal framework to study a variety of traits including sexual orientation, ethnic identity, religious affiliation and political allegiance. My evidence, which uses a new source of plausibly exogenous variation in male same-sex attraction coupled with measures of county-of-birth tolerance of homosexuality, documents patterns of concealment and its corresponding impact on individual outcomes. I find that men who were more likely to develop same-sex attraction yet born in more homophobic counties were significantly less likely to engage in same-sex cohabitation through age 45 compared to similar men from less homophobic counties. Changes in identity investments (conservative gender ideology and religious adherence) conform with this pattern, and sizable penalties to mental health and educational outcomes accrue to men from more homophobic backgrounds. Labor market outcomes, however, appear to generally be unaffected. I hypothesize that men who experience same-sex attraction yet conceal it are compensated for this choice despite their diminished human capital.

Examining the extension of legal rights to same-sex couples also provides insight into

how such rights shape individual outcomes. In "Same-Sex Partnership for What? Evidence from Swedish Register Data" (with Lina Aldén, Lena Edlund, and Mats Hammerstedt), we study the impact of legal recognition of same-sex couples on their labor earnings and fertility using Swedish longitudinal register data covering the period 1994-2007. In 1994, Registered Partnership for same-sex couples was introduced in Sweden, which conferred almost all rights and obligations of marriage with joint legal parenting (paternity presumption) being added in 2002. We find registered partnership to be important to both gays and lesbians but for different reasons. For gays, resource pooling emerges as the main function of registered partnership. For lesbians, registered partnership appears to be an important vehicle for family formation, especially after the 2002-adoption law. In contrast to heterosexual couples, we find no evidence of specialization among lesbians. This is noteworthy given comparable fertility effects of registered partnership and the fact that the lesbian couples were less assortatively matched than the heterosexual couples, two factors commonly believed to promote specialization.

Chapter 1

The Criminal and Labor Market Impacts of Incarceration

Michael Mueller-Smith

The United States currently has the highest incarceration rate in the world (Walmsley (2009)), a consequence of three decades of dramatic growth in the prison population since the late 1970s (Carson (2013)). Over this same time period governmental expenditures on police protection, judicial and legal systems, and corrections also surged (Bureau of Justice Statistics (1980) and Kyckelhahn (2013)). Recent estimates indicate that the annual U.S. correctional population included over 7 million adults (Glaze and Herberman (2013)), and combined federal, state and local expenditures on justice-related programs topped $260 billion per year. Despite the reach and cost associated with these changes to criminal justice policy, causal evidence on how this use of incarceration has impacted the population remains scarce (see Donohue III (2009)).

To help address this gap in the literature, I investigate the impacts of incarceration using original data from Harris County, Texas. The new data is comprised of over 2.6 million criminal court records accounting for 1.1 million unique defendants, which I collected and processed into an empirical dataset. It captures the universe of misdemeanor and felony criminal charges between 1980 and 2009 regardless of final conviction status. What makes the data especially unique is the ability to link the court records to a variety of other sources of administrative data including state prison and county jail data, unemployment insurance wage records, public assistance benefits, marriage and divorce records as well as future criminal behavior using individual identifiers available in the data. Taken together, the combined data allows me to estimate impacts on a broad range of policy-relevant outcomes, promoting a better understanding of the potential mechanisms underpinning the treatment effects and providing for a more complete cost benefit analysis of incarceration.

The research design leverages the random assignment of criminal defendants to courtrooms as a source of exogenous variation in both the extensive and intensive margins of incarceration. The courts are staffed by judges and prosecutors who differ in their propen-

sity to incarcerate. As a result, which courtroom a defendant is randomly assigned to strongly predicts whether he will be incarcerated and for how long.[1] This increasingly popular identification strategy has been used in a number of applications where judges, case workers, or other types of programs administrators are given discretion on how to respond to a randomly assigned caseload.[2]

The application considered in this paper is moderately more complex than standard uses of this research design. Sentencing takes on multiple dimensions (e.g. incarceration, fines, drug treatment, etc.) and judges display non-monotonic tendencies (e.g. a judge may incarcerate drug offenders at a relatively higher rate but property offenders at a relatively lower rate). Since failure to account for these features of the data could lead to violations of the *exclusion restriction* and *monotonicity assumption*, a new estimation procedure is developed.[3] In this new approach, I first construct instruments for each observed aspect of sentencing, not just incarceration, in order to control for court tendencies on non-focal sentencing dimensions. I also relax the first stage equation to allow the impact of court assignment on sentencing outcomes to flexibly respond to observed defendant characteristics. Because this second modification can generate many instruments due to the curse of dimensionality, the least absolute selection and shrinkage operator (LASSO) is used in conjunction with cross validation as a data-driven tool to achieve disciplined dimension reduction without skewing statistical testing.

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[1]Even though parole boards may adjust some sentences ex-post, my evidence indicates that the courts exert influence over actual time served.

[2]For studies specifically related to incarceration, see Kling (2006), Di Tella and Schargrodsky (2004), or Aizer and Doyle (2013). For research in other fields, see Doyle [2007, 2008], Autor and Houseman (2010), Belloni, D. Chen, Chernozhukov, and Hansen (2012), Munroe and Wilse-Samson (2012), Doyle, Graves, Gruber, and Kleiner (2012), French and Song (2012) Maestas, Mullen, and Strand (2013), Autor, Maestas, Mullen, and Strand (2013) and Dahl, Kostol, and Mogstad (2013).

[3]Prior researchers have acknowledged the potential for these features to also affect their findings, but data limitations have generally limited their ability to address these concerns in any formal way.

My empirical findings indicate that incarceration for marginal defendants is less attractive from a policy perspective than has been shown in prior work. I measure modest incapacitation effects while defendants are in jail or prison: felony defendants are 6 percentage points less likely to be charged with a new criminal offense while incarcerated. This benefit, however, is offset by increases in post-release criminal behavior: each additional year that a felony defendant was incarcerated increases the probability of facing new charges post-release by 5.6 percentage points per quarter. What is particularly concerning about these results is that the incapacitation effect is disproportionately driven by misdemeanor charges, while the post-release criminal behavior shows mainly increases in felony offenses. Partially driving this result is a pattern of former inmates being charged with new crime types. In particular, I find that former inmates are especially likely to commit more property (e.g. theft or burglary) and drug-related crimes after being released, even if these crimes were not their original offenses.

In contrast with prior work, I find strong evidence that incarceration has lasting negative effects on labor market outcomes after defendants have been released. I find that each additional year of incarceration reduces post-release employment by 3.6 percent points. Among felony defendants with stable pre-charge earnings incarcerated for one or more years, reemployment drops by at least 24 percent in the 5 years after being released. Misdemeanor defendants show a small increase in take-up of cash welfare payments, and felony defendants show increases in Food Stamps benefits, which provide further evidence of lasting economic hardship post-release.

The impacts of incarceration extend beyond recidivism and labor market outcomes. Incarceration appears to negatively impact family formation and stability as measured through marriage and divorce activity. While incarcerated, young felony defendants exhibit significantly lower rates of marriage that are not compensated post-release indicating a net decline

in marriage rather than a temporal shift. Further supporting this conclusion, I find that divorce rates among older felons increase while in prison and post-release.

Using these new estimates, I reevaluate the welfare impacts of incarceration. Because I cannot measure general deterrence effects in my research design, the cost benefit exercise is partial in nature and only accounts for the administrative expenses, criminal behavior effects and economic impacts associated with the defendant's own outcomes. Using the most conservative estimates, I find that a one-year prison term for marginal defendants decreases social welfare by $56,200 to $66,800 of which negative impacts to economic activity account for 41 to 48 percent of overall costs. In order for this sentence to be neutral in social welfare terms, a one-year prison term for a marginal (low-risk) offender would need to deter at least 0.4 rapes, 2.2 assaults, 2.5 robberies, 62 larcenies or 4.8 habitual drug users in the general population.[4]

The remainder of this paper organized into 8 sections. Section 1.1 briefly discusses the literature. Section 1.2 describes the setting of this study in Harris County, Texas. Section 1.3 documents the sources of data. Section 1.4 illustrates how multidimensional and non-monotonic sentencing patterns create opportunities for bias, and Section 1.5 proposes an alternative estimation strategy to address these concerns. Section 1.6 describes the panel model used in this study to estimate both the contemporaneous and post-release effects of incarceration. Section 1.7 reports the empirical results and discusses the robustness exercises. Section 1.8 conducts a cost benefit exercise using the newly estimated parameters. Section 1.9 concludes.

[4]This ignores potential intangible benefits of incarceration that might arise if victims gain utility from seeing their offender punished.

1.1 Related literature

Economic research on the incarceration has primarily focused on measuring its impacts on future criminal behavior. Incapacitation, in particular, has received significant focus. Credible estimates range from 2.8 to 15 crimes prevented per year of incarceration (see Levitt (1996), Owens (2009), Johnson and Raphael (2012), Buonanno and Raphael (2013), Kuziemko (2013)). Lower estimates generally rely on inmate records that are matched to their own future criminal activity, while larger estimates allow for incapacitation effects to also measure potential multiplier effects in the population. The potential for diminishing returns to incarceration as incarceration rates have increased over time has also been put forth as a potential explanation for the variation in the estimates (see Liedka, Piehl, and Useem (2006), Johnson and Raphael (2012)).

Existing work presents conflicting views on the degree to which general and specific deterrence inform criminal decision making. Poor prison conditions and three strikes laws appear to discourage criminal behavior (see Katz, Levitt, and Shustorovich (2003) and Helland and Tabarrok (2007)), yet sharp changes in the severity of sentencing at age of maturity and actual experiences of incarceration seem to have zero or positive effects on recidivism (see Lee and McCrary (2009) and McCrary and Sanga (2012), K. Chen and Shapiro (2007), Di Tella and Schargrodsky (2004), Green and Winik (2010), Nagin and Snodgrass (2013)). Perhaps at issue is the salience of the criminal penalty. Drago, Galbiati, and Vertova (2009)'s analysis of a collective pardon in Italy that allowed inmates to be released under the explicit condition that any future reoffense would reinstate the remainder of their original sentence finds that each additional month carried over to future potential sentencing decreases future criminal activity by 0.16 percentage points. Conversely, when offenders appear to get off easy on the terms of their original sentence through either early release or changes in sen-

tencing guidelines, recidivism rates tend to increase (see Maurin and Ouss (2009), Bushway and Owens (2012), Kuziemko (2013)).

An emerging agenda has begun to show that peer effects play an important role in criminality. Bayer, Hjalmarsson, and Pozen (2009) and Ouss (2013) both find evidence that inmate interactions influence their post-release criminal activity through encouraging new criminal patterns. Drago and Galbiati (2012) similarly find that inmates stimulate the criminal behavior of their non-incarcerated peers after being released. Yet Ludwig and Kling (2007)'s evaluation of the Moving to Opportunity experiment, on the other hand, found no measured correlation between the future criminality of the relocated study participants and the ambient levels of crime in their destination neighborhoods.

Data constraints have limited the ability of researchers to study outcomes beyond criminal activity. As a result, there is less rigorous evidence on the non-criminal effects of incarceration (see Donohue III (2009) for discussion). Several studies consider whether incarceration and criminal history generate stigma in the labor market (Pager (2003), Bushway (2004) and Finlay (2009)). Another group of studies use panel data with individual fixed effects to evaluate whether income increased after being released from incarceration (see Grogger (1996), Cho and Lalonde (2005), Western (2006), Sabol (2007), Pettit and Lyons (2007) and Raphael (2007)).

Two recent studies in particular are closely related to this paper. First, Kling (2006) studies the impact of incarceration length on labor market outcomes by linking inmate records of state and federal prisoners from Florida and California, respectively, to their labor market outcomes. He finds no evidence that longer prison sentences adversely affected labor market outcomes. His conclusions were based on panel data with individual fixed effects and an instrumental variable strategy using the average incarceration length for each defendant's randomly assigned federal court judge as an instrument for his actual incarceration length.

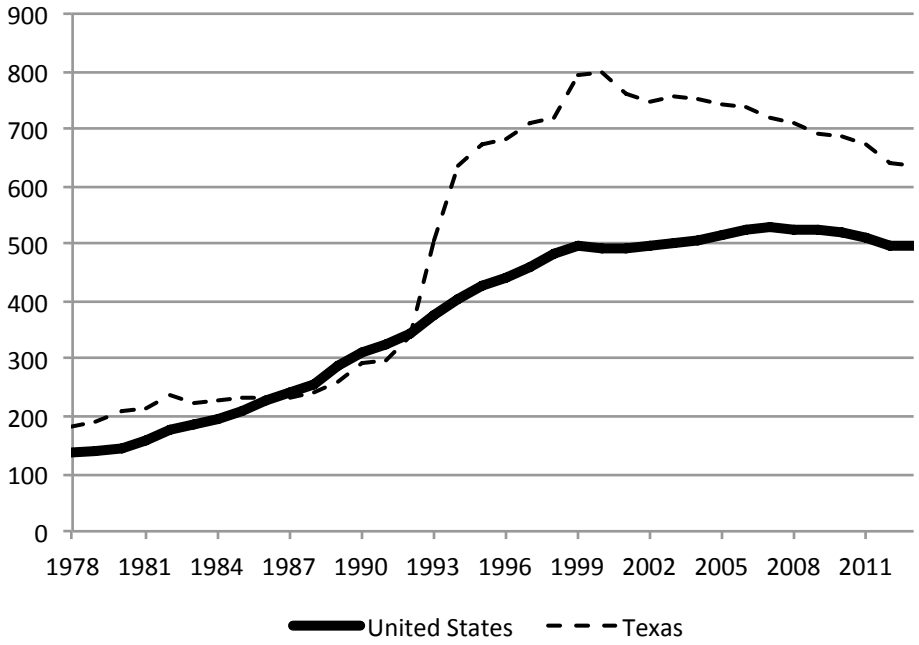
Second, Aizer and Doyle (2013) study the impact of incarceration among juvenile offenders in Chicago also using an instrumental variable strategy based on randomized judges. While their data does not allow them to evaluate labor market impacts, they find that being sentenced to a juvenile delinquency facility reduces the likelihood of high school graduation and increases the likelihood of adult incarceration. Since these two studies evaluate different populations (i.e. adult versus juvenile offenders) and margins of incarceration (extensive versus intensive) their disparate findings are not necessarily inconsistent. For instance, incarceration may have a particularly harmful effect on youth who are still in the midst of building their human capital. The stark divergence in their findings, however, is still surprising and raises the need for further investigation.

1.2 The Harris County Criminal Justice System

The setting for this study is Harris County, Texas, which includes the city of Houston as well as several surrounding municipalities. The Houston metropolitan statistical area has the fifth largest population in the United States and encompasses a geographical area slightly larger than the state of New Jersey. The population is economically and demographically diverse, which is reflected in the observed population of criminal defendants.

Texas is known for being particularly tough on crime. Figure 1.1 plots the imprisonment rate per 100,000 residents in the United States and Texas. Throughout the 1980's, Texas was actually close to the national trend due to overcrowding within the Texas prison system, but in the early 1990's, the newly elected governor, Ann Richards, began a massive prison expansion program relaxing the capacity constraint and resulting in sustained higher imprisonment rates. The widespread use of incarceration in Texas will imply that defendants on the margin of incarceration may be less dangerous than marginal defendants in other

Figure 1.1: National versus Texas imprisonment rate per 100,000 residents



Source: *Bureau of Justice Statistics, Corrections Statistical Analysis Tool.*

settings. This will tend to tip the scale in favor of finding welfare losses in this context, and the results should be interpreted with caution when applying them to other settings. But, given that Texas accounts for roughly 12 percent of the non-federal institutional population, a group that is understudied in general, this population is important to study in and of itself.

Two court systems operate in Harris County: the Criminal Courts at Law (CCL) and the State District Courts (SDC). The fifteen CCLs have jurisdiction over cases involving misdemeanor charges and serve slightly more than 4,500 cases per court per year.[5] Typical cases include traffic violations, non-habitual driving while intoxicated offenses, minor possession of marijuana, larceny of items worth less than $1,500, and non-aggravated assault. The

[5]In 1980, only 10 CCLs were active. Additional courts were added in 1983, 1985 and 1995.

twenty-two SDCs litigate cases involving felony charges and serve roughly 1,800 cases per court per year.[6] Typical cases include possession of controlled substances, drug manufacture and distribution, larceny involving more than $1,500 in stolen property, residential or vehicular burglary, aggravated assault as well as more heinous offenses like murder, rape and child abuse. The felony and misdemeanor courts are administratively segregated yet physically co-located at the Harris County Criminal Justice Center (1201 Franklin St., Houston, TX 77002).[7]

Table 1.1 shows summary statistics for misdemeanor and felony defendants. Both the misdemeanor and felony caseloads are predominantly male with mean age around 30 years old. Individuals facing misdemeanor charges have been charged with and convicted of fewer previous crimes compared to felony defendants; 60 percent of the misdemeanor cases are first-time offenders while only 45 percent of the felony caseload are. The most common crime types for misdemeanor cases are driving while intoxicated (DWI), other traffic related offenses, larceny involving less than $1,500 worth of property and minor possession of marijuana. For felony cases, the most common crimes are more serious drug possession (in terms of quantity or seriousness of the illicit drugs), more costly property crimes, and aggravated assault.

Roughly equal shares of non-Hispanic Caucasian, non-Hispanic African American and Hispanic defendants are represented in both caseloads. Misdemeanor cases have a relatively larger proportion of non-Hispanic Caucasians, whereas felony cases are more likely to be African Americans. A number of other physical descriptors are available in the data including: skin tone, height, weight, body type, eye color and hair color. These are mainly

[6] In 1980, only 18 SDCs were active. Additional courts were added in 1982 and 1984.

[7] In addition to the Criminal Courts at Law and the State District Courts, there are also Justices of the Peace who rule on misdemeanor level C charges, and Federal District Courts for the Southern District of Texas which address federal crimes. In addition, minor offenders are generally prosecuted through the Family District Court system. None of these institutions are considered in the analysis and so they are not addressed at length.

recorded in the event a warrant needs to be issued for the defendant. Coverage of these variables is much more reliable for cases from 1985 and onwards when record keeping in the court files improved.

When criminal charges are filed against a defendant in Harris County, his case is randomly assigned to a courtroom.[8] Randomization is viewed as an impartial assignment mechanism for defendants and an equitable division of labor between courtrooms. Up to the late 1990s, assignment was carried out using a bingo ball roller; this was later transitioned to a computerized system for automatic random case assignment. In order to ensure the case allocation mechanism is not manipulated by internal actors, the Harris County District Clerk's office, which is both physically and administratively segregated from the criminal court system, is solely responsible for courtroom assignment.

When a case is randomly assigned to a courtroom, a defendant is assigned to the jurisdiction of a specific judge and team of assistant district attorneys. The judges are elected to serve a specific bench and are responsible for presiding over all cases assigned to their courtroom while in office. Elections occur every two years, and the vast majority of judges are successfully reelected. As a result, a defendant's initial court assignment will likely determine the judge who presides over the entirety of his trial.

The Harris County District Attorney's office stations a team of three assistant district attorneys (one chief ADA and two subordinate ADAs) to each CCL and SDC. This team prosecutes all cases assigned to their courtroom with broad discretion over how to divide the workload within the team and the desired sentencing outcome.[9] The teams work in their

[8]Two types of cases do not undergo random assignment. If a defendant is already on probation from a specific court, his new charges will automatically be assigned to that same courtroom. In addition, charges at the Capital Felony level are not randomly assigned because they generally require significant resources to adjudicate. Because neither of these types of charges are randomly assigned, they are dropped from the analysis.

[9]Interviews with the District Attorney's office revealed that prosecutors' conviction rates or trial outcomes

Table 1.1: Characteristics of Harris County's Criminal Courts at Law and State District Courts' caseloads, 1980-2009

Defendant Characteristics	Criminal Court at Law (Misdemeanor Offenses)	State District Court (Felony Offenses)
Male	0.78	0.81
Age	29.84	30.26
First time offender	0.61	0.45
Total prior felony charges	0.44	0.93
Total prior misdemeanor charges	0.80	1.20
Type of criminal charge		
Driving while intoxicated	0.25	0.04
Traffic	0.11	0.01
Drug possession	0.11	0.26
Drug manufacture or distribution	0.00	0.09
Property	0.23	0.31
Violent	0.09	0.13
Median duration of trial (months)	1.35	2.14
Race/Ethnicity		
Caucasian	0.39	0.30
African American	0.31	0.46
Hispanic	0.29	0.23
Other	0.01	0.01
Skin tone		
Fair	0.14	0.13
Light	0.06	0.05
Light brown	0.07	0.06
Medium	0.22	0.22
Medium brown	0.09	0.10
Olive	0.03	0.03
Dark	0.04	0.07
Dark brown	0.09	0.14
Black	0.04	0.07
Missing	0.21	0.13
Height (in.)	68.13	68.4
Weight (lbs.)	169.82	172.51
Body type		
Skinny, light	0.11	0.13
Medium	0.60	0.66
Heavy, obese	0.08	0.09
Missing	0.21	0.13
Eye color		
Green, blue	0.18	0.15
Brown, black	0.65	0.75
Missing	0.17	0.10
Hair color		
Blonde, red	0.07	0.06
Black, brown	0.75	0.83
Bald, grey	0.02	0.02
Missing	0.17	0.10
Total cases	1,449,453	775,576

Source: Author's calculations using Harris County District Clerk's criminal court records.
Notes: Calculations do not include sealed court records, juvenile offenders or defendants charged with capital murder.

assigned courtroom until staffing needs or promotions reallocate them. Generally, ADAs serve anywhere between a couple months to several years in the same courtroom before receiving a new assignment.

Overall, there were 111 elected judges operating in the Harris County criminal court system between 1980 and 2009. In this same time period, 1,262 individuals worked as assistant district attorneys (ADAs) in Harris County. Among these, 923 worked at some point in their career in the felony courts while 1,154 spent time in the misdemeanor court system. Figure 1.2 shows the median tenure per assignment among judges and ADAs staffed in a given court per year. Due to the infrequency of elections and high likelihood of re-election, the median judge in both court systems spent the entire year in her courtroom. ADAs working in the state district court exhibit a high degree of stability in their staffing, with the median chief ADA spending generally over 300 days in his court each year. First and second assistant prosecutors generally worked between 150 and 250 days in a given court. The misdemeanor courts also exhibit the same pattern of judge stability, with the median judge spending the entire year in her courtroom. ADAs, however, have a higher degree of turnover with all team members generally spending between 75 and 200 days in their respective courtrooms.

Defendants court outcomes heavily depend of the discretion of the specific judge and prosecutor assigned to their case in Texas. Sentencing guidelines established by the Texas Penal Code (see Appendix A.1) provide broad recommendations on maximum and minimum sentencing for defendants based on the degree of criminal charges. For instance, a second degree felony can receive anywhere between two and twenty years incarceration in state prison, while a class A misdemeanor can receive up to a year in county jail. Despite the

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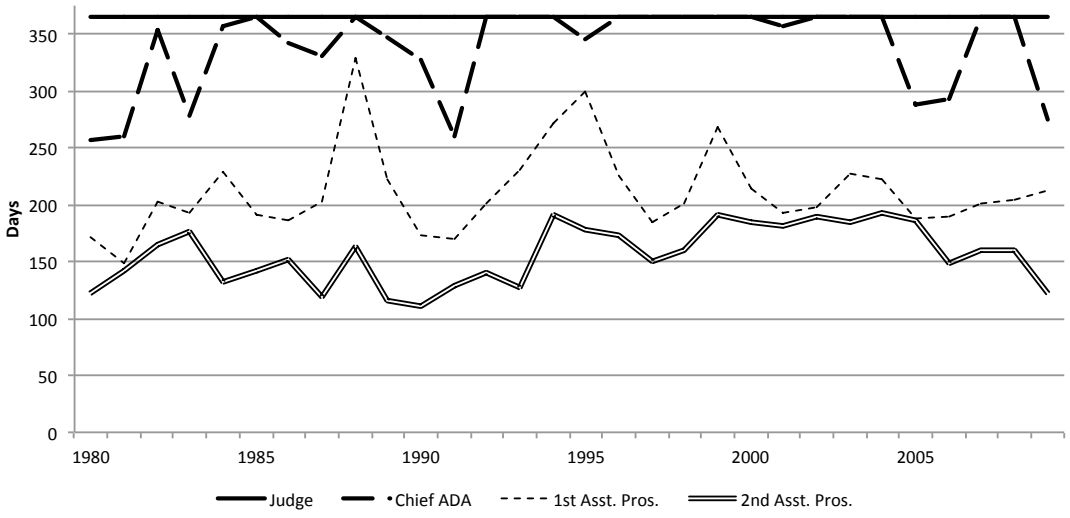
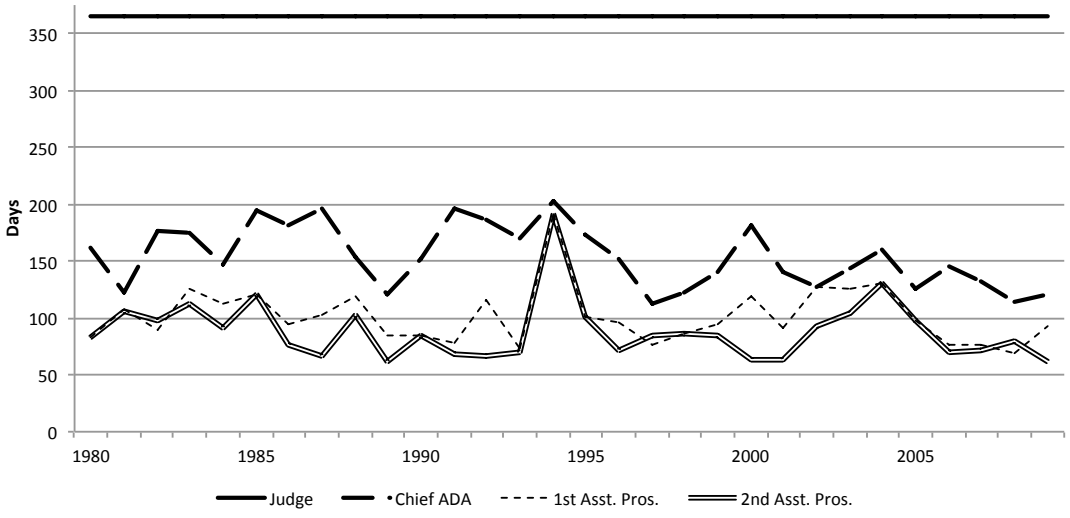
are not routinely monitored for performance evaluation. Instead, their ability to consistently "clear" cases from the docket in a timely manner determines their standing in the department.

Figure 1.2: Median days staffed in a specific court per year for judges and assistant district attorneys between 1980 and 2009

(a) State District Courts (felony offenses)



(b) Criminal Courts at Law (misdemeanor offenses)



Source: *Author's calculations using the Harris County Criminal Courts at Law, State District Courts, and District Attorney's staffing records.*

potential for mandatory minimums, the court can choose to suspend any sentence of 10 years or less in favor of probation for defendants convicted of non-aggravated felonies or misdemeanors. This allows the court to release defendants to community supervision and forgo incarceration altogether under terms similar to parole.

Texas subscribes to a combination of *determinate* and *indeterminate* sentencing systems depending on the degree of the criminal charge. Crimes that fall under determinate sentencing result in incarceration sentences that must be served in full regardless of behavioral considerations; the only way to modify these sentences is by court order. In contrast, indeterminate sentences can be changed after the fact by the Texas Department of Criminal Justice and the Texas Board of Pardons and Parole after taking into account an inmate's behavior and his participation in education and training programs. Sentence adjustments come in the form of granting "good time" credits to inmates and permitting supervised early release through parole. The court retains broad influence over incarceration duration however through establishing the maximum sentence length and, as a result, a corresponding minimum sentence length due to Texas's "truth in sentencing" law. Truth in sentencing requires that inmates serve a minimum percentage of their sentence prior to being eligible for early release.[10]

Several additional features give judges and prosecutors broad influence over court outcomes. These include determining the admissibility of evidence, prosecution strategy, sentencing enhancements and plea bargain terms. Local judges also play an important role in the indigent defense system. Until 2011 when a public defenders office was first opened in Houston, a defendant who could not afford legal representation would be appointed a lawyer by his trial judge. Bright (2000) describes judges in Harris County as "treating the

[10]The specific percent of the sentence that must be spent in jail or prison depends on the laws in effect when the inmate committed his offense and what type of crime he was convicted of. It can range between 25 and 100 percent.

appointment of counsel to defend poor defendants as political patronage and [...] assigning lawyers not to provide zealous advocacy but to help move their dockets." In fact, popular press in the early 2000's documented cases in Harris County where appointed counsel were under-qualified, intoxicated, and/or asleep at the time of trial (see Rimer and Bonner (2000)).

1.3 Sources of Data and Matching Methods

This project uses several sources of administrative data. Information on court assignment, defendant and crime characteristics as well as sentencing outcomes were acquired from the Harris County District Clerk. Initial filings of felony and misdemeanor charges between 1980 and 2009 are included in the data regardless whether the case resulted in a guilty or innocent verdict. Cases sealed to the public by order of the court, which account for less than half of a percentage point of the overall caseload, not were included in the data. Criminal appeals cases were also not included in the data.

For the purpose of the analysis, defendants charged with multiple criminal offenses or recharged for the same crime after a mistrial were collapsed to a single observation. For these cases, only the earliest filing date and original sentencing outcomes were retained. For all defendants, sentencing modifications were eliminated from the data (e.g. a defendant who violated the terms of his probation after three years and was incarcerated as a result was only coded as receiving probation in his original sentencing).

Administrative identifiers in the court data link defendants to their full historical criminal record in Harris County, allowing the research to evaluate local recidivism outcomes. Archival research gathered judge tenure and assistant district attorney staffing documents from the courts and transcribed the information into an electronic database. Judges and assistant

district attorneys were then mapped to criminal court cases using the defendant's filing date and assigned court number. Data on actual incarceration spans between 1978 and 2013 were acquired through Public Information Act requests from the Texas Department of Criminal Justice for state prisons and from the Harris County Sheriff's Office for the Harris County Jail, and matched using the defendant's full name and date of birth.

Quarterly unemployment insurance wage records for the entire state of Texas between 1994 and 2012 were accessed through a data sharing agreement with the Texas Workforce Commission. Monthly Food Stamps and Temporary Assistance for Needy Families benefits between 1994/1992 and 2011 were accessed through a data sharing agreement with the Texas Health and Human Services Commission. Matching between the various data sources was based on a combination of full name, sex, exact date of birth and social security number depending on what variables were available in each specific dataset.

Public marriage and divorce indices were also collected from the Texas Department of State Health Services. Unfortunately, this data is only identified at the full name and age at marriage or divorce level, making it prone to mismatch. Incorrect data linkages should be orthogonal to courtroom assignment which should lead to classic measurement error and push estimated coefficients towards zero.

1.4 The Complications of Multidimensional and Non-monotonic Sentencing

To evaluate the impact of incarceration, this study relies on exogenous variation in sentencing outcomes attributable to random assignment of defendants to criminal courts. Prior work

using this research design has generally been formalized using the following two equations:

$$Y_i = \beta_0 + \beta_1(X_i)D_i + \beta_2 X_i + \epsilon_i \ , \tag{1.1}$$

$$D_i = \gamma_0 + \gamma_1 J_i + \gamma_2 X_i + \nu_i \ , \tag{1.2}$$

where,

$$E[\epsilon_i, \nu_i | X_i] \neq 0 \ , \ E[\epsilon_i, J_i | X_i] = 0 \text{ and } \gamma_1 \neq 0 \ .$$

In this notation, Y_i is the outcome variable, D_i is a criminal sentence (such as an indicator variable for being incarcerated or a continuous measure of the duration of incarceration), X_i is the observed defendant characteristics and J_i is a vector of dummy variables for the defendant's randomly assigned judge.[11] The program effect can potentially be heterogeneous so $\beta_1(X_i)$ is allowed to depend on defendant traits. Non-zero coefficients in γ_1 indicate differences in average sentencing outcomes between judges who serve statistically equivalent populations. Such differences are often motivated on the basis that some judges are thought to be "tough" while others are "easy" on defendants.

Two additional assumptions are required in order to achieved unbiased results (see Imbens and Angrist (1994), Angrist, Imbens, and Rubin (1996)). First, the exclusion restriction requires that $E[Y_i|D_i, X_i, J_i] = E[Y_i|D_i, X_i, J_i']$. This means that judge assignment can only impact the final outcome through its influence on the criminal sentence. The second requirement is that the data must satisfy a monotonicity assumption: $\{E[D_i|X_i, J_i = j] \geq E[D_i|X_i, J_i = k] \ \forall i \text{ or } E[D_i|X_i, J_i = j] \leq E[D_i|X_i, J_i = k] \ \forall i\} \ \ \forall j, k.$

[11] In the specific context of this study, random court assignment results in both a random judge as well as a random team of assistant district attorneys. For the ease of notation and to remain consistent with the existing literature, however, I proceed using only judges in the model but knowing that they are a placeholder for all influential actors who are attached to a specific courtroom.

This means that defendants assigned to judges with higher overall incarceration rates must also be at weakly higher risk for incarceration if assigned to their caseload.

The parsimony of this standard model makes it quite appealing. The source of identification is intuitive, and the estimation is generally straightforward, particularly in settings where the researcher is constrained by data availability. The fact that my data exhibits multidimensional and non-monotonic sentencing patterns, however, limits the plausibility of satisfying the necessary assumptions for unbiasedness. Instead, application of the standard methods in this context results in two distinct biases which for the sake of clarity I term *omitted treatment bias* and *non-monotonic instruments bias*. The nature of these biases are described below.

Omitted treatment bias

In the Texas criminal justice system, judges and prosecutors have influence over several aspects of trial outcomes (e.g. guilt or innocence, incarceration versus probation, duration of punishment, amount of fine, etc.). I may, however, only be interested in a subset of the full range of sentencing outcomes just as incarceration is the focus of this present study. To distinguish between these sets, I define $D_i^f \subset D_i$ as the *focal* set of sentencing outcomes, while the remaining elements are the *non-focal* set D_i^n.

