

Surveillance and System Avoidance: Criminal Justice Contact and Institutional Attachment

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Abstract

The degree and scope of criminal justice surveillance increased dramatically in the United States over the past four decades. Recent qualitative research suggests the rise in surveillance may be met with a concomitant increase in efforts to evade it. To date, however, there has been no quantitative empirical test of this theory. In this article, I introduce the concept of “system avoidance,” whereby individuals who have had contact with the criminal justice system avoid surveilling institutions that keep formal records. Using data from Add Health ($n = 15,170$) and the NLSY97 ($n = 8,894$), I find that individuals who have been stopped by police, arrested, convicted, or incarcerated are less likely to interact with surveilling institutions, including medical, financial, labor market, and educational institutions, than their counterparts who have not had criminal justice contact. By contrast, individuals with criminal justice contact are no less likely to participate in civic or religious institutions. Because criminal justice contact is disproportionately distributed, this study suggests system avoidance is a potential mechanism through which the criminal justice system contributes to social stratification: it severs an already marginalized subpopulation from institutions that are pivotal to desistance from crime and their own integration into broader society.

Keywords

surveillance, crime, punishment, inequality, institutions

Over the past four decades, the degree and scope of surveillance in the United States increased dramatically. The proliferation of surveillance—the “collection and analysis of information about populations in order to govern their activity” (Haggerty and Ericson 2006:3)—has catalyzed significant theoretical reflection, with some scholars arguing that surveillance has become a salient characteristic of all modern societies (Foucault 1977; Garland 2001; Giddens 1990). Excluding routine traffic stops, 5.5 million people were involuntarily stopped by police in 2008, the majority of whom were released without

charge, and more than 40 million individuals had face-to-face police contact (Eith and Durose 2011). Fully one-quarter of the adult population—47 million Americans—now have a record on file with criminal justice agencies (Travis 2002). Much scholarship

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exists on the precipitous rise in the population under criminal justice surveillance. Although most of this research focuses on imprisonment and criminal records, police contact and criminal justice sanctions short of incarceration have led a growing swath of individuals—who previously would not have been involved in the criminal justice system for their minor offenses—to be placed under criminal justice supervision, a phenomenon termed “net widening” (Cohen 1985).

Beyond the broadening reach of the criminal justice system, there has been a creep of surveillance more generally. Institutions not typically associated with crime control have adopted the language and logic of crime control and surveillance (Garland 2001; Harris, Evans, and Beckett 2011; Innes 2001; Simon 2007), and formerly discrete institutions have become integrated into the “surveillant assemblage”—a system aimed at performing surveillance and social control functions (Haggerty and Ericson 2000). Examples of this assemblage include recent regulations requiring U.S. banks to link their clients’ financial holdings to a roster of individuals who owe child support (Haggerty and Ericson 2000) and fusion centers—surveillance centers that consolidate data from public and private agencies, including criminal, hospital, bank, and state motor vehicles records, and make them available to law enforcement agencies.

The consequences of surveillance are important for scholars and policymakers interested in inequality, institutions, and criminal justice policy. While most research on the topic focuses on the intended functions of surveillance, a burgeoning literature examines the unanticipated consequences (Merton 1936). Theory and recent ethnographic work (Goffman 2009) suggest the rise in surveillance—and, more importantly, individuals’ *perceptions* of pervasive surveillance—may be met with a concomitant increase in individuals’ efforts to evade it. To date, however, there has been no systematic test of whether criminal justice contact is indeed associated with individuals avoiding certain institutions.

To begin to test for this relationship, I introduce the concept of “system avoidance.”

System avoidance denotes the practice of individuals avoiding institutions that keep formal records (i.e., put them “in the system”) and therefore heighten the risk of surveillance and apprehension by authorities. I argue that system avoidance is an important concept that should be developed theoretically and operationalized in a way that can be empirically tested. Using data from the National Longitudinal Study of Adolescent Health ($n = 15,170$) and the National Longitudinal Survey of Youth 1997 ($n = 8,894$), I test the hypothesis that involvement with the criminal justice system at all levels—from police contact to incarceration—affects how people interact with medical, financial, labor market, educational, civic, and religious institutions. Specifically, I posit that the potential of surveillance may lead to lower levels of involvement in institutions that keep formal records, such as hospitals, banks, schools, and employment, and I hypothesize that a previously ignored mechanism—system avoidance—may be driving this relationship.

To identify wariness of surveillance as a motivation behind system avoidance, the following theoretical framework emphasizes the distinction between surveilling and non-surveilling institutions, arguing that an undertheorized characteristic of institutions is the degree to which they keep formal records of individuals’ behaviors, transactions, and interactions as a matter of course. Surveilling institutions keep detailed formal records; these records are critical to carrying out the institution’s functions and, in most instances, are required to be kept by law. Examples of surveilling institutions include hospitals, banks, formal employment, and schools. Non-surveilling institutions are characterized by a more casual relationship with individuals—formal records may be kept, but there is no legal imperative to do so. Examples of non-surveilling institutions include volunteer associations and religious groups.

To elucidate this distinction, consider how routine interactions with surveilling and non-surveilling institutions differ. One profound difference is in requirements for identification. Interactions with banks, hospitals, and

employers all require individuals to present state-issued papers to establish identity. For hospitals, establishing identity is essential for, among other things, insurance billing. For banks and employers, establishing identity is legally required in fulfillment with labor and financial regulations and tax compliance. By contrast, religious and volunteer associations do not, as a rule, require individuals to present identification to participate. These institutions may keep legal records of donations and other activities for tax purposes, but individuals can easily abstain from these more intrusive forms of record keeping without jeopardizing their ability to interact with the institution.

By focusing on institutional involvement, this article extends existing research on the consequences of criminal justice contact by analyzing a previously ignored outcome that literature suggests is consequential for social stratification and marginalization. Lack of attachment to medical care, banks, schools, and employment is associated with poorer outcomes for health, financial security, upward mobility, and desistance from crime, respectively. Furthermore, although surveillance is growing in all parts of society, its penetration is differential (Fiske 1998). Individuals who have been involved in the criminal justice system are under heavier surveillance than those who have not. Given that disadvantaged populations are more likely to have contact with the criminal justice system, any negative consequences of associated institutional avoidance will be similarly disproportionately distributed, thereby exacerbating preexisting inequalities. Therefore, this article advances a mechanism by which the criminal justice system may stratify and marginalize already disadvantaged individuals and groups.

EXPANSION OF CRIMINAL JUSTICE SUPERVISION

The U.S. criminal justice system has grown dramatically over the past four decades. Today, one in 31 Americans—approximately 10 million people—are under some form of

correctional supervision and 2.3 million people are in prison or jail, making the United States a world leader in its use of imprisonment (Pew Charitable Trusts 2010). Including those under community supervision, more than 47 million Americans—one-quarter of the adult population—have a criminal record on file with federal or state criminal justice agencies (Travis 2002). Excluding routine traffic stops, 5.5 million people were involuntarily stopped by the police in 2008, the majority of whom were released without charge, and over 40 million individuals had face-to-face police contact (Eith and Durose 2011).

Rates of criminal justice involvement—from street stops to incarceration—are highly stratified by class and race. Close police surveillance has become a part of everyday life for many residents of poor neighborhoods (Beckett, Nyrop, and Pfingst 2006; Gelman, Fagan, and Kiss 2007; Goffman 2009). However, this was not always the case. Ethnographies from the 1970s and 1980s describe urban minority communities as largely devoid of police presence (e.g., Anderson 1978; Williams 1992). Goffman's (2009) more recent ethnographic work, by contrast, illustrates the magnitude of police surveillance today. Due, in part, to changes in crime control laws, such as the 1994 Violent Crime Control and Law Enforcement Act, police saturation and frequent interactions with authorities have become a reality in many low-income communities.

