THE LABELING OF CONVICTED FELONS AND ITS CONSEQUENCES FOR RECIDIVISM*

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Florida law allows judges to withhold adjudication of guilt for individuals who have been found guilty of a felony and are being sentenced to probation. Such individuals lose no civil rights and may lawfully assert they had not been convicted of a felony. Labeling theory would predict that the receipt of a felony label could increase the likelihood of recidivism. Reconviction data for 95,919 men and women who were either adjudicated or had adjudication withheld show that those formally labeled are significantly more likely to recidivate in 2 years than those who are not. Labeling effects are stronger for women, whites, and those who reach the age of 30 years without a prior conviction. Second-level indicators of county characteristics (e.g., crime rates or concentrated disadvantage) have no significant effect on the adjudication/recidivism relationship.

Traditional labeling theory explains the potential "escalating" consequences of a criminal or delinquent labeling experience in two ways (Lofland, 1969; Sherman et al., 1992). The first consequence involves a

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transformation of identity,[1] and the second emphasizes structural impediments to conventional life that result from a labeling event.[2] Although labeling events have been variably operationalized to include police contact, arrest, conviction, and imprisonment, it is arguable that felony conviction is the most consequential in relation to the development of structural impediments. The label of "convicted felon" strips an individual of the right to vote, serve on juries, own firearms, or hold public office. In many states, convicted felons are prohibited from obtaining student loans, employment in state-licensed occupations, or employment with state-licensed companies. In addition, the label of convicted felon may contribute to various informal exclusions that can make access to noncriminal activities more difficult and criminal alternatives more attractive.[3]

The state of Florida has a law that allows individuals who have been found guilty of a felony, either by a judge, jury, or plea, to literally avoid the label of convicted felon. Judges have the option of "withholding adjudication" of guilt for convicted felons who are being sentenced to probation. The consequence of this unique labeling event is that offenders who are equivalent in terms of factual guilt can either be labeled a convicted felon or not. For those offenders who have adjudication withheld—half of the felony probationers in Florida in recent years—no civil rights are lost and such individuals may legitimately say on employment applications and elsewhere that a felony conviction did not occur. For those offenders who *are* formally adjudicated, all of the structural impediments of being a convicted felon are possible.

Labeling theory would hypothesize that being formally adjudicated should increase the chances of recidivism. The research question that we address here is whether the labeling event of adjudication or the withholding of adjudication has any effect on subsequent recidivism.[4] Drawing on

1. Early research involving self-esteem or "self-typing" was reported by Ageton and Elliott (1974), Gibbs (1974), Harris (1976), Jensen (1972), and Tittle (1972), among others. More recently, "reflected appraisals of self" (Heimer and Matsueda, 1996; Matsueda, 1992) have been implicated in identity-related labeling research.
2. For an excellent discussion of structural impediments related to labeling and supportive research, see Sampson and Laub (1997: 147–52).
3. Discussions of the collateral consequences of felony conviction have primarily focused on the loss of voting rights (Hull, 2006; Manza and Uggen, 2006; Uggen and Manza, 2002). But a broader range of consequences from imprisonment, which presumes conviction, has also been described (Mauer and Chesney-Lind, 2002; Western, 2002; Western and Pettit, 2005).
4. It is also possible, as specific deterrence theory would hypothesize, that being formally adjudicated could have the opposite effect of reducing the chances of recidivism. For discussions of specific deterrence, see, for example, Paternoster (1987), Nagin (1998), and Stafford and Warr (1993). At the same time, a positive relationship between formal sanctioning and subsequent criminal activity could

recent theory and research, we also examine whether any labeling effects on recidivism are contingent on characteristics of the defendant, such as sex, race, or prior record, as well as on characteristics of the county to which the sentenced defendant returns, such as rates of crime or levels of social disadvantage. To assess these questions, we examine the post-sentencing recidivism experience of 95,919 men and women who were found guilty of a violent, property, or drug felony and sentenced to probation in Florida between 2000 and 2002. This study is the first to examine the potential labeling effects of adult felony conviction.