Omitted treatment bias is the result of neglecting of D_i^n when estimating the causal effect of D_i^f on Y_i. Judicial tendencies on focal and non-focal sentencing outcomes may be correlated leading to violations of the exclusion restriction. For instance, if judges who have higher than average incarceration rates also are more likely to impose fines (and the estimated model omits fines), the estimated impact of incarceration will capture a weighted sum of the combined effect of incarceration and fines. It is unrealistic to think that researchers ever observe the full set of potential treatments a defendant may have received. For instance, a

judge may speak sternly to the defendant, which would likely not be measured in the data. But, to the extent that unmeasured treatments play minor roles in producing final outcomes and are uncorrelated with other judicial tendencies, the potential bias is minimal.[12]

Omitted treatment bias can be easily avoided by estimating to the full model, inclusive of both D_i^f and D_i^n. In this scenario, both the focal and non-focal elements of D_i would be simultaneously instrumented using random assignment of judges as the source of exogenous variation. This would ensure that, for instance, the impact of incarceration is identified off of judges who tend to incarcerate relatively more after accounting for their other sentencing tendencies. This approach, however, may undermine joint tests of the first stage and weak instrument robust inference for the focal variables since some non-focal sentencing outcomes may not exhibit sufficiently strong differences between judges.

To solve this problem, this study proposes constructing predicted values of $E[D_i^n|J_i, X_i]$ and adding them to the second stage equation to eliminate the omitted treatment bias. In practice, this entails estimating the first stage equation for each element of D_i^n, and then adding the predicted values to the second stage equation as reduced form controls.

Non-monotonic instruments bias

Non-monotonic sentencing patterns also create opportunities for bias. Some judges, for instance, are observed to have higher than average incarceration rates for specific subsets of their caseload like drug offenders while also exhibiting lower than average incarceration

[12]Compared to other settings, like research on the impact of going to a better school where treatments may include complex interactions between various school inputs and peer interactions, the criminal justice context relatively straightforward with respect to what the major components of D_i should include. These are: incarceration status and length, fine status and amount, probation status and length, and less common enrollment in alternative sentencing programs like electronic monitoring, drug treatment, boot camps, or driver's education. Since there is little to no interaction among defendants in the court room setting, there is minimal concern for peer influence at this stage.

rates for other groups like property offenders. This creates a situation where it is no longer necessarily true that being randomly assigned to a judge with a higher overall rate of incarceration actually increases the probability of incarceration for every defendant. Violations of the monotonicity assumption lead to an unsigned bias making it difficult to determine if the estimates under- or over-estimate the true effect.

To the extent that this complexity responds to observed characteristics, it is not insurmountable. The standard approach might result in biased estimates, but that is a consequence of the fact that the standard model is misspecified in this context. Judges form expectations as to what will best maximize their objective function given the facts of the case before them and their own subjective information. They don't simply see defendants interchangeably, but respond to the specific context of each case. This is not saying that judges are inconsistent in their application of the law; instead, their decision rules are just more complex that mean shifts between judges.

The alternative first stage equation I propose for this sentencing model is:

$$D_i = \Gamma_0 + \Gamma_1(X_i)J_i + \Gamma_2 X_i + \nu_i \ . \tag{1.3}$$

In contrast to Equation 1.2, Equation 1.3 allows judicial preference to flexibly adjust according to defendant characteristics. The implication is that the monotonicity of the impact of judge assignment no longer need hold across all defendants, but instead the impact of judge assignment must only remain consistent among a group of peers with similar observable characteristics (e.g. Caucasian male drug offenders or African American females convicted of driving while intoxicated).[13] While the modified approach adds complexity to the model,

[13]A more general model could adopt a random effects framework to account for unobserved variation as well (see Heckman and Vytlacil (1998) and Wooldridge [1997, 2003]), but is beyond the scope of this study.

it relaxes the assumptions necessary for unbiased results.

Empirical examples

To illustrate how the multidimensional and non-monotonic sentencing affect my estimates, I construct two examples using actual court data from Harris County, TX. The first example considers the impact of accounting for additional degrees of treatment while the second demonstrates how non-uniformities in sentencing can generate bias. The estimates shown in these examples are given to illustrate the features of the data; more refined estimates using the full sample of data are reserved for Section 1.7.

The first example estimates the "causal" impact of incarceration on one year recidivism rates in the felony caseload. The analysis uses all individuals who were charged with felony crimes between 2005 and 2006, and their court sentence is instrumented using their randomly assigned judge. While the coefficient of primary interest is a dummy variable measuring whether or not a defendant was incarcerated for any period of time, each specification progressively adds more controls for non-focal dimensions of sentencing to the model. The controls in this example are constructed using the judge-specific mean of each non-focal court outcome. The results are shown in Table 1.2.

In the first specification, the impact of incarceration is estimated without controlling for judicial tendencies on any other court outcomes. The estimated coefficient is positive and significant indicating that defendants assigned to incarceration are 6 percentage points more likely to be charged with a new crime in the year after charges were filed. The second column adds judicial tendencies on incarceration length to the model and the third specification adds judicial tendencies for guilt, deferred adjudication of guilt, fine status and amount as well as probation status and length.

The second and third specifications also produce positive and significant coefficients,

Table 1.2: Estimating "the causal effect" of incarceration in the presence of omitted treatment bias

	New criminal charges within 1 year		
	(1)	(2)	(3)
Sentenced to Incarceration	0.06**	0.15***	0.26***
	(0.03)	(0.03)	(0.07)
Total Observations	66,335	66,335	66,335
Judicial Tendency Controls:	No controls	Incar. length	Incar. length, guilt, def. adj. of guilt, fine status/amount, probation status/length
Testing equality of coefficients:	(1) = (2)	(1) = (3)	(2) = (3)
Chi-squared test	44.32	9.06	3.35
P-value	0.00	0.00	0.07

Source: Harris County District Clerk's criminal court records (cases filed in State District Court between 2005 and 2006).
Notes: "Sentenced to incarceration" is instrumented using fixed effects for the assigned judge at the time of charge. *** p<0.01, ** p<0.05, * p<0.1.

but now the estimated impact of being incarcerated increases dramatically, up to 125 to 300 percent larger. The smaller coefficient observed in the first specification is due to the fact that judges who tend to have relatively higher rates of incarceration also tend to exhibit longer average incarceration lengths in their caseloads. Judicial tendencies on incarceration length are negatively correlated with short run recidivism (not shown), which results in coefficient in specification (1) being negatively biased. Similar mechanisms explain the difference between specifications (2) and (3). Statistical tests reject the null hypothesis that the estimated effects are equal.

To illustrate the consequences of non-monotonic instruments bias, I construct an empirical example using two years of the misdemeanor court data. The exercise uses data for two courtrooms between 2005 and 2006. For the entirety of the period, each court is served by a single elected judge (one Democrat, one Republican) and the cases are randomly assigned. To simplify the example, I have limited the caseload composition to two prominent crime types: driving while intoxicated and possession of marijuana. The total number of

Table 1.3: Incarceration rates per judge, overall and by crime type

	Incarceration rate			Caseload size		
	DWI & Drug Poss.	DWI	Drug Poss.	DWI & Drug Poss.	DWI	Drug Poss.
Judge A	65.7%	66.6%	64.6%	2,271	1,274	997
Judge B	64.8%	59.1%	71.9%	2,277	1,261	1,016
Difference	0.9%	7.5%	-7.3%			

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).

observations is 4,548 criminal cases.

Table 1.3 shows the incarceration rates by judge as well as their corresponding crime-specific incarceration rates. Judge A exhibits a higher overall incarceration rate and defendants randomly assigned to this courtroom are roughly 1 percentage point more likely to be incarcerated. This aggregate statistic, however, masks substantial subgroup variation. When looking by crime type, Judge A remains the tougher judge for individuals charged with driving while intoxicated (+7.5 percentage points); this relationship, however, is reversed for individuals charged with marijuana drug possession, where now Judge A is 7.3 percentage points less likely incarcerate relative to Judge B.

Knowing that the impact of judge assignment depends on crime type, I compute four estimates of "the causal effect" of incarceration on short-run recidivism.[14] In the first estimation, I use an indicator variable for judge assignment as an instrument for incarceration status in the overall caseload. In the second and third estimations, I continue to use an

[14]The maximum duration of incarceration in the county jail system is 1 year, so this should capture the short-run net effects of incarceration on criminal activity collapsing both the incapacitation and post-release effects. To the extent that these two judges adjust other dimensions of sentencing (e.g. sentence length, fines, or use of other alternative sentencing programs), these estimates will be biased. The purpose of this example is not to improve our understanding of the relationship between incarceration and recidivism, but instead illustrate the consequences of failures in monotonicity in a straightforward example. More refined estimates on the impact of incarceration on future criminal behavior are presented in Section 1.7.

indicator variable for judge assignment as an instrument for incarceration status, but I split the sample by crime type and estimate the impact separately. In the final estimation, I use interactions between judge assignment and crime type as instruments for incarceration.

The results of this exercise are presented in Table 1.4. When I use judicial assignment as an instrument in the overall caseload, ignoring potential crime type interactions but still controlling linearly for type of crime, I find a negative correlation between incarceration and recidivism within one year. The estimate is noisy and I cannot reject the null hypothesis that there is zero correlation. In columns 2 and 3, where I separate by subgroup, the estimated coefficients for both subgroups are positive and significant however. For defendants charged with driving while intoxicated, I find that being sentenced to incarceration increases the likelihood of being charged with a new crime within one year by 32 percentage points, which is significant at the five percent level. The effect for those charged with drug possession is even larger at 51 percentage points although only significant at the 10 percent level. Given that each subgroup shows significant and positive impacts of incarceration on recidivism, it is surprising that the results from the overall sample were negative and insignificant. What explains this pattern is the fact that the judges' rank ordering changes when looking at the incarceration rates for specific subgroups. In fact, when I return to the pooled sample and allow the impact of judge assignment to vary according to crime type, I find a strong correlation between incarceration and short-run recidivism (41 percentage points), significant at the 1 percent level, that is a weighted average between the effect for drug offenders and DWIs.

The magnitude of the bias depends on the degree to which monotonicity is violated and the treatment effect for the group that defies treatment:

$$\left(\hat{\beta}_1^{LATE} - \beta_1^{LATE}\right) = \frac{Pr[\text{Defier}]}{Pr[\text{Complier}] - Pr[\text{Defier}]} \times \left(\beta_1^{\text{Complier}} - \beta_1^{\text{Defier}}\right) . \qquad (1.4)$$

Table 1.4: Estimating the "causal effect" of incarceration in the presence of non-monotonic instruments bias

	New criminal charges within 1 year			
Sentenced to Incarceration	-0.31	0.32**	0.51*	0.41***
	(1.31)	(0.16)	(0.28)	(0.15)
Crime type = DWI	-0.21***			-0.17***
	(0.072)			(0.014)
N	4,548	2,535	2,013	4,548
Sample	DWI and Drug	DWI	Drug	DWI and Drug
Instrument	Judge	Judge	Judge	Judge × Crime
Anderson canon. Correlation LM statistic	0.46	15.2	12.2	27.4
Cragg-Donald Wald F statistic	0.46	15.3	12.3	13.8

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).
Notes: *** p<0.01, ** p<0.05, * p<0.1.

If the probability of being a defier is close to zero, then the bias will also be close to zero. Likewise, if the treatment effects for the group of compliers and defiers is similar, the bias will also be negligible. Problems arise, however, when the ratio of defiers to compliers grows and the treatment effects for the two groups systematically differ.

Given this formula, I can directly compute the magnitude of the bias from using the judge assignment without crime type interactions as an instrument. This requires estimating four parameters: $Pr[\text{Complier}]$, $Pr[\text{Defier}]$, $\beta_1^{\text{Complier}}$ and β_1^{Defier}. The compliers in this example are a subset of the individuals charged with driving while intoxicated while the defiers are those charged with possession of marijuana. The complier rate will be equal to difference in the incarceration rates between the judges for DWI's (0.0748) times the percent of the sample that is charged with DWI (0.557). The defier rate is equal to difference in the incarceration rates between the judges for drug possession (0.0726) times the percent of the sample that is charged with DWI (0.443). For the remaining two parameters, $\hat{\beta}_1^{\text{Complier}}$ is shown in the

second column of Table 1.4, while $\hat{\beta}_1^{\text{Defier}}$ is listed in the third column. This results in the following calculation:

$$\begin{aligned}\text{Bias} &= \frac{0.0726 \times 0.443}{0.0748 \times 0.557 - 0.0726 \times 0.443} \times (0.3248 - 0.5147) \\ &= -0.6367\end{aligned}$$

When adding together the impact of incarceration for individuals charged with driving while intoxicated (e.g. the compliers in the example) with the estimate of the bias, I recover the point estimate recorded in Column 1 of Table 1.4 (i.e. $\hat{\beta}^{\text{DWI}} + \text{Bias} = -0.31$).

Subgroup analysis based on the standard model, however, is not sufficient to eliminate this bias. Table 1.5 shows the results of separate estimations of the impact of incarceration based on splitting the sample by crime type, sex, first time offender status, age and race. Each reported coefficient is the result of a separate regression. The first column shows the effects estimated off of judge fixed effects within the given subgroup. The second column shows the effects estimated using judge fixed effects interacted with crime type within the given subgroup as the instrumental variable. The first versus the second columns present starkly divergent conclusions regarding the impacts of being incarcerated. When using uninteracted judge fixed effects, only the coefficients for the crime type subgroups are found to be statistically significant, which happen to be equivalent to allowing judge fixed effects to interact with crime type. In addition, coefficients vacillate between positive and negative and a chi-squared test of the joint significance across all specifications fails to reject the null hypothesis. In contrast, the second column shows systematic positive effects of incarceration on short-run recidivism across all subgroups, with the joint test strongly rejecting the null. The problem with the subgroup analysis here is that splitting the sample by sex or age eliminates potential violations of monotonicity only along those specific dimensions, but fails to

correct the known violation based on crime type.

Whether or not judges, case workers or other program administrators exhibit non-uniform preferences depends on the specific research context. Empirical work provides several examples of situations in which decision makers demonstrate non-uniform within-caseload preferences (see Korn and Baumrind (1998), Korn, Teeter, and Baumrind (2001), Waldfogel (1998), Abrams, Bertrand, and Mullainathan (2010) and Price and Wolfers (2010)). These settings include medical care, criminal law and professional sports. This is not to say that non-monotonicities necessarily bias studies based on this type of research design; even when non-uniform preferences exist, to the extent they represent a small portion of the overall variation (i.e. $Pr[\text{Defier}] \approx 0$) or that the range of potential treatment effects is not very large, the resulting bias will be minimal. It does, however, provide motivation for developing more rigorous empirical methods.

Table 1.5: Estimated impact of incarceration using Judge versus Judge × Crime fixed effects as instrumental variables

Sugroup	N	New criminal charges within 1 year	
DWI	2,535	0.32**	0.32**
		(0.16)	(0.16)
Drug Poss.	2,013	0.51*	0.51*
		(0.28)	(0.28)
Female	682	0.88	0.31*
		(0.66)	(0.17)
Male	3,866	0.27	0.48**
		(0.56)	(0.20)
First	2,434	0.087	0.19
		(0.73)	(0.14)
Repeat	2,114	-0.065	0.77*
		(1.23)	(0.46)
Age < 25	1,919	0.75	0.45*
		(0.67)	(0.24)
Age >= 25	2,625	0.33	0.37*
		(0.32)	(0.20)
White	1,656	-0.36	0.23
		(1.34)	(0.19)
Black	1,195	2.30	1.01*
		(3.95)	(0.61)
Hispanic	1,697	0.90	0.26
		(1.38)	(0.18)
Chi-squared test of joint significance		10.87	89.11
P-value		0.45	0.00
Instrumental Variable:		Incarceration rate by Judge	Incarceration rate by Judge × Crime type

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).
Notes: *** p<0.01, ** p<0.05, * p<0.1.

1.5 Estimating Instrumental Variable Models in the Presence of Non-Monotonic Instruments

The solution to non-uniform judge preferences is straightforward if judges base their decisions on a single defendant characteristic that is categorical in nature (e.g. crime type). Using the judges' crime-specific incarceration rates in lieu of their average incarceration rates as the source of exogenous variation in sentencing will eliminate monotonicity violations resulting from the non-uniform preferences. But, the answer becomes more complicated when the data contains many defendant characteristics some of which might be categorical (e.g. sex, race or skin tone) and others might be continuous (e.g. age, time since last criminal charge or total prior convictions) and it is unknown over which judges base their decisions.

A fully non-parametric estimation of $\Gamma_1(X_i)$ from Equation 1.3 would be the most straightforward approach from a theoretical perspective. Multivariate regression including judge fixed effects fully interacted with all potential pre-existing covariates and combinations thereof would provide consistent estimates of the flexible judge-specific decision rules. But, due to the curse of dimensionality such models often are not practical. The problem could be simplified if the combination of traits included as interactions with judge assignment were pre-specified, which could be motivated by detailed institutional knowledge of the research setting. However, putting this choice in the hands of the researcher unfortunately opens the door to undisciplined specification searching which limits the reliability of the produced estimates.

A semi-parametric approach where $\Gamma_1(X_i)$ is approximated in a linear model using a series of basis functions provides a feasible compromise. In this framework,

$$\Gamma_1(X_i)J_i = \sum_{k=1}^{K} \omega_k b_k(X_i, J_i) + \eta_i, \tag{1.5}$$

where $b_k(\cdot)$ is a basis function using information on defendant traits (X_i) and judge assignment (J_i) that measures relative judicial preferences, the parameters ω_k provide weights to each $b_k(\cdot)$ and η_i is an approximation error.

Any number of basis functions could be utilized here. The functions I focus on measure how judicial preferences deviate from caseload wide trends after conditioning on various combinations of defendant traits. The defendant traits I consider are: crime type, degree of charge, race, skin tone, sex, body type (i.e. thin, medium or heavy), height, weight, whether the defendant has a visible scar, whether the defendant has a visible tattoo, eye color, age, time since last criminal charge, time since last criminal conviction, total prior felony charges, total prior felony convictions, total prior misdemeanor charges, and total prior misdemeanor convictions.[15] For continuous characteristics, two equations are estimated. The first equation is the caseload-wide relationship between the sentencing outcome and the trait, and the second re-estimates the model allowing the parameters to vary by judge. The equations are parameterized using an indicator function for the value being non-zero to deal with potential censoring and a second order polynomial to allow for some curvature in preferences:

$$\begin{aligned} D_i &= \phi_0 1[x_i > 0] + \phi_1 x_i + \phi_2 x_i^2 + e_i \ , \\ D_i &= \sum_{j \in \mathcal{J}} \left[\phi_0^j 1[x_i > 0] + \phi_1^j x_i + \phi_2^j x_i^2\right] \times 1[\mathrm{J}_i = j] + \mathrm{e}_i \ . \end{aligned}$$

The candidate basis function $b_k(\cdot)$ is then computed by taking the difference between the predicted value of D_i based on the judge-specific and general model. To avoid any degree of mechanical correlation in the first stage, several researchers have recommended using "leave-one-out" or "jackknife" estimators wherein data for all defendants except for individual i are

[15]For continuous characteristics, I winsorize the top and bottom 5 percent of the distribution to improve boundary performance of the basis functions.

used to estimate $b_k(\cdot)$ for individual i (see Kling (2006), Doyle (2007), and Aizer and Doyle (2013)). One can implement this strategy without having to reestimate the two models for each observation by simply computing the diagonal elements of the Hessian matrix H_k. The value $h_{k;ii}$, which represents the ith diagonal element of H_k, measures the impact that observation i has on his predicted value, which is known in statistics as i's *leverage*. The jackknife residual is then reverse engineered by dividing the fitted residual from the full regression by $(1 - h_{k,ii})$. This results in the following formula to estimate the jackknife version of $b_k(\cdot)$:

$$b_{k,\not{i}}(\cdot) \quad = \quad \left[D_i - \frac{\hat{\mathrm{e}}_i}{1 - \hat{\mathrm{h}}_{ii}}\right] - \left[D_i - \frac{\hat{e}_i}{1 - \hat{h}_{ii}}\right] \ ,$$

where $\not{i}$ reflects the fact that the parameter has been stripped of all information from individual i.

The basis function for categorical characteristics are much more straightforward. Rather than estimating multiple regressions, $b_k(\cdot)$ is implemented as the difference in means between the judge and the overall caseload for various subgroups in the population:

$$b_{k,\not{i}}(\cdot) \quad = \quad \sum_{\kappa} \sum_{j} 1[x_i = \kappa, \mathrm{J}_i = j] \times \left(\sum_{\iota=1, \iota \neq i}^{N} 1[x_\iota = \kappa] \times \left[\frac{1[\mathrm{J}_\iota = 1] \times D_\iota}{\sum_{\iota, \iota \neq i} 1[\mathrm{J}_\iota = j]} - \frac{D_\iota}{\sum_{\iota, \iota \neq i} 1} \right] \right) \ .$$

In this notation, κ represents the potential values that the categorical variable x_i takes and j records judge assignment. Again to avoid a mechanical correlation in the first stage the sentencing means are calculated over all observations except for individual i. The resulting estimator will be numerically equivalent to but computationally faster than the prior strategy of estimating caseload-wide and judge-specific regressions models and using the leverage to remove individual i's data from the estimates.

Estimating preferences based on interactions of defendant characteristics (e.g. crime type by race) requires only trivial adjustments to the formulas described above and is not described in detail. To set an upper limit on the total number of potential basis functions to be constructed, the analysis presented in this study only uses two-way interactions among defendant characteristics. While this will limit the flexibility of the estimated decision rule, which could have implications for non-monotonicity, it is assumed that mismeasurement at this point will merely be an approximation error.[16]

The full set of basis functions could be used jointly as instrumental variables without explicitly estimating their respective weights ω_k. However, since I explicitly do not want to arbitrarily constrain the set of defendant traits that may influence judicial preferences, the set of constructed preference measures can be very large and may lead to many instruments bias (see Hansen, Hausman, and Newey (2008)). This problem is easily solved by invoking a cross validation sample splitting technique (see Angrist and Krueger (1995)) wherein the overall sample is randomly divided into two halves, ω_k for one half of the data is estimated using the other half of the data, and vice versa. Through using "out-of-sample" observations to construct the final weighting of the $b_k(\cdot)$ to estimate $\hat{\Gamma}_1(X_i)$, overfitting the first stage is avoided and test statistics will not need to be adjusted.

The difficulty with cross validation in this context is that the full set of candidate instruments likely contains many variables that contribute little to no additional information on judicial preferences. These variables add noise to the estimation and decrease prediction accuracy. For instance, after controlling for judicial preference by crime type, it is unlikely that measured preference by crime type interacted with eye color adds a significant amount of new variation to the estimation. A variety of shrinkage procedures can be employed to

[16]To the extent that remaining violations of monotonicity are between defendants with similar local average treatment effects, the impacts of this assumption should be minimal.

reduce dimensionality and isolate the key sources of variation in a model (see Hastie, Tibshirani, and Friedman (2009)). While it is acknowledged that these procedures introduce bias into the estimation of model parameters, the potential variance reduction has generally been shown to result in improved prediction accuracy (Leeb and Pötscher [2008a, 2008b]), which is precisely the goal given that this is being implemented in a first stage equation using cross validation. Many procedures have been explored in the context of instrumental variables including boosting (Bai and Ng (2009)), common factor analysis (Bai and Ng (2010)), and ridge regression (Hansen and Kozbur (2013)). I adopt the *least absolute shrinkage and selection operator* (Lasso) originally proposed in Tibshirani (1996), which has received growing interest in recent years in the literature (see Belloni, Chernozhukov, and Hansen (2014) for discussion of recent work).

I follow Belloni, D. Chen, et al. (2012)'s implementation of Lasso by estimating following objective function to solve for ω:

$$\hat{\omega}^{\text{Lasso}} \in \underset{\omega \in R^p}{\text{argmin}} \sum_{i \in \mathcal{C}} \left[\left(D_i - \sum_k \omega_k b_k(X_i, J_i) \right)^2 \right] + \frac{\lambda}{N} ||\Lambda \omega||_1 \ .$$

The objective function tries to minimize the sum of the squared residuals, but is penalized by the weighted sum of the absolute value of the coefficients. This creates a kink at zero in the domain of the objective function which forces coefficients that would otherwise be close to (but not exactly) zero under ordinary least squares (OLS) to be exactly zero under Lasso. Among the full set of p potential instruments, only s optimal instruments exhibit non-zero coefficients which are referred to as the *sparse* set.

In order to estimate this equation, both a penalty level (λ) and a penalty loading matrix ($\Lambda \equiv diag(\lambda_1, \lambda_2, \ldots)$) need to be specified. The elements of the optimal penalty loading matrix Λ^o defined as $\lambda_k^o = \sqrt{\mathbf{E}\left[f_k(x_{k,i})^2 \eta_i^2\right]}$ are infeasible since η_i, the error term from

Equation 1.5, is not observed in practice, but Λ^o can be approximated through an iterative process wherein conservative values initialize the Λ matrix. Given the initial penalty loadings, estimates of $\hat{\eta}_i$ can be recovered which can then be used to produce new penalty loadings based on $\hat{\lambda}_k = \sqrt{\frac{1}{N}\sum_i [b_k(X_i, J_i)^2 \hat{\eta}_i^2]}$. The process is repeated until the penalty loadings stabilize and converge.

The penalty level determines the degree of the kink in the objective function. Higher values of λ will result in relatively more coefficients being set to exactly zero. Belloni, D. Chen, et al. (2012) recommend setting $\lambda = c\,2\sqrt{N}\Phi^{-1}\left(1 - \gamma/(2p)\right)$, where the constant $c = 1.1$ and $\gamma = 0.1/\log(p \vee N)$. The combination of the iterated penalty loadings and this penalty level ensure that the Lasso estimator obeys the following near-oracle performance bounds,

$$||\hat{\Gamma}_1(X_i) - \Gamma_1(X_i)||_{2,N} \lesssim_P \sqrt{\frac{s\log(p \vee N)}{N}}\,,$$

meaning that estimates will coincide up to a $\sqrt{\log(p)}$ factor with the bounds achievable when the correct sparse set of significant variables is known ex-ante.

The traditional implementation of Lasso generally assumes there exists only a fixed number of optimal instruments, which is known as *exact sparsity*. Belloni, D. Chen, et al. (2012) show that their implementation of Lasso can relax this assumption to an *approximate sparsity* assumption, which states that $\frac{s^2 \log^2(p \vee N)}{N} \to 0$. Instead of setting a fixed bound on the number of optimal instruments, this assumption places an upper bound on the growth rate of the number of optimal instruments relative to the sample size. They show this assumption can be relaxed even further when employing a sample splitting procedure (as used in this study) to $s\log(p \vee N) = o(N)$, which effectively allows for an even faster growth rate of s in the sample size.

A closely related estimator known as the *Post-Lasso* estimator takes the sparse subset of

instruments selected by Lasso and re-estimates their coefficients using OLS. This addresses a known issue in the Lasso estimator that non-zero coefficients are biased towards zero. Post-Lasso eliminates some of this shrinkage bias, and achieves the same rates of convergence without requiring additional assumptions. It is for this reason that the preferred estimates of $\hat{\Gamma}_1(X_i)$ used in Section 1.7 are constructed using Post-Lasso coefficients rather than Lasso coefficients.[17]

Compared to other shrinkage procedures, Lasso and post-Lasso are particularly interesting because they identify a subset of variables that have high explanatory power. Isolating these variables gives the researcher an opportunity to learn about the dimensions over which judges exhibit differential behavior. Thus, the algorithm not only increases the power of our instruments, but also improves our understanding of judicial decision making.

1.6 A Panel Model of the Impact of Incarceration

In contrast to existing work using judge randomization, this study adopts a panel framework to estimate both the immediate and lasting effects of incarceration. Outcome Y for individual i, q quarters after being charged at time t is modeled as a linear function of his incarceration status and history, estimated court tendencies for non-focal sentencing outcomes ($\hat{D}_i^n$), and individual characteristics (X_i). Incarceration status and history are formalized as three specific variables: (1) the percent of days in a quarter that a defendant was incarcerated, (2) whether the defendant was previously incarcerated if he is not currently incarcerated, and (3) the total amount of time the defendant has spent incarcerated if he is not currently incarcerated. Quantifying these variables on the quarterly versus monthly or weekly basis

[17] In practice, the Lasso and Post-Lasso predictions of $\hat{\Gamma}_1(X_i)$ are very similar and this choice does not substantively alter the conclusions of this paper.

may introduce measurement error into the analysis; this is unavoidable, however, as several outcome variables are only measured on the quarterly basis.

Sixty to seventy percent of defendants are booked in county jail the week charges are filed. This generates a positive, mechanical correlation between incarceration status and criminal charges in any given quarter during my followup period. To deal with this issue, I recode incarceration status to zero once in the days after new charges are filed until a new quarter has started. This breaks the mechanical relationship between new charges and imprisonment and eliminates the simultaneity bias. This modification has minimal impacts on estimates for the felony caseload since incarceration spells generally span several quarters if not years, but is important for the misdemeanor caseload where the median incarceration spell is 10 days.

To account for any unobserved changes based on the timing of defendant's original charge or the amount of follow-up time since the charge was filed, fixed effects μ_t and μ_q are also included. This model is presented below:

$$Y_{i,t+q} = \delta_1 Incarcerated_{i,t+q} + \delta_2 Released_{i,t+q} + \delta_3 \left(Released_{i,t+q} \times Exposure_{i,t+q}\right) + \quad (1.6)$$
$$\delta_{4,q} \hat{D}_i^n + \delta_{5,q} X_i + \mu_t + \mu_q + \xi_{i,t+q} \ ,$$

where the primary variables of interest are defined as:

$$
\begin{aligned}
Incarcerated_{i,t+q} &= \frac{\text{Days Incarcerated}_{i,t+q}}{\text{Days in Quarter}_{t+q}} \ , \\
Released_{i,t+q} &= 1\left[\sum_{\tau=1}^{T} Incarcerated_{i,t+q-\tau} > 0\right] \times 1\left[Incarcerated_{i,t+q} < 1\right] \ , \\
Exposure_{i,t+q} &= \sum_{\rho=1978q1}^{t+q} Incarcerated_{i,\rho} \ .
\end{aligned}
$$

To compute $Released_{i,t+q}$, a maximum retrospective window (denoted by T) is necessary. When a more narrow window is used, δ_2 and δ_3 will capture primarily short-run effects. A longer window will average short-run impacts with medium-term and long-term impacts. In order to strike a balance between short and medium-run outcomes, I set T equal to 5 years.

Total incarceration exposure is measured as the cumulative time spent incarcerated since the first quarter of 1978, which is when the state prison data first begins. I cap the total exposure variable to 5 years to reflect the likely diminishing returns to incarceration length, and to improve the precision in the construction of the instruments. The model only allows total exposure to impact outcomes once an inmate has been released in order to avoid confounding the impact of duration after being released with any potential incapacitation effects.

Five years of post-charge outcomes are included in the panel data. In order to account for repeated observations for the same defendant over time as well as the same defendant appearing in the data for multiple charges, the standard errors are clustered at the defendant level.

The primary variables of interest will be instrumented using the methodology discussed

in Section 1.5. Because the data collected for this study spans 30 years of court filings, a time during which some judges remain in office for over 20 years, the full set of instruments are recalculated every 2 years over the range of t. This allows the estimated preferences of judges and assistant district attorneys who remain in the court system for many years to change with time. Their relative preferences will correspondingly adjust according to the court composition around the time charges were filed (e.g. a "tough" judge becomes relatively less tough when all of the "easy" judges are elected out of office and replaced with other "tough" judges).

Unlike fixed court outcomes such as guilt or fines, incarceration status and history change with time. As a result, instruments for these variables must be recalculated each quarter since the date of charge. This amounts to comparing the relative portion of each court's caseload that is incarcerated as of one quarter after charges were filed, two quarters after charges were filed and so on. A benefit of recalculating the instruments over time in the panel framework is that the estimation will leverage non-linear differences in the distribution of sentencing outcomes between courtrooms rather than focus on average differences in overall sentence length. As an example, some courts may be characterized by bimodal distributions of primarily short-term and long-term incarceration, whereas others might utilize a more uniform distribution of sentences. While the courts' average sentence lengths might be equal, the realization of these sentences over time will vary substantially.

The misdemeanor caseload does not have a wide distribution in the length of incarceration; the median incarceration length in this caseload is only 10 days. This severely limits the feasibility of estimating the full panel model proposed. Instead, for this caseload, I will estimate the same model but exclude the $[Released_{i,t+q} \times Exposure_{i,t+q}]$ variable. This means that the misdemeanor analysis will be unable to speak to the post-release impact along the intensive margin, and instead will only measure the incapacitation effects and the

post-release effects on the extensive margin.