Research clearly demonstrates that racial minorities—namely black and Latino residents—have higher rates of police contact. To be sure, areas with high crime rates often have high concentrations of minority citizens (Massey and Denton 1993). However, differences in stop rates among racial and ethnic groups cannot be explained by precinct or previous arrest rates alone (Beckett et al. 2006; Gelman et al. 2007). Similar patterns exist for more serious criminal justice involvement—the risk of imprisonment for white men born since the late 1960s is less than one in 30, whereas the risk for black men is one in five (Western and Wildeman 2009), with 60 percent of black male high school dropouts spending some time in prison (Pettit and Western 2004). The

sheer magnitude of the criminal justice system, coupled with the large race and class disparities in rates of involvement, raises a host of sociological questions about the consequences of such differential involvement for inequality.

CONSEQUENCES OF CRIMINAL JUSTICE CONTACT

The rise in imprisonment has catalyzed research on the criminal justice system as a powerful mechanism of stratification. Existing literature suggests incarceration has negative consequences for employment (Pager 2007; Pettit and Western 2004; Western 2002), political participation (Manza and Uggen 2006), children and families (Comfort 2007; Foster and Hagan 2007; Western and McLanahan 2000; Wildeman 2009), neighborhood stability (Clear 2007), and access to housing and public assistance (Travis 2002).

In addition to diminished opportunities, procedural justice literature suggests negative interactions with legal authorities can erode public perceptions of police legitimacy and trust in government, or “the system,” more broadly (Fagan and Davies 2000; Soss 1999; Sunshine and Tyler 2003). In poor neighborhoods, residents’ most frequent contact with the state is often through street-level bureaucrats, such as the police (Lipsky 1983). Such interactions are routinely adversarial, infused with suspicion and involuntary (Skogan 2006). The “legal cynicism” (Sampson and Bartusch 1998) that can develop through interactions with law enforcement can shape normative orientations toward a wide variety of bureaucratic institutions. Moreover, contact with the criminal justice system has real political consequences—Weaver and Lerman (2010) find it is associated with lower levels of trust in government and political participation.

In addition to implications for perceptions of police and the state, involvement with the criminal justice system can trigger labeling consequences (Becker 1963). Pager (2007) finds the mark of a criminal record confers “negative credentials” on individuals attempting to enter the labor market. Likewise,

Garland (2001:142) argues that the modern era is characterized by a penal strategy of “marked, monitored existence” for those in the criminal justice system. Being labeled a deviant not only confers a stigma that shapes how others relate to that person, but it may also lead individuals to alter their behavior in ways unintended by state agents, a behavior termed “secondary deviance” (Lemert 1967; Matza 1969).

SURVEILLANCE AND ITS CONSEQUENCES

Rising incarceration rates, the growth of stop-and-frisk databases, and police saturation in low-income minority neighborhoods are fueling a heightened sense of surveillance. This growth of criminal justice surveillance is coupled with the expansion of surveillance in a wide variety of organizations (Garland 2001; Harris et al. 2011). Enabled by technological advancement, formerly discrete institutions have become integrated into what has been described as a “surveillant assemblage” (Haggerty and Ericson 2000).

A large body of literature analyzes the increase in surveillance in state and non-state institutions (e.g., Foucault 1977; Garland 2001; Giddens 1990; Haggerty and Ericson 2006). Foucault (1977:214) suggests that modern societies are characterized by omnipresent surveillance, accumulated in formal “reports and registers” comprising “an immense police text” that creates a “permanent account of individuals’ behavior.” More recently, Garland (2001) describes a “culture of control” in which surveillance pervades institutions not typically associated with a crime control function. Crime control now entails institutions formally mandated to reduce crime, such as the police and prisons, as well as informal institutions of social control in broader society that are “embedded in the everyday activities and interactions of civil society” (Garland 2001:5). By extension, crime control includes not only the actions of criminal justice authorities, but also “private actors and agencies *as they go about their daily lives and ordinary routines*” (Garland

2001:6, emphasis added). Individuals interact on a daily basis with institutions that keep records, contributing to a real awareness of the prospect of being surveilled and raising concerns over “contextual integrity” (Nissenbaum 2004)—the collection of personal data in one context and use of it in another.

Note that a social control or surveillance motive need not be assumed in all organizations that conduct record keeping and data sharing. Initially, records from different agencies were linked under a “welfarist ideology of service delivery” (Haggerty and Ericson 2000:611). However, recent research suggests institutions such as hospitals, schools, workplaces, and banks have increasingly been “drawn into the harder edge of social control” (Haggerty and Ericson 2000:611) and oriented toward surveillance (Harris et al. 2011). Record-keeping practices initially introduced with one intention are often expanded to address new problems and situations. Information accumulation and data migration are characterized by unintended expansion, whereby the simple everyday use of institutions leads to the amassing of more personal data (Innes 2001). In short, regardless of the reason they were kept in the first place, data and records are increasingly integrated and deployed by law enforcement agencies for a broad range of surveillance purposes.

A number of recent studies explore some of the unanticipated consequences of the spread of surveillance across institutional settings, including the concomitant increase in people’s efforts to evade it. In a study of Temporary Assistance for Needy Families (TANF) recipients, O’Brien (2008) finds that individuals’ concern that social service employees could monitor transactions with formal financial institutions and find some way to deem them ineligible for assistance led them to avoid banks. Notably, program participants feared such surveillance even in states that did not require formal records of recent bank transactions to qualify for assistance, suggesting *perceptions* of surveillance may be as consequential in shaping individuals’ behavior as surveillance practices themselves.¹ Similarly, in Harris, Evans, and Beckett’s

(2010:1782) research on legal financial obligations stemming from involvement in the criminal justice system, some respondents reported “fear of being sanctioned for non-payment led them to hide from authorities.” After the U.S. Congress passed the Child Support Recovery Act in 1992, which administratively linked child support and public assistance records (Rich, Garfinkel, and Gao 2007; Wimberly 2000), research suggests the fear of detection led some men to withdraw from formal employment and increase underground work (Holzer, Offner, and Sorensen 2005; Waller and Plotnick 2001).

Additionally, research on surveillance in public assistance programs suggests a growing overlap between welfare and criminal justice systems. Gilliom (2001) describes the common experience of systematic surveillance between individuals on public assistance and those under criminal justice supervision and argues it is magnified by the recent computerization of welfare administration. Gustafson (2011) cites Operation Talon as an example of information exchange between welfare offices and various branches of the criminal justice system. Under this program, sting operations were set up in food stamp offices—individuals with outstanding warrants would receive a phone call indicating there was a problem with their benefits or they were eligible for a bonus and instructing them to report to a welfare office. Upon arrival, an officer from the sheriff’s department would serve them an arrest warrant. More than 10,000 individuals were arrested through the program between 1997 and 2006 (Gustafson 2011). In this way, “the ‘left hand’ of the welfare state and the ‘right hand’ of the carceral state now work together as a single system of poverty governance” (Soss, Fording, and Schram 2011:6). Knowledge of such programs and the overlap of welfare and criminal justice systems may discourage some individuals from utilizing government benefits.