Hierarchical linear modeling (HLM) is used to assess the direct effect of having adjudication applied or withheld, as well as to assess the effects of other individual-level variables. In addition, we assess the direct effect of county-level characteristics on recidivism and the conditioning relevance that these second-level county characteristics may have on the relationship between adjudication status and recidivism.

LABELING EFFECTS: CONCEPTUAL ISSUES

A recurrent theme in the recent renewal of interest in labeling effects (Bernburg and Krohn, 2003; Bernburg, Krohn, and Rivera, 2006; Sampson and Laub, 1997) is the recognition that consequences of formal sanctions may be contingent on the characteristics of offenders. As noted by Paternoster and Iovanni (1989: 381), "we should not expect labeling effects to be invariant across societal subgroups." With this in mind, four individual-level contingencies will inform parts of our analysis. They are race, sex, prior record, and "stakes in conformity." Their conceptual relevance for potentially observed labeling effects is reviewed briefly here.

Race was one of the first issues to be raised in discussions of contingent labeling outcomes. Harris (1976: 433) argued that the effects of official labeling would be least consequential for those individuals most excluded from "ascriptive membership" in dominant groups—particularly minorities. Earlier, Jensen (1972) as well as Ageton and Elliott (1974) hypothesized that higher status persons—presumably including non-minorities in most contexts—may be more susceptible to labeling effects. Similarly, Paternoster and Iovanni suggested that persons who grant less "legitimacy to the legal order and its agents" may, as a result, be "more impervious to labeling effects" than others (1989: 382).

Alternatively, although not referencing race per se, Sampson and Laub (1997: 153) discussed the potential life-course consequences of official sanctions for the disadvantaged urban poor. They noted that "among the

be the consequence of factors other than those attributed to labeling. Among these alternatives could be general strain (Agnew, 2006), defiance (Sherman, 1993), and control–balance (Tittle, 1995).

disadvantaged, things seem to work differently. Deficits and disadvantages pile up faster and this has continuing negative consequences for later development. . . .” Building on that argument, Bernburg and Krohn (2003) hypothesized that labeling effects would be strongest among “disadvantaged offenders” as distinguished by minority and poverty status. Their expectations were further supported by the observation that official labeling of disadvantaged youth could be “enhanced by the negative stereotypes that are already associated with these youths in mainstream culture” (2003: 1290).

The relevance of sex for labeling outcomes has been substantially overlooked in criminological discourse. Even a book with the promising title of *Labeling Women Deviant* (Schur, 1983) makes no mention of whether or how labeling effects for women would be different from those for men. Regarding juveniles, Ageton and Elliott (1974) hypothesized that “males are more likely to be affected negatively by police contact than females,” but no rationale was associated with that expectation. Somewhat later, Ray and Downs (1986), also in the context of adolescents, articulated the opposite expectation. On the premise that “females are expected to be more attentive to interpersonal relationships than men,” they hypothesized that “labels may exert more of an influence on behavior for females than males” (1986: 171).

The possibility that women could be more adversely affected by labeling than men has also been noted in passing references that were concerned primarily with broader issues. For example, in his discussion of “gender blind criminology,” Messerschmidt (1993) conjectured how females might be factored into traditional criminological theories if one took gender into account. In this context, he speculated that because men exercise greater power in society than women, “men should therefore have increased opportunity to counteract official labeling . . .” (1993: 4). Similarly, Giordano, Cernkovich, and Lowery (2004) were developing alternative expectations concerning the relative prospects of women as compared with men for desistance from crime, without specific reference to formal labeling. In that context, they observed that “the greater social stigma attached to female in contrast to male involvement in antisocial behavior” could in the long run “be more limiting to life chances/opportunities for a return to conventional roles” (2004: 189).

Interestingly, the antithesis to labeling, deterrence, which anticipates that official sanctions will reduce rather than amplify criminal involvement, has generally argued that women are more likely to be deterred than men because they have more to lose if caught committing a crime. The argument has been made that women have been socialized to feel more responsible for social relationships than men (Blackwell and Eschholz, 2002) and to have a “greater investment in conformity” than is

true for men (Richards and Tittle, 1981), both of which could presumably be lost as a consequence of a formal criminal sanction. If sanctions are expected to have stronger deterrent effects for women than for men, then logically, labeling effects should be weaker for women than they are for men. However, these arguments have been made in the context of general deterrence and not specific deterrence, which would be more appropriate here.