1.7 The contemporaneous and post-release effects of incarceration

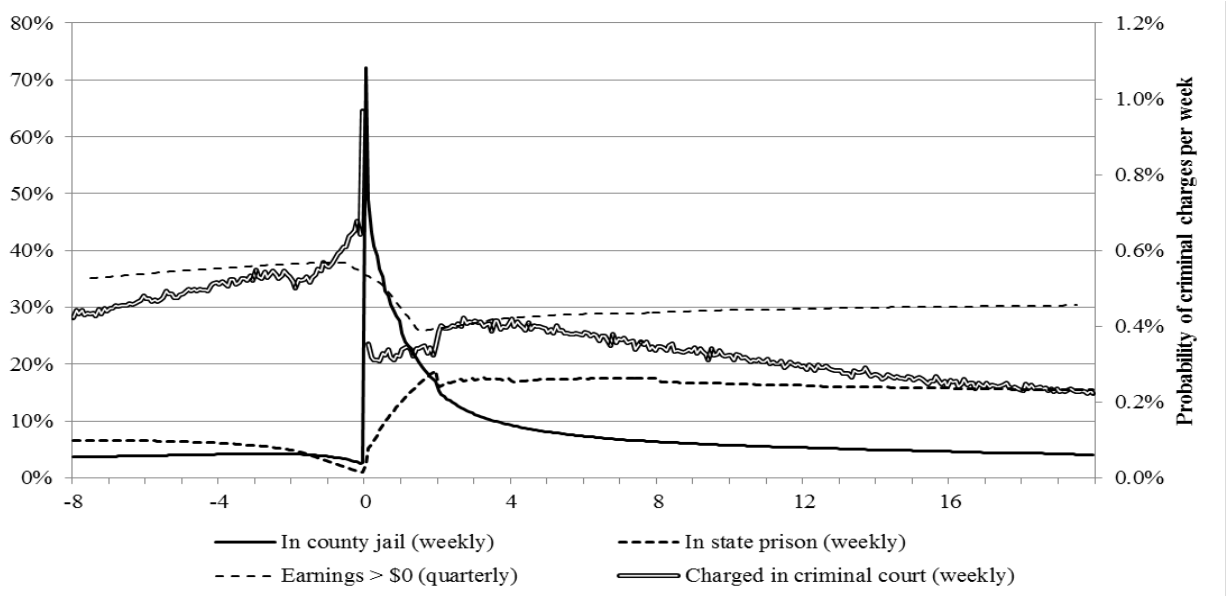
This section presents the empirical findings of the study. It begins with a descriptive analysis of how incarceration status, criminal activity and employment develop over time for defendants. I then report several results to confirm the validity of the proposed research design. Finally, I present the instrumental variable analysis which estimates the pre- and post-release effects of incarceration on defendant and household outcomes. Particular attention is paid to distinguishing mechanisms that underlie these results where possible. The results of my robustness exercises are discussed at the end of the section.

Figure 1.3 shows the incarceration, criminal charge and employment rates of felony and misdemeanor defendants relative to the timing of their criminal charges. Incarceration status, which is separated out into being in county jail and state prison, as well as criminal charges are measured on a weekly basis, whereas employment is measured quarterly. In order to preserve the scale of the figures, criminal charges in Week 0 are excluded since by definition all defendants would be charged in this week.

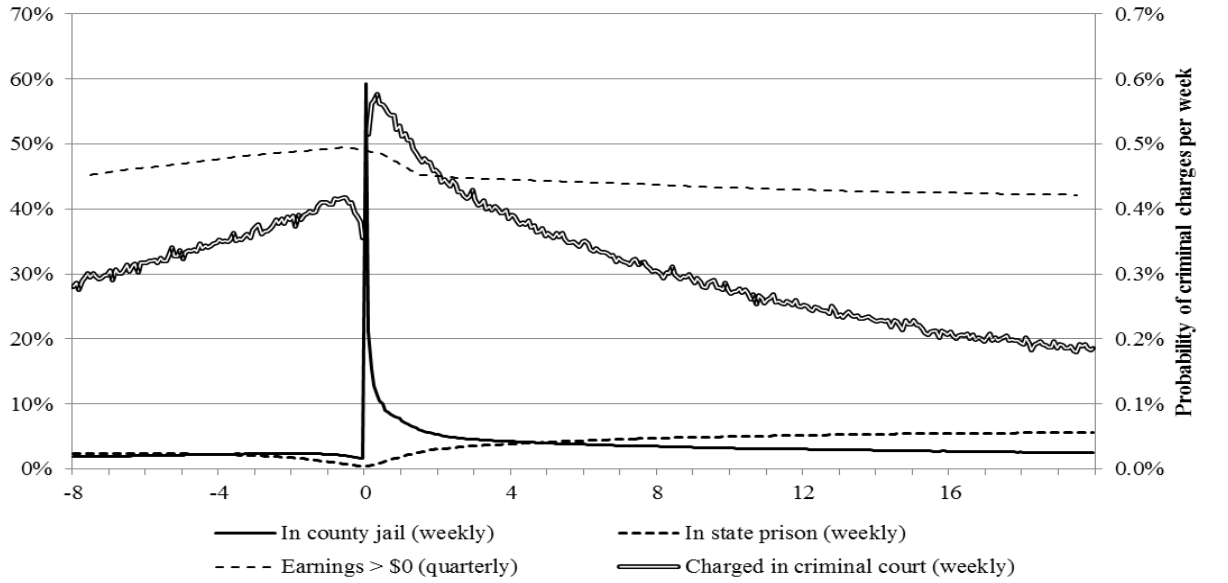
In the run up to Week 0, there is a relative decline in the incarceration rate of both felony and misdemeanor defendants. This is mirrored by an increase in criminal activity. Once charges are filed, there is an immediate increase in the likelihood of being jailed which is later displaced by prison for felony defendants. These increases in incarceration coincide with a distinct drop in criminal activity and employment. However, as inmates are released (months for felony defendants, weeks for misdemeanor defendants), there appears to be a modest short-run increase in criminal activity potentially upon release. In the 5 years of

Figure 1.3: Timeline of incarceration, additional criminal charges, and employment

(a) Felony defendants



(b) Misdemeanor defendants



Source*: Author's calculations using Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice's state prison records (1980-2013), and Texas Workforce Commission's unemployment insurance wage records (1994-2012).*

post-charge data, employment does not ever return to pre-charge levels.

This study relies on the fact that defendants are randomly assigned to courtrooms. While there is no reason to doubt the random assignment because it was implemented by an external office, the fact that the data only includes non-sealed records means that the observed caseloads for each courtroom may be censored in a non-random way. This can be tested by estimating the following equation.

$$x_{i,t} = \alpha + \tau_t + \beta Court_{i,t} \otimes \tau_t + \epsilon_{i,t}$$

In this equation, $x_{i,t}$ is a defendant covariate, $Court_{i,t}$ is a vector of court assignment dummy variables and τ_t is a vector of charge year dummy variables. Because Harris County introduced several new courtrooms in response to growing caseloads over time, it is necessary to include time dummies to absorb this variation. In addition, since the composition of courtroom actors (i.e. judge, chief prosecutor, etc) is changing over time, I fully interact courtroom and year fixed effects so that courtroom deviations are not arbitrarily constrained over time. In order to evaluate if the observed caseloads are statistically equivalent, I conduct an F-test of the joint significance of β. This procedure can be repeated using a sentencing outcome instead of a defendant covariate to establish a baseline of the instrument relevance if only using average differences between courtrooms.

Table 1.6 shows the results of this exercise. The first panel shows the F-tests for differences in the balance of defendant covariates. The second panel shows the F-tests for differences in the balance of various sentencing outcomes. The test statistics for defendant characteristics generally range between 1 and 1.4. These indicate a technical rejection of the null hypothesis, but capture very minor differences in court balance. In contrast, the test statistics for sentencing outcomes generally are all greater than 10, indicating a much

stronger rejection of the null.

While there are statistically significant average differences between courtrooms, particularly with respect to being sentenced to incarceration, I also rely on characteristic-specific differences in order to avoid non-monotonic instruments bias. Figure 1.4 shows the distribution of constructed instrument values for sentenced incarceration status and length for the felony and misdemeanor caseloads. Instruments for the felony caseload have more variation compared to the misdemeanor caseload, particularly with regard to incarceration length.

In order to document which traits have the most influence on relative court tendencies, Table 1.7 reports the ten strongest predictors of incarceration status and length selected by Post-Lasso among the full set of candidate interactions between defendant characteristics and judge/prosecutor assignment. Because certain defendant-court interactions exhibit greater variance than others, each candidate instrument is normalized to mean zero and standard deviation one. As such, the largest coefficients will identify the characteristics over which court actors exhibit the most divergent preferences, which will have greatest influence over the final constructed instrument.[18]

Members of the prosecution team are featured prominently in each set of selected variables reflecting their role in the courtroom and establishing plea bargains. In the felony caseload, judge and chief prosecutor preferences are difficult to disentangle due to the colinearity in their court tenures and so either individual measure should be thought to reflect the joint tendencies of judge and chief prosecutor. The defendant characteristics that most heavily influence relative court opinion are interactions between the defendant's type of crime, degree of charge, and criminal history. Felony courts pay closer attention to prior felonies while the

[18]Instruments for actual incarceration status over time instead of static sentenced incarceration status and length are used in my main analysis. Because the Post-Lasso selects similar predictors for both sets of variables, sentenced incarceration is presented here for simplicity. The sample splitting technique was not employed in the construction of this table in order to avoid adding unnecessary complexity to the discussion.

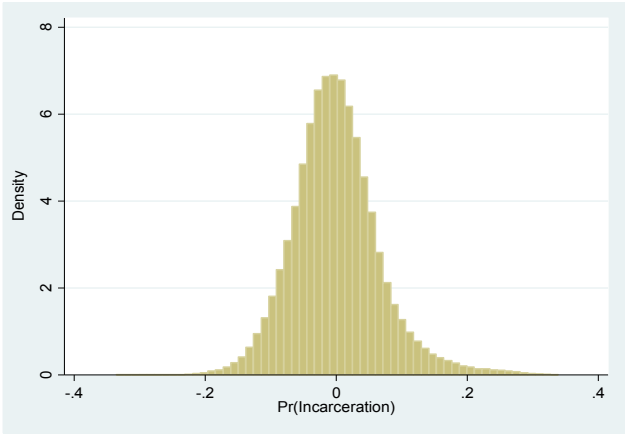
Table 1.6: Testing for significant differences between courts

	F-Test	
	Felony	Misd.
Panel A: Defendant Characteristics		
Female	0.9	1.1
Race/Ethnicity = Caucasian	1.1	1.2
Race/Ethnicity = African American	1.2	1.2
Race/Ethnicity = Hispanic	1.1	1.3
Age	2.3	1.0
Weight	1.0	1.1
Height	1.0	1.0
First Time Offender	1.3	1.2
Crime = Driving while intoxicated	1.2	1.1
Crime = Drug Possession	1.3	1.1
Crime = Traffic	1.4	1.1
Crime = Property Crime	1.4	1.2
Crime = Violent Crime	1.2	1.0
Panel B: Sentencing Outcomes		
Verdict = Guilty	14.0	15.0
Verdict = Deferred Adjudication of Guilt	19.3	22.8
Sentenced to Incarceration	14.8	19.7
Incarceration Length	3.4	11.0
Given Fine	24.5	8.1
Fine Amount	10.7	242.5
Sentenced to Probation	18.8	26.3
Probation Length	18.7	24.3
Felony Conviction Reduced to Misdemeanor	7.4	-
Sentenced to Drug Rehabilitation Program	9.5	4.9
Sentenced to Boot Camp Program	6.3	5.5
Sentenced to Incarceration during Off-Work Hours	6.0	27.6
Enrolled in Traffic School	1.0	11.6
Enrolled in Drug Education	7.1	12.6
Ignition Device Required	2.8	35.7
Electronic Monitoring	1.6	24.1

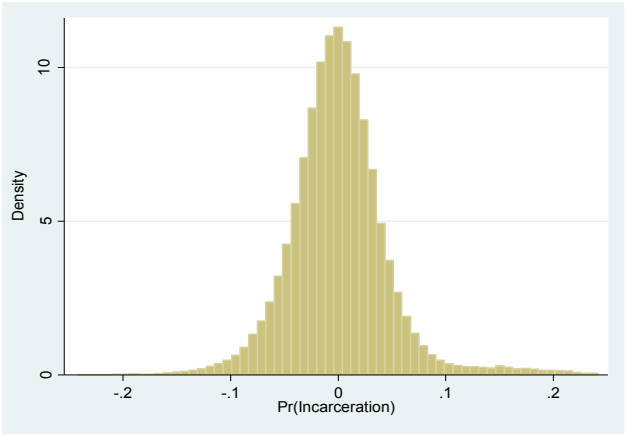
Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).

Figure 1.4: Histograms of constructed instruments for intensive and extensive margins of incarceration

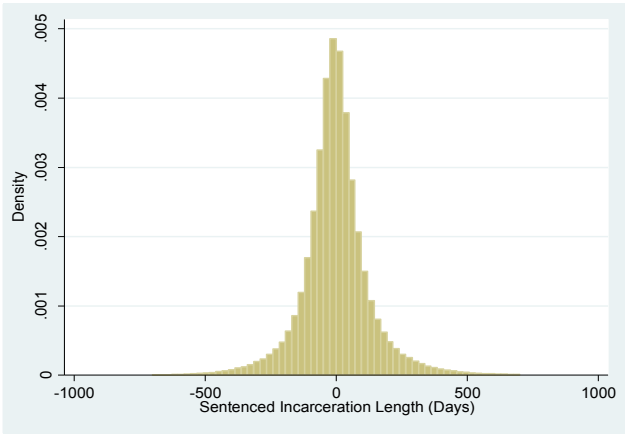
(a) Extensive margin, felony defendants



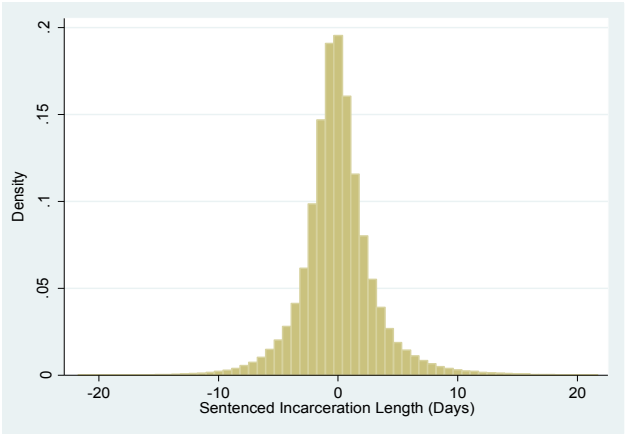
(b) Extensive margin, misdemeanor defendants



(c) Intensive margin, felony defendants



(d) Intensive margin, misdemeanor defendants



Source*: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).*

lower courts rely more on misdemeanor records. Defendants' age and race also appear to systematically influence felony court decision making, while age, sex, and skin tone are more prominent in the misdemeanor caseload. For each sentencing outcome, when the algorithm is given the opportunity to select among conditional and unconditional sentencing propensities, unconditional sentencing propensities were never chosen.

One might be concerned that allowing for flexible interactions will overfit the first stage, even if sparse and cross-validation methods are employed. One strategy to evaluate this concern is to examine whether the proposed methodology constructs instruments that are predictive of pre-charge incarceration status. Because judges and prosecutors have no jurisdiction over a defendant until he is assigned to their courtroom, courtroom assignment should have no predictive power before the filing of charges.

To evaluate this hypothesis I construct "instruments" for the defendant's incarceration status for the 8 quarters before and 8 quarters after the filing of the defendant's charge using my proposed algorithm from Section 1.5. I then regress the constructed instruments on actual incarceration status and record the R^2 for each quarter. Figure 1.5 shows the results of this exercise. In the lead up to criminal charges, the constructed instruments have effectively zero explanatory power in both the felony and misdemeanor caseloads. There is a sharp break, however, once charges are filed indicating that court assignment has a clear albeit modest impact on incarceration status. Court influence is most pronounced during the first year after charges are filed. At its peak, the R^2 is 0.095 in the first quarter after charges were filed for the felony caseload and 0.0025 for the misdemeanor caseload. Despite the decline, the predictive power remains non-zero in the post period for the felony caseload. This pattern is precisely what should be expected for this research design.

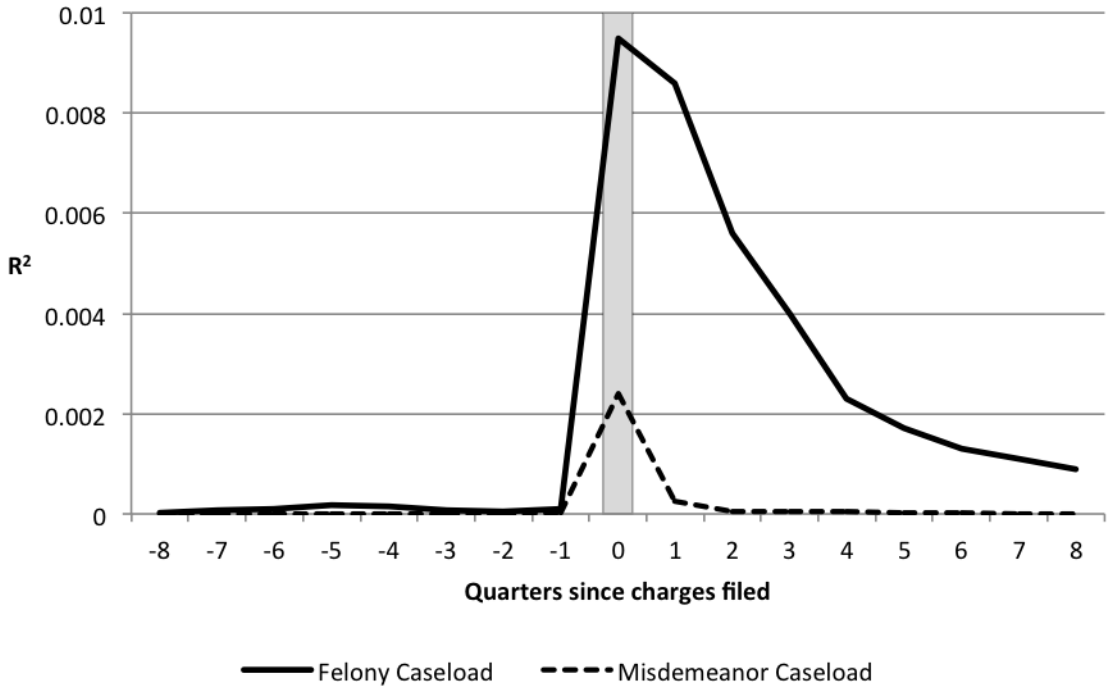
I now turn to discussing the main findings of this paper. The first set of outcomes I consider is the impact of incarceration on future criminal activity in the 5 years after

Table 1.7: Ten strongest normalized predictors selected by Post-Lasso for constructed instrumental variable

Court Actors	Defendant/Crime Characteristic	$\hat{\omega}$
Panel A: Incarceration status, felony caseload		
2nd Asst. Pros.	Defendant age × Crime type	0.008
Judge	Total prior felony convictions × Crime type	0.008
2nd Asst. Pros.	Crime type × Charge degree	0.008
2nd Asst. Pros.	Crime type × Defendant race	0.006
1st Asst. Pros.	Crime type × Charge degree	0.005
Judge	Total felony convictions × Defendant race	0.005
1st Asst. Pros.	Crime type × Defendant race	0.004
2nd Asst. Pros.	Total prior felony charges × Charge degree	0.004
1st Asst. Pros.	Defendant age × Crime type	0.004
Judge	Total prior felony charges × Defendant Race	0.004
Panel B: Incarceration length, felony caseload		
1st Asst. Pros.	Total prior felony convictions × Time since last charge	0.225
Chief Pros.	Time since last conviction × Charge degree	0.097
2nd Asst. Pros.	Defendant age × Crime type	0.085
2nd Asst. Pros.	Total prior felony charges × Crime type	0.055
2nd Asst. Pros.	Crime type × Charge degree	0.054
2nd Asst. Pros.	Defendant age × Charge degree	0.051
Chief Pros.	Defendant age × Crime type	0.050
Judge	Crime type × Charge degree	0.043
Judge	Total prior felony charges × Charge degree	0.043
1st Asst. Pros.	Crime type × Charge degree	0.042
Panel C: Incarceration status, misdemeanor caseload		
2nd Asst. Pros.	Total prior misd. charges × Time since last charge	0.025
Judge	Crime type × First-time/Repeat offender	0.007
Chief Pros.	Crime type × Charge degree	0.004
2nd Asst. Pros.	Defendant age × Crime type	0.004
1st Asst. Pros.	Crime type × Charge degree	0.004
Chief Pros.	Total prior misd. charges × Time since last charge	0.004
Judge	Total prior misd. convictions × Crime type	0.004
Judge	Time since last conviction × Crime type	0.003
Chief Pros.	Total prior misd. charges × Skin tone	0.003
2nd Asst. Pros.	Crime type × Charge degree	0.003
Panel D: Incarceration length, misdemeanor caseload		
Chief Pros.	Time since last charge × Time since last conviction	0.005
Judge	Total prior misd. convictions × Crime type	0.002
2nd Asst. Pros.	Total prior misd. convictions × Sex	0.001
2nd Asst. Pros.	Total prior misd. charges × Crime type	0.001
Chief Pros.	Total prior misd. charges × Time since last charge	0.001
1st Asst. Pros.	Total prior misd. charges × Sex	0.001
Chief Pros.	Total prior misd. convictions × Charge degree	0.001
Chief Pros.	Total prior felony charges × Time since last conviction	0.001
Chief Pros.	Total prior misd. convictions × Skin tone	0.001
Chief Pros.	Total prior misd. convictions × Crime type	0.001

Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).

Figure 1.5: R^2 of incarceration status first stage regression, by quarter relative to charges



Source*: Author's calculations using Harris County District Clerk's criminal court records (1980-2009), Harris County Sheriff's county jail records (1980-2013), and Texas Department of Criminal Justice's state prison records (1980-2013).*

the defendant's charge date. Criminal activity is measured using three different variables: being booked into county jail for a new arrest, being charged in a Harris County criminal court with a new offense, and being convicted of a criminal offense anywhere in the state of Texas. Each of these measures comes from a different data source, and they are not perfectly nested as a result. Table 1.8 shows the coefficient estimates separately for the felony and misdemeanor caseloads using OLS in the first and third columns and IV in the second and fourth columns. Each panel shows the coefficients for a different outcome (booking for new arrest, new criminal charges in the county court, and any statewide convictions), and each controls for time and relative quarter fixed effects as well as interactions between relative quarter fixed effects and defendant covariates and non-focal sentencing outcomes (or non-focal instruments in the case of IV). To account for repeated observations of defendants across multiple criminal episodes and over time, the standard errors are clustered at the defendant level.

The OLS estimates show a negative impact of incarceration on criminal activity while defendants are in jail or prison. The estimates indicate that about 2 to 4 percent of defendants would be arrested, charged or convicted in relation to a new criminal offense per quarter in the absence of incarceration. Once defendants are released from incarceration, however, they are more likely to be involved in criminal activity based on my three measures, especially for longer incarceration durations. The incapacitation effects measured here, however, likely underestimate the true effect of incarceration as those with the greatest probability of reoffending are usually incarcerated longer. Likewise, post-release estimates may be biased upwards given that those who are incarcerated also are thought to have unobserved characteristics that increase their probability of committing crimes.

The IV estimates show an incapacitation rate of 3 to 6 percentage points per quarter for marginal felony defendants depending on the measure. This decline in criminal activity,

however, is offset by an increase in post-release criminal activity of 4 to 7 percentage points per quarter for each additional year spent incarcerated. The increase in future charges should be of particular concern since it rapidly reverses any cost savings from crime prevented.

In the misdemeanor caseload, the first finding that jumps out is that marginal offenders are more likely to be booked in a county jail for a new arrest or charged in court while incarcerated. Taken literally, these results are misleading. The median incarceration sentence in the misdemeanor caseload is only 10 days, which is much shorter than the resolution at which the data is constructed.[19] While it is not impossible, it is extremely uncommon in the data for inmates to be charged with a new crime while in county jail. Instead, what this coefficient is measuring is the combined effect of temporary incapacitation as well as the immediate post-release effect in the remaining weeks or months in the quarter. Because the median defendant spends only slightly more than one-tenth of a quarter incarceration, the coefficient should be scaled down substantially for accurate interpretation. With this scaling adjustment, the measured effect is essentially brought inline with the post-release coefficient estimates. Given this interpretation, it appears that, like the felony caseload, marginal misdemeanor defendants tend to become more criminally involved as a result of incarceration rather than less.

It is assumed that the probability of being arrested or charged in court conditional on committing a new offense does not change in response to being incarcerated. One potential violation of this assumption would be if probation versus parole officers exerted differential effort in monitoring their assignees or were more or less stringent in reporting observed illegal behavior. Unlike in many states, in Texas neither probation nor parole officers are considered *peace officers* meaning they do not have the ability to make arrests on their own

[19]Efforts to reestimate the model at the weekly level were explored, but not feasible due to computational constraints.

Table 1.8: Impact of incarceration on criminal activity

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Booked in county jail for new arrest				
In jail or prison	-0.023***	-0.033***	-0.035***	0.22***
	(0.00032)	(0.0080)	(0.00048)	(0.024)
Released from incarceration	0.023***	0.0038	0.033***	0.020***
	(0.00024)	(0.0074)	(0.00018)	(0.0046)
[Released × Duration]	0.025***	0.067***		
	(0.00021)	(0.0058)		
Panel B: Charged in Harris County criminal court with new offense				
In jail or prison	-0.023***	-0.060***	-0.031***	0.11***
	(0.00028)	(0.0068)	(0.00044)	(0.021)
Released from incarceration	0.018***	0.00092	0.028***	0.015***
	(0.00020)	(0.0066)	(0.00016)	(0.0041)
[Released × Duration]	0.020***	0.056***		
	(0.00020)	(0.0053)		
Panel C: Convicted of criminal offense in Texas				
In jail or prison	-0.0025***	-0.028***	-0.016***	-0.025
	(0.00029)	(0.0074)	(0.00034)	(0.020)
Released from incarceration	0.015***	-0.00071	0.015***	-0.0060*
	(0.00020)	(0.0058)	(0.00013)	(0.0036)
[Released × Duration]	0.012***	0.036***		
	(0.00019)	(0.0047)		
Kleibergen-Paap rk LM stat.		536.3		610.5
Kleibergen-Paap rk Wald F stat.		181.1		307.5
Unique defendants	462,377	431,422	897,934	887,019
Total observations	15,425,207	13,744,324	29,976,888	29,222,981

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013), Texas Department of Public Safety statewide criminal conviction history database (1980-2013).

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

which should help limit this potential bias.

The measured incapacitation rate in this study is notably lower than other researchers' estimates. While this may be attributable to being a feature of the local context or the fact that I measure incapacitation using bookings, court charges and statewide convictions, the post-release increases in criminality suggest an alternative explanation. The strongest evidence in the literature on incapacitation comes from research designs that rely on quasi-random variation in sentence reductions among inmates who are already in jail or prison. Given their exposure to incarceration leading up to the potential sentence reduction, their likelihood of reoffending increases upon release which would translate into higher measured incapacitation rates. In comparison, the counterfactual in this study are individuals released on probation or not convicted due to their court assignment, whose natural rate of criminal behavior will be lower due to their insulation from the prison system.

Among felony defendants, the types of criminal charges prevented as a result of incarceration tend to be evenly split between misdemeanor and felony offenses (see Table 1.9). The crimes encouraged through incarceration's impact on post-release behavior, on the other hand, tend to be primarily felony-level crimes. This is particularly concerning because this indicates that criminal activity not only appears to be going up on net, but also appears to be getting more serious in nature. The misdemeanor caseload does not follow this trend. Instead, the increase in criminal activity overall tends to be more weighted towards new misdemeanor charges. This could explain why no statistically significant effects were observed when using statewide convictions as the dependent variable since the TDPS dataset has relatively poor coverage of less serious crimes.

Several mechanisms could explain the increased likelihood of new criminal charges post-release. Incarceration may facilitate the transmission of criminal capital through peer interactions among inmates; penalties to labor market outcomes could increase material hardship,

Table 1.9: Comparing impacts on felony versus misdemeanor charges

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Charged in Harris County criminal court with misdemeanor offense				
In jail or prison	-0.013***	-0.031***	-0.022***	0.046***
	(0.00019)	(0.0048)	(0.00033)	(0.016)
Released from incarceration	0.012***	0.0049	0.017***	0.014***
	(0.00015)	(0.0044)	(0.00013)	(0.0034)
[Released × Duration]	0.0063***	0.014***		
	(0.00011)	(0.0033)		
Panel B: Charged in Harris County criminal court with felony offense				
In jail or prison	-0.011***	-0.034***	-0.010***	0.064***
	(0.00019)	(0.0047)	(0.00025)	(0.013)
Released from incarceration	0.0074***	-0.0022	0.013***	0.0032
	(0.00013)	(0.0046)	(0.000088)	(0.0023)
[Released × Duration]	0.015***	0.047***		
	(0.00015)	(0.0041)		
Kleibergen-Paap rk LM stat.		536.3		610.5
Kleibergen-Paap rk Wald F stat.		181.1		307.5
Unique defendants	462,377	431,422	897,934	887019
Total observations	15,425,207	13,744,324	29,976,888	29222981

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

encouraging theft or pursuit of illegal income sources; or, diminished social capital may reduce one's incentives to avoid future incarceration. To evaluate the first of these hypotheses, Table 1.10 documents whether defendants were more or less likely to be charged with new types of crimes compared to their original offense. Each column in the table considers whether incarceration affected the likelihood of committing a specific type of crime (i.e. property, drug possession, drug manufacture or distribution, violent, and driving while intoxicated) for the group of defendants not originally charged with this specific crime. These five crime groupings account for 70 percent of the charges in the data.

The first panel in this table shows the results for felony defendants. I find that longer exposure to jail and prison increases the likelihood of new criminal behavior with the largest effects observed for drug possession and property crimes. While the increase in property crimes could be an indication that incarceration impacts income stability, the effect on drug offenses, which are the most common crime type among inmates, suggests a distinct possibility for criminal learning in this context. Impacts on drug manufacture or distribution follow similar patterns. The second panel shows the results for misdemeanor defendants. Like the felony context, misdemeanor defendants are more likely to be charged with drug possession or dealing post-release, even if their prior offense did not relate to drugs. In addition, I also observe a small but significant increase in the likelihood of violent offenses post-release.

Table 1.11 shows how incarceration impacts quarterly employment, income and log income. The first and third columns show the OLS coefficients for felony and misdemeanor defendants respectively, while the second and fourth columns show the IV estimates. While the specific magnitudes differ, the panels present similar stories: incarceration has a substantial impact on labor market outcomes while inmates are confined and a smaller but significant lasting negative impact on post-release outcomes. The OLS estimates are larger

Table 1.10: Impact of incarceration on committing new types of offenses

Type of criminal offense:	Property	Drug poss.	Drug mfr. or distr.	Violent	DWI
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.011***	-0.013***	-0.0042***	-0.0059***	-0.0026**
	(0.0033)	(0.0030)	(0.0013)	(0.0021)	(0.0013)
Released from incarceration	-0.0035	-0.000052	-0.00015	0.0021	0.00065
	(0.0033)	(0.0030)	(0.0013)	(0.0018)	(0.0013)
[Released × Duration]	0.015***	0.013***	0.0045***	0.00085	-0.00095
	(0.0028)	(0.0031)	(0.0012)	(0.0014)	(0.00078)
Kleibergen-Paap rk LM stat.	390.0	286.4	433.2	504.6	518.9
Kleibergen-Paap rk Wald F stat.	131.5	96.0	146.0	170.4	175.2
Unique defendants	344,395	347,337	408,013	359,991	413,127
Total observations	10,228,285	9,829,092	12,458,737	11,355,229	13,157,796
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.0042	0.018**	0.0089**	0.010	-0.0017
	(0.011)	(0.0089)	(0.0045)	(0.0074)	(0.0046)
Released from incarceration	-0.00030	0.00027	0.00031	0.0032**	0.00046
	(0.0018)	(0.0016)	(0.00069)	(0.0013)	(0.0013)
Kleibergen-Paap rk LM stat.	415.0	576.3	607.7	524.5	491.6
Kleibergen-Paap rk Wald F stat.	208.6	290.4	306.0	264.0	247.6
Unique defendants	747,535	816,217	882,885	822,456	673,906
Total observations	23,525,669	25,709,334	29,088,997	26,299,327	21,806,616

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
Notes: Each column excludes defendants originally charged with the type of crime being considered as the outcome variable. Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

in magnitude, likely driven by omitted variable bias, but the IV results still remain negative and significant. Based on the IV estimates, felony and misdemeanor defendants were respectively 32 to 40 percentage points less likely to be employed while incarcerated. Stated another way, marginal defendants who were not incarcerated were roughly five times more likely to be gainfully employed than be charged with another criminal offense if not incarcerated.

In contrast with prior research, I find the negative effect of incarceration extends beyond just the period of incapacitation. For each additional year of incarceration, felony were 3.6 percentage points less likely to be employed and earned 0.34 less log income. Misdemeanor defendants are 4.5 percentage points less likely to be employed and earn 0.42 less log income after being incarcerated, which are both marginally insignificant. As these magnitudes are well below the estimated incapacitation effects, many inmates likely return to pre-charge income levels.

To further explore the impact on labor market outcomes, Table 1.12 breaks out the impacts on employment and log income according to pre-charge income levels. Defendants were classified as either having $0 in average annual income, between $1 and $17,050 (the cutoff for living below poverty level for a family of four), or having greater than $17,050 in annual income. Prior earnings were calculating using up to 3 years of pre-charge data. A number of defendants were excluded from this analysis because their charge dates were before 1994 when the unemployment insurance wage records begin, making it impossible to calculate their pre-charge income level.

This table shows that labor market impacts for felony defendants are primarily concentrated among individuals with the strongest pre-charge earnings (see Panel A). The employment loss for individuals who previously earned over $17,050 per year was 46 percentage points while incarcerated (i.e. in the absence of incarceration, about half of inmates of this type would have continued being employed). For those serving at least two years, at least

Table 1.11: Impact of incarceration on labor market outcomes

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Quarterly employment				
In jail or prison	-0.40***	-0.32***	-0.41***	-0.40***
	(0.0019)	(0.037)	(0.0016)	(0.12)
Released from incarceration	-0.088***	-0.054	-0.082***	-0.045
	(0.0018)	(0.043)	(0.0012)	(0.031)
[Released × Duration]	-0.019***	-0.036*		
	(0.00053)	(0.019)		
Panel B: Quarterly log(earnings+1)				
In jail or prison	-3.30***	-2.59***	-3.30***	-3.25***
	(0.016)	(0.30)	(0.013)	(0.98)
Released from incarceration	-0.90***	-0.55	-0.86***	-0.42
	(0.015)	(0.35)	(0.010)	(0.27)
[Released × Duration]	-0.17***	-0.34**		
	(0.0042)	(0.16)		
Panel C: Total quarterly earnings				
In jail or prison	-2247.1***	-1632.1***	-2265.0***	-1641.0*
	(16.8)	(293.0)	(13.2)	(951.3)
Released from incarceration	-1119.3***	-683.5**	-1244.0***	-466.0
	(16.3)	(345.3)	(11.4)	(298.8)
[Released × Duration]	-140.5***	-246.5		
	(3.55)	(150.3)		
Kleibergen-Paap rk LM stat.		327.6		148.4
Kleibergen-Paap rk Wald F stat.		110.5		74.4
Unique defendants	259,698	243,491	424,306	419,432
Total observations	8,035,049	7,263,800	13,401,574	13,098,771

Source: Harris County District Clerk's criminal court records (1989-2009), Harris County Sheriff's county jail records (1994-2012), Texas Department of Criminal Justice state prison records (1994-2012), Texas Workforce Commission's unemployment insurance records (1994-2012).