Recent ethnographic work describes individuals’ wariness of surveillance and consequent avoidance of institutions in great detail. In her study of the impact of criminal justice surveillance in a Philadelphia neighborhood,

Goffman (2009:353) concludes that due to the prevailing “climate of fear and suspicion in poor communities,” individuals wary of being apprehended for anything from technical parole violations to outstanding court fines and fees “avoid institutions, places, and relations on which they formerly relied.” Institutions and activities that “others rely on to maintain a decent and respectable identity,” Goffman (2009:353) argues, are “transformed into a system that the authorities make use of to arrest and confine them. The police and courts become dangerous to interact with, as does showing up to work or going to places like hospitals.” Individuals avoid going to hospitals to obtain medical care or to attend the birth of their children for fear they could be tracked and apprehended by authorities. This wariness of hospitals appears justified—in an interview with two police officers, Goffman (2009) found that in addition to surveilling Social Security, bank, and employment records, officers routinely run the names on hospital admission records when they bring someone to the emergency room. Interestingly, even people with no pending legal action expressed concern that, if given the opportunity, the police would run their name through the system and “find some reason to hold them” (Goffman 2009:344) or pressure them to inform on a friend or family member. These efforts to evade authorities ultimately undermine attachment to important institutions.²

This research suggests that individuals wary of surveillance may deliberately and systematically avoid institutional contact that puts them “in the system,” because of the prospect they will come under heightened surveillance, thus increasing their risk of detection by authorities. I label this behavioral response “system avoidance”—the practice of individuals avoiding institutions that keep formal records. Preexisting literature on the fear of surveillance raises two questions about system avoidance. First, is there generalizable evidence to support the qualitative findings that individuals who have had criminal justice contact avoid institutions that put them in the system? Second, do individuals abstain from using institutions across the

board, or are they selective in their institutional avoidance? To gain analytic leverage on these questions, I introduce the distinction between surveilling and non-surveilling institutions. As previously described, surveilling institutions are legally required to keep formal records. Simply keeping records heightens the perception that the police, parole officers, or probation officers could access these data. Surveilling institutions highlighted in existing qualitative literature include hospitals, banks, schools, employment, and public assistance (Garland 2001; Gilliom 2001; Goffman 2009; Gustafson 2011; Harris et al. 2010, 2011; O’Brien 2008; Soss et al. 2011). By contrast, individuals can easily opt out of formal record keeping in non-surveilling institutions. Examples of such institutions are volunteer and religious associations.

Appreciating system avoidance is critical to recognizing the full range of consequences of criminal justice contact, as well as understanding an unexplored pathway through which such contact may have real stratifying consequences for individuals. Attachment to surveilling institutions is consequential for life outcomes: obtaining medical care is important for health; banks are necessary for full financial participation in society, savings, credit, and upward mobility; life course literature suggests attachment to school and employment is important in the transition into adult roles; and public assistance is an important social safety net for the economically vulnerable. Moreover, in addition to the stratifying and marginalizing consequences of system avoidance, attempts at social control through surveillance may actually fuel the very behaviors it is trying to suppress. When people go off the books, their attachment to institutions key to desistance from crime, such as formal employment, are undermined (Hirschi and Gottfredson 1993; Laub and Sampson 1993).

HYPOTHESES

The aim of this empirical investigation is to test for a relationship between criminal justice contact and institutional attachment. I test the following hypotheses, which are

motivated by theories of social control and surveillance, and existing qualitative literature (e.g., Goffman 2009). Net of a host of individual-level sociodemographic and behavioral characteristics that could be associated with both criminal justice contact and institutional avoidance, (1) individuals who have had contact with the criminal justice system at all levels—that is, have been stopped and questioned by the police, arrested, convicted, or incarcerated—will have higher odds of not participating in surveilling institutions that keep formal records, such as (1a) hospitals; (1b) banks; and (1c) school or work.³ (2) These individuals will be no less likely to interact with non-surveilling institutions, such as (2a) volunteer and (2b) religious associations. (3) Individuals with deeper levels of criminal justice involvement will have higher odds of not participating in surveilling institutions than will those with lower levels of contact.⁴

Testing for the association between criminal justice contact and non-surveilling institutions serves two functions. First, it tests an alternative empirical outcome—that involvement with the criminal justice system will depress all types of institutional involvement, as individuals abstain from participation due to associated feelings of disempowerment and distrust in institutions in general. Indeed, research suggests that individuals who have been involved in the criminal justice system may develop a cognitive framework of distrust and cynicism of institutions. Second, it serves as a theoretical robustness check—if unobservable selection dynamics are driving contact with the criminal justice system *and* institutional avoidance, results should demonstrate that individuals with involvement in the criminal justice system participate less in all institutions, not merely those that keep formal records.

DATA

To test these hypotheses, I primarily utilize data from the National Longitudinal Study of Adolescent Health (Add Health) (Harris et al. 2009), a nationally representative panel survey of adolescents interviewed at four points

in time. The sample of adolescents was selected in 1994 and 1995 from 132 schools, and the study includes in-school and in-home surveys and interviews with the adolescents and their teachers, school administrators, parents, siblings, peers, and romantic partners. I use data from Waves 3 and 4, when respondents were age 18 to 26 and 24 to 34.⁵ Wave 3 ($n = 15,170$) includes a battery of questions on criminal justice contact and institutional involvement, providing a unique opportunity to estimate the association between the two.⁶ I also use data from the National Longitudinal Survey of Youth 1997 (NLSY97) (Bureau of Labor Statistics 2012) in targeted supplementary analyses. NLSY97 ($n = 8,984$) is a nationally representative panel survey of individuals interviewed at 14 points in time. The analyses presented here use data from four rounds in which respondents were of comparable age to the Add Health sample.⁷

I coded respondents' contact with the criminal justice system into five mutually exclusive categories: (1) no contact, (2) stopped and questioned, (3) arrested, (4) convicted, and (5) incarcerated.⁸ Individuals are categorized based on their most serious degree of criminal justice contact (i.e., if an individual was both questioned by the police and arrested, they are classified as "arrested" only). I also constructed an overall measure of criminal justice contact that is a binary indicator for whether a respondent reported any criminal justice contact, regardless of level. The outcome of interest—institutional involvement—is divided into surveilling and non-surveilling institutions. Surveilling institutions in this analysis are medical facilities (e.g., hospitals, doctors' offices, and clinics), formal financial institutions (e.g., banks), employment, and schools (e.g., high schools, colleges, and universities). Institutional involvement is coded using a series of binary measures, with lack of attachment coded as 1. Medical institutional involvement is coded based on whether respondents reported not obtaining medical care when they thought they needed it in the past 12 months (1 = did not obtain necessary medical care, 0 = did not report *not* obtaining necessary medical care).

Financial institutional attachment is coded based on whether respondents reported not having a checking account (1 = no account, 0 = account). Employment and school enrollment are combined in one binary indicator for whether a respondent was “neither in school nor work” (1 = no school/work, 0 = school/work), because these overlap as relevant age-graded institutions for adults in this age range.⁹ In addition, because having a child under 12 years of age at home is the strongest predictor, for women, of being neither in school nor working (it is nonsignificant for men), women with a young child are excluded from the neither-in-school-nor-working group.¹⁰

Non-surveilling institutions in these analyses are volunteer associations and religious institutions. Involvement with volunteer associations is indicated using a binary measure for whether respondents performed unpaid volunteer or community service work in the past 12 months (1 = no volunteer, 0 = volunteer). Participation in religious groups is indicated using a binary measure for whether respondents participated in activities for young adults such as Bible classes, retreats, youth groups, or choir at churches, synagogues, or other places of worship in the past 12 months (1 = no participation, 0 = participation).

ANALYTIC APPROACH

Pathways into involvement with the criminal justice system and the consequences that stem from such involvement are complex. Therefore, there are a number of challenges in establishing causal inference in this analysis. First, literature suggests many of the characteristics that predict involvement in the criminal justice system—such as socioeconomic disadvantage or being a racial/ethnic minority—may also shape patterns of institutional attachment. Similarly, from a behavioral perspective, it is plausible that individuals who tend to be unpredictable or evasive may be more likely to become involved in the criminal justice system and less likely to develop institutional ties. Therefore, I use a number of analytic strategies to attempt to isolate the direct relationship

between criminal justice contact and institutional attachment.

The first strategy is to include an extensive battery of individual-level sociodemographic and behavioral controls that preexisting literature suggests may be associated with both criminal justice contact and institutional avoidance—age, sex, education, parents’ education, employment status, school status, race, citizenship, marital status, military service, and household configuration (number in household and whether respondents live with parents). Add Health also has a uniquely rich set of self-reported behavioral measures, including drug use (cocaine, methamphetamine), drug sales, property crime (damaging property, theft under \$50, theft over \$50), violent behavior (fighting, stabbing), carrying a knife or gun to school or work, gang membership, and impulsivity (measured as individuals who self-report liking to “take risks,” “lose control” of themselves,¹¹ or wish there were “no rules or restrictions”). Controlling for this range of behaviors that may be driving both outcomes assists in isolating the net effect of criminal justice contact.