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

40 percent then fail to reintegrate into the labor market after release, resulting in long-term earnings loss. As a point of comparison, von Wachter, Song, and Manchester (2009) finds job displacements from mass layoffs result in an immediate loss of 30 percent in annual earnings and long term loss of 20 percent after 15 to 20 years.

Prior research has had difficulty establishing causal evidence of human capital atrophy from adult incarceration. One factor contributing to this may relate to what is observed for the majority of inmates who earn little to no income prior to charges. The job loss rate during incarceration for marginal low-income defendants ranges from 8 to 38 percentage points, meaning that most very low-income defendants would not be employed even in the absence of incarceration. This indicates that most marginal defendants are only weakly attached to the formal labor force, and so earnings in the formal sector may be a poor proxy for human capital due to lack of variation.

To determine whether incarceration increased or decreased dependence on government programs, Table 1.13 shows the impacts of incarceration on the take-up of the Food Stamps/Supplemental Nutrition Assistance Program as well as the take-up of Aid to Families with Dependent Children/Temporary Assistance for Needy Families. While policy dictates that inmates lose benefits while they are incarcerated, there is little evidence (based on the IV estimates) that incarceration terminates benefit take-up. Post-release, felony defendants were 5 percentage points more likely to receive Food Stamps benefits per quarter, while misdemeanor defendants were 1 percentage point more likely to receive cash welfare. This increased reliance on social programs serves as additional evidence that inmates struggle with self-sufficiency after being released from incarceration.

Whether incarceration also affects social capital in addition to human capital is an important question. Marriage rates have significantly declined in recent decades, particularly among low-income and African American communities (B. Stevenson and Wolfers (2007)).

Table 1.12: Labor market impacts by pre-charge income level

	Employment			Log Wages		
Panel A: Felony defendants, Instrumental variables						
In jail or prison	-0.080*	-0.38***	-0.46***	-0.60*	-2.91***	-4.23***
	(0.044)	(0.051)	(0.15)	(0.35)	(0.41)	(1.35)
Released from incarceration	-0.023	-0.067	-0.094	-0.16	-0.65	-1.11
	(0.063)	(0.060)	(0.10)	(0.49)	(0.48)	(0.96)
[Released × Duration]	0.0064	-0.020	-0.15	0.019	-0.19	-1.34
	(0.021)	(0.029)	(0.11)	(0.17)	(0.23)	(1.00)
Net post-release effect:						
6 months in prison	-0.02	-0.08	-0.17*	-0.15	-0.75*	-1.78**
1 year in prison	-0.02	-0.09*	-0.24**	-0.14	-0.85**	-2.45**
2 years in prison	-0.01	-0.11*	-0.39**	-0.12	-1.04**	-3.79**
Kleibergen-Paap rk LM stat.	119.7	142.3	20.1	119.7	142.3	20.1
Kleibergen-Paap rk Wald F stat.	40.3	47.8	6.73	40.3	47.8	6.73
Annual Pre-Charge Income	0	$1 - $17,050	$17,051+	0	$1 - $17,050	$17,051+
Unique defendants	65,334	132,042	25,963	65,334	132,042	25,963
Total observations	2,013,657	3,796,562	572,857	2,013,657	3,796,562	572,857
Panel B: Misdemeanor defendants, Instrumental variables						
In jail or prison	-0.046	-0.47***	-0.17	-0.037	-3.58***	-1.63
	(0.14)	(0.17)	(0.56)	(1.13)	(1.36)	(5.21)
Released from incarceration	-0.028	-0.010	-0.048	-0.36	-0.0098	-0.51
	(0.063)	(0.046)	(0.057)	(0.51)	(0.38)	(0.54)
Kleibergen-Paap rk LM stat.	40.0	71.8	14.1	40.0	71.8	14.1
Kleibergen-Paap rk Wald F stat.	20.0	36.0	7.06	20.0	36.0	7.06
Annual Pre-Charge Income	0	$1 - $17,050	$17,051+	0	$1 - $17,050	$17,051+
Unique defendants	92,526	228,499	70,048	92,526	228,499	70,048
Total Observations	2,712,784	7,088,968	1,714,330	2,712,784	7,088,968	1,714,330

Source: Harris County District Clerk's criminal court records (1994-2009), Harris County Sheriff's county jail records (1994-2012), Texas Department of Criminal Justice state prison records (1994-2012), Texas Workforce Commission's unemployment insurance records (1994-2012).
Notes: Pre-charge income calculated using up to 12 quarters of pre-charge data. Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.13: Incarceration and public benefit receipt

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Quarterly Food Stamps receipt				
In jail or prison	-0.026***	-0.0087	-0.045***	-0.016
	(0.00090)	(0.018)	(0.00077)	(0.068)
Released from incarceration	0.037***	0.049**	0.033***	0.024
	(0.00089)	(0.020)	(0.00058)	(0.015)
[Released × Duration]	0.0023***	-0.016		
	(0.00031)	(0.011)		
Kleibergen-Paap rk LM stat.		464.4		186.1
Kleibergen-Paap rk Wald F stat.		157.1		93.3
Unique defendants	358,619	333,888	654,624	645,576
Total observations	9,785,345	8,864,396	17,982,294	17,583,624
Panel B: Quarterly cash welfare receipt (AFDC or TANF)				
In jail or prison	-0.0083***	-0.00049	-0.0088***	-0.024
	(0.00037)	(0.0084)	(0.00029)	(0.021)
Released from incarceration	0.0043***	0.0094	0.0039***	0.010*
	(0.00040)	(0.0093)	(0.00023)	(0.0061)
[Released × Duration]	-0.0015***	-0.0044		
	(0.000094)	(0.0039)		
Kleibergen-Paap rk LM stat.		505.5		413.4
Kleibergen-Paap rk Wald F stat.		171.0		207.7
Unique defendants	388,825	363,260	714,886	705,473
Total observations	10,955,406	9,879,373	20,165,101	19,700,866

Source: Harris County District Clerk's criminal court records (1987-2009), Harris County Sheriff's county jail records (1992-2012), Texas Department of Criminal Justice state prison records (1992-2012), Texas Health and Human Service Commission's Food Stamps records (1994-2011), Texas Health and Human Service Commission's AFDC/TANF records (1992-2011).

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

Researchers point to the growth in mass incarceration as an important contributor to this trend (Western and Wildeman (2009)). Table 1.14 reports the effects of incarceration on marriage and divorce activity. The estimates are separated for those under 25 years of age at time of charge and those over 40 years of age at time of charge. The purpose of these breakouts is to target the timing of when marriage and divorce are most likely to occur. Statistically and substantively significant impacts are observed in the felony caseload. Individuals under 25 years old are 1 percentage point less likely to get married while incarcerated. While the absolute magnitude of this coefficient is relatively small, the relative effect is substantial given that defendants face a 0.27 percent likelihood of getting married each quarter in the 2 years leading up to charges. The fact that these estimates are substantially larger than the average marriage rate indicates that marginal defendants likely exhibit a higher likelihood of marriage than non-marginal defendants.

The decline in marital activity does not appear to be simply a temporal displacement as post-release effects are quite small if not negative. In fact, among the felony defendants over 40 years old, who have the highest pre-charge marriage rate, the probability of divorce increases by 1 percentage point while incarcerated with positive coefficients on the post-release measures. I find that defendants serving three or more years in prison are at a statistically significant elevated risk of divorce post-release.[20] This further solidifies the argument that incarceration appears to play a causal role in preventing and dissolving marriages.

Robustness Exercises

Several robustness exercises, which can be found in Appendix A.3, were conducted to confirm the stability of the results. The results are generally robust, but some instability is observed

[20]Test not shown but available upon request.

Table 1.14: The effects of incarceration on marriage and divorce activity

Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Marriage, Defendant Age at Charge < 25				
In jail or prison	-0.003***	-0.012**	-0.003***	0.00038
	(0.0001)	(0.006)	(0.0001)	(0.0081)
Released from incarceration	-0.001***	-0.008	-0.001***	-0.0016
	(0.0001)	(0.006)	(0.0001)	(0.0018)
[Released × Duration]	-0.0001	0.0004		
	(0.0001)	(0.003)		
Kleibergen-Paap rk LM stat.		135.9		573.6
Kleibergen-Paap rk Wald F stat.		45.3		100.6
Unique defendants	175,609	175,609	374,636	374,636
Total observations	4,447,204	4,446,062	10,848,746	10,847,455
Panel B: Divorce, Defendant Age at Charge >= 40				
In jail or prison	-0.0006***	0.011**	-0.0007***	-0.003
	(0.0001)	(0.005)	(0.0001)	(.009)
Released from incarceration	-0.0004***	0.002	-0.0006***	0.001
	(0.0001)	(0.006)	(0.0001)	(0.002)
[Released × Duration]	-0.00002**	0.002		
	(0.00002)	(0.001)		
Kleibergen-Paap rk LM stat.		64.8		111.9
Kleibergen-Paap rk Wald F stat.		21.8		56.1
Unique defendants	83,962	83,962	188,833	188,833
Total observations	2,307,326	2,304,959	5,103,583	5,101,709

Source: Harris County District Clerk's criminal court records (1980-2009), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013), Texas Department of State Health Services marriage and divorce indices (1980-2012).

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

in the incapacitation effects measured in the misdemeanor caseload. As such, these results should be interpreted with caution.

In the first robustness check, I replicate the findings on criminal behavior using a statewide criminal conviction database maintained by the Texas Department of Public Safety. It is known that this database has incomplete coverage[21], but is generally thought to capture the most serious crimes. While the incapacitation effects measured in this data are weaker from a statistical perspective, I do capture the increase in criminality post-release for felony defendants. This ensures that these findings are not the result of differential intra-state mobility. The estimates for the misdemeanor caseload are much less precise, but this could be a function of the fact that fewer misdemeanor crimes are reported by counties to this statewide database.

The second robustness exercise re-estimates my results using a more conservative level of clustering: the court interacted with quarter of charge. Clustering at the defendant level accounts for correlation in the error term between repeated observations in the panel, but fails to account for correlation between defendants charged in the same courtroom. One example that could generate this relationship is if defendants generate peer effects while in courtroom. Taking this more conservative approach, however, leaves my standard errors virtually unchanged.

The third exercise intentionally omits crime type from the construction of the instruments. This explores the extent to which other covariates can account for variation that is of first order importance with regard to non-monotonic sentencing yet omitted from the estimation. This is an important concern considering that there are a number of potentially important defendant traits like educational attainment or marital status that are not

[21]State auditors have generally found the submission rates from local authorities to the statewide repository to be roughly 60 to 70 percent over the years

included in my data. Reestimating my findings with the new set of instruments does not statistically or substantively change my results, indicating that an incomplete set of covariates still can potentially capture the important dimensions of non-uniformity.

The fourth robustness check employs a two-step procedure wherein first stage residuals were estimated using OLS and then used to construct a fourth order polynomial control function that was added to outcome equation. The IV coefficients were then re-estimated using two-step GMM to ensure the estimates were insensitive to potential misspecification in the first stage. The magnitudes of the coefficients do not change noticeably and in fact the statistical precision in these specifications generally improves.

The fifth exercise trimmed the top 99th and bottom 1st percentiles in the instrument values to ensure that extreme values did not drive the results. I find the precision of the point estimates for the $[Released \times Duration]$ variable are somewhat sensitive to the trimming exercise with some loss of significance on specific coefficients, but this is not entirely surprising because I am eliminating variation. But taken as a whole, the general conclusions appear qualitatively similar.

The sixth check replaces the Post-Lasso coefficients with the original Lasso coefficients to weight the basis functions in the instrument construction. This should demonstrate that the results are not an arbitrary artifact of the specific estimation process I used. What I find in this exercise is that the estimates are very close together with similar magnitudes and precision, indicating that the Lasso versus Post-Lasso distinction in this application is somewhat arbitrary.

The final robustness exercise drops the shrinkage procedure entirely and uses only cross-validated OLS to weight the basis functions. These estimates do not deviate substantively from my main results.

1.8 Reexamining the costs and benefits of incarceration

A common exercise when presented with incapacitation effects is to evaluate whether the cost savings from crimes prevented outweigh the expense of keeping the inmate incarcerated (see Levitt (1996) or Owens (2009)). Without taking into account general deterrence effects, this type of calculation has been interpreted as a lower bound on the social gain from incarceration. But, such an exercise is not necessarily so straightforward; Donohue III (2009) compiles a detailed listing of additional mechanisms through which incarceration could impact welfare. At issue are concerns regarding losses to inmate productivity, spillovers to household members, and impacts on post-release behavior that could increase the overall costs. Many parameters needed for this more detailed accounting have not been credibly estimated, and so attempts at evaluating this question are either incomplete or rely heavily on untested assumptions.

The new estimates developed in this paper address some of the gaps in prior estimates. I cannot conduct a full cost benefit analysis as my research design does not measure general deterrence effects, and even if I could, there is no clear measure for other intangible benefits of punishment like a retributive utility gain for victims. But, through aggregating the impacts on the defendants own pre- and post-release criminal charges, labor market outcomes and public assistance payments in addition known institutional costs of incarceration I can provide improved partial estimates.[22] The remaining question is then to ask whether general deterrent or other unmeasured benefits in society are large enough to justify these documented costs.

[22]Because there is no clear way to value the impacts to marriage and divorce, these are excluded from the calculation.

Researchers have used a number of ways to monetize the social cost of crimes. These include hedonic pricing models (Bartley (2000)), compensating wage differentials (Viscusi (2000)), estimates based on jury awards (Miller, Cohen, and Wiersema (1996)) and contingent valuation studies (Cohen, Rust, Steen, and Tidd (2004)). Where necessary, these estimates are supplemented by simple accounting exercises. Additional complications arise when considering whether to include or exclude property transfers from theft in the calculation since criminals gain utility from consuming stolen goods. I follow Donohue III (2009) in using the costs proposed in jury award studies, which are the most commonly used strategy in the literature, excluding transfers as lower bound estimates and contingent valuation prices as upper bound estimates. Fewer crimes have been priced by the contingent valuation methodology[23], and so jury award prices inclusive of the value of stolen property supplement these figures.[24]

Resources are also expended in the criminal justice and legal system in order to arrest, charge, convict and punish individuals who commit crimes. In the absence of such crimes, these resources could reallocated for alternative uses. Additionally, facing new criminal charges increases the likelihood that a defendant is convicted and potentially incarcerated,

[23]Crimes that have been explicitly priced by the contingent valuation methodology are murder, rape, assault, robbery and burglary.

[24]Neither approach has priced the cost of drug consumption. To address this gap, I construct a naive price using aggregate cost estimates from the Department of Justice and aggregate usage rates rom the National Survey on Drug Use and Health. National Drug Intelligence Center (2011) estimates that the economic impact of illicit drug use in the United States in 2007 was $193 billion. This number is inclusive of impacts to criminal justice expenditures, defendant productivity and health. Because criminal justice expenditures and defendant wage losses (due to incarceration) are accounted for elsewhere in the calculation, I exclude them which results in aggregate costs of $84.8 billion mainly attributed to decreases in productivity and increases in health expenditures. Substance Abuse and Mental Services Administration (2011) findings indicate that roughly 22.6 million individuals in 2010 report having used illegal drugs in the prior month, and 39 percent used for 20 or more days. I conservatively assume that the remaining 61 percent of respondents only used drugs 1 day in the month, which generates an average frequency rate of 8.4 drug episodes per user. Finally, I divide aggregate costs by total estimated drug episodes in the year, which results in a price of $37 per act of drug consumption.

Table 1.15: The Social Costs of Charged Criminal Activity (2010 USD)

Criminal Activity	Lower Bound[a] ($)	Upper Bound[b] ($)
Homicide	4,301,817	11,559,713
Rape	187,680	343,859
Robbery	73,196	333,701
Assault	41,046	109,903
Burglary	21,617	50,291
Larceny	9,598	9,974
Motor Vehicle Theft	10,590	15,192
Drug Possession	2,544	2,544
Driving while Intoxicated	25,842	25,842

[a] *Based on Miller et al. (1996) and excluding the average value of property transfer,* [b] *Based on Cohen et al. (2004); Miller et al. (1996) (inclusive of the average value of property transfer). Both sets of estimates are inclusive of criminal justice costs originally taken from Donohue III (2009) but re-adjusted to eliminate arrest rate scaling. Both sets of estimates are inclusive of productivity losses using the author's estimates from Table 1.11. Figures reflect 5 percent discount rate.*

which has implications for defendant productivity. Donohue III (2009) incorporates both of these features into his work, but relies on untested assumptions in their construction. I substitute my own IV estimates of the impacts to defendant productivity while incarcerated, which results in effects that are roughly half the size of what Donohue III (2009) proposes. I do, however, rely on his estimates regarding costs to the criminal justice system.[25] For the sake of simplicity, I ignore the recursive aspects of this calculation: being incarcerated increases the likelihood of criminal charges, which then increases the likelihood of additional incarceration, which further increases the likelihood of more criminal charges and so on. Leaving this component out of the calculation underestimates the true social costs but is of second-order importance. The final set of cost estimates are displayed in Table 1.15.

I complement the instrumental variable parameter estimates from Section 1.7 with de-

[25] Because I measure changes in criminal behavior with court charges rather than criminal activity I eliminate the arrest rate scaling used in his estimates. Comparable figures for drug possession and driving while intoxicated were added to complete the list.

tailed crime-specific estimates which can be found in Appendix A.4. These are used to determine the exact changes in behavior associated with each type of criminal activity listed in Table 1.15.[26] These costs and savings are added to direct impacts on earnings and public benefit receipt and the cost of incarceration. I use Vera Institute of Justice (2012)'s estimate that each year an inmate spends in prison in Texas costs $21,390.[27]

To evaluate whether savings or costs dominate in this exercise, I compute four estimates: the incapacitation benefits, the institutional costs of incarceration, the post-release costs of increased criminality and the total economic impact. The general deterrence effect is left explicitly unmeasured. This exercise is repeated for three candidate sentence lengths: 6 months in prison, 1 year in prison and 2 years in prison.[28] The impacts take into account both the time served and five years of post-release outcomes and are discounted at a 5 percent annual discount rate (1.75 percent on a quarterly basis). Setting the impact after five years to zero is a strong assumption, and may lead to underestimates of the true cost. The exercise

[26]Not all types of crimes are priced, and so they are excluded from the calculation. This is equivalent to assuming that the social costs of unpriced crimes is zero. The vast majority of felony crimes are covered and the major types of misdemeanor crimes missing are traffic violations, public disturbance or disorderly conduct and fraud.

[27]Owens (2009) uses an estimated marginal cost rather than the reported average cost of incarceration. She finds that the marginal cost is slightly over half of the average cost in Maryland where her study is located. If this also holds in my setting, the correctional costs and shares presented later in this section would tend to be overstated.

[28]This exercise could be replicated for longer prison terms like 5 years, 10 years or 20 years. The results of this exercise could be misleading as local average treatment effects could differ for more serious offenders, which this study is likely not identified off of. As such, these results are not reported.

is formalized in the following equations:

$$
\begin{aligned}
\text{Incapacitation} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1}\times\left(\sum_{\mathcal{C}}(cost_{\mathcal{C}}\times\hat{\delta}_1^{\mathcal{C}})\right)\right],\\
\text{Institutional Costs} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1}\times\left(\frac{\$21,390}{4}\right)\right],\\
\text{Post-Release Criminality} &= \sum_{q=1}^{20}\left[(0.9825)^{Q+q-1}\times\left(\sum_{\mathcal{C}}cost_{\mathcal{C}}\times\left(\hat{\delta}_2^{\mathcal{C}}+\hat{\delta}_3^{\mathcal{C}}\times\frac{Q}{4}\right)\right)\right],\\
\text{Economic Impacts} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1}\times\left(-\hat{\delta}_1^{Wage}+\hat{\delta}_1^{FS}+\hat{\delta}_1^{TANF}\right)\right]\\
&\quad+\sum_{q=1}^{20}\left[(0.9825)^{Q+q-1}\times\left(-\hat{\delta}_2^{Wage}+\hat{\delta}_2^{FS}+\hat{\delta}_2^{TANF}\right)\right]\\
&\quad+\sum_{q=1}^{20}\left[(0.9825)^{Q+q-1}\times\left(-\hat{\delta}_3^{Wage}+\hat{\delta}_3^{FS}+\hat{\delta}_3^{TANF}\right)\times\frac{Q}{4}\right],
\end{aligned}
$$

where the parameters refer to the various estimated coefficients from Equation 1.6, Q represents the length of the prison term (measured in quarters), $\mathcal{C}$ is the set of crime types, and $cost_{\mathcal{C}}$ refers to the lower or upper bound social costs of different crimes plus the criminal justice and productivity costs added in. I compute the test statistics on the total measured change to evaluate if the estimates are significantly different from zero. This is accomplished using two stage least squares with seemingly unrelated regression employed in the second stage to allow for cross-equation correlation in the error terms. The results are presented in Table 1.16.

Across all specifications, the estimated costs outweigh the short-run incapacitation benefits and these effects are significantly different from zero. I find that a prison term of one year decreases welfare by roughly \$56,000 to \$67,000 based on correctional expenditures and defendant behavior. Close to half of these costs are driven by economic impacts, while post-

Table 1.16: Partial net costs based on cost of incarceration and defendant criminal, labor and public benefit outcomes

	Lower Bound		Upper Bound	
	Benefits	Costs	Benefits	Costs
Prison Term: 6 Months				
Incapacitation	$774		$1,936	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$10,601		$10,601
Post-Release Criminal Behavior		$6,078		$15,029
Economic Impacts		$21,433		$21,433
Total Measured Change	**- $37,338*** **		**- $45,127*** **	
Prison Term: 1 Year				
Incapacitation	$1,521		$3,805	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$20,835		$20,835
Post-Release Criminal Behavior		$9,736		$22,615
Economic Impacts		$27,114		$27,114
Total Measured Change	**- $56,164*** **		**- $66,759*** **	
Prison Term: 2 Years				
Incapacitation	$2,939		$7,350	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$40,249		$40,249
Post-Release Criminal Behavior		$16,281		$36,182
Economic Impacts		$37,653		$37,653
Total Measured Change	**- $91,246*** **		**- $106,735*** **	

*Notes: Estimates exclude murders, which are very expensive but noisily estimated. These estimates, which point to potentially substantially larger costs, are available upon request. *** p<0.01, ** p<0.05, * p<0.1.*

release criminal behavior accounts for between one fifth and one third of costs depending on how costs are defined. The total measured change worsens with incarceration length, which should not be surprising given my results. The costs associated with a six-month prison term range between $37,000 to $45,000 and those of a two-year prison term range between $91,000 to $107,000.

One caveat worth noting is that these estimates explicitly exclude the impacts on capital offenses (e.g. murder). While these crimes are rare events, particularly amongst the population of marginal offenders, the social costs associated with them are quite significant and substantially increase the measured costs overall.

To evaluate whether the measured costs can be justified based on a general deterrent effect alone, one can restate the cost benefit analysis in terms of the number of crimes that would need to be prevented in order for incarceration to be welfare neutral.[29] For instance, based on the lower bound cost estimates a one-year prison term would be welfare neutral if it prevented 0.4 rapes, 2.2 assaults, 2.5 robberies, 62 larcenies or 4.8 habitual drug users in the general population.[30] It is worth emphasizing that these are impacts that would need to be observed in the criminal behavior of the general population and not the defendant as his criminal behavior is already taken into account in this analysis. The higher costs of crime associated with the upper bound estimates present a more modest picture: one year in prison would need to prevent 0.2 rapes, 0.7 assaults, 0.2 robberies, 52.5 larcenies or 5.7 habitual drug users to be welfare neutral. But, these estimates are still quite high, especially if we consider the thought experiment of imprisoning of a low-risk offender whose incarceration is unlikely to deter the high cost crime categories like rape or assault.

[29]Because general deterrence would impact crimes that both are and are not arrested, I used the arrest rate scaled estimates provided by Donohue III (2009) for this portion of the analysis.

[30]A habitual drug user is defined as an individual who uses illicit drugs 200 or more times in a year.

1.9 Conclusion

Criminal justice policy in the United States has grown increasingly reliant on incarceration in the past three decades. Only in recent years has the incarcerated population begun to plateau. Previous work showing substantial incapacitation gains has helped encourage this trend. The findings of this study, however, should give pause to policy-makers. Measured incapacitation rates in the context of this study are quite low. This might be a function of the fact that I mainly measure incapacitation using court charges instead of criminal acts, which has led to lower estimates in other settings. But, in spite of this caveat, I still find that incarceration led to increased criminality for inmates after re-entry, which calls into question whether incapacitation is actually achieving true cost savings.

A number of non-crime outcomes are also negatively impacted by incarceration further diminishing its appeal. Effects on employment, earnings, benefit take-up and family formation indicate that inmates face significant barriers to re-entry. Decreased economic self-sufficiency coincides with greater use of government safety net programs. Lower marriage rates and increased divorce rates indicate social isolation and raise important questions regarding potential spillovers to families and communities.

This study cannot provide any evidence regarding potential general deterrent effects of incarceration. However, based on the impacts to defendant outcomes alone, I estimate that incarceration generates sizable social costs to society. Unless the benefits of general deterrence are at the upper bound of estimates found in the literature or there are other sizable intangible benefits to incarceration, it is unlikely that incarceration for low-risk offenders in Texas is welfare improving.

Chapter 2

Discrimination with Concealable Characteristics: Evidence and Application to Sexual Orientation in the United States

Michael Mueller-Smith

2.1 Introduction

Economic research on discrimination, both theoretical (G. Becker (1957)) and empirical (e.g. Goldin and Rouse (2000), Bertrand and Mullainathan (2004), Charles and Guryan (2008)), has classically assumed that minority traits are perfectly observable. In the context of race and sex, which form the general foundation of existing research, such an assumption is innocuous. In this paper, however, I propose a departure from this body of work through the consideration of an alternative class of traits: *concealable characteristics*. These traits are precisely defined by the fact that they are not publicly observable. Instead, agents make an active decision whether to disclose or conceal their minority status.

Concealment has important implications from both a theoretical and empirical perspective. Given the option to conceal, individuals who self-identify their minority status only represent a subset of individuals for whom the benefits of disclosure outweigh the costs, which is a classic case of selection bias. A potential consequence of this bias, for example, could be that those likely to face the worst discrimination conceal their type and generate censoring in the distribution of realized discrimination (i.e. what is actually measurable ex-post in equilibrium). As a result, estimates based on self-reported status would underestimate the true magnitude of discrimination.

If we suppose that the researcher could measure innate preferences, selection bias could be avoided but standard models which focus on wage penalties may fail to capture the nuanced implications of discrimination. For instance, the costs of discrimination may be dispersed across multiple outlets (e.g. labor market penalties, mental health costs, etc) and the relevant channel will uniquely depend on the individual's concealment status. This stands in contrast to G. Becker (1957), which concludes that zero or minimal measured wage penalties is an indication that all or most minorities have found non-discriminating firms

and avoided punishment.[1]

Psychologists have long recognized that there exists numerous channels through which stigma can affect minority individuals. Herek (2004) focuses on three distinct modes of transmission: *enacted* stigma, *felt* stigma and *internalized* stigma. This formulation distinguishes between prejudicial acts carried out against minorities (enacted), minority fear and anticipation of prejudicial acts (felt), and self-directed stigma (internalized). Concealment can reduce the chances of experiencing enacted stigma, but may exacerbate felt and internalized stigma compounding mental health and anxiety disorders. Psychologists also point to an additional drain on cognitive resources resulting from the constant vigilance needed maintain a false identity [Major and O'Brien (2005)].

The specific application being considered in this paper is *innate* sexual orientation, a concept that is theoretically distinct from *self-identified* sexual orientation. The former category measures an individual's private sexual attraction, while the latter is the public presentation of one's sexual orientation. The features of a concealable characteristics framework, however, also extend to work on cultural identity and assimilation for immigrants (Arai and Skogman-Thoursie (2009), Constant, Gataullina, and Zimmermann (2009)), ethnic identification (Mason (2004)), religious attachment (D. Chen (2010)), political allegiance (Kuran (1995)), racial identification (Nix and Qian (2015), Saperstein and Penner (2010)) and student ability (Akerlof and Kranton (2002), Austen-Smith and Fryer (2005)). All echo similar mechanisms (e.g. motives to conceal identity or other opaque attributes). A generalized concealable characteristics framework could serve to unify these disparate research agendas.

Original empirical analysis is presented to illustrate how potential concealment shapes

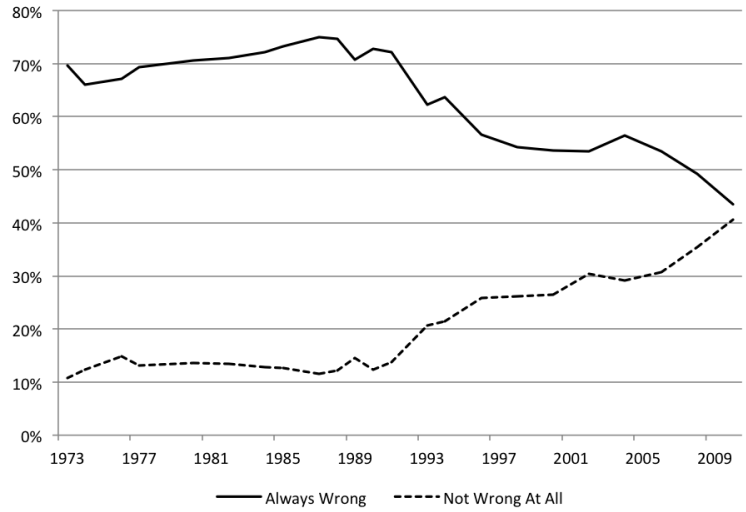
[1]As an extreme example, consider the case of Iran where homosexuality is punishable by death. In thirty years, relatively few individuals have been executed [Boroumand Foundation (2010)], but this is not evidence the law is not enforced. Instead, homosexuals in Iran are highly motivated to conceal, even to the point of leading Mahmoud Ahmadinejad, the president of Iran, to say in 2007, "In Iran, we do not have homosexuals like you do in [the United States]. It is not a problem."

life-cycle outcomes using data from the National Longitudinal Survey of Youth 1979. The study leverages the fraternal birth order (FBO) hypothesis from developmental psychology as a proxy measure for innate sexual orientation in lieu of self-identified sexual orientation. As will be discussed in greater detail in the following sections, the FBO hypothesis is the culmination of numerous studies that have consistently found that men with more older brothers are more likely to identify as homosexual or report same-sex attraction. This proxy for innate sexual orientation is used in conjunction with varying degrees of juvenile exposure local discrimination against the lesbian, gay and bisexual community based on the respondent's county of birth in the United States. Studying how these two sources of variation interact will allow the paper to consider how life-cycle trajectories change in response to increasing motives to conceal, and whether early life exposure to anti-gay policies and attitudes has long-term implications past adolescence.

This paper does not seek to explain why discrimination against the lesbian, gay, and bisexual (LGB) community exists, nor why discriminatory attitudes have changed over time (see Kuran (1995)). Additionally, it does not seek to develop a theory of identity formation and management (see Akerlof and Kranton (2000), Benabou and Tirole (2011)). Those questions are beyond the scope of this study.

The remainder of this paper is organized in the following manner. Section 2.2 further motivates the application being considered in this study. Section 2.3 presents a theoretical framework to consider discrimination in the context of concealable characteristics. Section 2.4 describes the empirical research design. Section 2.5 presents the empirical results. Finally, Section 2.6 provides concluding remarks.

Figure 2.1: Attitudes towards sexual relations between two adults of the same sex in the United States, 1973-2010



Source: General Social Survey

2.2 Motivation

The lesbian and gay community in the United States has been highly stigmatized for many years. Throughout the 1970's and 1980's roughly 70 percent of respondents to the General Social Survey thought that sexual relations between adults of the same sex were "always wrong" (see Figure 2.1). In fact, Moore (1993) found that 37 percent of Americans (30 percent of women and 45 percent of men) preferred that "homosexuals stay in the closet", a colloquialism for hiding same-sex attraction. Recent decades have witnessed warming attitudes, yet these changes have been slow to translate into the legislative change. Physical and verbal harassment also remain relatively common experiences, and, if looking beyond the United States, many countries around the world still outlaw homosexuality with consequences as severe as capital punishment.

Studies seeking to measure the extent of employment discrimination generally find neg-

ative wage impacts for gay men (see Badgett, Lay, Sears, and Ho (2007) for review of this literature). Conditional on human capital and family structure, gay men earn roughly 10 to 32 percent less than similarly qualified heterosexual men. Lesbians, however, have more mixed evidence, sometimes showing no difference with similarly qualified women, sometimes earning a positive premium over similarly qualified premium. Audit studies and other experimental techniques, in contrast, consistently show large negative biases against sexual minorities, including both gay men and lesbian women (see Tilcsik (2011), Hebl, Foster, Mannix, and Dovidio (2002), Jones (1996), Walters and Curran (1996)).

The impacts of stigma related to sexuality also extend beyond labor markets. The LGB community has also been associated with elevated rates of violence and report more fear of victimization (see Herek (1991), Dunbar (2006), Harris Interactive (2006)). Researchers have also found gay youth are more likely to report physical, verbal and sexual abuse in schools (Bochenek and Brown (2001), O'Shaughnessy, M. Heck, Calhoun, and Laub (2004), Kosciw, Diaz, and Greytak (2008), Saewyc et al. (2006)).