Furthermore, given the nature of the investigation and potential selection into refusing to answer questions about illegal behaviors, it could be problematic to handle missing data by employing listwise deletion and excluding all cases in which respondents refused to answer. Therefore, in this analysis, missings on behavioral questions are included in models as binary indicators, because respondents might refuse to answer these questions in a systematic way, therefore potentially biasing any estimated association between criminal justice contact and institutional involvement.¹²

Additionally, I employ three robustness checks, one theoretical and two empirical. As a theoretical robustness check, as previously noted, I test for the association between criminal justice contact and non-surveilling institutions. If unobservable measures of selection are driving contact with the criminal justice system and institutional avoidance, results should demonstrate that individuals with involvement in the criminal justice system avoid all institutions, not merely those that keep formal records.

Although the cross-sectional models are useful in estimating an association between criminal justice contact and institutional involvement, more complex models are required to delve more deeply into potential mechanisms. As a first empirical robustness check, I use propensity score matching—a nonparametric estimation method—and doubly robust estimation. Because criminal justice contact is not randomly distributed, it is useful to model selection into the criminal justice system. Propensity score matching and the doubly robust estimation strategy are designed to gain analytic leverage by allowing for a better-specified treatment definition and establishing a direct test of the counterfactual model by making treatment and control groups more comparable. Propensity score matching offers a number of advantages over basic logistic regression models, namely that it does not rely on assumptions about functional forms, as parametric methods do (Harding 2003; Rosenbaum and Rubin 1983; Winship and Morgan 1999). In this analysis, it makes it possible to control for characteristics and behaviors likely to predict criminal justice contact, maximize covariate balance between treatment and control groups, and test the marginal effect of criminal justice contact on institutional attachment. Cases are matched based on their propensity to have contact with the criminal justice system; propensity scores are generated using logistic regressions that include all sociodemographic and behavioral covariates in the parametric models. This analysis uses nearest available pair matching with replacement.¹³ The range of the difference in propensity scores between treated and untreated matched cases is very small (.01 is the maximum), and comparing covariate means for each of the five matches demonstrates excellent balance was achieved.¹⁴ I then conduct a doubly robust estimation in which propensity scores serve as analytic weights in the logistic regression model. In line with existing research (e.g., Brand and Xie 2010; Heckman, Ichimura, and Todd 1997), I restrict analyses to the region of common support, that is, the region of propensity scores in which both treatment

and control cases are observed. Including cases in which the propensity score of treatment observations is lower than the minimum or higher than the maximum propensity score of all control cases represents a violation of the common support condition (or minima and maxima criterion) and could be a source of evaluation bias.¹⁵

As a final empirical test, I estimate individual fixed-effects regressions to net out time-invariant, individual-level unobservable characteristics that could potentially be associated with both criminal justice contact and institutional involvement. The estimation relies on cases with change in both the outcome and predictor variables, in other words, when individuals had no criminal contact in Wave 3, but did in Wave 4, or their levels of criminal justice contact and institutional involvement changed across waves. Although a benefit of fixed-effects analysis is that it exploits longitudinal data, it can only estimate change in variables consistent across different waves of the survey. Therefore, I recoded criminal justice contact to include only arrest, conviction, and incarceration, because individuals were not asked if they had been stopped by the police in Wave 4 of Add Health. Additionally, individuals were not asked if they have a bank account in Wave 4 of Add Health. Therefore, I employ data from NLSY97 in targeted supplemental analyses to estimate the banking outcome. Analysis of NLSY97 data is limited to any criminal justice contact, arrest, and incarceration categories. I included wave fixed effects in all models to net out the average difference between the two time periods. Triangulating the cross-sectional, propensity score matching, and fixed-effects analyses provides considerable analytic leverage to estimate the relationship between criminal justice contact and institutional involvement.

RESULTS

Criminal Justice Contact

Figure 1 depicts respondents' involvement with the criminal justice system. Among

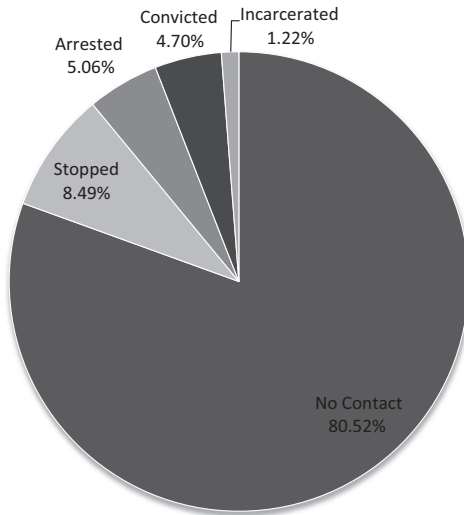


Figure 1. Criminal Justice Contact within Sample

Source: National Longitudinal Study of Adolescent Health.

Note: Percentages may not add to 100 due to rounding.

Wave 3 respondents, 2,927 (19.48 percent) had contact with the criminal justice system; 1,276 (8.49 percent) had been stopped and questioned by the police but never arrested; 761 (5.06 percent) had been arrested but never convicted; 706 (4.70 percent) had been convicted but never incarcerated; and 184 (1.22 percent) served time in prison or jail.

Table 1 presents results of five discrete logistic regression models predicting criminal justice contact. The reference category in each model is people who had no criminal justice contact. The findings on sociodemographic characteristics largely comport with existing literature on the topic: net of other covariates, being male, U.S.-born, unemployed, or having lower levels of educational attainment is associated with higher odds of having been incarcerated (e.g., Beckett et al. 2006; Davies and Fagan 2012; Kubrin and Ishizawa 2012; MacDonald and Saunders 2012; Pager 2007; Peterson and Krivo 2005; Pettit and Western 2004; Wakefield and Uggen 2010; Western 2006).¹⁶ In terms of behaviors, having gang ties, being impulsive, reporting having damaged property, or

reporting pulling a gun or knife on someone is associated with higher odds of having been incarcerated, controlling for all other variables in the model. Note that in these data, impulsivity is associated with higher odds of having been arrested, convicted, or incarcerated, but not with having been stopped by the police (e.g., Gottfredson and Hirschi 1990; Pratt and Cullen 2000; Sampson and Laub 1993; Weaver and Lerman 2010).

Institutional Involvement

Approximately 23 percent of respondents reported not obtaining medical care when they needed it, 28 percent did not have a bank account, 14 percent were neither in school nor working, 72 percent did not participate in volunteer activities, and 74 percent were not involved in religious organizations.

Logistic regression models. Table 2 presents results from logistic regression models predicting institutional involvement by criminal justice contact. I estimated two discrete models for each of the five institutional outcomes—medical care, bank accounts, school/work, volunteer associations, and religious groups. In the first model, for each outcome I use a single binary indicator for any criminal justice contact, and in the second, criminal justice contact is disaggregated into four mutually exclusive categories. All 10 models include a battery of individual-level controls, including sociodemographic and behavioral characteristics.