Epidemiologists and psychologists have consistently linked homosexuality with elevated rates of suicide (Gibson (1989), Meyer (2003), King, McKeown, et al. (2003)), substance abuse (Marshal et al. (2008)), and mental health disorders (Cochran, Sullivan, and Mays (2003)). To quantify these relationships, King, Semlyen, et al. (2008)'s meta-analysis of 28 population-based studies found that members of the LGB community were 2.5 times more likely to attempt suicide (4.3 among gay men), 1.5 times more likely to have depression or other anxiety disorders, and 1.5 times more likely to abuse alcohol or other substances. While the earliest work on homosexuality thought same-sex sexual behavior was a symptom or type of a mental health disorder, the consensus among psychologists for several decades has concluded that negative mental health outcomes observed for gay men and women reflect a psychological response to the animus and prejudice faced by sexual minorities in everyday

life (Meyer (2003)).

With substantial risks to disclosure, sexual minorities should be highly motivated to conceal their type. Supporting this concept, Badgett, Donnelly, and Kibbe (1992) estimate that between 28 and 72 percent of self-identified gay men and women actively conceal their sexual orientation to some degree to avoid discrimination.[2] Because this estimate is based on a convenience sample of individuals who self-identify as gay to some degree, it is hard to say whether or not rates of concealment in the overall population should be higher or lower.

The fact that some gay men and women still choose to disclose their identity suggests there are benefits to disclosure and costs to nondisclosure. Goffman (1963) discusses the active effort require to maintain a secret or manage a stigmatized identity for sexual minorities, attenuating the productivity and effectiveness of "closeted" individuals. Case studies of gay men and lesbians (Weinberg and Williams (1974), Schneider (1986), Hall (1989), Woods (1993), Friskopp and Silverstein (1995)) also provide tentative evidence regarding modes of concealment and the repercussions of nondisclosure in the work setting. Findings from this literature indicate that sexual minorities actively engage in identity management to avoid disclosure including avoidance of social situations as well as figurative and concrete expressions of heterosexual identity. The corresponding impacts on productivity have been theorized but have not been empirically established.

Related work in health psychology has considered the relationship between nondisclosure of sexual orientation and physical health. Cole, Kemeny, Taylor, Visscher, and Fahey (1996) found that among a sample of healthy HIV-seropositive gay men, individuals who reported concealing their identity experienced an accelerated course of HIV infection. Individuals were followed for nine years, and impacts were measured using CD4 T lymphocyte levels,

[2]See also Francis (2008) for analysis of the impact of the AIDS epidemic on the sexual identity of men and women in the United States.

time to AIDS diagnosis and time to AIDS mortality. Impacts were strongest for those who reported the highest degree of concealment, even when controlling for demographic characteristics, health and sexual practices, as well as mental health status. Recent work in Strachan, Bennett, Russo, and Roy-Byrne (2007) supports these conclusions, finding that disclosure of sexual orientation improved CD4 T lymphocyte levels. In another sample of HIV-seropositive gay men, Cole, Kemeny, Taylor, and Visscher (1996) found elevated rates of cancer and infectious disease among those who concealed their sexual orientation. Given the potential link between concealment behavior, socio-economic background, and, unobserved health practices, however, it remains unclear whether nondisclosure has a truly independent effect on physical health.

A remaining problem in the literature is the lack of knowledge regarding individuals who experience same-sex attraction but do not report their minority status. Existing empirical work relies on individuals self-identifying their sexual orientation or having a same-sex partner in their household. This raises the question of the extent to which researchers should rely on self-reported or revealed identity when attempting to measure the impacts of discrimination. To the extent that researchers can innovate ways to measure sexual orientation through non-disclosed means, the literature will be better able to capture the broader dynamics associated with stigma, prejudice and life-cycle choices.

2.3 Discrimination with Concealable Characteristics

Incorporating concealment into traditional discrimination models requires enabling identity choice for agents.[3] The relevant model for this feature depends on whether society discrim-

[3] "Identity" in this context is not a arbitrary pooling mechanism as is the case in Akerlof and Kranton (2000) and related work. Instead, fundamental differences in the population are endowed by nature. When I refer to identity, I simply mean the public expression of the privately known differences.

inates based on the underlying status itself or simply the expression of this status. For underlying preferences, signaling models would be more appropriate whereas for behavior-based discrimination, discrete choice is sufficient. In this work, I focus on behavior-based discrimination both for its simplicity as well as its relevance to the application.

In this simple model, two agents are randomly matched and engage in joint production with the resulting output π. The presenting agent (p) is endowed by nature with a concealable trait τ_p, which can take the value M if the presenting agent is a member of the majority or m if he is a minority. The presenting agent's type is private information. What he decides to publicly disclose regarding τ_p is represented by $T_p \in \{M, m\}$. It is assumed that if $\tau_p = m$ the presenting agent can still credibly conceal his type because he is randomly drawn from a population containing both majority and minority types.

The responding agent (r) has distaste parameter (d_r) over being paired with a minority, which is randomly drawn from the distribution $\mathcal{D} \in \mathbb{R}^+$ and is only observed by the presenting agent once the pair is matched. She sells π at a price of 1, and decides how to divide the proceeds.

The responding agent seeks to optimize the following objective function:

Definition 1. *The Responding Agent's Objective Function*

$$\max_{\lambda_M, \lambda_m} \quad Q_r(\lambda_M, \lambda_m, \pi, d_r) \equiv 1\left[T_p = M\right] \times q_r(\lambda_M \pi) + \left(1 - 1\left[T_p = M\right]\right) \times \left(q_r(\lambda_m \pi) - d_r\right)$$

where, $1[T_p = M]$ is an indicator function recording whether the presenting agent announces he is member of the majority. The model is neutral on the particular functional form of q_r in order to accommodate a range of possible applications. For instance, $-q_r$ could be the firm's cost minimization equation with $q_r = \lambda\pi - \pi$. In this setting, $\lambda\pi$ would be akin to the wage rate paid to the presenting agent. It could also model an altruistic parent who values

her child's utility: $q_r = V_r((1-\lambda)\pi) + \theta V_p(\lambda\pi)$.

The responding agent optimizes this objective function by setting two different compensation rates λ_M and λ_m which correspond to the presenting agent's announced type. The differential pricing serves two purposes. First, it allows the responding agent to allocate more of the π to herself in the event that the presenting agent decides to reveal his type, which will compensate her for pairing with a minority (i.e. she can set λ_m and λ_M such that $q_r(\lambda_m\pi) - d_r = q_r(\lambda_M\pi)$). Second, it gives the responding agent the ability to manipulate the presenting agent's concealment choice. Even if $q_r(\lambda_m\pi) - d_r < q_r(\lambda_M\pi)$, the responding agent will be satisfied with λ_m and λ_M if the discrepancy between the two compensation rates forces the presenting agent to select $T_p = M$. Because of these two dynamics, the relative compensation for majority types will be weakly higher compared to minority types (i.e. $\lambda_M \geq \lambda_m$ and $\lambda_M > \lambda_m$, $\forall\ d_r > 0$).

Depending on the specification of q_r, it may be necessary to assume that factors outside the model to prevent the responding agent from arbitrarily setting either or both λ equal to zero. In the context of an employer-employee relationship, introducing mobility into the model and allowing competition between firms would generate a price floor for both λ_M and λ_m. In the context of an altruistic parent (where mobility is an unrealistic feature), such assumptions can be unnecessary as the model can internally generate non-zero λ's.

The presenting agent seeks to maximize his indirect utility:

Definition 2. *The Presenting Agent's Objective Function*

$$\begin{aligned}\max_{T_p}\ Q_p \quad \equiv \quad & 1\left[T_p = M\right] \times V_p(\lambda_M\pi, \psi - 1[\tau_p = m] \times \rho) \\ & + \ (1 - 1\left[T_p = M\right]) \times V_p(\lambda_m\pi, \psi - 1[\tau_p = M] \times \rho)\end{aligned}$$

V_p is an indirect utility function that is increasing but concave in consumption. If the

presenting agent chooses to conceal his type, he will receive $\lambda_M \pi$ in total consumption. If he chooses to disclose his type, he will instead receive $\lambda_m \pi$.

Without additional features, a trivial solution for the presenting agent is to choose concealment when facing any positive amount of animus in order to avoid the costs of disclosure. Costless concealment, however, does not fit observed patterns in the real world; minorities with a choice to conceal regularly disclose their true identity in spite of the potential negative consequences. This indicates that concealment comes at a cost or disclosure has a non-pecuniary benefit.[4] These two potential motivations are isomorphic, and so for simplicity, I frame the model just in terms of costs to nondisclosure.

To this end, I introduce mental health capital (ψ) to the production of indirect utility which positively contributes to utility production (with decreasing returns). When the presenting agent chooses to conceal (i.e. $\tau_p \neq T_p$, he must sacrifice a ρ mental health as consequence of nondisclosure, which results in a lower amount of mental health capital: ψ'. As such, a minority presenting agent faces a trade-off between sacrificing additional consumption or sacrificing mental health both of which lower overall utility. Whether to conceal or disclose depends on whether $V_p(\lambda_M \pi, \psi')$ is greater or less than $V_p(\lambda_m \pi, \psi)$, which can be decomposed into two components:

$$\begin{aligned} V_p(\lambda_M \pi, \psi') - V_p(\lambda_m \pi, \psi) &= \underbrace{V_p(\lambda_M \pi, \psi') - V_p(\lambda_M \pi, \psi)}_{\Psi} + \underbrace{V_p(\lambda_M \pi, \psi) - V_p(\lambda_m \pi, \psi)}_{\Lambda(d)} \end{aligned}$$

Ψ is the utility cost of the negative shock to mental health imposed by concealment condi-

[4] In fact, psychologists note that concealing sexual orientation utilizes cognitive resources, which can have a corresponding negative impact on well-being (Pachankis (2007), Smart and Wegner (2000) and Lewis, Derlega, and Clarke (2006)), and may be manifested through psychological distress and other physical health problems (Cole (2006), Morris, Waldo, and Rothblum (2001), Strachan et al. (2007), Ullrich, Lutgendorf, and Stapleton (2003)).

tional on a given level of income. $\Lambda(d)$ is the utility gain of additional consumption from the avoidance of direct discrimination. The presenting agent will choose concealment when $-\Psi < \Lambda(d)$ and disclosure when $-\Psi > \Lambda(d)$. Supposing there exists $d_r = d_r^*$, such that $-\Psi = \Lambda(d*)$, the presenting agent will be neutral between the two choices. For $d < d*$, the presenting agent will choose to disclose and for $d > d*$ he will choose to conceal.

To better emulate real world dynamics, I incorporate two final features. First, I allow the costs of concealment to covary with the level of potential discrimination, which allows Ψ to depend d_r. Second, I introduce heterogeneity in the presenting agent's ability (α) which can influence both Ψ and Λ. The relationship between α and Ψ can be motivated by the potential for ability level to help mediate or exacerbate mental health costs. The relationship between α and Λ is the direct result of allowing π to be a function of α, where higher ability individuals are able to produce more total output. The resulting choice to conceal or disclose the comes down to whether $-\Psi(d_r, \alpha_p)$ is greater than or less than $\Lambda(d_r, \alpha_p)$.

Proposition 1. *Given a population of responding agents, $\mathcal{R}$, with heterogeneity over $d_r \in \mathcal{D}$ optimizing Definition 1; a population of presenting agents, $\mathcal{P}$, with randomly assigned values of $\tau_p \in \{M, m\}$ and $\alpha_p \in \mathbb{R}$ optimizing Definition 2.1; a matching process that randomly pairs members of $\mathcal{R}$ with $\mathcal{P}$; and, assuming that $\exists\ \mathcal{D}^* \subset \mathcal{D}$ such that $\forall\ d_r \in \mathcal{D}^*$ then $-\Psi(d_r, \alpha_p) < \Lambda(d_r, \alpha_p)$ holds; then, the following is true:*

- $\tau_p = T_p \quad \forall\, p \in P \quad s.t. \quad \tau_p = M$
- $\tau_p = T_p \quad \forall\, p \in P \quad s.t. \quad \tau_p = m \quad and \quad d_r \notin D^*$
- $\tau_p \neq T_p \quad \forall\, p \in P \quad s.t. \quad \tau_p = m \quad and \quad d_r \in D^*$

Proposition 1 states that given the setup of the model: (1) all majority presenting agents will truthfully reveal their type, (2) a subset of the minority presenting agents will disclose

their type because the concealment costs outweigh the disclosure gains, and (3) the remaining minority presenting agents will not disclose their type because the gains cannot justify the costs. While the fact that some minorities choose to conceal their type is a straightforward conclusion in this model, it is worth highlighting because it is a mechanism that has not been previously explored in the literature.

In G. Becker (1957), sorting between firms is the only option for avoiding discrimination in the labor market, and given sufficient non-discriminatory firms, the market wage for minorities will not be penalized. As a result, the labor market will be segregated. By contrast, in my model, concealment is the sole escape mechanism. The underlying difference between these two approaches is based in different assumptions regarding search costs and information asymmetries. The conclusions of the Becker model rely on costless search and mobility between firms as well as perfect observability of d_r. Instead, my model implicitly assumes infinite search costs and zero observability of d_r ex-ante. These assumptions are the direct result of the model setup in which agents are randomly paired (i.e. $\tau_p \perp d_r$) and the presenting agents cannot rematch. Since the goal of the model is to isolate the influence of concealment, I am comfortable with these assumptions. In reality, however, there is likely a mixture these two extreme positions, making observed outcomes a combination of Becker's and my own theoretical results.

Corollary 1. *Given the assumptions made in Proposition 1, and further assuming that* $D^* \neq R^{++}$*:*

- $\exists\, p \in P \quad s.t. \quad \tau_p = m,\, \tau_p = T_p$ *and* $d_r \neq 0.$

Corollary 1 states that despite the concealment behavior pointed out in Proposition 1 some minorities will still be observably penalized directly by the responding agent for their type. The range of $d_r \in D^*$ will be censored due to concealment and hence unobservable

in equilibrium, but, because in some cases the concealment costs will outweigh the benefit of avoiding direct penalties, measurable discrimination in the population will exist.

At this point, several interesting questions can be asked with this setup. What type of censoring are we still likely to observe in the realized distribution of d_r? Does this censoring lead to over or under estimates of $E[d_r]$? How does the incentive to conceal change over the ability spectrum? Are those who are most likely to disclose their type positively or negatively selected? Will the range of observed d_r vary across α? Without imposing additional assumptions, these questions are theoretically ambiguous and need to be addressed empirically. But, in order to understand the mechanisms that could generate the different results, I introduce several potential assumptions and trace out their implications for these questions.

Given this framework, the relative utility gain from consumption changes due to concealment ($\Lambda(d_r, \alpha_p)$) will be weakly positive. This gain will grow with respect to d_r and shrink with respect to α_p due to the concavity of indirect utility. $\Psi(d_r, \alpha_p)$ measures the utility loss from the mental health penalty and will be weakly negative. Whether this gap grows or shrinks with d_r or α_p depends on the specific features of the model. For instance, if Ψ is influenced by guilt of concealment and lessens with high d_r, the gap could shrink. On the other hand, if changes in the mental health penalty are driven by the anxiety of being found out which may grow with animus, the gap would widen. This leads to two potential assumptions.

Assumption 1. *"Guilt Costs"* $\quad \frac{\partial \Psi}{\partial d_r} < 0$

Assumption 2. *"Anxiety Costs"* $\quad \frac{\partial \Psi}{\partial d_r} > 0$

Under Assumption 1, the relative incentive to conceal becomes more attractive as d_r grows. The reductions in indirect utility from the consumption losses will get larger and

mental health penalties will get smaller with higher levels of d_r making concealment increasingly more attractive. In this context, the observed average level of d_r in the population would underestimate the true average level of d_r because the higher ranges of d_r would never be observed in equilibrium. Under Assumption 2, however, the relationship between the motive to conceal and d_r is ambiguous. As d_r gets larger, presenting agents face a larger consumption penalty but also higher psychic costs. If $-\frac{\partial \Psi}{\partial d_r} < \frac{\partial \Lambda}{\partial d_r}$, meaning that mental health penalties grow with d_r but at a sufficiently slow rate to not overweigh the direct consequence of d_r, then the same conclusions can be made as in the scenario with Assumption 1. These conclusions are summarized in Proposition 2.

Proposition 2. *Given the structure of Proposition 1, and assuming that Assumption 1 holds or that* $-\frac{\partial \Psi}{\partial d_r} < \frac{\partial \Lambda}{\partial d_r}$*:*

- $Pr[\tau_p = T_p | d_r] \leq Pr[\tau_p = T_p | d_r'] \quad \forall\ d_r > d_r'$
- $\int d_r \times 1[\tau_p = T_p | d_r]\, dd_r \ \leq \int d_r\, dd_r$

The relationship between psychic costs and ability level is unknown and may vary across the ability spectrum. Higher ability agents may be better equipped to cope with stress which could lessen the utility loss from mental health penalties. They also may be more introspective and self-critical which could exacerbate mental health costs.

Assumption 3. "*Coping Psyche*" $\quad \frac{\partial \Psi}{\partial \alpha_p} < 0$

Assumption 4. "*Critical Psyche*" $\quad \frac{\partial \Psi}{\partial \alpha_p} > 0$

Under Assumption 3, higher ability individuals face a lower mental health penalty. Whether the motive to conceal increases or decreases with ability in this scenario will depend on the relative magnitude of $-\frac{\partial \Psi}{\partial \alpha_p}$ and $\frac{\partial \Lambda}{\partial \alpha_p}$. If the mental health penalty is sufficiently small and

the potential discrimination is sufficiently large, censoring could be observed in the top of the ability distribution. However, if Assumption 4 holds, the attractiveness of concealment unambiguously declines with ability level. This would generate censoring in the ability distribution of true minority types, with only those of high ability level revealing their type.

Proposition 3. *Given structure of Proposition 1, and assuming that Assumption 4 holds or that* $-\frac{\partial \Psi}{\partial d_r} < \frac{\partial \Lambda}{\partial d_r}$*:*

- $Pr[\tau_p = T_p|\alpha_p] \geq Pr[\tau_p = T_p|\alpha'_p] \quad \forall\ \alpha_p > \alpha'_p$

- $\int \alpha_p \times 1[\tau_p = T_p|\alpha_p]\, d\alpha \ \geq \int \alpha_p\, d\alpha$

If rather than random matching between α_p and d_r, the model was further relaxed to allow α_p and d_r to be negatively correlated,[5] this would also generate censoring in both observed discrimination levels and ability level in the minorities community. High ability individuals would be relatively more likely to reveal their type and face the least amount of discrimination, while low ability individuals would be relatively less likely to come out. This would create the impression that minorities are affluent and empowered, in spite of the fact minority status could be completely uncorrelated to ability and discriminatory views might be highly prevalent.

This framework provides two important conclusions. First, cross sectional wage penalties may be upward or downward biased due to endogenous identity selection. Depending on the covariance between concealment costs, discrimination tastes and productivity, the sign of this bias can potentially be determined. Many plausible scenarios lead to an underestimate of market discrimination when using the realized wage distribution of revealed minorities.

[5]This could be motivated on the basis of observed correlates with socioeconomic status.

Second, despite the fact that two areas observe the same wage penalty, it would be incorrect to conclude that both exhibit similar distributions of d_j. Instead, the concealment option imposes a floor on wage penalties indicating that individuals are incurring incremental gains in discrimination through alternative channels rather than wage rates. One such outlet could be negative impacts to mental health.

2.4 Empirical Research Design

The empirical research design utilizes two sources of variation to explore how discrimination against concealable characteristics generates non-standard results. First, I proxy same-sex attraction using the fraternal birth order effect, which has its origins in the fields of developmental psychology and microbiology. The empirical relationship on which this is based is the consistent finding that men with more older brothers are more likely to express same-sex attraction and identify as homosexual. There is no effect of older brothers on women, and there is no effect of sisters or younger brothers on men or women. The evidence on this finding is presented in Section 2.4.1.

The second source of variation is a measure of the degree of discrimination or anti-gay attitudes in the county of birth of individuals. Information on the adoption of policies that impact the LGB community as well as the concentration of demographic groups that are known to oppose LBG rights are combined in a principle components analysis to generate a single discrimination index. The goal of this index is to identify respondents who were exposed to more and less discriminatory attitudes during their childhood. The exposure could be an indication of parental preferences, attitudes of peers or the general local attitude towards sexual minorities. The variation in exposure differential incentives to conceal. Detailed description on the generation of the index is described in Section 2.4.3.

To conduct my analysis, I use the National Longitudinal Survey of Youth 1979 (NLSY79) panel data files linked to the location geocodes through an agreement with the Bureau for Labor Statistics. This data is one of the few instances in the United States where it is possible to observe birth order information with sex of siblings identified, location of birth and extensive information on adult outcomes over the life-cycle.[6]

The NLSY79 started in 1979 with a sample of over 10,000 unique respondents between the ages of 12 and 18. Survey participants were reinterviewed on an annual basis until 1994 and a biannual basis thereafter. Data collection is still ongoing. Information was collected on a variety of life-cycle issues, with particular emphasis on adolescent transition to adulthood during the early years. I am forced to remove approximately 1,200 respondents from my analysis for two reasons: first, I restrict my analysis to respondents who have a valid U.S. state and county for place of birth, location at age 14 or in 1979; second, respondents were required to participate in their 1993 interview when sibling composition was collected.[7] My final base sample is 8,763 unique respondents.

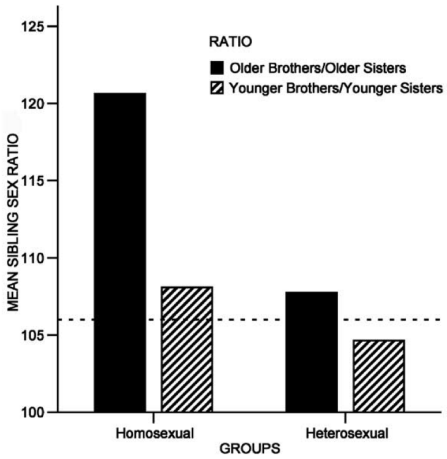
2.4.1 Fraternal Birth Order Effect

The epidemiological relationship known as fraternal birth order effect can be illustrated in Figure 2.2. This figure shows that men who identify as homosexual are much more likely to have more older brothers than older sisters. This empirical relationship has been documented in numerous studies (see Blanchard (1997)). Consistently, they find that each additional

[6]The only other potential sources of sibship composition, geographic information and adult follow up were the ADD Health dataset which started in 1994 and the National Longitudinal Survey of Youth 1997. I choose to focus on the NLSY79 due to the greater amount of geographic variation as well as life-cycle information. In the NLSY79 I am able to track outcomes through age 45, whereas the corresponding maximum age in ADD Health is only 28 years old and for the NLSY79 is 24 years old.

[7]The majority of excluded cases were removed due to non-interview in 1993 rather than missing place of birth information.

Figure 2.2: The Fraternal Birth Order Effect



Source: Blanchard (1997)

older brother for men increases the odds of homosexual identity by 25 to 33 percent.

The dominant hypothesized mechanism (known as the Maternal Immune Hypothesis[8]) is that male-specific, Y-linked minor histocompatibility (H-Y) antigens form in the mother's body after exposure to male tissue (e.g., blood) during the pregnancy and childbirth of a male child (see Müller (1996) for review of H-Y antigens; see Blanchard (1997) for complete discussion of biological hypothesis). This tissue would be considered foreign to the mother's body and would generate an immune response. The mother's antigens would then remain in the mother's immune system and affect the development of future male children. It could be either that each successive male child increases the number of antigens circulating thereby increasing the odds of abnormal development or instead each successive male child increases the probability of antigens ever forming in the first place.

Researchers point to several types of evidence to support this specific mechanism (see Blanchard and Klassen (1997)). First, epidemiologists have long understood that male fe-

[8]The origins of this theory come from MacCulloch and Waddington (1981)'s work, except for the fact these authors focused on fetal testosterone rather than H-Y antigens.

tuses are more antigenic to human mothers than female fetuses leading to more maternal immune reactions (Gualtieri and Hicks (1985), Komlos et al. (1990)). Studies of tissue localization indicate that the H-Y antigen is strongly represented on the surfaces of brain cells, and therefore may play an active role in brain development. Finally, animal research suggests it is plausible that H-Y antibodies could be present in sufficient quantities to affect sexual differentiation in the fetal brain, without also affecting the development of the genitalia. Singh and Verma (1987) found that male offspring of mother mice immunized against H-Y prior to pregnancy were much less likely to mate successfully with receptive females compared to a control sample.

The most popular rival hypothesis to explain the fraternal birth order effect is based on a postnatal, psychosocial channel rather than a prenatal, biological channel. Researchers from this perspective argue that a boy's chances of engaging in same-same sexual interaction with older males increases in proportion to the number of his older brothers (Bern (2000), Sulloway (1996); see Blanchard (1997) for complete discussion of all psychosocial hypotheses). This argument posits that increased same-sex sex-play during childhood increases a boy's probability of developing a homosexual orientation later in life (Slater (1958)).

Research disputes the logic in Slater's argument. In contrast to what Slater would expect, Wellings, Field, Johnson, and Wadsworth (1994) found that boys attending all-male boarding schools were more likely to have same-sex sexual experiences as adolescents compared to those attending co-ed schools, yet no difference was observed in later life sexual identification.

Several studies have sought to further test the biological rather than social nature of this relationship. The best piece of evidence is in Bogaert (2006) which finds that in sample of complex families (e.g., children from multiple parents, divorced parents that separate children, etc) the number of biological older brothers (i.e. not male step siblings) was the only significant predictor of homosexuality. Furthermore, the amount of time spent with spe-

cific siblings during childhood (biological/non-biological, male/female) did not significantly predict the sexual orientation of a child.

Finally, critics of the fraternal birth order effect point to lack of national, population-based evidence that support the empirical relationship being observed. Almost all studies that find the fraternal birth order hypothesis utilize convenience sampling of college students and openly gay individuals, sex offenders, or representative surveys of specific metropolitan areas (Bogaert (2005)). Attempts at nationally representative population-based studies find weakened statistical relationships. For instance, Francis (2008)'s study of ADD Health data finds a positive but marginally insignificant coefficient on older brothers and homosexuality.[9]

2.4.2 Family Composition and Birth Order Literature

The findings of Butcher and Case (1994), S. Black, Devereux, and Salvanes (2005) and related literature should raise caution when considering use of birth order and sibship information as a proxy to measure sexuality. Butcher and Case (1994) found that women's educational choices varied systematically with the composition of her siblings while men's did not: having an older brother significantly increased women's education. Using Norwegian data, S. Black et al. (2005) found that later born children complete fewer years of education, with corresponding negative impacts on adult earnings and employment, particularly for women.

[9]The work presented in this paper can reconcile these conflicting results. Because of convenience sampling at universities or representative sampling from major U.S. cities, the FBO literature has been estimated off of a sample of individuals who are least likely to conceal. Out of the fact that they study individuals who live in the non-discriminatory areas, they observe a population for which self-identified sexual orientation and same-sex attraction are highly co-linear.

In contrast, population-based studies broaden the sample to individuals facing high discrimination costs, thereby weakening the relationship between self-identified sexual orientation and innate same-sex attraction. This interpretation is consistent with my finding that men with more older brothers are more likely to have same-sex, unrelated adults in their households rosters through age 45, but those facing higher discrimination costs back away from these "relationships" enough so that more than half of individuals experiences same-sex attraction would be indistinguishable from heterosexual respondents.

The potential bias incurred from alternative channels through which fraternal birth order may affect outcomes is important to acknowledge. However, given the state of the birth order literature, I can attempt to identify the sign of the bias in my estimation. The details of my econometric specification are discussed later in this section, but generally speaking I seek to examine the relative impact of older brothers on men and women. The Butcher and Case findings would tend to negatively bias any coefficients on human capital accumulation due to women's relative gain from older brothers. Positive impacts on human capital would therefore be a lower bound.

The S. Black et al. (2005) results present the opposite issue. While their finding that children systematically suffer worse educational and employment outcomes from higher parity births should be addressed by comparing relative outcomes for men and women with more older brothers, what poses a greater problem is that women's outcomes tend to suffer more relative to men's outcomes. This creates a positive bias in human capital accumulation.

2.4.3 County Index of Discrimination

The goal of this project is to study how minority individuals with the option of concealment respond to increasing costs of disclosure. To this end, I estimate a index of local discrimination based on the timing of adoption of various pieces of LGB-focused public policy initiatives at the city, county and state-level as well as a variety of community characteristics measured at the county level. The measures of homosexual protections and exclusions as well as community characteristics are combined using principle component analysis, with the first factor representing cross-sectional variation in discrimination against sexual minorities across counties in the United States. Focusing on how the correlation between the birth order proxy and individual outcomes changes across the spectrum of discriminatory background may help eliminate other confounding factors associated with birth order.

My measure of local discrimination is mapped onto individual observations based on their county of birth.[10] If innate sexual preference is biologically determined, information about county of birth is unlikely to be correlated with underlying sexual preferences. County information should, however, provide a measure of the attitudes that parents, friends, employers and local communities have towards sexual minorities. Despite the fact that respondents may migrate away from these specific counties at later points in their lives, the index will continue to measure the lasting affect of juvenile and adolescent exposure to discriminatory attitudes which might include the alienation of family and friends conditional on disclosure.

The variables that I use in the factor analysis are based on local adoption of public policies impacting the gay community as well as local characteristics that have been linked to homophobic attitudes in the literature. The public policy measures include the year of adoption of anti-discrimination legislation for private employment and public employment, the first year of formal recognition of same-sex relationships[11], the first year domestic partner benefits were offered from county government, the first year of hate crimes legislation inclusive of sexual orientation, the first year civil union or same-sex marriage was offered, the first year same-sex marriage was banned at the state-level by constitutional amendment or legislation, and the earliest adoption of other protections based on sexual orientation.[12] In addition, I also construct indicator variables to measure whether localities have never adopted each of these policies as of the end of 2012.

The timing of policy adoption was collected from several sources. Reports compiled by the Lambda Legal, the National Gay and Lesbian Task Force and the Human Rights Campaign

[10]When county of birth is not observed or not located in the United States, I use the respondent's U.S. county at age 14 or U.S. county in 1979 depending on which first identifies individuals living in the United States.

[11]This includes domestic partnership registries, civil unions and same-sex marriage.

[12]These include credit, housing, union, eduction and public accommodation protections

served as the foundation of this information. When the timing of adoption conflicted between sources, dates were verified directly with county statutes. Finally, there were many instances in which policies were adopted at both the county and state level. When the timing of these enactments varied, the earliest year of adoption was selected for the analysis.

The county-level community characteristics used in the factor analysis include the Republican vote share, the percentage of residents identified as religious adherents, the log population density (measured as persons per square mile), the percent of residents who have graduated from college, and the percent of land identified as urban. The Republican vote share was gathered from the Congressional Quarterly's *Voting and Elections Collection.*[13]. The per capita rate of religious adherents was estimated using the *Churches and Church Membership in the United States* collected by the Glenmary Research Center.[14] All other variables were collected from the United States Census.

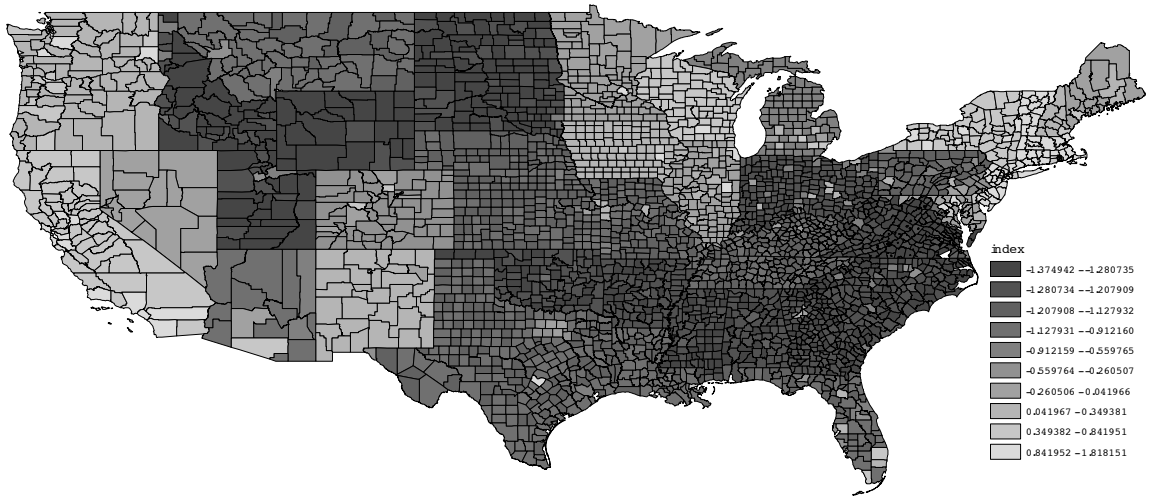
The results of the factor analysis are presented in Figure 2.3. This figure presents a mapping of the predicted first factor, which I interpret as an index of the discriminatory environment faced by the LGB community in United States counties. The index has been normalized to a mean of zero and standard deviation of one using total population per county in 1970 as weights. The average individual would thus be exposed to an index score of zero. Counties that have positive index scores are shaded light gray and are relatively more protective of and friendly to gay men and women. In contrast, dark gray counties have negative index scores and are more antagonistic towards sexual minorities.

Regional trends are apparent; Southern and Midwestern states tend to have the least favorable climate towards gay men and women while New England and Western States tend

[13]To focus on social conservative Republicanism, I average over Republican vote share in a county between 1992 and 2008

[14]To minimize statistical noise in this data, I average over membership data from 1971, 1980, 1990 and 2000.

Figure 2.3: Map of County-Level Index Scores in the United States



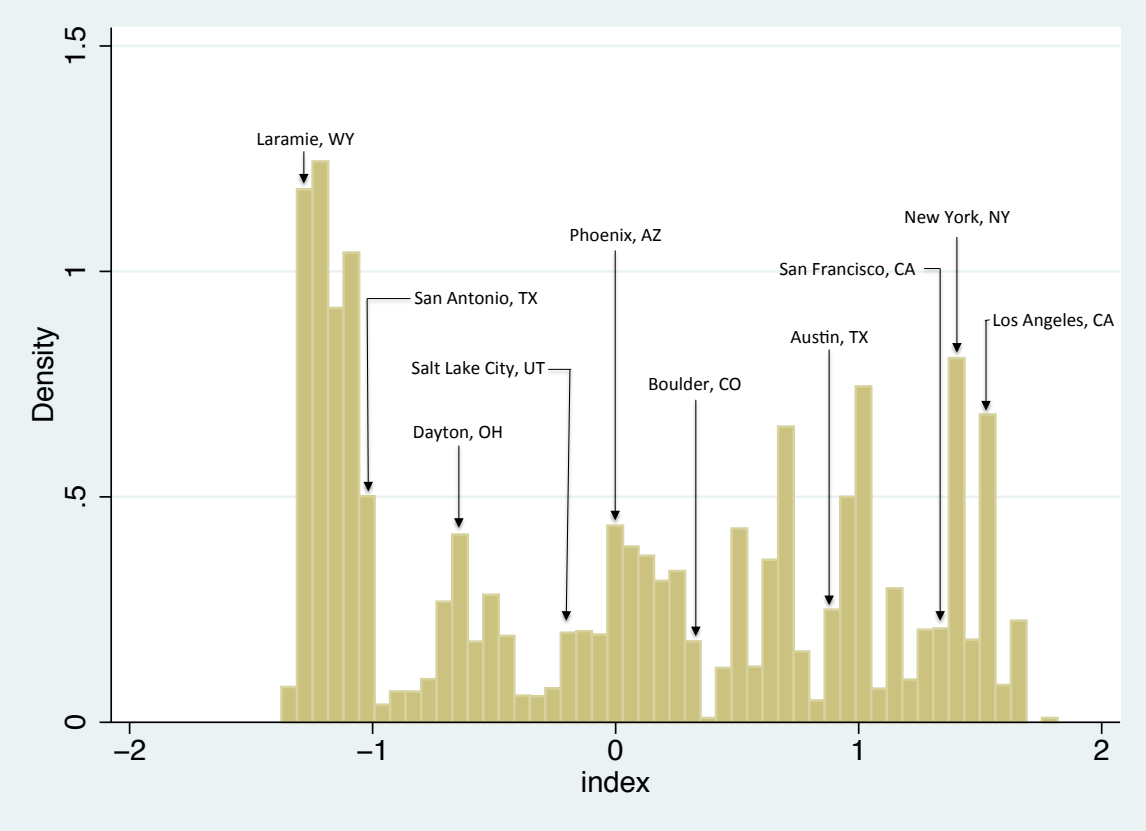
to be more favorable. Inclusion of regional controls or state fixed effects will be an important consideration. Despite strong regional trends, however, I still observe a fair amount of within-state variation. Counties that include major metropolitan areas or college communities tend to be more favorable towards sexual minorities.

Figure 2.4 shows a histogram of population exposure to various index values. Several cities are highlighted along the distribution to help quantify the meaning of standard deviation change in the index. The distribution is not unimodal; there is a larger percentage of the population concentrated at the lower tail of the distribution. This trend reflects the fact that most counties have not enacted any protections for sexual minorities.

2.4.4 Econometric Specification

The sources of variation discussed above motivate a quasi-triple differencing strategy in the econometric specification. Intuitively, I examine how men with more older brothers facing different discrimination costs compare to women with more older brothers facing different discrimination costs. Since women's sexuality does not respond to the number of

Figure 2.4: Distribution of Index Scores



older brothers, this should allow us to identity the impacts of same-sex attraction and its interaction with increasing discrimination. This is summarized in the following equation.

$$y_{i,t,b} = \alpha_{0,t} + \beta_t X_i + \delta_{1,t}[ob_i \times m_i \times \iota_b] + \delta_{2,t}[ob_i \times m_i] + \delta_{3,t}[ob_i \times \iota_b] + \delta_{4,t} ob_i + \delta_{5,t} m_i + \delta_{6,t}\iota_i$$
$$+ \gamma_{1,t}[ts_i \times m_i \times \iota_b] + \gamma_{2,t}[ts_i \times m_i] + \gamma_{3,t}[ts_i \times \iota_b] + \gamma_{4,t} ts_i + \tau_B + e_{i,t,b}$$

In this equation, ob_i is the number of older brothers, m_i is an indicator variable for the respondent being male, and ι_b is the index assigned according to county of birth. To guarantee that my estimates are not contaminated a differential effect of family size on men and women in more and less discriminatory areas, I also include interactions with total siblings (ts_i). Likewise, I include the total number of older sisters, younger sisters and younger brothers as well as additional controls in X_i. To absorb regional variation, I include state of birth fixed effects (τ_B). Finally, standard errors are clustered at the county of birth (b) to address potential common shocks at this level.

The coefficient of interest from this equation is $\delta_{1,t}$. It should estimate the effect of decreasing discrimination faced by individuals more likely to experience same-sex attraction. The coefficient is indexed on t to account for the fact that the coefficient may evolve over time as the individuals age. Respondents in NLSY79 started out between the ages of 12 and 18, and in the most recent data available are in the 45 and 51 age range. My analysis focuses on outcomes observed between the ages of 18 and 45 for all respondents to maximize the overall sample.

There are clear advantages to performing an instrumental variable regression over the reduced-form specification outlined above. Instrumental variable analysis would scale the coefficients so that the magnitude of the effect on the affected population could be determined without relying on untested assumptions. However, this is not possible because the aim

of this research is to study how concealment of underlying preferences impacts life-cycle behavior, and the variable I would instrument for is simply not observed in the data.

2.4.5 Robustness Check

To rule out systematic bias in my econometric specification, I run a series of robustness checks to confirm that my identification is uncorrelated to factors which pre-date the onset of puberty and sexual maturation. Tables 2.1 and 2.2 present these results.

In Table 2.1, I test whether parents' characteristics at birth and during the respondent's childhood are correlated to the explanatory variables of interest. The dependent variables are: mother's age at respondent's birth, mother's educational attainment, whether the respondent lived with their biological mother at birth and age 5, whether the mother was reported to be working when the respondent was 14 years old, father's age at respondent's birth, father's educational attainment, whether the respondent lived with their biological father at birth and age 5, and whether the father was reported to be working when the respondent was 14 years old. The coefficient of interest ($\delta_{1,t}$) is presented in the first row of the table. There is no systematic correlation observed between the triple interaction and maternal or paternal characteristics.

In Table 2.2, I consider whether children's characteristics that pre-date the development and expression of sexual attraction covary with the triple interaction of older brothers, male and the index. I test whether there is a significant relationship with preschool attendance, presence of a foreign language spoken at home during childhood, whether the respondent reported being shy at age 6, household receipt of magazines/newspaper at age 14, household having a library card at age 14 and whether the household lived in an urban area when the respondent was 14 years old. These tests show no statistically significant relationship between the key independent variable and pre-pubescent respondent characteristics. This

Table 2.1: Robustness Check: Preexisting Parental Characteristics

Variables	Mom Age at Birth	Mom Educ.	Lived with Mom at: Birth	Lived with Mom at: Age 5	Mom Worked (Age 14)	Dad Age at Birth	Dad Educ.	Lived with Dad at: Birth	Lived with Dad at: Age 5	Dad Worked (Age 14)
# Older Brothers × Male × Index	0.0584	-0.0834	0.00313	0.00411	0.00125	0.0837	0.0226	0.00399	0.0117	-0.00350
	(0.138)	(0.0775)	(0.00346)	(0.00495)	(0.0119)	(0.170)	(0.0986)	(0.00793)	(0.00932)	(0.0503)
Constant	25.18***	12.61***	0.958***	0.935***	0.662***	28.06***	13.20***	0.885***	0.825***	0.243***
	(0.174)	(0.0889)	(0.00638)	(0.00760)	(0.0145)	(0.211)	(0.113)	(0.0101)	(0.0119)	(0.0574)
Observations	8,046	8,296	8,485	8,487	8,788	7,327	7,479	8,478	8,460	8,788
R-squared	0.371	0.111	0.008	0.005	0.023	0.304	0.116	0.005	0.004	0.009

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 2.2: Robustness Check: Prepubescent Child Characteristics

	Attend Preschool	Foreign Lang. at Home	Shy at Age 6	Rec'd Magazines (Age 14)	Rec'd Newspaper (Age 14)	Library Card (Age 14)	HH in Urban Area (Age 14)
# Older Brothers × Male × Index	0.00736	0.0110	-0.0145	0.00596	-0.0123	-0.0113	-0.00318
	(0.0112)	(0.0101)	(0.0245)	(0.0117)	(0.0110)	(0.0110)	(0.00907)
Constant	0.303***	0.153***	-0.129***	0.746***	0.893***	0.822***	0.811***
	(0.0142)	(0.0123)	(0.0311)	(0.0142)	(0.0125)	(0.0127)	(0.0114)
Observations	8,639	8,991	8,692	8,943	8,965	8,958	8,995
R-squared	0.005	0.066	0.011	0.056	0.038	0.051	0.044

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

provides the most concrete evidence that $\delta_{1,t}$ is not biased by the birth parity and parental investment issues considered in Butcher and Case (1994) and S. Black et al. (2005).

2.5 Results

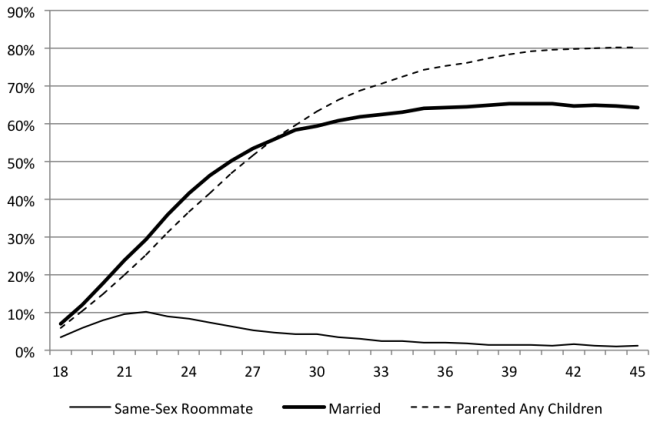
In this section, I present the results of my empirical analysis. The aim of the section is twofold. First, I seek to provide documentation that nondisclosure actually is present in my data. Second, given the concealment behavior, I want to document how this impacts life-cycle outcomes. The results are separated by broad categories, including: (1) relationship and family formation, (2) migration and identity, (3) mental health, mental outlook and substance abuse, and (4) educational attainment and employment.

2.5.1 Relationships and Family Formation

I begin my analysis by looking at relationships and family formation. While the National Longitudinal Survey of Youth 1979 (NLSY79) does not have any survey questions asking respondents to self-identify their sexual orientation or to report same-sex domestic partners, I proxy for sexual preference through observing same-sex, unrelated roommates appearing on the respondent's household rosters over time. Complementing this analysis, I also consider marital and parenting status.

Clearly, not all individuals with same-sex roommates are gay. Figure 2.5 shows the trends in these variables in the NLSY79 sample over the age range of 18 to 45. Having a same-sex roommate peaks when respondents are in their early 20's, and declines to roughly 2% during later years. The strength of this proxy as a measure for sexual orientation will be weakest when respondents are in their late teens and early 20's when having a roommate is a standard practice related to educational attainment and cost savings. In later years,

Figure 2.5: Timeline of Relationships and Family Formation



however, the remaining presence of a same-sex roommate is likely a better proxy for of homosexuality.

Table 2.3 reports the coefficients for the triple interaction from regressions considering respondents relationships and family formation behavior between ages 18 and 45. The table is split into three panels. The first considers the presence of same-sex, unrelated adult roommates on the respondent's household roster. The second examines being married and the third reports on whether respondents have ever parented a child. The regressions are split into four distinct age bins: 18 to 24, 25 to 30, 31 to 37 and 38 to 45.

In the first panel, we observe evidence that both the fraternal birth order effect is operating in the NLSY79 sample and that discrimination discourages disclosure. As expected, regressions considering respondents between ages of 18 to 30 are noisily estimated and not statistically significant. The large number of heterosexual respondents making use of platonic roommates for cost savings purposes biases the results in earlier years towards zero. In later years, when respondents are older and heterosexual individuals cease having same-sex roommates, there are a statistically significant coefficients of 0.00958 (ages 31 to 37) and 0.00868 (ages 38 to 45).

One interpretation of these results is that for each standard deviation increase in the index (which is normalized to mean of zero and standard deviation of one), the fraternal birth order effect is strengthened by 0.91 percentage points. For men born in San Francisco County, CA, which has an index score of 1.39, this would translate into a 1.27 percentage point relative increased likelihood of having a same-sex partner for each older brother. Conversely, for men born in Mobile County, AL, which has an index score of -1.21, this would translate into a -1.1 percentage point relative decline in having a same-sex partner for each older brother. The coefficient $\delta_{2,t}$ (not reported in Table 2.3) measures the average impact of older brothers for men in the sample across all discrimination environments. During the later period, average effect is measured as -0.000155 (0.00286) for ages 31 to 37 and -0.000829 (0.00911) for ages 38 to 45. The resulting total effect of $\delta_{1,t}$ and $\delta_{2,t}$ is close to zero or negative for many counties in the United States, which may help explain why developmental psychologists have had difficulty in replicating the fraternal birth order effects using nationally representative samples.

To put the magnitude of the results in perspective, I consult existing estimates in the fraternal birth order literature. The fraternal birth order effect is traditionally measured as an increase in the odds rather a linear increase in probability, with most authors reporting an increase between 25 percent to 33 percent for each older brother for men. In order to translate the results from the literature into comparable statistics for this analysis, one needs to identify an overall prevalence rate of homosexuality in the United States.[15] Assuming that 5 percent of the male population is gay, the estimates in the literature translate into about a 1.07 to 1.33 percentage point increased probability of homosexuality among men for each older brother.

[15]Researchers have suggested the prevalence of homosexuality in the United States ranges between 2 percent and 10 percent of the overall population, although recent measurements of same-sex coupling consistently suggest estimates closer to the lower bound.

Before turning to other results, an additional source of downward bias needs to be addressed. Gay men exhibit strikingly low partnership rates in the general population. These individuals, who opt against cohabitation, will show no measurable response in the data to changing levels of discrimination even though true expression of sexual identity may still be impacted. D. Black, Gates, Sanders, and Taylor (2000) report that among men in the General Social Survey or the National Health and Social Life Survey who had same-sex sex in the last year, only 28.6 percent were currently cohabitating with a domestic partner. Partnership rates by age group are not reported making it difficult to determine the proper scaling of the coefficients by age bin. Using 28.6 percent would suggest a scaling of 3.497 to the coefficients. A more conservative partnership rate of 40 percent would translate to scale factor of 2.5. The resulting scaled coefficient for individuals over 30 years old would be 0.02275, which I will use as my best approximation of a "first stage" in interpreting the magnitude of impacts on other results.

The second and third panel consider secondary indicators of concealment behavior: marital and parenting status. Neither show strong correlations with the triple interaction. It may be that the prevalence of single, heterosexual bachelors through age 45 makes marriage and childbearing an unnecessary signal for successful concealment.

2.5.2 Migration and Identity

Another interpretation of the results in Table 2.3 is that gay men from discriminatory areas do not have the opportunity to match with same-sex partners. Given recent interest in spousal matching behavior and marriage premiums among economists, this is another interesting possibility.

To distinguish between these two potential theories, I test whether the triple interaction term is correlated with migration behavior and identity expression. If gay men from dis-

Table 2.3: Relationships and Family Formation

Variable	**Average Outcome in Age Range**			
	18 to 24	**25 to 30**	**31 to 37**	**38 to 45**
	Same-Sex, Unrelated Adult in Household Roster			
# Older Brothers × Male × Index	0.00582	0.000363	0.00958**	0.00868***
	(0.00468)	(0.00464)	(0.00387)	(0.00318)
N	54,614	58,766	34,124	25,524
	Married			
# Older Brothers × Male × Index	-0.00389	0.00315	0.00123	-0.00949
	(0.00911)	(0.0119)	(0.0127)	(0.0126)
N	54,621	59,333	38,189	30,965
	Parented Any Children			
# Older Brothers × Male × Index	-0.0178**	-0.00451	-0.0120	-0.00391
	(0.00891)	(0.0121)	(0.0128)	(0.0123)
N	54,896	60,230	38,386	30,973

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

criminatory areas cannot match to sexual partners in their hometowns, they face the highest incentive to migrate away to more favorable counties. Likewise, these individuals would exhibit a relatively larger gap in measures of conservative identity (e.g., religiosity and gender ideology) compared to heterosexual individuals born in the same county. Tables 2.4 and 2.5, however, show the opposite conclusions.

Men with more older brothers from more favorable counties are more likely to move away from their county of residence at age 14 between the ages of 18 to 24. There is no significant difference in migration rates during later years. I interpret this result as indicating that men who are able to express their sexual identity are more likely to accelerate their first move away from home.

There is also evidence that men with more older brothers from less discriminatory areas migrate to even more favorable locations. In the second panel of Table 2.4, I report the correlations between the triple interaction and the index score of their new destination counties after migration. Between ages 25 and 30, I see a significant correlation of 0.0398 indicating that men with more older brothers who are born to more favorable locations choose to migrate to even more accepting counties. Similarly, across all age bins I observe a negative correlation between -0.0431 and -0.0504 for the triple interaction and the standardized Republican vote share in the destination county. Republican vote share is measured as the average percent of votes cast for Republican presidential candidates in a given county between 1992 and 2008, which is then demeaned and divided by the standard deviation. Given a "first stage" of 0.02275, the interpretation of this result is that gay men opt to move to counties that are 1.74 standard deviations more favorable towards sexual minorities and about 2 standard deviations less likely to vote for Republican presidential candidates for each 1 standard deviation increase in the index based in their county of birth. This consolidation of gay men in the least discriminating areas may help explain why large gay communities

Table 2.4: Migration

Variable	Average Outcome in Age Range 18 to 24	25 to 30	31 to 37	38 to 45
	Migrated away from Home County[a]			
# Older Brothers × Male × Index	0.0209*	0.00997	-0.00437	0.00842
	(0.0119)	(0.0134)	(0.0147)	(0.0147)
N	50,295	55,180	35,858	29,488
	Contemporaneous Discrimination Index			
# Older Brothers × Male × Index	0.00617	0.0398**	0.0250	-0.000920
	(0.0175)	(0.0201)	(0.0216)	(0.0228)
N	52,051	57,229	37,273	30,401
	Standardized Republican Vote Share			
# Older Brothers × Male × Index	-0.0431**	-0.0504**	-0.0498**	-0.0507*
	(0.0219)	(0.0239)	(0.0249)	(0.0264)
N	52,050	57,229	37,273	30,401

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

[a] Home county defined as the county the respondent lived in at age 14.

Table 2.5: Identity Investment

Variable	**Attends Religious Services Weekly**[a]	**Gender Ideology Score**[b]
# Older Brothers × Male × Index	-0.0148*	0.0189**
	(0.00758)	(0.00846)
N	25,524	33,678

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
[a]Religiosity was measured by the NLSY79 in 1979, 1982 and 2000.
[b]Gender ideology was measured by the NLSY79 in 1979, 1982, 1987 and 2004.

developed in a few isolated locations.

I also consider measures of conservative identity formation in Table 2.5 through looking at whether respondents report attending religious services a weekly basis or more as well as their self-reported views on gender and the role of women in society. Attendance to religious services was asked to respondents in 1979, 1982 and 2000. The gender ideology score was collected in 1979, 1982, 1987 and 2004. The gender ideology score was created using a battery of questions on the role of women in society. Respondents are asked to report how much they agree or disagree with seven separate statements like, "A woman's place is in the home, not the office or shop" or "It is much better if the man is the achiever outside the home and the woman takes care of the home and family". The responses were aggregated such that a higher score reflects more progressive views of gender, and then were standardized to a mean of zero and a standard deviation of one.

The results in Table 2.5 suggest that gay men from more favorable locations are least likely to embrace social conservatism. For each standard deviation change in the index, men with more older brothers are 1.48 percentage points less likely to attend religious services weekly

and score 0.0189 standard deviations higher on the gender ideology spectrum. Scaling by the "first stage", these results indicate that gay men from areas with a one standard deviation increase in index observe a 65 percentage point decline in likelihood of attending weekly religious services and a .83 standard deviation increase in gender ideology score.

These results are inconsistent with the hypothesis that gay men from discriminating counties are not concealing their identity, but instead simply cannot match to a romantic partner. Men with more older brothers from conservative localities wait to move away from their home county until later in life and they move to locations that are relatively more conservative. Compared to their peers, they are more likely to invest in social conservative signals as measured by religiosity and views on gender. Instead, these results seem more consistent with the hypothesis that these men are exerting extra effort to signal heterosexuality and social conservatism to mask their underlying nonconformity.

2.5.3 Mental Health, Mental Outlook and Substance Abuse

I now turn my focus to considering what the implications are of concealment on life-cycle outcomes. As the theory in Section 2.3 suggests, minorities may opt to experience the impact of discrimination through indirect channels, mainly negative mental health shocks. To test this concept, I study the relationship between the triple interaction and several measure of mental health and outlook. The NLSY79 collected several rounds of mental health assessments allowing one to observe respondents' self-esteem and depression symptoms almost every 5 years. The two main instruments used by the NLSY79 were the Rosenberg Self-Esteem Scale and the Center for Epidemiological Studies Depression (CES-D) Scale.

The Rosenberg scale is designed to measure self-esteem through asking respondents to report to what degree the agree or disagree with a variety of statements. Examples of these questions include, "I am a person of worth, at least on an equal plane with others" and

Table 2.6: Mental Health: Rosenberg Self-Esteem Scale and Center for Epidemiological Studies Depression Scale (CES-D)

	Standardized Score		
Variable	**Overall**	**1980[a]**	**1987[a]**
# Older Brothers × Male × Index	0.0468**	0.0610**	0.0801***
	(0.0185)	(0.0288)	(0.0307)
N	45,441	8,593	8,390
	1992[b]	**1994[b]**	**2006[a]**
# Older Brothers × Male × Index	0.0495*	0.0234	0.0383
	(0.0295)	(0.0292)	(0.0317)
N	8,759	8,713	7,205

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
[a] Standardized Rosenberg Self-Esteem Scale
[b] Standardized Center for Epidemiological Studies Depression Scale

"On the whole, I am satisfied with my life." The Rosenberg scale was administered in 1980, 1987 and 2006. The CES-D scale collects information on both negative symptoms (e.g., feeling lonely, depressed, poor appetite, etc.) and positive symptoms (e.g., feeling happy) and aggregates the information into a composite score. Symptom severity is measured by asking the frequency of occurrence of each item over the preceding week, with responses ranging from 0 (rarely or none of the time/1 day) to 3 (most or all of the time/5-7 days). The CES-D scale was administered in 1992 and 1994. I standardized each mental health score to mean zero and standard deviation one to make results comparable. I also normalized the scale so that a higher score indicates a better outcome. Table 2.6 presents the results.

Point estimates consistently show a positive correlation between the triple interaction and measures of mental health; statistically significant results are observed in 1980, 1987 and 1992. The Rosenberg scale and the CES-D scale both find strikingly similar magnitudes. Men with more older brothers experience a 0.0468 standard deviation improvement in mental

health outcomes between 1980 and 2006 for each standard deviation increase in the index. Given the "first stage," these results indicate that gay men's mental health improves by 2.06 standard deviations for each 1 standard deviation improvement in the index of their county of birth.[16]

The weakening statistical relationship over time could be the result of several factors. First, the sample size is decreasing in successive waves due to respondent attrition in the NLSY79. It could also be the case that changing attitudes towards the LGB community nationwide in the 1990's and 2000's improved mental health outcomes disproportionately for those from the most discriminatory backgrounds.

Two measures of mental outlook were also collected in the NLSY79. In 1979, respondents completed the Rotter Internal-External Locus on Control questionnaire, and in 1992, they also completed the Pearlin Mastery survey. Both of these instruments aim to measure how much respondents believe they have control over their own outcomes. As in the previous analysis, I have standardized the scores to ease interpretation with a higher score indicating respondents experience a greater sense of control over one's life. Table 2.7 shows the results.

I find $\delta_{1,t}$ is consistently positive when regressing mental outlook on my specification. The results are not statistically significant, but are consistent with the previous findings regarding mental health. The interpretation of the coefficients is that men with more older brothers from more favorable birth counties are more likely to identify self-motivation and self-determination as the primary factor affecting their outcomes instead of chance or luck. The pooled regression coefficient is 0.0313. Scaling by the "first stage," gay men experience a 1.37 standard deviation improvement on the mental outlook score for each 1 standard deviation increase in the birth county index.

[16]The large magnitude observed in Table 2.6 suggest that the coefficients from Table 2.3 perhaps should be scaled at a higher rate. This would increase the size of the "first stage" and spread the reduced form effect across a larger set of individuals.

Table 2.7: Mental Outlook: Rotter Locus of Control and Pearlin Mastery Scores

	Standardized Score		
Variable	**Overall**	**1979[a]**	**1992[a]**
# Older Brothers × Male × Index	0.0313	0.0334	0.0302
	(0.0212)	(0.0280)	(0.0284)
N	17,622	8,902	8,720

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1
[a] Standardized Rotter Locus of Control Score
[b] Standardized Pearlin Mastery Score

Table 2.8: Alcohol Use Patterns

	Days Consumed 6+ Alcoholic Drinks			
Variable	1982	1988	1994	2002
# Older Brothers × Male × Index	-0.00190	-0.0289	-0.00760	-0.0658*
	(0.0508)	(0.0462)	(0.0462)	(0.0340)
N	7,972	8,112	7,072	7,551

	Experienced Aggressive Emotions while Inebriated		
	1984	1985	1988
# Older Brothers × Male × Index	-0.033***	-0.0234**	-0.0060
	(0.0108)	(0.00967)	(0.0100)
N	8,268	8,712	8,113

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

The last set of outcomes I consider in this section relate to alcohol use patterns. I consider impacts on binge drinking (days spent consuming six or more alcoholic drinks in a day) and reported feelings of aggression while drinking in the past month. Table 2.8 shows the results of this analysis. I find suggestive evidence that men with more older brothers in the most discriminatory areas may have elevated rates of alcohol abuse. In 2002, the $\delta_{1,t}$ is negative and significant with a coefficient of -0.0658. Smaller, negative coefficients are reported for other time periods. Stronger results are observed for expression of aggressive emotions during consumption of alcohol. These results suggest that alcohol may serve as a coping mechanism to deal with the mental health impacts of concealment.

2.5.4 Educational Attainment and Employment

The sizable effects on mental health suggest that concealment should also impact human capital accumulation and labor market experience. Table 2.9 shows the impacts on AFQT scores, highest grade completed by age 30 and college graduation rates. AFQT scores and educational attainment are demeaned and standardized by dividing by their standard deviation to make the coefficients more comparable. The results show that men with more older brothers born in more favorable counties exhibit relatively better educational attainment across all three measures. The estimated coefficients are 0.0479 (AFQT), 0.0651 (years of education) and 0.0258 (college graduate); all are statistically significant.

The magnitudes of these coefficients are similar in size to the result observed for mental health outcomes. Interpreting these results in a quasi-instrumental variable context suggest impacts ranging from 2 to 3 standard deviations in AFQT scores and educational attainment for a 1 standard deviation change in birth county index among gay men. Probability of college graduation increases by 113 percent points. These exceedingly large magnitudes may be exaggerated by a conservative estimate of partnership rates for gay men between the ages

Table 2.9: AFQT Score and Educational Attainment

Variable	Standardized Score		
	AFQT Score	**Years of Schooling**	**Graduate College**
# Older Brothers × Male × Index	0.0479**	0.0651***	0.0258**
	(0.0240)	(0.0248)	(0.0110)
N	8,561	8,990	8,990

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

of 31 to 45 used in scaling the first stage. If I revise the first stage to scale by the 28.6 percent cohabitation rate reported in D. Black et al. (2000), the impacts would be scaled down to 1.48 to 2 standard deviation increases in AFQT and years of schooling and 80 percent point increased likelihood of graduating from college.

Table 2.10 shows the reduced form impact on employment, income and career advancement. Compared to the previous results on mental health and educational attainment, there are relatively few statistically significant impacts observed. There is a small negative coefficient on employment in the early age bin of 18 to 24, most likely related to pursuit of post-secondary education. Positive, significant impacts on log income and occupational status are observed between the ages of 38 to 45. In spite of the large effects on mental health and educational attainment, however, no significant effects are observed in earlier years. It is possible that the men who do not conceal face some degree of discrimination in the market (either directly or through sorting to non-discriminating, lower compensation industries) which depresses their wages in earlier years.

Table 2.10: Employment and Income

Variable	**Average Outcome in Age Range**			
	18 to 24	**25 to 30**	**31 to 37**	**38 to 45**
	Employed			
# Older Brothers × Male × Index	-0.0138*	-0.00596	-0.0118	-0.00297
	(0.00816)	(0.00798)	(0.00888)	(0.00931)
N	54,629	59,339	37,704	25,878
	Log Income			
# Older Brothers × Male × Index	-0.0113	0.0211	0.0173	0.0781***
	(0.0286)	(0.0236)	(0.0272)	(0.0294)
N	20,654	38,468	24,131	19,344
	Occupation = Managerial or Professional			
# Older Brothers × Male × Index	-0.00219	0.00412	0.0179	0.0411***
	(0.00645)	(0.00882)	(0.0111)	(0.0125)
N	30,703	50,070	32,629	27,208

Notes: Robust standard errors clustered at the county of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

2.6 Conclusion

This paper presents a framework to study discrimination in the context of concealable characteristics. I highlight two important implications of concealability: first, current estimates likely underestimate the true distribution of discrimination faced by the lesbian and gay community in the United States, and second, focusing purely on labor market outcomes can fail to capture the broader dynamics at work. To support these conclusions, I present evidence that same-sex cohabitation, mobility and identity investments respond to increasing discrimination risks. The concealment behavior is linked with negative impacts on the mental health of respondents and educational attainment. Income and career progression, however, show weaker correlations relationships suggesting that disclosure may come at a cost of labor market discrimination. Data limitations prevent analysis of the externalities of concealment on friends and family, but the high levels of observed negative mental health outcomes and substance abuse patterns raise the potential for substantial negative externalities.

The results show the first plausible evidence for men who experience same-sex attraction but never act on or self-identify these feelings. Because the natural rate of homosexuality is highly contested, it is hard to know the true reach of this phenomenon. But, given the large negative impacts associated with concealment, even a small prevalence rate could have significant implications especially if it is geographically concentrated.

This work suggests the importance of continuing to incorporate sexual orientation into public policy initiatives, particularly anti-discrimination legislation. Providing protections for the lesbian and gay community may help reduce the costs of discrimination and decrease rates of concealment. Given that many gay men and women may already be living under concealment with spouses and children, it will be important to anticipate the implications of changing attitudes towards gay men and women as these individuals potentially transition

out of concealment and disrupt the family unit.

Chapter 3

Same-Sex Partnership for What? Evidence from Swedish Register Data

Lina Aldén, Lena Edlund, Mats Hammarstedt and Michael Mueller-Smith

3.1 Introduction

In 1989, Denmark became the first country to legally recognize same-sex unions. Since then, some 30 countries have followed suit, France being the latest to strengthen their protections for same-sex couples in the form of same-sex marriage. In the US, more than half of the states allow same-sex marriage and what rights and protections to be afforded homosexuals is an ongoing debate. Advocates invoke equality, fairness, and human rights; opponents see a threat to family and society Chamie and Mirkin (2011).

Despite growing demand, relatively little is known about the function of legal same-sex unions. What is it that legal status confers that cannot be achieved through private contracts or actions such as cohabitation? Arguably, the same might be asked of opposite-sex marriage, an institution that has proven long lived.

But what holds for opposite-sex unions need not carry over to same-sex ones. For instance, the returns to marriage in the Beckerian framework rests on returns to specialization and same-sex couples appear to specialize less (L. K. Jepsen and C. A. Jepsen (2002)). Long-term commitment is another celebrated function of marriage that may or may not translate to same-sex couples (Andersson, Noack, Seierstad, and Weedon-Fekjr (2006)). A potentially more thorny issue, however, is the so-called paternity presumption: the husband is the presumed father of children borne by the wife (Appleton (2006)). Paternity presumption has until now been a universal feature of marriage and one that may even constitute its very core (Posner (1992)). In fact, most same-sex unions carve out paternity presumption, but even when included, its application is far from straightforward. This is so because of the strong rights accorded birth mothers. By default, the mother is the woman who gives birth. If a man in a same-sex partnership acknowledges paternity of a child born to an unmarried woman, will the child have three parents? And if parental rights are at the heart of legal

unions, then what is its relevance to all-male, and thus sterile, couples?

This paper seeks to shed some light on the practical implications for same-sex couples of greater access to legal rights formerly reserved for opposite-sex couples by studying an expansion of rights in Sweden. Starting in January 1995, same-sex couples could enter registered partnership, a contract that conferred almost the same rights and obligations as opposite-sex marriage. However, paternity presumption was carved out in an innocuous sounding exemption of rights extended to one sex only. It would be another eight years until same-sex partners gained the right to adopt jointly or as step-parents. The new adoption law was enacted in 2002 and took effect January 1, 2003.

In this paper, we analyze Swedish administrative data covering the period from 1994 to 2007. Derived from Swedish registers, these data are high quality, have universal coverage, and allow us to follow individuals. Using these administrative data, we identify and follow all individuals who entered into registered partnerships in 1995-2006 (to allow for a post and pre-union year). For comparison, we include all who entered opposite-sex marriage in the said period. The data contain detailed information on earnings and children living in the household, which enables us to shed new light on how entry into partnership/marriage affects labor market and parental outcomes. Our empirical strategy is to compare outcomes of earnings and presence of children before and after union entry controlling for individual fixed effects so that the person serves as his or her own control group.

For ease of exposition, "marriage" in the text will refer to opposite-sex marriage unless otherwise noted. Registered partnership (RP) will be referred to as such, or abbreviated to partnership, context allowing.[1]

By exploiting longitudinal data we can avoid the problem of selection into partnership

[1]In 2009, outside our sample period, registered partnership was replaced by same-sex marriage. However, other than the name, the principal change was to allow the ceremony to take place in the Swedish Church.