Models 6 and 7 present results of logistic regressions predicting the odds of individuals not obtaining needed medical care. In addition to the host of sociodemographic and behavioral controls, these models also control for general health and possession of medical insurance. Individuals who had contact with the criminal justice system had 31 percent higher odds of not obtaining medical care when they needed it, compared to those who did not have contact. Model 7 disaggregates criminal justice contact into four levels. Individuals who have only been stopped by the police had 33 percent higher odds of not

Table 1. Predicting Criminal Justice Contact

	Model 1: Any CJ Contact	Model 2: Stopped	Model 3: Arrested	Model 4: Convicted	Model 5: Incarcerated
Sociodemographic					
Male	3.151*** (.157)	2.231*** (.146)	3.606*** (.335)	4.617*** (.486)	11.812*** (3.333)
Black	.927 (.056)	.825* (.070)	1.238* (.129)	.749* (.091)	1.249 (.278)
Hispanic	1.149* (.081)	1.220* (.115)	1.372** (.166)	.812 (.112)	1.512 (.366)
Other race	.933 (.080)	.872 (.101)	.934 (.146)	.986 (.155)	1.193 (.398)
Age	1.015 (.299)	.465 (.183)	2.812 (1.497)	2.400 (1.359)	3.636 (3.829)
U.S. born	1.247* (.122)	.948 (.117)	1.232 (.213)	2.116*** (.489)	6.385* (4.673)
Education	.930*** (.013)	1.047* (.020)	.897*** (.023)	.819*** (.022)	.619*** (.036)
Parent college	1.240*** (.065)	1.279*** (.093)	1.238* (.113)	1.292** (.125)	.912 (.167)
Have job	.890* (.048)	.970 (.071)	.779** (.072)	.891 (.092)	.535*** (.101)
In school	1.140* (.061)	1.167* (.082)	1.076 (.103)	1.149 (.118)	.780 (.190)
Behavioral					
Theft over \$50	1.353* (.164)	1.129 (.184)	1.469* (.271)	1.238 (.249)	1.566 (.535)
Theft under \$50	1.678*** (.142)	1.793*** (.193)	1.599*** (.228)	1.710*** (.253)	1.267 (.394)
Damaged prop.	1.950*** (.142)	1.764*** (.173)	2.006*** (.240)	2.205*** (.276)	2.506*** (.598)
Carry gun/knife	.760 (.146)	.711 (.210)	.639 (.202)	.579 (.190)	1.571 (.604)
Pulled gun/knife	2.123*** (.397)	1.995** (.531)	2.128** (.615)	2.189** (.661)	2.480* (1.028)
Stabbed	1.212 (.384)	.433 (.266)	1.591 (.702)	.735 (.391)	1.515 (.923)
Used meth	1.665*** (.159)	1.167 (.169)	1.541** (.236)	2.225*** (.315)	1.822* (.477)
Used cocaine	2.294*** (.174)	1.811*** (.197)	2.263*** (.286)	2.886*** (.357)	2.916*** (.683)
Sold drugs	2.088*** (.161)	1.701*** (.188)	1.988*** (.247)	2.305*** (.281)	2.441*** (.532)
Gang	1.334*** (.081)	1.134 (.098)	1.486*** (.150)	1.519*** (.166)	1.943*** (.373)
Impulsive	1.290*** (.067)	1.152 (.084)	1.458*** (.128)	1.316** (.124)	1.602** (.282)
N	14,557	12,917	12,456	12,410	11,901
Pseudo R-squared	.150	.082	.147	.218	.335

Note: All coefficients expressed as odds ratios. Standard errors are in parentheses. Includes controls for household size, live with parents, military, age squared, missing self-reported drug use, and criminal behavior. Reference category in all models is respondents with no criminal justice contact.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table 2. Logistic Regression Predicting Institutional Avoidance

	Avoided Surveilling Institutions					Avoided Non-surveilling Institutions				
	Medical Care		Bank Account		School/Work		Volunteer		Religious Group	
	Model 6	Model 7	Model 8	Model 9	Model 10	Model 11	Model 12	Model 13	Model 14	Model 15
Any Criminal Justice Contact	1.309*** (.017)		1.186** (.068)		1.314*** (.101)		.906 (.049)		1.084 (.072)	
Stopped		1.332*** (.096)		.939 (.076)		1.198 (.130)		.920 (.064)		.994 (.086)
Arrested		1.293** (.119)		1.294** (.124)		1.302* (.162)		.932 (.089)		1.141 (.135)
Convicted		1.331** (.128)		1.535*** (.153)		1.304* (.169)		.867 (.089)		1.238 (.167)
Incarcerated		1.102 (.195)		1.509* (.273)		2.181*** (.426)		.732 (.149)		1.410 (.369)
Sociodemographic Controls	Yes†	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Behavioral Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	14,458	14,411	14,515	14,468	14,167	14,120	14,510	14,463	14,400	14,354
Pseudo R-squared	.071	.071	.207	.209	.089	.090	.095	.095	.283	.284

Note: All coefficients expressed as odds ratios. Standard errors are in parentheses. Sociodemographic controls include sex, race, age, education, parental education, marital status, nativity, household configuration (i.e., number in household and whether individuals live with parents), military service, and whether respondents are in school or have a job. Behavioral controls include whether individuals self-report stealing over or under \$50, damaging property, carrying a gun or knife to school or work, stabbing someone, using cocaine or methamphetamine, selling drugs, or being in a gang, and whether respondents are classified as impulsive or candid.

†Includes controls for general health and possession of medical insurance.
‡Includes controls for religiosity and regular church attendance.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

obtaining medical care when needed; those who have been arrested had 29 percent higher odds, and those who have been convicted had 33 percent higher odds of not obtaining needed medical care. These results provide appreciable evidence in support of Hypothesis 1a: contact with the criminal justice system is associated with higher odds of not obtaining medical care when individuals thought they needed it. However, there is no evidence to support Hypothesis 3, that the more serious the level of involvement in the criminal justice system, the higher the odds of not interacting with surveilling institutions. This finding suggests lower levels of criminal justice involvement may be as consequential for institutional involvement as more serious contact.

To describe the relationship in terms of predicted probabilities, roughly 22 percent of respondents who never had criminal justice contact did not obtain medical care when they needed it, compared to 30 percent of those who did have contact (holding all other variables in the model at their means). The confidence intervals for these predicted probabilities are non-overlapping, suggesting they are indeed statistically different from one another.

Models 8 and 9 estimate individuals' odds of not having a bank account. Model 8 suggests that criminal justice contact is associated with 19 percent higher odds of not having a bank account. When levels of criminal justice contact are disaggregated in Model 9, being arrested, convicted, or incarcerated stand out as significant predictors—they increase an individual's odds of not having a bank account by 29, 54, and 51 percent, respectively, providing evidence in support of Hypothesis 1b. Among respondents with no criminal justice contact, the predicted probability of not having a bank account is 26 percent, compared to 34 percent for respondents who had contact with the criminal justice system (holding all other variables in the model at their means). Again, the confidence intervals for these probabilities do not overlap.

Models 10 and 11 estimate the relationship between criminal justice contact and individuals neither working nor being in school. Respondents who had any contact with the

criminal justice system had 31 percent higher odds of neither working nor being in school compared to those who had no contact. This finding is statistically significant at the $p < .001$ level. Similarly, individuals who had been arrested, convicted, or incarcerated had 30, 30, and 118 percent higher odds of neither working nor being in school than did respondents who did not have contact with the criminal justice system. Holding the rest of the variables in the model at their means, among respondents who had criminal justice contact, the predicted probability of neither working nor being in school is 9 percent, compared to 14 percent among those who have been involved in the criminal justice system. The confidence intervals around the predicted probabilities do not overlap.

Models 6 through 11 provide evidence in support of Hypotheses 1a, 1b, and 1c—contact with the criminal justice system at various levels, net of a host of sociodemographic and behavioral controls, is associated with lower levels of involvement with surveilling institutions, specifically, medical, financial, educational, and labor market institutions. Whereas arrest and conviction are significant predictors of institutional attachment in all of these models, being stopped is not a significant predictor of account ownership or being in school/working, nor is incarceration a significant predictor of not obtaining necessary medical care.¹⁷

Given that it is impossible to control for all potential predictors of criminal justice contact and system avoidance using cross-sectional data, lingering questions of selection remain. In the next set of analyses, Models 12 through 15 test for the association between criminal justice contact and individuals' attachment to non-surveilling institutions, including volunteer and religious groups. If selection into the criminal justice system also influences institutional attachment, we would expect to find reduced attachment across the board. Yet, Models 12 through 15 suggest criminal justice contact does not reduce the odds that individuals will interact with non-surveilling institutions. For example, in Model 12, the binary indicator for criminal justice contact is not a significant predictor of volunteerism;

similarly, in the disaggregated model, no level of criminal justice contact is a significant predictor of participating in volunteer activities. Moreover, Models 14 and 15 illustrate no statistically significant association between criminal justice contact at any level and individuals' odds of not participating in religious group activities, net of sociodemographic and behavioral controls and model-specific controls for religiosity and church attendance. Models 12 through 15, therefore, comport with Hypotheses 2a and 2b: there is no evidence to suggest interacting with the criminal justice system reduces involvement with non-surveilling institutions. Overall, these cross-sectional models provide empirical evidence for the negative association between criminal justice contact and involvement in surveilling institutions. The following analyses employ propensity score and fixed-effects methods to delve more deeply into testing hypothesized mechanisms.

Propensity score matching models. Table 3 presents results of the propensity score matching and doubly robust estimations. When a matched sample is employed to estimate the net effect of criminal justice contact on institutional involvement (i.e., the average treatment effect on the treated, or ATT), criminal justice contact appears to significantly reduce attachment to surveilling institutions, such as medical care, banks, and school/work, but not to non-surveilling institutions, such as volunteer associations and religious groups.

Furthermore, results of the doubly robust estimations—in which propensity scores serve as analytic weights in the logistic regression models including the full set of covariates—provide additional evidence that criminal justice contact is associated with higher odds of not participating in surveilling institutions, but it is not a significant predictor of involvement in non-surveilling institutions.

Fixed-effects regression models. Whereas the previous models provide analytic leverage by accounting for a particularly rich number of observable measures of

selection available in Add Health data, the following individual fixed-effects regression models are employed as an additional strategy to address potential selection bias. In estimating the following models, individual fixed-effects models net out time-invariant individual-level characteristics that could be associated with both changes in criminal justice contact and changes in institutional attachment across time. In Wave 4 of Add Health, individuals were not asked if they had been stopped by the police. Therefore, I recoded criminal justice contact in the fixed-effects analyses to include only arrest, conviction, and incarceration. Wave fixed effects are included in all models to net out the average difference between the two time periods. All outcomes are estimated using Add Health data, except the bank account outcome, which is not included in Wave 4 of the survey. I estimated bank account models using data from the NLSY97. To ensure categorical comparability across datasets, analyses using NLSY97 data are limited to any criminal justice contact, arrested, and incarcerated categories.

Table 4 presents results from 10 discrete individual-level fixed-effects logistic regression models. Model 16 suggests individuals who transition from no contact to contact with the criminal justice system between Waves 3 and 4 have 48 percent higher odds of not obtaining medical care when needed. Similarly, when levels of criminal justice contact are disaggregated in Model 17, the statistically significant coefficients suggest an increase in criminal justice contact at all levels is associated with an increase in the odds of not obtaining needed medical care in Wave 4.

Models 18 and 19 estimate the relationship between changes in criminal justice contact and changes in bank account ownership—individuals who change from no contact to contact have 90 percent higher odds of changing from having a bank account to not having a bank account, and individuals who shift from no contact to arrested (without conviction or incarceration) demonstrate 83 percent higher odds of no longer having a bank account.

Table 3. Effect of Criminal Justice Treatment on Matched Samples

Avoidance	Propensity Score Matching					Doubly Robust Estimation				
	Treated	Controls	Difference	SE	T-stat	Significance	OR	SE	N	Pseudo R ²
Surveilling Institutions										
Medical care	.321	.282	.039	.019	2.070	<i>p</i> < .05	1.186*	.096	3,148	.057
Bank account	.410	.301	.109	.019	5.620	<i>p</i> < .001	1.704***	.146	3,160	.191
School/work	.157	.126	.031	.014	2.170	<i>p</i> < .05	1.321*	.144	3,088	.096
Non-surveilling Institutions										
Volunteer	.748	.753	-.005	.018	-.280	n.s.	.951	.083	3,162	.087
Religious groups	.837	.818	.018	.016	1.160	n.s.	1.176	.129	3,134	.249

Note: Models include same suite of sociodemographic and behavioral controls as in Models 6 through 15. Sociodemographic controls include sex, race, age, education, parental education, marital status, nativity, household configuration, military service, and whether respondents are in school or have a job. Behavioral controls include whether individuals self-report stealing over or under 50 dollars, damaging property, carrying a gun or knife to school or work, stabbing someone, using coke or meth, selling drugs, or being in a gang, and whether respondents are classified as impulsive or candid. In light of cross-sectional results, criminal justice treatment is defined as arrested, convicted, or incarcerated, although results remain substantially unchanged when stopped is included, with one exception—bank account is only marginally significant at the *p* < .1 level.

p* < .05; *p* < .01; ****p* < .001 (two-tailed tests).

Table 4. Individual-Level Fixed-Effects Logistic Regressions Predicting Institutional Avoidance

	Avoided Surveilling Institutions						Avoided Non-surveilling Institutions					
	Medical Care ^a		Bank Acct.		Work		Volunteer		Religious Group ^b			
	Model 16	Model 17	Model 18	Model 19	Model 20	Model 21	Model 22	Model 23	Model 24	Model 25		
Any Criminal Justice Contact	1.478*** (.115)	1.904*** (.417)	1.904*** (.417)	1.411*** (.147)	1.411*** (.147)	1.411*** (.147)	.915 (.178)	.915 (.178)	1.067 (.093)	1.067 (.093)		
Arrested	1.359** (.14)	1.359** (.14)	1.827*** (.406)	1.827*** (.406)	1.287 (.185)	1.287 (.185)	.807 (.216)	.807 (.216)	1.025 (.121)	1.025 (.121)		
Convicted	1.345* (.174)	1.345* (.174)	1.345* (.174)	1.345* (.174)	1.011 (.169)	1.011 (.169)	1.052 (.343)	1.052 (.343)	.841 (.136)	.841 (.136)		
Incarcerated	1.588*** (.160)	1.588*** (.160)	1.702 (.842)	1.702 (.842)	1.750*** (.224)	1.750*** (.224)	.892 (.226)	.892 (.226)	1.247 (.144)	1.247 (.144)		
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
N	7,620	7,574	2,753	2,753	4,422	4,396	16,576	16,426	6,910	6,882		
Pseudo R-squared	.069	.069	.092	.092	.137	.137	.87	.869	.106	.108		

Note: All coefficients expressed as odds ratios. Models 16, 17, and 20 through 25 estimated using Add Health; Models 18 and 19 estimated using NLSY97.

Standard errors are in parentheses.

^aModels include controls for general health and possession of health insurance.

^bModels include controls for religiosity and church attendance.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Models 20 and 21 present fixed-effects estimates of the association between criminal justice contact and having a job. Note that whereas descriptive statistics suggest school and work are overlapping age-graded institutions in Wave 3, they are less likely to overlap in Wave 4 when respondents are in the 24 to 34 age range. A very small, select percentage of respondents are still in school at that age, and their pathways for enrollment (or re-enrollment) in school are difficult to disentangle. Therefore, informed by life course literature on the subject (e.g., Sampson and Laub 2005), I limited the analysis to simply having a job in each wave. Consistent with the cross-sectional analyses, women at home caring for children who do not work at a job for pay are not included in the not-working category. Between-wave change from no contact to contact with the criminal justice system is associated with 41 percent higher odds of no longer having a job, and individuals who changed from lower levels of contact to incarceration had 75 percent higher odds of no longer having a job by Wave 4.¹⁸

By contrast, none of the results from Models 22 through 25—models predicting change in involvement with non-surveilling institutions by change in criminal justice contact—are statistically significant, providing further support for the original hypothesis that criminal justice contact, net of individual-level characteristics, is associated only with avoidance of surveilling institutions.

These fixed-effects models are largely consistent with results of the cross-sectional models. Whereas the propensity score matching results demonstrate that among matched pairs, receiving the treatment of criminal justice contact is associated with reduced involvement with surveilling institutions, results of the fixed-effect regressions demonstrate how change in criminal justice contact changes individuals' attachment to important institutions over time. In interpreting fixed-effects results, note that coefficients for each disaggregated level of criminal justice contact (arrested, convicted, and incarcerated) represent, in part, the marginal effect of that deeper

level of contact over the next lowest level of contact. In other words, the category of individuals who move into the incarceration category between waves includes respondents who had no criminal justice contact in the prior wave as well as those who previously may have been only arrested or convicted. Therefore, the incarceration coefficient only captures, in part, the marginal effect of this deepening criminal justice contact.