(or marriage) that arises in cross-sectional comparisons. However, the possibility that partnership/marriage entry is timed to coincide with other life changes remains. Milestones such as graduation or steady employment may both trigger marriage and presage earnings growth, resulting in an upward bias. On the other hand, a downward bias would result if partnership/marriage were timed to coincide with a downshift in labor market attachment (e.g., due to parenthood or retirement). Therefore, our estimates provide a description of labor market and parenting responses to partnership/marriage entry, but cannot isolate the causal effect of entry into partnership/marriage.

Our most noteworthy finding pertains to parenthood. Following the 2002-adoption law giving partners in a registered partnership the right to joint or step-parent adoption, we see both a noticeable increase in lesbian partnership and children living with lesbians in partnership. The net effect of union entry on presence of children, especially after the 2002 reform, reveals similar effects of entry into legal union status for lesbian and opposite sex couples – couples with at least one woman. These findings highlight the importance of a legal framework for parental rights; indeed it underscores the role of joint legal parenting for fertility decisions.

The found fertility effects, however, are limited to unions with women. The absence of positive fertility effects among gays could be due to a number of reasons including lower demand for children. It is also the case that the route to joint parenthood is more difficult for partnered men. For step-parent adoption, the child's mother's rights need to be severed, which for practical purposes means that the child is motherless or born by a gestational carrier. For regular adoption, the hurdle again is the supply of children. Many adoption agencies restrict adoptions to husband-wife couples or single persons.

Turning to earnings, we find a substantial decline in individual earnings for gay men (-12 percent) whereas for lesbian women the effect is small (-2 percent) and highly insignif-

icant. As for couple earnings, the pronounced decline seen for gays is absent, suggesting a high degree of income buffering (or negative sorting). By contrast, among lesbians, the income reduction seen at the individual level is amplified once viewed at the couple level, suggestive of within-couple positively correlated labor market responses to partnership entry. Within-couple earnings gaps change in a direction consistent with this interpretation. Among lesbians, there is a sizable (but statistically insignificant) reduction in the within-couple earnings gap, whereas among gays there is only a small and highly insignificant effect on the gap.

As a point of reference, we also look at heterosexual couples and we find effects of marriage largely in line with what has been documented in the literature: fertility increases, earnings of women decrease and there is an increase in the within-couple earnings gap. Men earn substantially more after marriage than before, but we find no evidence of a marriage premium employing our within-individual comparison. Instead, we find a strong ramp up of earnings in the years leading up to marriage. Given the negative marriage premium for women and the absence of a positive premium for men, our finding that the combined earnings for the couple decline on marriage is perhaps unsurprising.

Taken together, these findings paint a picture of same-sex registered partnership filling a different role for same sex couples than marriage does for opposite sex couples, and the roles are different for gays and lesbians. Generally speaking, as evidenced by the earnings gap, specialization on union entry is much more pronounced among heterosexual couples, and if anything, higher among gays than lesbians. This is particularly noteworthy given the close to zero fertility effect among gays and similar fertility effects for women, whether in a same- or opposite-sex union.

One possibility is that the specialization seen in opposite-sex couples stems from the fact that only one of the spouses, the woman, can bear children. If home production is defined

as the bearing of children, then the inability of men in that department gives them infinite comparative advantage in market work – a candidate reason why the sexual division of labor remains qualitatively unchanged despite significant gains women have made on men in the labor market in the last half century.

In separate work, one of us has argued that marriage, because of paternity presumption, is a contract in which men gain paternity and obtain parental rights (Edlund (2006, 2013), Edlund and Korn (2002)). The argument is based on the twin observations that: (i) the act of giving birth assigns motherhood, and an unmarried mother is by default the child's only known parent and its sole custodian; and (ii) a married mother shares custody with her husband and presumed father of the child. In other words, an unmarried man has no guaranteed rights to either legal fatherhood or custodial rights regardless of biological parentage, whereas these rights are guaranteed a married man (again, regardless of biology). This "transfer of children" to men in marriage could form the basis for a transfer in the other direction, that is unearned income to the woman (spousal earnings being a form of unearned income, e.g., Juhn and Murphy (1997)).

Marriage, viewed through this lens, does not translate 1:1 to the same sex context. Gay couples are sterile and lesbian couples lack the asymmetry of opposite sex couples: either party can bear a child and neither party needs a formal union for legal recognition of the parent-child relationship established at birth.

The remainder of the paper is organized as follows. Section 3.2 provides a literature review, a brief discussion of possible channels, and background on the institution. Section 3.3 describes our data. Section 3.4 considers individual and couple responses to union entry. Section 3.5 concludes.

3.2 Background

Our study is in the tradition of the literature on the so-called *marriage premium*, in the cross-section estimated to be in the 10-percentage range for men – a robust but intriguing association (Antonovics and Town (2004), Cornwell and Rupert (1997), Dougherty (2006), Ginther and Zavodny (2001), Korenman and Neumark (1991), Krashinsky (2004)). Our findings for heterosexual men are in line with Dougherty (2006) who, analyzing the 1979 National Longitudinal Survey of Youth, used a similar individual fixed-effects framework and found the marriage event to be largely indistinguishable from a smooth earnings profile. Zavodny (2008) studied the effect of cohabitation on earnings among US homosexual men in a cross-sectional comparison using the General Social Survey and the National Health and Social Life Survey and found no evidence of a "cohabitation premium."

Turning to earnings and sexual orientation, a number of studies have found gay men to earn less than heterosexual men while lesbians typically out-earn heterosexual women (for the US, see Allegretto and Arthur (2001), Badgett (1995, 2001), D. Black, Gates, Sanders, and Taylor (2008), C. Carpenter (2004), C. S. Carpenter (2005), Clain and Leppel (2001), Klawitter and Flatt (1998); for Australia, see C. Carpenter (2008); for Europe, see G. Arabsheibani, Marin, and Wadsworth (2004); for the UK, see G. R. Arabsheibani, Marin, and Wadsworth (2005); for the Netherlands, see Plug and Berkhout (2004); for Greece, see Drydakis (2011); for Sweden, see Ahmed and Hammarstedt (2010), Ahmed, Andersson, and Hammarstedt (2011, forthcoming)).

Homosexuality – particularly relations between men – has a long history of criminalization, e.g., Frank, Camp, and Boutcher (2010). While rarely stated in so many words, resistance to male homosexuality may lie in a suspicion that if allowed, men would willingly trade their role of breadwinning for a hedonistic existence and women and children would

suffer. And while one father is considered better than none, how does two stack up? Does societal acceptance of same-sex unions reduce the interest in traditional marriage? Questions like these may speak to the controversy surrounding recognition of same-sex unions. Legal recognition of same-sex couples provide a unique opportunity to shed further light on issues like these.

Our paper adds to a small but growing literature on marriage-like contracts for same-sex couples afforded by the recent expansion of such rights (Burn and Jackson (2014), C. Carpenter and Gates (2008), Dillender (2014, Forthcoming), Langbein and Yost (2009), Trandafir (2015)). Exploiting cross-sectional and time variation, a number of studies have found no evidence of legalization of same-sex unions eroding traditional values as measured by marriage, divorce, or abortion rates, for the United States see Langbein and Yost (2009) and Dillender (2014), for the OECD, see Trandafir (2015). In fact, for one outcome, syphilis, legalization appears to have reduced the incidence, possibly by encouraging fidelity among among same-sex partners although the data cannot speak to whether the disease reduction stemmed from more safe same-sex or opposite-sex sex (Dee (2008)).[2] While the Swedish context does not allow for geographic and time variation, it can be noted that the expansion of right to same-sex couples in Sweden coincided with an increase in both the propensity to enter, and stability of, opposite-sex marriages (Andersson and Kolk (2011)).

Burn and Jackson (2014) studied the marriage premium for gay men using a difference-in-difference-in-difference approach where the earnings growth of men in same-sex couples relative to married men over the 1990-2011 period was compared. They found that the earning growth had been substantially higher for men in same-sex couples relative to hetero-sexually married men in the six U.S. states that had legalized same-sex marriage compared

[2]Syphilis, a sexually transmitted disease "relatively common among men who have sex with men" Dee (2008, page 1056).

to such men living in states that had not legalized same-sex marriage.[3]

Consistent with children being important reasons for formal unions and the limited fertility of gay couples, C. Carpenter and Gates (2008) in their study of homosexual Californians found lesbian couples to be more prone to "legalize" their relationship, a finding echoed in Sweden after 2002.[4]

The paper perhaps closest to our study is Dillender (Forthcoming) who found that access to same-sex marriage reduced the labor market participation of lesbian couples, shifting these families from dual- to single-earner households, a change that he attributed to access to partner's health insurance benefits.

Lastly, and not strictly about same-sex marriage, Rosenfeld (2010) found same-sex couples to be equally effective in raising children, as measured by the children's educational attainment. However, the results by Rosenfeld have been called into question by Allen, Pakaluk, and Price (2013) and Allen (2013). Allen et al. (2013) re-evaluated Rosenfeld (2010) by using the same US data set but other comparison groups and other sample restrictions. They found that children raised by same-sex couples to be significantly less likely than children raised by married couples to make normal progress through school. Allen (2013) examined the association between household type and children's graduation rates from high school with the help of Canadian census data. The result showed that children living with gay and lesbian parents were less likely to graduate from high school than children living with opposite sex parents. Girls with same-sex parents did considerably worse than boys with same-sex parents. The graduation rates were especially low among daughters of gay

[3]The states are: Massachusetts (2004), Connecticut (2008), Vermont (2009), Iowa (2009), Washington, D.C. (2010), and New Hampshire (2010).

[4]Statistics Sweden. 2009. Fler kvinnor än män gifte sig med person av samma kön (Pressmeddelande) [More women than men married someone of the same sex (Press Release)] http://www.scb.se/sv_/Hitta-statistik/Statistik-efteramne/Befolkning/Befolkningsframskrivningar/Demografisk-analys/55349/55356/Behallare-for-Press/Infor-Stockholm-Pride/

parents.

3.2.1 Channels

Registered partnership may be an important institution for reasons similar to marriage. In this section we discuss some possible channels. We start by juxtaposing two different theories of marriage and their respective implications for labor market effects. We then turn to the specific institutional context in Sweden in which income and asset pooling is the default. This resource pooling can obviously impact observed labor market behavior and therefore merits our attention. However, it is a function that could be achieved by private contracting, and while it may be mandated by marriage or partnership, it does not define these institutions, and therefore we discussed it separately. Finally, we discuss the role of social recognition.

Theories of Marriage

In this section will discuss two theories of marriage – G. S. Becker (1973)'s canonical theory and a more obscure theory advanced by one of us in separate work (Edlund (2006, 2013), Edlund and Korn (2002)) – and their respective relevance and predictions for registered partnership. In brief, the gains from marriage in Becker's theory are realized through intra-household specialization. It is a fundamentally gender neutral theory and therefore could be eminently applicable to same-sex couples, assuming the presence of a "household commodity." Edlund's theory, by contrast, hinges on biological asymmetries in reproduction, and marriage is viewed as a contract that effectuates trade in children. Viewed from this vantage point, opposite-sex marriage serves as a poor template for same-sex unions. In fact, same-sex unions may help separate the two theories.

Household Commodity In his seminal "A Theory of Marriage," G. S. Becker (1973) advanced the notion of a household commodity produced using non-market time and market goods. Household commodities are "not marketable or transferable among households, although they may be transferable among members of the same household...[examples include] quality of meals, the quality and quantity of children, prestige, recreation, companionship, love and health status."

The theory is essentially gender symmetric and within-couple specialization arises from the need for *non-market* time – a need that could be circumvented if the good were transferable between households, but it is not. As above quote makes clear, the household commodity is *not marketable or transferable among households.* These are important distinctions for (at-least) two reasons. First, they separate the person providing the non-market time from hired help, the wife from the maid. Second, they drive the case for negative sorting on wages (one of the most criticized predictions of the theory). To see why, note that negative sorting requires not only the high-wage man marries the low-wage woman (which is known to have happened), but also the low-wage man marries the high-wage woman (less observed). This prediction is all the more jarring today when increasingly women are both well educated and attached to the labor force, resulting in a sizable group of women with high wages and whose family-formation mores can be observed.

While Becker did not limit household commodities to children, of the examples listed, children are of particular salience, both because of their importance and their lack of marketability.[5]

[5]Of the examples listed by Becker – quality of meals, the quality and quantity of children, prestige, recreation, companionship, love and health status – markets exist for quality of meals, prestige, recreation, health status. Companionship and love may not have markets, but it is also hard to see how they relate to marriage or could be produced through specialization or be transferable between spouses.

Paternity presumption Children are at the heart of marriage in Edlund (2013) and unlike Becker's theory, Edlund emphasized formality.[6] Her view of marriage focuses on formal marriage as a contract on children, where women sell and men buy. The rational for this characterization lies in the asymmetries of reproduction. While everybody has exactly one mother and one father, the woman's contribution is more critical. Women are not only bottlenecks in reproduction, they are also the only readily identifiable parent.

It is perhaps then not surprising that in the vast majority of jurisdictions the woman who give birth is also the default mother – if unmarried, she is also the child's sole custodian and the father is unknown. It is very difficult for a man to claim paternity against the will of the mother, unless the mother is also his wife, which brings us to paternity presumption.

If the mother is married, the husband is the presumed father and his guaranteed paternal rights exceeds those of an unmarried father. This is known as paternity presumption is a universal feature of marriage. It is also a unique feature of marriage. Formality is needed because a private contract would amount to trade in children and lack legal standing in jurisdictions that do not allow contracts on rights in people (the vast majority, that is).

Marriage thus conceptualized amounts to a contract whereby a husband hires a wife to produce children. While the wife needs to be a woman, the husband can be of either sex. Many traditional African societies allowed barren women to take wives, an early form of same-sex marriage (Evans-Pritchard (1951)). In fact, Appleton (2006) has argued that same-sex marriage, including paternity presumption, should be reserved for women based on the complication mentioned in the introduction: male same-sex marriage could easily result in three legal parents, a concept that for now at least is alien to Western society. (However, as cross-racial adoptions illustrate, biological plausibility is not a *sine qua non* for

[6]G. S. Becker (1973, pages 815-816) abstracted from formal marriage "...two persons, M and F, who must decide whether to marry each other or remain single. For the present, 'marriage' simply means that they share the same household."

legal parent-child relations.)

Marriage is commonly conceptualized as a transfer of resources from the man to the woman, e.g., Akerlof, Yellen, and Katz (1996). What is transferred the other way is, however, often not articulated or justified as stemming from an inability of women to support themselves. However, if marriage is the transfer of parental rights from the wife to the husband; material transfers in the opposite direction may be endogenous to the transfer in parental rights. The sexual division of labor commonly observed could arise from comparative advantage but could equally be the result of women's unearned income. Furthermore, negative sorting does not arise as readily as in the Beckerian theory, since negative sorting would require the pairing of a high-wage seller and a low-wage buyer. Thus, paternity presumption offers an explanation (other than gender roles) for why high-wage women would rather remain single than marry low-wage men.

Predictions, general Turning to the predictions of the two theories for same-sex registered partnership, it may be useful to distinguish between children and other household commodities. Excluding children, the Beckerian theory predicts negative sorting and specialization. By contrast, a theory of marriage based on paternity presumption does not apply in a world without children.

If the household commodity is indeed children (which seems reasonable given Becker's definition, his other examples notwithstanding), the two theories have observationally different predictions for same-sex and opposite-sex couples. Again, Becker's theory predicts specialization, one person specializing in market work and the other person providing nonmarket time for the household commodity, regardless of whether same-sex or opposite sex. By contrast, Edlund's theory predicts less specialization for same-sex than for opposite sex couples. The reason is that marriage amounts to trade in children, from the woman to

the man. The resulting compensation from the man to the woman allows women to enjoy more leisure, observationally equivalent to reduced labor market attachment. However, in the context of same-sex couples, the basis for this payment is moot. Either or none of the partners can bear children.

Registered partnership in Sweden carved out paternity presumption (a carve out that remains for same-sex marriage, registered partnership's 2009 incarnation). Thus, to the extent that the effects of marriage are tied to the transfer of parental rights they may not carry over to partnership. This carve-out is common to same-sex legal unions but not universal, e.g., Anderson (2006). The legal landscape is changing rapidly however. Whereas paternity presumption tends to remain carved out, legal unions are recognized and granted by an increasing number of jurisdictions, and may pave the way for greater parental rights. For instance, in France, the discussion of same-sex marriage has precipitated a discussion of same-sex adoption of children.

In Sweden, lesbians entering registered partnership do not automatically share parental rights to children borne by the partner. That is, if one of the women becomes a mother, her partner does not automatically become a mother and custody is not joint. Since the 2002-adoption law, however, those in registered partnership have the right to adopt jointly or as a step-parent. Furthermore, in 2005, lesbian women gained the right to artificial insemination under the auspices of the national health care system (its significance for fertility can be questioned on *a priori* grounds however).

Assuming that children are the main household commodity, both theories of marriage predicts a rise in lesbian partnership entry, as well as higher fertility, following the 2002-adoption law. (The effects of the 2005-law cannot be examined with our data since our last year is 2007, allowing for at most one "treated" year.)

While the situation for gays is legally the same as for lesbians, the 2002-adoption law

has little practical significance. For the law to be applicable, a child is needed and a man not married to a woman lacks default parental rights. Absent that, gay couples' options are limited to adoption or surrogacy. The supply of children for adoption is limited and many countries do not allow same-sex couples to adopt. As for surrogacy, although not illegal, surrogacy contracts are typically not enforced. This legal gray zone makes surrogacy emotionally and financially taxing and unpractical for the majority of couples.[7] Thus, the gay couple may be for all practical purposes sterile, removing an important reason for household specialization.

Given the difficulty gay men face in obtaining children, lesbian couples emerge as a testing ground of particular interest. Does the possibility of joint-legal parenting boost partnership entry. Is partnership entry associated with more children for lesbians? If so, do lesbian couples specialize or not?

Financial Motives/Income Pooling

A number of financial incentives and programs are organized around the institution of marriage. However, there are few financial benefits tied to marriage in Sweden today. For instance, tax filing status is strictly individual and all residents are covered by national health insurance. Additionally, the public retirement pension is not inherited by the surviving spouse and there is no gift or inheritance tax.

The main financial consequences of registered partnership (and marriage) are: (i) all assets are treated as marital property (individual ownership but restrictions on disposal), unless otherwise specified in a prenuptial agreement or given as a gift expressly designated to be individual property; (ii) all assets accumulated during the partnership (or marriage) are

[7]The red-tape, uncertainty and high cost surrounding adoption and surrogacy can be traced to both being conceptually close to contracts on children.

community property; (iii) partners (spouses) have the right and obligation of mutual support and specifically have the right to the same standard of living; and (iv) default inheritance rights of the surviving partner (spouse).

Thus partnership (and marriage) entails a resource transfer to the financially weaker partner. This could dull work incentives for both the higher and the lower earner in the couple. For the higher earner, pooling amounts to a tax. For the lower earner, pooling amounts to unearned income. Furthermore, income pooling offers insurance and therefore reduced incentives to earn enough to maintain a buffer (potentially reducing the fiscal benefits of same-sex partnership recognition, e.g., A. Stevenson (2012)).

For these reasons, we expect partnership (or marriage) to reduce earnings, *ceteris paribus*. While reduced earnings has been widely documented for women on marriage entry, the same cannot be said for men, suggesting that other factors are at play. As discussed in the previous section, children are candidate explanations. Both theories of marriage predict marriage to boost earnings of one party (the person specializing in market work in Becker's theory; the person who acquires parental right because of marriage in Edlund's theory).

Whether work disincentives or work incentives dominate is an empirical question, but one simple prediction presents itself: for childless partnerships, lowered incentives to earn dominate (since the sole mechanism). For the reasons discussed above, gay couples are more likely to be childless, and if so we may expect partnership to result in lower earnings for gays.

Recognition and Social Acceptance

Since the 1970s, Western societies have seen the improved ability of unmarried fathers to establish paternity and obtain parental (and other, see e.g., Perelli-Harris and Sanchez Gassen (2012), Waaldijk (2005)) rights formerly reserved for marriage, as well as increasing accep-

tance and incidence of non-marital cohabitation and fertility. As a result, the practical and social significance of marriage has been reduced. Increasingly, marriage is viewed as a choice rather than a necessity and has emerged as a marker of prestige (Cherlin (2004), Holland (2013)).

Social acceptance and prestige may be one reason same-sex marriage is demanded. Legal acknowledgment of ongoing commitment may translate into broad social acceptance of homosexual unions among friends, family and coworkers, and may thus bestow non-pecuniary benefits. Advocates of this idea focus on the importance of common institutions (i.e., marriage rather than registered partnership) to promote the idea that homosexual relationships are no different from heterosexual relationships.

Thus, partnership entry may boost mental and physical health. In the preliminary analysis we looked into the uptake of health related benefits in our administrative data. However, our data did not reveal a detectable effect of partnership entry (not reported).

3.2.2 Institutional Background

We analyze Swedish administrative data spanning 1994-2007, a period during which several rights were extended to homosexuals. The date in boldface indicates when the legal change takes effect.

1995 The Partnership Act of 1994 took effect January 1, 1995. It grants all rights provided to married couples, with an important exception for paternity presumption. Savolainen (2003, page 28): "...the presumption of paternity does not apply where a female partner gives birth to a child. The other partner does not become the legal parent of the child or acquire any parental rights or duties at the birth of the child by operation of law as is the case in respect of a child born in wedlock. These [Finnish and Swedish Part-

nership] Acts do not know any special procedure, agreement, consent or 'recognition of parenthood' whereby a partner could become a legal parent of a child produced by the other partner." Savolainen (2003) noted that this arguably important carve-out is buried in an exception for rights conferred by marriage to one sex but not the other, Swedish Partnership Act, Chapter 3, section 3.

Registered partners could neither jointly adopt a child adopt as step-parents, these forms being only open to married couples (Savolainen (2003, page 36)).[8]

The Partnership Act did however expand parenting ability by allowing for parental leave accorded one partner to be shared between partners.

1999 Banning of workplace discrimination based on sexual orientation. An Ombudsman office is introduced. This law strengthened the 1987 law banning discrimination based on sexual orientation.

2003 The 2002-adoption law gave registered partners the right to adopt jointly or as step-parents.[9]

In Sweden, married couples can only adopt jointly, and for a man and a woman to adopt as a couple, they have to be married. Likewise, following the 2002-adoption law, same-sex couples in a partnership can only adopt jointly. Since some countries do not allow adoption by same-sex couples, the 2002-adoption law may be an impediment to partnership entry. Children available for adoption are limited. Therefore, the right to adopt as a step-parent may be the empirically more relevant right. Moreover, this right is more likely to be of use to lesbian than gay couples.

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[8]http://www.notisum.se/rnp/sls/lag/19941117.HTM, http://www.regeringen.se/sb/d/1522/a/17834

[9]http://www.adoptionpolicy.org/pdf/eu-sweden.pdf

Consider a lesbian couple where one of the women is pregnant. The other woman could adopt her step child. Granted, the father of the child would need to relinquish his parental rights but that could be sidestepped if the mother declared the father unknown. Interestingly, the possibility of one woman bearing a child by an unknown father and raising it jointly with her partner precedes the ability to do so as joint legal parents. Thus, any effects of partnership combined with this legal right on fertility would be testimony to the importance of the designation of parental rights.

For men, these rights are likely less consequential. If they had a child (say from a previous marriage), the mother would need to surrender her parental rights for a step-parent adoption to take place. Note that paternity presumption makes the spouse of the wife a parent, not the spouse of a husband. In other words, a married man who acknowledges paternity of a child born to a woman that is not his wife does not make the wife a mother.

These adoption rights allow partnership to be potentially at par with marriage. However, unlike marriage, it is an add-on requiring both partners' consent. (If same-sex partners are both legal parents, they have joint custody during partnership, and this is also the default custody arrangement on dissolution of the partnership.)

2003 The cohabitation law (sambo-lag) makes the joint residence communal property and in 2003 it was extended to same-sex couples. However, since there is no court-verifiable action that designates a couple as co-habitants, the protection offered by this law is weak. For opposite-sex couples, the focal event is the birth of a child where both partners are listed on the birth certificate and under the same address. For same-sex couples, there is no similar event since joint parenthood is predicated on partnership.

When unmarried parents separate, the default custody arrangement is for the mother

to retain sole custody.

2005, July 1 Women in a partnership gain the right to artificial insemination or IVF treatment through the national health care system, a right previously reserved to married or cohabiting women (single women are still denied).

2009, November 1 Although outside our sample period, in 2009 same-sex marriage replaced same-sex partnership. Couples in same-sex partnership can convert their partnership into same-sex marriage (or remain in the partnership). The change from partnership to marriage was mainly cosmetic as the chief additional right was the right to marry in the "Svenska Kyrkan" [http://www.rfsl.se/?p=420]. The Swedish Church used to be the State Church of Sweden, and remains the dominant religious institution. Thereby, the blessings, tradition, liturgy, and venues administered by the Swedish Church were made available to same-sex couples. Paternity presumption remains excluded from same-sex marriage.

3.3 Data and Descriptive Statistics

We use data from LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), a register-based longitudinal database developed by Statistics Sweden. Coverage is universal and includes demographic characteristics, labor market characteristics, and use of social benefits. Our analysis data set covers the period 1994 to 2007. In order to compare labor market outcomes before and after entry into partnership or marriage, we restrict the sample to individuals who entered partnership or marriage in the period 1995-2006.

All individuals who have entered a registered partnership are defined as homosexual and

all opposite-sex couples who have entered marriage are defined as heterosexuals, following Ahmed and Hammarstedt (2010), Ahmed et al. (2011, forthcoming).

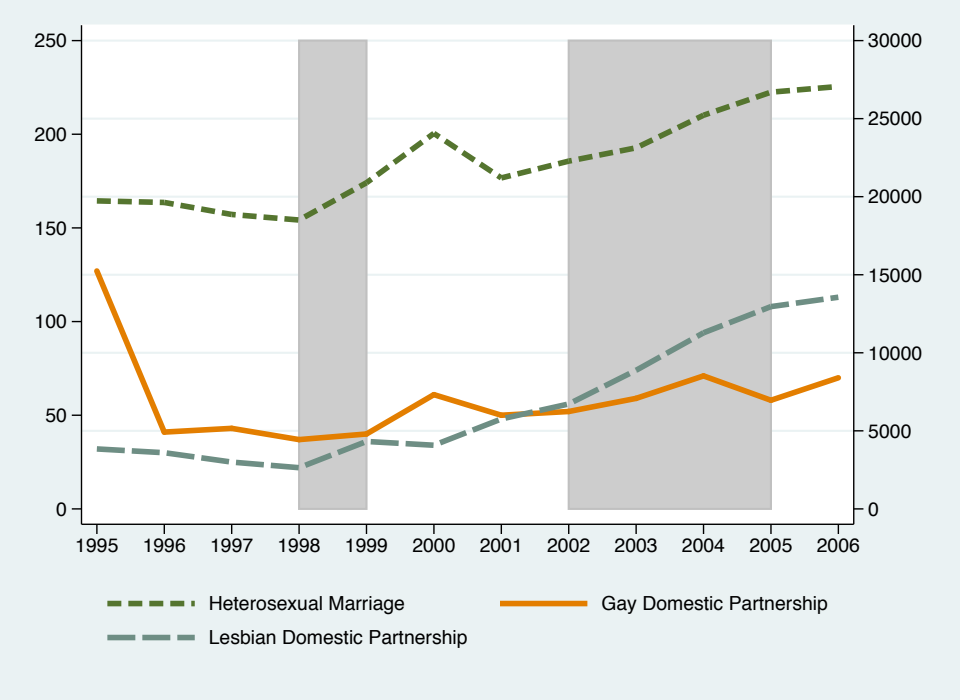
We are interested in the effect of partnership entry and arguably entry into first marriage corresponds most closely to partnership entry. For greater homogeneity, we also restrict attention to couples for which it is the first union for both. Furthermore, we restrict the sample to couples where both partners were between the ages 20 and 64 at the time of union entry.[10] After these restrictions, our sample consists of 672 female and 709 male homosexual couples, and 267,264 heterosexual couples. The panel is not completely balanced but the vast majority of couples were observed for all years (1994 to 2007).

We focus on the following labor market outcomes: individual and couple annual labor earnings, within-couple earnings differential, and the number of coresiding children. Annual labor earnings comprise earnings from wage employment and self-employment as well as other work-related benefits.

Figure 3.1 shows the number of heterosexual and homosexual marriages by year of union entry for our sample. The number of gay partnerships averages between 50-75 per year, except for the first year (1995) in which 127 gay couples entered partnership. Lesbian partnership, on the other hand, did not spike in the first year. Instead it is flat at around 40 per year until 2000, after which there is a steady increase. In the last year for partnership entry for our sample, 2006, about 120 lesbian couples entered partnership. The difference in pent-up demand for legal union status between gays and lesbians is intriguing. Can it be that men marry for retrospective reasons more than women, and if so, why? One possibility is that gays enter partnership for income pooling and estate planning, whereas lesbians are drawn to registered partnership for the joint parenting possibility, a motive that, at least

[10]Retirement is mandatory at age 65. Employment beyond that is at the employers discretion, extensions are easy for the first two years. The self-employed are exempt.

Figure 3.1: Union Entry, by Year



Notes: In-sample year of union entry. These numbers differ from official statistics because of the sample restrictions we have imposed.

viewed from the perspective of the daily juggle, loses its relevance once children are grown.

The shaded areas show the years of parliamentary legislation against workplace discrimination based on sexual orientation (enacted in 1998) and the right to adopt jointly or as a step-parent (enacted in 2002).[11] Partnership entry is a public act that reveals sexual orientation and in principle the 1998 law offering greater workplace protection could have encouraged partnership entry. However, no such response is evident in Figure 3.1. It is possible that the law was toothless. Alternatively, work place discrimination may have been negligible or irrelevant for the partnership decision.

Whether the 2002-adoption law enabling joint- or step-adoption boosted partnership entry by lesbians can be debated, but we see in that year that the number of lesbian partnerships

[11] Generally, laws take force January 1 the year following enactment.

overtakes the number of gay partnerships and the gap widens every year thenceforth. We also present the number of heterosexual marriages (right scale) for reference, and the most noteworthy feature is a spike in 2000. We are not aware of any particular event directly linked to family formation that can explain this increase in marriages. The spike may simply be related to salience attached to the number "2000" (e.g., see Ohlsson-Wijk (2014)).

3.3.1 Descriptive Statistics

Tables 3.1 and 3.2 present descriptive statistics for our samples. Homosexuals in our sample are older (due to sample construction and older age at union entry) by three years for women and nine years for men, with an average age for lesbians of 34 years and 42 years for gays. Homosexuals earn more than the heterosexuals in our sample, not surprising given the age and education differences. Whereas 42 percent of heterosexual women have a college degree, this is true of 52 percent of lesbian women. The numbers for men are 37 and 47 percent respectively. These findings remind us that by conditioning on partnership/marriage entry in a country where a high proportion of couples chose informal cohabitation, we are dealing with a (positively) selected sample. The extent to which this is truer of the homosexual sample is hard to ascertain since the underlying population is unknown, but is a conjecture that would be consistent with the literature (Badgett, Gates, and Maisel (2008)).

As for children, homosexuals have very few children living with them before partnership entry, perhaps unsurprising given that we exclude the previously married. By contrast, heterosexuals have on average "half" a child living with them before marriage. (The average is for all years before marriage, so for instance, if we observed a person for four years before marriage, and a child appears in year three, that would show up as 0.5 children.) The number of children after union entry stays at close to zero for gay men, but increases among the other groups, with the greatest increase among heterosexual couples.

Table 3.1: Characteristics of Individuals Entering Marriage or Partnership in Sweden 1994-2007

	Female		Male	
	Homosexual	Heterosexual	Homosexual	Heterosexual
Labor earnings[a]				
before union[b]	148.99	127.48	226.45	199.02
after union	228.15	193.61	267.18	320.69
Labor earnings>0, %				
before union	89	90	92	93
after union	90	92	86	96
Parental leave uptake, %				
before union	4	26	0	20
after union	20	67	1	56
Age	33.61	30.52	41.74	32.61
Metropolitan	60	41	74	41
Primary school	12	10	13	11
Secondary school	36	48	39	52
University degree	52	42	47	37
Unknown	0	0	1	
Years of schooling				
before union	12.71	12.39	12.59	12.31
after union	13.38	13.11	13.10	12.73
N individuals	1,418	267,264	1,344	267,264

[a] – Annual 2007 Swedish Krona (SEK) '000.
[b] – Registered partnership of marriage.
The variables are averaged across all years 1994-2007.

Table 3.2: Couple Characteristics of Individuals Entering Marriage or Partnership in Sweden 1994-2007

	Homosexuals		Heterosexuals
	Females	Males	
Couple earnings[a]			
before union[b]	298	452.89	326.5
after union	456.3	534.37	514.3
Couple earnings gap			
before union	98	144.2	113.15
after union	125.7	174.34	169.72
Dual earner, %			
before union	81	86	85
after union	84	77	89
Couple schooling gap (years)			
before union	1.48	1.97	1.29
after union	1.36	1.85	1.31
Couples with children			
before union	0.12	0.01	0.48
after union	0.43	0.02	0.91
Number of children at union entry[c]	1.16	1.25	1.69
N couples	672	709	267,264

[a] – Annual 2007 Swedish Krona (SEK) '000.
[b] – Registered partnership of marriage. [c] conditional on having children.

We are also interested in couple-level outcomes. We treat the persons who enter a union in our sample as a couple throughout the period we observe them, although strictly speaking they may not be a couple for the entirety of the period. Table 3.2 shows couple level characteristics. Joint earnings are highest for gay couples, closely followed by heterosexual couples (after marriage). Lesbian couples have the lowest joint earnings, perhaps unsurprisingly.