These results provide compelling empirical evidence for Hypotheses 1 and 2: there is a strong, robust negative relationship between criminal justice contact and involvement with surveilling institutions, net of sociodemographic and behavioral characteristics. The cross-sectional models demonstrate the strong negative association between contact with the criminal justice system and surveilling institutions such as hospitals, banks, employment, and school. The lack of significant results for volunteer and religious institutions suggests the surveilling nature of institutions may lead to lower levels of institutional involvement. Results did not yield considerable evidence to support Hypothesis 3, that the more serious the level of criminal justice contact, the lower the rates of institutional involvement. This finding suggests the grade of contact may be less important than the contact itself. Propensity score matching, doubly robust estimation, and individual fixed-effects models provide further evidence that the association is not merely an artifact of selection. These analyses suggest system avoidance may be an important mechanism mediating the relationship between criminal justice contact and institutional involvement.

ALTERNATIVE MECHANISMS TO SYSTEM AVOIDANCE

Individuals' pathways into the criminal justice system and the consequences stemming from that involvement are complex social processes. Given the nature of observational data, it is therefore important to carefully consider other possible explanations for the empirical relationship between criminal

justice contact and lack of engagement with surveilling institutions demonstrated in this analysis. The first is selection: individuals' preexisting characteristics may lead them to select into both criminal justice contact and institutional avoidance. The fixed-effects and propensity score models, alongside the battery of controls and theoretical robustness check of non-surveilling institutions, reduce the likelihood this is the case.

A second possibility is that institutions may systematically exclude individuals with criminal justice contact. Harris and colleagues' (2010) research on legal financial obligations and research on unbanked and underbanked populations (e.g., Blank and Barr 2009; Caskey 2005; FDIC 2009) are instructive when considering alternative explanations for the relationship between criminal justice contact and account ownership. Harris and colleagues (2010) suggest nonpayment of monetary sanctions can damage individuals' credit, making it difficult for them to obtain loans. Poor credit, however, cannot explain reduced bank account ownership—it is possible to open a bank account with no credit. The only types of contact with the criminal justice system that can preclude an individual from opening a checking account or other financial instrument are convictions related to fraud. Another possibility is that because employment is the strongest predictor of bank account ownership (Blank and Barr 2009; Caskey 2005; FDIC 2009), and criminal justice contact is associated with lower rates of employment, job status could potentially confound the relationship between criminal justice contact and bank account ownership. However, results are robust to supplementary fixed-effects model specifications in which the analysis is limited to respondents who had a job in both waves.

Adjudicating between exclusion and avoidance mechanisms is particularly complex for the employment outcome. Although there is strong ethnographic research suggesting individuals wary of the police "cultivate unpredictability" by avoiding regular employment (Goffman 2009:340), a robust body of research also suggests a criminal record is a

significant barrier to getting a job (e.g., Pager 2007; Pettit and Western 2004). Therefore, it is helpful to refer to the arrested category of individuals, as they do not have a criminal record. In the cross-sectional model, being arrested is a significant predictor of not being in school or work, and in alternative model specifications—for example, analyzing only employment (not school) as a cross-sectional outcome—arrest continues to be an important and significant predictor. In the fixed-effects model, the arrest coefficient approaches significance ($p < .08$). However, as some arrest records can now be found online (regardless of whether they subsequently result in conviction), it would be ideal to estimate the job outcome among respondents who have only been stopped. Unfortunately, the dataset does not include questions on police stops in Wave 4. Looking to results for the arrested category, however, mitigates the impact prevalent job application practices, such as "check the box" (if you have a criminal or felony conviction) or public criminal record checks, may have on the relationship between criminal justice contact and job status.

Another important consideration is that financial obligations resulting from entanglement in the criminal justice system may create a disincentive to work, because individuals' wages could be garnished (Beckett and Harris 2011). However, this possibility is not irreconcilable with the theoretical framework presented here—nonpayment of monetary sanctions can result in incarceration and therefore provides considerable incentive to avoid employment.

Finally, in terms of exclusion from medical institutions, criminal justice contact does not directly affect an individual's ability to obtain medical care—physicians cannot deny care based on criminal justice involvement, and doctors regularly treat custodial patients. Another possible explanation for the relationship between criminal justice contact and not obtaining medical care might be that changing patterns of drug or alcohol use—which could be associated with criminal justice contact and perceived or actual need for medical care—may be confounding the relationship.

However, results of a number of sensitivity analyses—including limiting the analysis by drug and alcohol use levels and including a wide range of potentially changing covariates in the fixed-effects models—yield substantively similar results.

Of course, this discussion is not theoretically exhaustive. Because involvement in the criminal justice system is the cause and consequence of complex social processes, it is possible that multiple mechanisms may be at work in conjunction with one another. However, these alternative mechanisms are not necessarily competing—they have the same implications for inequality in that they contribute to the disconnection of a growing swath of individuals from important institutions. It is impossible to exhaust all theoretical possibilities, so this analysis aims to interrogate the strongest qualitative research on this topic (e.g., Goffman 2009) to understand whether experiences of avoidance represent a more systematic pattern. Results strongly suggest avoidance is indeed an important, previously ignored part of the story.

DISCUSSION AND CONCLUSION

This investigation seeks to better elucidate the relationship between contact with the criminal justice system and institutional involvement. Motivated by recent ethnographic work detailing how fear of surveillance leads to institutional evasion, I introduced the concept of system avoidance to capture individuals' deliberate avoidance of surveilling institutions that keep formal records. Results of this analysis provide the first quantitative empirical evidence that individuals who have been stopped, arrested, convicted, or incarcerated are less likely to interact with institutions that keep formal records, such as hospitals, banks, employment, and schools, than their counterparts without criminal justice contact. Results also suggest that institutional involvement is not uniformly affected by involvement in the criminal justice system: the relationship does

not hold for non-surveilling institutions, such as volunteer organizations and religious groups. In other words, individuals retract specifically from institutions that keep formal records and thus are more likely to increase risk of re-exposure to the criminal justice system. Empirical and theoretical robustness checks suggest fear of surveillance and subsequent system avoidance, rather than sociodemographic characteristics, behavioral characteristics, or an aversion to institutions in general, shape individuals' behavior and involvement with institutions that are consequential for future outcomes.

This study contributes to the sociological literature on criminal justice, surveillance, and stratification, and the findings have direct implications for public policy. Understanding the consequences of surveillance is increasingly relevant for academics, policy architects, and practitioners alike, as technological advances in data integration, electronic trails, and tracking systems continue, including the proliferation of fusion centers that consolidate data from public and private institutions and make it available to law enforcement agencies.

Moreover, system avoidance and subsequent unequal institutional involvement may have real consequences for inequality. Given that involvement with the criminal justice system is highly stratified by race and class, the negative consequences of system avoidance will be similarly disproportionately distributed, thus exacerbating preexisting inequalities for an expanding group of already disadvantaged individuals. Furthermore, lack of attachment to important institutions such as hospitals, banks, schools, and the labor market leads to marginalization and impedes opportunities for financial security and upward mobility. As Haggerty and Ericson (2000:619) suggest, "efforts to evade the gaze of different systems involves an attendant trade-off." That trade-off is full participation in society.

The negative consequences of avoiding the specific institutions examined in this analysis are myriad. Failing to obtain medical care can be detrimental to future health outcomes, as regular medical care is associated

with earlier detection of health conditions and lower rates of morbidity and mortality (Kaiser Family Foundation 2011; Weissman et al. 1991). Not having a bank account precludes individuals from building credit and securing financing for mobility-enhancing investments and can lead to increased reliance on alternative financial services such as predatory lenders (Blank and Barr 2009; FDIC 2009). Furthermore, life course literature identifies attachment to educational and employment institutions as important in shaping outcomes during the transition to adulthood. “Temporally-embedded” social engagement (Emirbayer and Mische 1998:63; Wikström and Sampson 2006) is important at this critical juncture; lack of attachment to institutions such as schools and banks can lead to capital deficiencies (Caspi et al. 1998). Involvement with the criminal justice system in young adulthood, therefore, can have a powerful effect on life trajectories; paternalistic contact with the state may lead people to avoid institutions that promote prosocial adult activity. Finally, institutional avoidance has yet another unanticipated consequence (Merton 1936)—attempts at social control through surveillance may actually fuel the very behaviors it is trying to suppress. When people go off the books, their attachment to institutions key to desistance from crime, such as formal employment, is undermined (Hirschi and Gottfredson 1993; Laub and Sampson 1993).

Although data integration provides many positive opportunities for service delivery, policymakers need to consider increasing the transparency of when and how personal data is shared across institutions, particularly with regard to criminal justice actors having access to individuals’ data in other institutional settings. Beyond transparency, policymakers should consider establishing certain safe harbors, where individuals’ interactions with an institution will not put them in jeopardy of apprehension. For example, as a matter of policy, the Internal Revenue Service does not share data with Immigration and Customs Enforcement, in an effort to not penalize positive civic behavior such as paying taxes (IRS

2012). Similarly, data-sharing firewalls could ensure a parent taking a child to the hospital or enrolling the child in public medical assistance would not increase the risk of being apprehended. Regulatory efforts in Europe—specifically Germany and the United Kingdom—related to the accumulation and sharing of personal data may provide a useful guide for initiating similar policy conversations in the United States.¹⁹

Finally, this study suggests that lower levels of criminal justice involvement—such as police stops and questioning without arrest—may have unintended stratifying consequences that have not received sufficient attention in the literature. System avoidance is not merely an operationalization of going “on the run”—it involves a wariness of institutional surveillance beyond individuals who have criminal records or outstanding warrants. This article provides empirical evidence that some penalties of involvement with the criminal justice system come into effect with low levels of contact. Consequently, this has policy implications for debates over alternatives to incarceration, which may fail to appreciate the collateral consequences of lower levels of involvement in the criminal justice system such as police stops. There is a need for more research on the effects of police questioning on outcomes beyond recidivism and aggregate crime rates.

Future research should consider system avoidance in the context of other institutions, such as public assistance, which the literature suggests may operate differently than other surveilling institutions. Moreover, existing observational data do not readily permit a direct test of the mechanism of system avoidance. Although recent ethnographic research (Goffman 2009), in concert with the behavioral differences between involvement with surveilling and non-surveilling institutions demonstrated in these data, lend substantial support to the theory of system avoidance, future qualitative and quantitative data collection should explicitly consider strategies for exploring this mechanism. At the same time, subsequent analyses should explore a broader

range of institutional settings, thereby permitting a more systematic examination of the institutional characteristics associated with avoidance by liminal populations.

By suggesting a potential pathway through which individuals involved in the criminal justice system may become disadvantaged and marginalized, the findings of this study are relevant for other fields of inquiry, such as research on undocumented immigrants or other groups likely to engage in institutional evasion. Future research should explore whether system avoidance may lead individuals to rely more heavily on informal financial arrangements and social networks, how the consequences of institutional detachment may extend beyond individuals to families and communities, and the precise ways it may be implicated in the accumulation of disadvantage.

The increasing integration of institutional databases and monitoring practices is transforming the way individuals interact with institutions and has implications that researchers are only beginning to appreciate. As modern technology enables surveillance activities across institutions, scholars and policymakers need to think seriously about the unintended consequences. System avoidance may sever an already marginalized subpopulation from the very institutions pivotal to their integration into broader society, leaving a growing class of individuals further and further behind.

Data

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Notes

1. Existing literature details various reasons why low-income and minority individuals avoid using mainstream financial institutions, one of which is fear of surveillance (Blank and Barr 2009; Caskey 2005).
2. Goffman's (2009) subjects were a relatively select group of individuals involved with the criminal justice system from a young age. For example, nearly half of the young men surveyed in the neighborhood had outstanding warrants.
3. Previous literature (e.g., Gilliom 2001; Gustafson 2011; Soss et al. 2011) suggests that criminal justice contact may also affect welfare institutional involvement, and that surveillance may play a complex role in shaping this relationship. One hypothesis is that criminal justice contact and concerns over surveillance will indeed result in lower rates of receipt. However, a contrasting hypothesis is that criminal justice and welfare systems may be interpenetrated institutions of poverty governance and surveillance, and thus criminal justice contact and welfare receipt may be positively associated (i.e., criminal justice contact would not deter individuals from obtaining public assistance and may even be associated with an increase in participation in government welfare programs). Although I cannot adjudicate between these two hypotheses given limitations of these data, I conducted supplementary analyses testing for an association between criminal justice and welfare institutional attachment (see note 17).
4. For example, individuals who have been incarcerated will have higher odds of not obtaining medical care than individuals who have been convicted but never incarcerated.
5. I also include parental education data from Wave 1 as a control.

6. Bias due to differential sample attrition is less than 1 percent (Chantala, Kalsbeek, and Andraca n.d.). Moreover, Add Health itself can be seen as a surveilling institution, so selective attrition would produce more conservative estimates and downwardly bias results because the study does not include individuals with the most extreme system avoidance.
7. A banking outcome in multiple rounds of the NLSY97 permitted me to conduct longitudinal analysis on that outcome; on all other counts, however, Add Health is a more suitable dataset for the research questions explored in this article and was therefore the primary dataset employed. In particular, the NLSY97 does not include key independent variables, including measures of police stops and questioning without arrest, and key dependent variables, such as not obtaining medical care when needed.
8. In their analysis of political participation, Weaver and Lerman (2010) use a comparable operationalization of criminal justice contact.
9. The question "Are you currently working more than 10 hours per week for pay?" was used to measure employment. I also conducted analyses using the question "Do you currently have a job?" and a combination of the two, and results were not significantly altered.
10. Regression results available from the author.
11. For a discussion on the role of self-control in predicting deviant and criminal behavior, see Gottfredson and Hirschi (1990), Laub and Sampson (1993), and Pratt and Cullen (2000).
12. Each behavioral question is coded using two dummy variables. The first is a binary indicator coded 1 if a respondent answered "yes" and 0 for those who responded "no" or refused to answer. A second binary indicator of "missingness" is included where 1 indicates the respondent refused to answer and 0 indicates the respondent provided an answer. Results were not substantively affected by including missing data in the model instead of employing listwise deletion.
13. As a robustness check, I used the kernel matching technique with a Gaussian kernel and a bandwidth of .06 to estimate the average treatment effect. This technique yielded results similar to the nearest neighbor matching (results available from the author upon request). The difference between the treated and untreated cases for each outcome was in the predicted direction and, in fact, the *t*-statistic on the difference between groups is greater for each outcome using the kernel matching technique rather than the nearest neighbor. Therefore, I present results from the nearest neighbor matching, because it is the more conservative test in this instance.
14. Tables displaying covariate balance are available from the author upon request.
15. Very few cases need to be dropped to meet this requirement—in total, I excluded seven cases from the medical care PSM analysis, three from the bank account analysis, five from school/work, three from volunteer activities, and four from religious groups.
16. Note that other national surveys typically find larger racial differences in criminal justice contact.
17. In supplementary analyses of public assistance, logistic regressions predicting participation in SNAP (food stamps) by criminal justice contact yielded null results, as did the propensity score matching and doubly robust estimations (results available from the author). In an alternative model specification, when nonmedical public assistance was used as the outcome (e.g., TANF), criminal justice contact was similarly not a significant predictor, suggesting the relationship between criminal justice contact and welfare may operate differently than other surveilling institutions (see Discussion and Conclusion for elaboration). All public assistance models included the same suite of sociodemographic and behavioral controls as models for the other surveilling institutions.
18. Supplementary analyses estimating change from school or work to work between waves yielded substantively similar results.
19. For example, see the population census decision of the German Federal Constitutional Court's 1983 decision on "informational self determination."

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