The pronounced earnings advantage of homosexual gay couples before union entry (453' SEK vs 326' SEK for heterosexual couples) is attenuated after union entry. There is also a noticeable fall in employment among homosexual couples. Whereas some 86 percent of gay couples were dual earners before union entry, this number falls to 77 percent after union entry. By contrast, the percent dual earners increases among both heterosexuals and lesbians, from 85 to 89 percent among heterosexuals and from 81 to 84 percent among lesbians.

The couple-earnings gap increases on union entry for all types, but is more muted among homosexuals.

Turning to educational sorting, homosexual couples are less assortatively matched, the gap being the greatest for gays with on average almost two years of schooling separating partners compared to 1.3 years among heterosexuals. Union entry does not appear to change that much, which is perhaps unsurprising given that our sample catches people in their 30s and 40s.

While a higher share of married and lesbian couples are dual earners after union entry, there is a pronounced drop among gay households (from 86 to 77 percent).

To control for the effect of time-varying characteristics we now turn to regression analysis to parse the role of union entry. Motivated by the findings of Andersson, Noack, et al. (2006) who showed substantial differences on observables by the sex composition of the couples (as well as union stability), we choose to estimate our models on gay, lesbian and heterosexual couples respectively rather than pooling our samples.

3.4 Econometric Analysis

Exploiting panel data for the years 1994-2007 we estimate the within-individual effect of partnership using a model of the form:

$$y_{it} = \beta UNION_{it} + X_{it} + \phi_i + \phi_t + \epsilon_{it} \tag{3.1}$$

where y_{it} is the outcome variable of interest: individual or couple earnings (logged); within-couple earnings gap (logged); and the number of co-residing children. Thus the unit of observation is either the individual or the couple. We will refer to the pair formed by the

two individuals who enter registered partnership or marriage during our study period as a couple, even if they are not a couple for the entire period (before union entry or after union entry because of divorce, the term used by Statistics Sweden for same and opposite sex couples equally). $UNION_{it}$ is a dummy variable that is 1 from the year of union entry and onwards. That is, if t^* denotes the year of union entry, then

$$UNION = \begin{cases} 1 & \text{if } t \geq t^*, \\ 0 & \text{if } t < t^*. \end{cases}$$

The parameter β can be interpreted as the effect of partnership/marriage on the outcome variable. In Sweden, the vast majority of marriages are preceded by cohabitation and therefore marriage or partnership effects likely isolate effects of change in legal status.

X_{it} is a vector of time varying individual or couple characteristics and includes dummy variables for age (average age in the case of a couple), year, county, dummy variables indicating divorce, receipt of disability pension (self, one partner in the couple, both partners in couple), and age>65 (self, one partner in the couple, both partners in couple). The reason for including divorce is that the effects of union entry may conceivably extend beyond divorce and therefore we keep couples that divorce in our analysis sample, but at the same time there are fewer reasons to expect specialization following divorce. A reason for including disability pension is that it clearly affects earnings, and the same can be said for reaching the retirement age of 65 (retirement in Sweden is mandatory).

In our preliminary analysis, we also included education as a control variable (despite it being potentially endogenous) but we present results without controlling for education because changes were small and its inclusion had minimal impact on results.

The presence of children, on the other hand, changed significantly on union entry and therefore we present results with and without controlling for children (despite this variable

being even more endogenous to union entry then education). The purpose of presenting results controlling for children is give the reader a sense of the extent to which earnings effects are mediated by the presence of children. Heterogeneity across individuals (couples) is captured by individual (couple) fixed effects, ϕ_i. Year-specific effects, ϕ_t, capture the earnings growth common to all individuals (households).

To allow for within-individual (couple) correlation, we cluster the error term ϵ_{it} at the individual (couple) level.

Union entry is, at least in the case of marriage, a decision that is often many years in the making, preceded not only by an engagement but in many cases cohabitation and to a lesser extent joint children. To drill down on the question of dynamics surrounding union entry, for earnings we estimate a version of Equation 3.1 that allows for both lead and lag effects of union entry:

$$y_{it} = \sum_{k=-3}^{3+} \beta_k UNIONk_{it} + X_{it} + \phi_i + \phi_t + \epsilon_{it} \tag{3.2}$$

where

$$UNIONk = \begin{cases} 1 & \text{if } t = t^* + k, \\ 0 & \text{if otherwise.} \end{cases}$$

The reference period is four years or more before union entry. Years three and higher are treated as one group (3+).

For fertility outcomes we are also interested in the possibility of the 2002-adoption law affecting fertility for same-sex couples, lesbians in particular. To that end, we include an interaction term allowing for a differential effect after the 2002-adoption law, but drop lead effects to keep the specification tractable. That is, we estimate a regression model of the form:

$$y_{it} = \sum_{k=0}^{3+} (\beta_k + \gamma_k \mathbf{1}(t^* > 2002)) UNIONk_{it} + X_{it} + \phi_i + \phi_t + \epsilon_{it} \tag{3.3}$$

where y_{it} is the number of children living with couple i, year t, and $\mathbf{1}(t^* > 2002) = 1$ if $t^* > 2002$ and 0 otherwise. The reference period is the year before union entry; years three and higher are treated as one group (3+).

3.4.1 Earnings

Table 3.3 shows the results from estimating Equations 3.1 and 3.2 for (log) individual earnings. Unlike the raw before and after difference, we see that union entry does not have a positive effect and for heterosexual women and gays, the negative effect is statistically as well as economically significant at 16 and 12 percent earnings reduction (panel A). Our main specification does not control for children, but we now turn to how including the number of children affects results. Inclusion of children attenuates the effect for heterosexual women, consistent with women reducing earnings in response to children. However, the 12 percent negative effect for homosexual men remains, perhaps unsurprising given the low presence of children among this group (panel B).

Panel C shows the results from estimating lag and lead effects per Equation 3.2. The reference period is 4 years or more before union entry, and we exclude controls for children because of their endogenous nature. The results for homosexuals remain largely unchanged, although the negative effect for gays loses statistical significance in this specification. As for heterosexuals, we see a ramp up of male earnings in the years leading into marriage, which may account for the lack of positive marriage premium usually found in the literature. One possibility is that the ramp-up itself can be attributed to anticipated marriage, in which case, we underestimate the marriage premium. Alternatively, the ramp-up may be a response to

Table 3.3: Individual Earnings Effects of Partnership or Marriage Entry

	Female		Male	
	Homosexual	Heterosexual	Homosexual	Heterosexual
	Panel A.	Not controlling for number of children		
Union[a]	-0.0246	-0.1577***	-0.1161**	-0.0024
	(0.0625)	(0.0042)	(0.0525)	(0.0033)
Adj-R^2	0.201	0.167	0.205	0.220
	Panel B.	Controlling for number of children		
Union	-0.0026	-0.0806***	-0.1167**	0.0004
	(0.0634)	(0.0041)	(0.0524)	(0.0033)
Adj-R^2	0.201	0.183	0.205	0.220
	Panel C.	Leads and Lags, reference period: 4+ years before union (No child controls)		
Union, years since				
-3	0.0437	0.0226***	0.0757	0.0496***
	(0.0635)	(0.0046)	(0.0563)	(0.0039)
-2	0.0686	0.0242***	0.0779	0.0658***
	(0.0790)	(0.0055)	(0.0710)	(0.0046)
-1	0.1339	-0.0019	0.0091	0.0840***
	(0.0888)	(0.0062)	(0.0795)	(0.0052)
0	0.1143	-0.0517***	-0.0399	0.0831***
	(0.1034)	(0.0070)	(0.0902)	(0.0057)
1	0.0679	-0.1559***	-0.0914	0.0613***
	(0.1157)	(0.0077)	(0.0995)	(0.0063)
2	-0.0264	-0.2967***	-0.1045	0.0378***
	(0.1353)	(0.0088)	(0.1126)	(0.0071)
3+	-0.0529	-0.3346***	-0.1964	-0.0038
	(0.1526)	(0.0101)	(0.1304)	(0.0085)
Adj-R^2	0.202	0.169	0.205	0.220
Observations	17860	3609338	18498	3609338

[a] – Marriage or partnership.

Standard errors in parentheses. All regressions include individual fixed effects, dummy variables for age, year, county, and dummy variables indicating legally separated, receipt of disability pension, and age>65. Standard errors are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01

greater familial responsibilities (children, cohabitation), with the formalization of the union being of little additional significance.

The findings for heterosexual women are qualitatively in line with what has been found in the literature: earnings dip markedly with marriage entry, a reduction that is attenuated once the number of children is controlled for (not reported).

In Table 3.4 we turn our attention to joint earnings and earnings gap. The unit of observation is the couple and we see that the estimated effect of union entry is negative for all groups, but only statistically significant for heterosexual couples. The latter is perhaps unsurprising given the absence of a positive marriage premium for men and the substantial marriage penalty for women (c.f. Table 3.3).

For gays, the pronounced decline found for individual earnings is absent once earnings are measured at the couple level, suggesting a high degree of income buffering (or negative sorting). By contrast, among lesbians, the income reduction seen at the individual level is amplified once viewed at the couple level, suggestive of within-couple positively correlated labor market responses to partnership entry. Within-couple earnings gaps change in a direction consistent with this interpretation. Among lesbians, there is a sizable (but statistically insignificant) reduction in the within-couple earnings gap, whereas among gays there is only a small and highly insignificant effect.

The lack of evidence of specialization among lesbians could be the result of greater similarity pre-partnership. But as seen in the descriptives, that is not the case. Lesbians are if anything less positively matched on education than the men and women in our heterosexual sample. The absence of specialization among lesbians is also noteworthy in view of partnership entry's effect on fertility, the topic we now turn to.

Table 3.4: Couple Earnings Effects of Partnership or Marriage

	Homosexuals		Heterosexuals
	Females	Males	
	Joint Earnings		
	w/o controls for number of children		
Union[a]	-0.0703	-0.0075	-0.0365***
	(0.0575)	(0.0377)	(0.0021)
Adj-R^2	0.275	0.295	0.333
	w/ controls for number of children		
Union	-0.0623	-0.0075	-0.0160***
	(0.0573)	(0.0377)	(0.0021)
Adj-R^2	0.275	0.295	0.336
	Earnings Gap		
	w/o controls for number of children		
Union	-0.0742	0.0132	0.0318***
	(0.0662)	(0.0540)	(0.0027)
Adj-R^2	0.067	0.119	0.104
	w/ controls for number of children		
Union	-0.1019	0.0138	-0.0004
	(0.0656)	(0.0541)	(0.0027)
Adj-R^2	0.068	0.120	0.109
Observations	8,930	9,249	3,609,338

[a] – Marriage or partnership.

Standard errors in parentheses All regressions include couple fixed effects, couple average age, year, county, and dummy variables indicating divorce, receipt of disability pension (one partner, both), and age>65 (one partner, both). Standard errors are clustered at the couple level.

* p<0.10, ** p<0.05, *** p<0.01.

3.4.2 Children

We now turn to parenthood. As discussed above, the exclusion of paternity presumption from the Partnership Act of 1994 means that the birth of a child to one partner does not make the other partner a parent and consequently cannot confer any parental rights to that partner. The 2002-adoption law, however, allowed registered partners joint or step-child adoption. This right may have been of little practical importance for gay couples since a child is still required. While a man may father a child and be the legal father, the child would in the vast majority of cases have a legal mother who would have to surrender her parental rights in favor of the father's partner in order for an adoption to take place. The child of an unmarried woman, however, is by default fatherless and Swedish praxis is to not pursue positive paternity claims (cases pressed by men). Thus, an unmarried woman who declared the father unknown would be the sole legal parent and custodian. With the possibility of partners to jointly adopt, she also has the capacity to bestow parenthood on her partner (in a registered partnership).

Thus, one reason for analyzing the fertility response of partnership, especially after the 2002-adoption law, is that it may help unpack the demand for registered partnership. Fertility response to partnership may also help clarify the channels through which partnership impacted earnings for men and women in partnerships.

We now turn to children raised by the couple. Unfortunately, we do not have natality data. Instead, we use information on the number of co-residing children younger than 18 (a number that can change in either direction, aging and moving out being the most important drivers of reductions). For brevity, we will refer to this measure as fertility, although strictly speaking it is not. We are particularly interested in investigating any fertility effects of the 2002 law that allowed for joint or step-parent adoption by individuals in registered partnership.

The results are presented in Table 3.5. Column 1 presents results from estimating a version of Equation 3.3 where we focus on children that follow union entry (that is, we ignore lead effects and the reference period is the time before union entry). We see a clear fertility effect of partnership on lesbians (panel A) but none among gays (panel B).

We next turn attention to the importance of the 2002-adoption law allowing joint- or step-adoption by same-sex couples. Column 2 presents results allowing for a trend-break reflecting this law.

For women (panel A, Column 2) there is a strong and positive fertility effect after 2002.

Columns 3 and 4 present the analogous results for heterosexual couples and we see that fertility actually decreased after 2002. The pronounced effect of the 2002-adoption law among lesbian women is notable and points to the role of joint legal parenting, as opposed to joint *de facto* parenting (registered partners have had the right to share parental leave since 1995). While lesbians also gained the right to artificial insemination, women's free access to sperm arguably precedes its coverage by national health insurance. Incidentally, 2003 marks the year partnership entry by lesbians overtook that of gays (Figure 3.1).

For gays, Columns 1 and 2, partnership is if anything associated with a (small) reduction in the number of children, possibly reflecting gay men being almost 10 years older than heterosexual men and consistent with registered partnership being entered into for reasons other than joint parenting.

In sum, we see strong fertility effects of partnership entry among lesbians – stronger than for heterosexual women once same-sex joint adoption is allowed. The stronger effect is consistent with registered partnership being a legally more enabling contract with respect to parental rights for same-sex couples than marriage is for opposite sex couples. A man and a woman seeking joint parental rights can achieve that without marriage (through paternity acknowledgment and custody assignment).

Table 3.5: Fertility Effects

	(1)	(2)	(3)	(4)
	Women			
	Homosexual		Heterosexual	
Years since Union[a]				
0	0.1522***	0.1385***	0.0497***	0.0707***
	(0.0136)	(0.0196)	(0.0012)	(0.0016)
1	0.2114***	0.1757***	0.2066***	0.2372***
	(0.0160)	(0.0211)	(0.0016)	(0.0020)
2	0.2722***	0.2338***	0.3112***	0.3431***
	(0.0203)	(0.0265)	(0.0020)	(0.0023)
3+	0.3597***	0.3242***	0.4996***	0.5222***
	(0.0257)	(0.0284)	(0.0026)	(0.0027)
Years×I(post-2002)				
0		0.0245		-0.0432***
		(0.0285)		(0.0024)
1		0.0765**		-0.0751***
		(0.0334)		(0.0029)
2		0.0934**		-0.0910***
		(0.0437)		(0.0035)
3+		0.1893***		-0.1720***
		(0.0592)		(0.0044)
Observations	17,860	17,860	3,609,338	3,609,338
Adj-R^2	0.246	0.247	0.552	0.552

[a] – Marriage or partnership.

All regressions include individual fixed effects, dummy variables for age, year, county, and dummy variables indicating legally separated, receipt of disability pension, and age>65. (The indicator variable for whether the marriage/partnership was entered after 2002 drops since it is a constant for the couple). Standard errors are clustered at the individual level.

Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 3.5: Fertility Effects (continued)

	(1)	(2)	(3)	(4)
	Men			
	Homosexual		Heterosexual	
Years since Union				
0	0.0010	-0.0014	0.1238***	0.1420***
	(0.0030)	(0.0030)	(0.0014)	(0.0018)
1	-0.0026	-0.0037	0.2836***	0.3093***
	(0.0033)	(0.0032)	(0.0018)	(0.0022)
2	-0.0055*	-0.0039	0.3890***	0.4148***
	(0.0033)	(0.0034)	(0.0022)	(0.0025)
3+	-0.0010	-0.0011	0.5680***	0.5867***
	(0.0044)	(0.0046)	(0.0028)	(0.0029)
Years×I(post-2002)				
0		0.0066		-0.0375***
		(0.0072)		(0.0027)
1		0.0029		-0.0629***
		(0.0068)		(0.0032)
2		-0.0055		-0.0731***
		(0.0045)		(0.0038)
3+		0.0009		-0.1422***
		(0.0054)		(0.0047)
Observations	18,498	18,498	3,609,338	3,609,338
Adj-R^2	0.005	0.005	0.506	0.506

[a] – Marriage or partnership.

All regressions include individual fixed effects, dummy variables for age, year, county, and dummy variables indicating legally separated, receipt of disability pension, and age>65. (The indicator variable for whether the marriage/partnership was entered after 2002 drops since it is a constant for the couple). Standard errors are clustered at the individual level.

Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

3.4.3 Other

There may also be less tangible benefits from social recognition of union status. Homosexual individuals have been identified as suffering worse health outcomes (Cochran (2001), Gilman et al. (2001), Herrell et al. (1999), Sandfort, de Graaf, Bijl, and Schnabel (2001)).

While our data are not particularly suited to look at mental or physical health outcomes, we have information on uptake of unemployment, disability and sickness benefits. Estimating Equation 3.1 with unemployment or disability pension as the left-hand side variable, we find no effects for homosexuals (but slight negative effects for heterosexuals).[12] These are crude measures of mental or physical health but taken at face value do not suggest health benefits of partnership.

3.5 Conclusion

Whether to allow same-sex couples to enter marriage-like legal unions is a contested issue currently on the legislative agenda of a number of countries and U.S. states. Despite the heated debate, the need for such unions is rarely articulated. Rather, it is often assumed that the benefits of marriage would carry over to the same-sex setting. In this paper, we have exploited legal reforms in Sweden to study the impact of an extension of rights to same-sex couples. In 1994, the Swedish parliament passed the Registered Partnership Act that gave same-sex couples entering registered partnership the same rights and obligations of marriage except in one sphere: joint parenting. Paternity presumption, the keystone of marriage, was carved out. Furthermore, joint legal parenthood through adoption was not possible. A step towards joint legal parenthood was taken with the enactment in 2002 of an adoption law giving those in registered partnership the right to joint- or step-child adoption.

[12]Not reported, available from the authors on request.

Using registration data, we created a panel of all individuals who enter registered partnership in the period 1995-2006 and studied their earnings and fertility outcomes. For comparison, we also created a similar panel of individuals who enter marriage in the same period. Our analysis sample thus contains men and women who entered either partnership or marriage and the effect of union entry is measured using a before-and-after comparison controlling for time varying characteristics, notably age.

We find registered partnership to be important to both gays and lesbians, but for distinctly different reasons. The overhang of gay couples entering partnerships in the first year allowed, the reduction in the combined earnings and the couple-earnings gap, and the virtual absence of children before and after union entry suggest that the main function of registered partnership for gays is resource pooling.

For lesbians, on the other hand, the right to joint or step-parent adoption allowed in 2002 raised fertility and possibly entry into partnership. Although the trend precedes the 2002 law, 2002 marks the year more women than men enter registered partnership, and the gap has continued to widen. These findings underscore both the centrality of the woman for family formation and the importance of legal parenthood. Thus, for lesbians, – low initial uptake, the decrease in combined earnings and narrowing of the couple-earnings gap, and fertility effects of union entry comparable to heterosexual couples especially after the 2002 reform – point to registered partnership being an important vehicle for family formation.

The lack of specialization among lesbians is largely consistent with the literature that has found same-sex couples to be less traditional in their division of labor than opposite-sex married couples (e.g., Grossbard and L. K. Jepsen (2008), Rothblum (2009)). However, it is at odds with Dillender (Forthcoming) who found access to legal marriage to lead to more single earner families among female same-sex couples. The explanations of the different findings may lie in institutional differences in the respective countries. Unlike the U.S.,

Sweden has universal health insurance coverage. Another difference is that childcare is highly subsidized and all but universal once the child reaches age one. Before that, generous parental leave policies enable parents to stay home. While these are policies that apply to same- and opposite sex couples alike, they have contributed to making housewife status highly optional.

The lack of specialization among lesbians is noteworthy given that they have children and are less positively assortatively matched (on education) than heterosexual couples. This finding casts new light on the source of the earnings divergence typically observed among heterosexual couples and routinely attributed to the woman specializing in non-market work. The different findings for lesbian partners and married couples are consistent with men paying women for the ability to bear children. Among lesbian couples, the basis for such payment is undermined by the fact that both partners are endowed with that capability.

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Appendix

A.1 The Texas Sentencing Guidelines

Table A.1.1: Charges, Crimes and Recommended Sentences

Charge	Typical Crimes	Eligible Penalty	Sentencing System
Capital Felony	Murder of a public safety officer, Multiple Murders, Murder of a child	Death, Life in Prison or Life in Prison without Parole	Indeter./Deter.
First-degree Felony	Murder, Possession of a controlled substance (CS) with intent to distribute, Theft over $200,000	5 to 99 years in a state prison and/or a fine of not more than $10,000	Indeterminate
Second-degree Felony	Possession of a CS $>$ 4 grams and $\leq$ 200 grams, Aggravated Assault with a deadly weapon, Indecency with a child (by contact), Intoxicated Manslaughter	2 to 20 years in a state prison and/or a fine of not more than $10,000	Indeterminate
Third-degree Felony	Possession of CS $>$ 1 gram and $\leq$ 4 grams, Aggravated Assault, DWI (3rd Offense), Solicitation of a minor	2 to 10 years in a state prison and/or a fine of not more than $10,000	Indeterminate
State jail Felony	Possession of CS $\leq$ 1 gram, DWI with a minor under the age of 15 in the vehicle, Third theft conviction of any amount	180 days to 2 years in a state jail and/or a fine of not more than $10,000	Determinate
Class A Misdemeanor	DWI (2nd offense), Assault causing bodily injury, Possession of marijuana (between 2 oz. and 4 oz.), Illegal possession of prescription drugs	Not more than 1 year in a county jail and/or a fine of not more than $4,000	Determinate[a]
Class B Misdemeanor	DWI (1st offense), Possession of Marijuana (less than 2 oz.), Prostitution	Not more than 180 days in a county jail and/or a fine of not more than $2,000	Determinate[a]
Class C Misdemeanor	Assault by contact, Drug paraphernalia, Disorderly conduct, Theft under $50	A fine of not more than $500	Not Applicable

Source: Texas Code of Criminal Procedure (2014).
Notes: (a) In 2010, the Harris County Sheriff's Department enacted an Early Release Program that allows inmates to earn "good time" for participation in education, employment or community service related activities. This technically makes sentencing of misdemeanor crimes indeterminate since 2010.

A.2 Comparing new and old estimation strategies

Table A.2.1: Comparing estimates between standard and new methodology

	Charged in Harris County criminal court with new offense			
In jail or prison	-0.060***	-0.027**	0.11***	0.69***
	(0.0068)	(0.011)	(0.021)	(0.12)
Released from incarceration	0.00092	0.047***	0.015***	-0.014
	(0.0066)	(0.015)	(0.0041)	(0.014)
[Released × Duration]	0.056***	0.055***		
	(0.0053)	(0.012)		
Kleibergen-Paap rk LM stat.	536.3	97.8	610.5	46.2
Kleibergen-Paap rk Wald F stat.	181.1	32.6	307.5	23.1
Unique defendants	431,422	462,374	887019	897,934
Total observations	13,744,324	15,425,102	29222981	29,976,867
Instrument type	Interacted	Average	Interacted	Average
Caseload	Felony		Misdemeanor	

	Quarterly log(earnings+1)			
In jail or prison	-2.59***	-1.98***	-3.25***	-1.57
	(0.30)	(0.39)	(0.98)	(3.26)
Released from incarceration	-0.55	-0.55	-0.42	-0.27
	(0.35)	(0.65)	(0.27)	(0.45)
[Released × Duration]	-0.34**	-0.015		
	(0.16)	(0.39)		
Kleibergen-Paap rk LM stat.	327.6	65.7	148.4	23.7
Kleibergen-Paap rk Wald F stat.	110.5	21.9	74.4	11.9
Unique defendants	243,491	259,698	419,432	424,306
Total observations	7,263,800	8,035,049	13,098,771	13,401,574
Instrument type	Interacted	Average	Interacted	Average
Caseload	Felony		Misdemeanor	

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects, instrumental variable controls for non-focal treatments and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

A.3 Robustness exercises

Table A.3.1: Robustness Exercise 1 - Impacts of incarceration on criminal activity using Texas Department of Public Safety statewide criminal conviction database

Type of criminal offense:	Property	Drug poss.	Drug mfr. or distr.	Violent	DWI
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.0038	-0.0097**	-0.0029	-0.0047*	-0.0035**
	(0.0038)	(0.0039)	(0.0019)	(0.0024)	(0.0016)
Released from incarceration	0.00048	0.00083	-0.0013	-0.0017	0.00090
	(0.0029)	(0.0027)	(0.0012)	(0.0017)	(0.0013)
[Released × Duration]	0.0090***	0.016***	0.0040***	0.0010	-0.00076
	(0.0022)	(0.0026)	(0.0012)	(0.0012)	(0.00089)
Underidentification statistic	536.3	536.3	536.3	536.3	536.3
Weak Identification statistic	181.1	181.1	181.1	181.1	181.1
Unique defendants	431,422	431,422	431,422	431,422	431,422
Total observations	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.020*	-0.0036	0.0034	-0.0080	0.0074
	(0.010)	(0.0085)	(0.0029)	(0.0064)	(0.0068)
Released from incarceration	0.0014	-0.0022	0.00025	-0.00023	0.00016
	(0.0016)	(0.0015)	(0.00046)	(0.00095)	(0.0011)
Underidentification statistic	610.5	610.5	610.5	610.5	610.5
Weak Identification statistic	307.5	307.5	307.5	307.5	307.5
Unique defendants	887,019	887,019	887,019	887,019	887,019
Total observations	29,222,981	29,222,981	29,222,981	29,222,981	29,222,981

Source: Harris County District Clerk's criminal court records (1980-2013), Texas Department of Public Safety criminal conviction records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects, instrumental variable controls for non-focal treatments and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3.2: Robustness Exercise 2 - Impacts of incarceration while clustering at Court × Quarter of Charge Level

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.060***	-0.32***	-2.59***	-0.0087	-0.00049
	(0.0074)	(0.037)	(0.31)	(0.019)	(0.0091)
Released from incarceration	0.00092	-0.054	-0.55	0.049**	0.0094
	(0.0070)	(0.043)	(0.36)	(0.022)	(0.010)
[Released × Duration]	0.056***	-0.036*	-0.34**	-0.016	-0.0044
	(0.0059)	(0.020)	(0.16)	(0.011)	(0.0043)
Total clusters	2,613	1,848	1,848	1,738	1,980
Total observations	13,744,324	7,263,800	7,263,800	8,864,396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.11***	-0.40***	-3.25***	-0.016	-0.024
	(0.030)	(0.12)	(0.99)	(0.077)	(0.025)
Released from incarceration	0.015***	-0.045	-0.42	0.024	0.010
	(0.0048)	(0.030)	(0.26)	(0.017)	(0.0070)
Total clusters	1,738	1,235	1,235	1,165	1,319
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

Table A.3.3: Robustness Exercise 3 - Impacts of incarceration excluding crime type in instrument construction

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.059***	-0.32***	-2.59***	-0.012	-0.0018
	(0.0069)	(0.037)	(0.31)	(0.018)	(0.0085)
Released from incarceration	0.0014	-0.059	-0.58	0.049**	0.0091
	(0.0067)	(0.043)	(0.35)	(0.021)	(0.0093)
[Released × Duration]	0.056***	-0.034*	-0.31*	-0.017	-0.0047
	(0.0055)	(0.020)	(0.16)	(0.011)	(0.0040)
Unique defendants	431,387	243,467	243,467	333,853	363,235
Total observations	13,741,071	7,261,945	7,261,945	8,862,474	9,877,134
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.11***	-0.37***	-3.01***	-0.019	-0.027
	(0.021)	(0.12)	(1.01)	(0.070)	(0.021)
Released from incarceration	0.016***	-0.044	-0.41	0.024	0.0100
	(0.0042)	(0.031)	(0.27)	(0.015)	(0.0061)
Unique defendants	887,016	419,421	419,421	645,564	705,463
Total observations	29,219,846	13,097,438	13,097,438	17,582,142	19,699,189

Table A.3.4: Robustness Exercise 4 - Impacts of incarceration after controlling for a quartic in the first-stage residuals

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.074***	-0.28***	-2.23***	0.0071	0.0041
	(0.0054)	(0.028)	(0.23)	(0.015)	(0.0070)
Released from incarceration	0.0074	-0.033	-0.37	0.037**	0.0085
	(0.0048)	(0.034)	(0.28)	(0.017)	(0.0078)
[Released × Duration]	0.055***	-0.042***	-0.39***	-0.013*	-0.0045
	(0.0037)	(0.014)	(0.11)	(0.0079)	(0.0029)
Unique defendants	431,422	243,491	243,491	333,888	363,260
Total observations	13,744,324	7,263,800	7,263,800	8,864.396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.074***	-0.32**	-2.42**	-0.025	-0.021
	(0.021)	(0.13)	(1.03)	(0.071)	(0.021)
Released from incarceration	0.018***	-0.043	-0.38	0.025	0.012*
	(0.0043)	(0.032)	(0.27)	(0.015)	(0.0062)
Unique defendants	887,019	419,432	419,432	645,576	705,473
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

Table A.3.5: Robustness Exercise 5 - Impacts of incarceration after trimming extreme valued instruments

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.053***	-0.35***	-2.82***	-0.012	-0.0043
	(0.0079)	(0.044)	(0.36)	(0.022)	(0.010)
Released from incarceration	0.0072	-0.091*	-0.81**	0.049**	0.0067
	(0.0074)	(0.049)	(0.41)	(0.024)	(0.011)
[Released × Duration]	0.022***	-0.021	-0.24	-0.022	-0.0051
	(0.0084)	(0.037)	(0.30)	(0.020)	(0.0075)
Unique defendants	431,299	243,422	243,422	333,700	363,115
Total observations	13,099,543	6,944,516	6,944,516	8,468,617	9,433,805
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.13***	-0.54***	-4.35***	0.079	-0.023
	(0.030)	(0.20)	(1.62)	(0.11)	(0.030)
Released from incarceration	0.0087*	-0.0091	-0.15	0.025	0.0075
	(0.0049)	(0.035)	(0.30)	(0.017)	(0.0065)
Unique defendants	886,545	418,793	418,793	644,465	704,903
Total observations	28,098,153	12,594,146	12,594,146	16,904,904	18,942,217

Table A.3.6: Robustness Exercise 6 - Impacts of incarceration using Lasso-weight instruments

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.057***	-0.32***	-2.57***	-0.0079	0.00020
	(0.0070)	(0.035)	(0.28)	(0.018)	(0.0082)
Released from incarceration	-0.0089	-0.043	-0.49	0.041**	0.0073
	(0.0070)	(0.042)	(0.35)	(0.020)	(0.0094)
[Released × Duration]	0.071***	-0.041**	-0.36**	-0.010	-0.0028
	(0.0054)	(0.018)	(0.14)	(0.0096)	(0.0036)
Unique defendants	431,422	243,491	243,491	333,888	363,260
Total observations	13,744,324	7,263,800	7,263,800	8,864,396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.18***	-0.30**	-2.40**	-0.035	-0.025
	(0.022)	(0.12)	(0.95)	(0.064)	(0.022)
Released from incarceration	0.014***	-0.029	-0.27	0.021	0.0097
	(0.0044)	(0.033)	(0.28)	(0.016)	(0.0063)
Unique defendants	887,019	419,432	419,432	645,576	705,473
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

Table A.3.7: Robustness Exercise 7 - Impacts of incarceration using cross validation without shrinkage procedure

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.049***	-0.35***	-2.89***	0.0014	-0.012
	(0.0086)	(0.041)	(0.33)	(0.021)	(0.0099)
Released from incarceration	0.019**	-0.026	-0.39	0.064***	-0.0010
	(0.0074)	(0.043)	(0.35)	(0.021)	(0.0100)
[Released × Duration]	0.034***	-0.036**	-0.33***	-0.014	-0.0058
	(0.0048)	(0.015)	(0.12)	(0.0093)	(0.0037)
Unique defendants	421,679	237,414	237,414	325,879	355,050
Total observations	13,183,828	6,977,260	6,977,260	8,548,485	9,509,945
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.021	-0.38***	-2.93***	-0.053	-0.0071
	(0.016)	(0.067)	(0.55)	(0.041)	(0.015)
Released from incarceration	0.022***	-0.032	-0.31	-0.0065	0.0054
	(0.0050)	(0.033)	(0.28)	(0.024)	(0.0065)
Unique defendants	885,565	418,474	418,474	644,099	703,984
Total observations	29,094,032	13,040,814	13,040,814	17,514,605	19,619,405

A.4 Crime-specific estimates for Cost Benefit Exercise

Table A.4.1: Impacts of incarceration on specific types of criminal charges

Type of criminal offense:	Murder	Sexual Assault	Robbery	Assault	Burglary	Larceny	Drug Possession	Driving While Intoxicated
In jail or prison	0.00076	0.00080*	-0.0014	-0.0026	-0.0076***	-0.0043	-0.023***	-0.0032**
	(0.00046)	(0.00045)	(0.00093)	(0.0017)	(0.0021)	(0.0027)	(0.0032)	(0.0014)
Released from incarceration	0.00022	0.00024	0.00099	0.0011	-0.0022	0.0030	-0.0044	0.00077
	(0.00041)	(0.00042)	(0.00086)	(0.0015)	(0.0020)	(0.0024)	(0.0031)	(0.0014)
[Released × Duration]	0.00038	0.00021	0.00038	0.00094	0.0099***	0.0086***	0.026***	-0.0014
	(0.00027)	(0.00028)	(0.00069)	(0.0012)	(0.0018)	(0.0019)	(0.0029)	(0.00083)
Kleibergen-Paap rk LM stat.	536.3	536.3	536.3	536.3	536.3	536.3	536.3	536.3
Kleibergen-Paap rk Wald F stat.	181.1	181.1	181.1	181.1	181.1	181.1	181.1	181.1
Unique defendants	431,422	431,422	431,422	431,422	431,422	431,422	431,422	431,422
Total observations	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects, instrumental variable controls for non-focal treatments and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions. *** p<0.01, ** p<0.05, * p<0.1.