Expectations concerning the relevance of prior record for potential labeling effects have not been extensively developed, but a limited consensus favors the prospect that the consequences of formal sanctions will be greater for naïve, as opposed to repeat offenders. Noting that some analysts have speculated that novice criminals would be more susceptible to deterrence, Horwitz and Wasserman (1979) offered "an alternative hypothesis." They suggested that punishments of "increasing severity will lead to a greater increase in the rate of subsequent criminal offenses for primary than for secondary deviants" (1979: 57). Smith and Gartin (1989) articulated a similar hypothesis, although like Horwitz and Wasserman, they provided no supporting rationale. What that rationale could be is suggested by Paternoster and Iovanni (1989: 386) who observed that once an individual has been labeled, "it is doubtful that further increments in labeling will continue to produce further deviance." They attributed this occurrence to a "leveling off" of labeling effects on the premise that once an individual has experienced social exclusions on the basis of a label, "it is not unreasonable to assume that further exclusion would have little additional meaning . . ." (1989: 386).

Regarding "stakes in conformity," Sherman et al. have shown that the issue has actually been used to hypothesize contradictory labeling effects. What they characterize as the "greater vulnerability" version of labeling theory expects that those individuals who "care more about the opinions of conventional society" will be more vulnerable to the escalating effects of labeling (1992: 682). From this point of view, nonminorities and higher status individuals would presumably be more vulnerable to labeling effects because they have more to lose. But it has been argued that stakes in conformity could have the opposite consequence. Specifically, persons with a higher stake in conformity, such as those who are employed and/or married, could be more insulated from the negative consequences of official labels. Sherman et al. (1992) refer to this position as the "less vulnerability" approach to labeling effects. In this view, those individuals who have more to lose because of their informal social bonds may as a consequence have "other social resources that overcome the impact of labeling" (1992: 682).

This "less vulnerability" position is implicit in the observation by Sampson and Laub (1997: 152) that "sanctioning tends to aggravate crime

among populations with low 'stakes in conformity'." They also note, with Braithwaite (1989) and Sherman (1993), that criminal sanctions may actually promote future defiance of the law when applied to those individuals with weaker social bonds to conventional society (Sampson and Laub, 1997: 153). Such a position is consistent with recent ethnographic (Anderson, 1999) and autobiographic (Shakur, 1993) accounts of the effects of official sanctions on individuals from highly disadvantaged inner-city environments who presumably have fewer stakes in conformity than others.

COMMUNITY-LEVEL CONTEXTS

However much individuals with particular characteristics may vary in their vulnerability to labeling effects, Paternoster and Iovanni (1989: 373) remind us that "the effect of status characteristics on labeling outcomes is not invariant, but varies substantially across different social contexts." In a related manner, Sampson and Laub (1997: 153) noted that in some social environments, "deficits and disadvantages pile up faster" and as a result "one cannot ignore the effects of larger social contexts" in studying the life-course development of individuals who may have experienced official labeling and some of its negative consequences. Kubrin and Stewart (2006: 172) have similarly argued that "where ex-offenders live greatly affects their ability to reintegrate into society" after being formally sanctioned.

For these reasons, we have chosen to examine several social contexts aggregated at the county level that may interact with individual traits in producing greater or lesser labeling effects. To our knowledge, this study is the first study of labeling outcomes to do so. Specifically, we include in our analyses measures of concentrated disadvantage (CD), concentrated extremes (affluence and poverty), index crime rates, and police presence. The conceptual relevance of each measure for potentially influencing labeling outcomes is briefly reviewed here.

As noted by Sampson, Raudenbush, and Earls (1997), among others, CD is a measure of "underclass concentration," which involves high levels of minority presence, poverty, welfare, and female-headed families. Such places not only have frequently increased levels of crime (Rountree, Land, and Miethe, 1994; Sampson, Raudenbush, and Earls, 1997; Silver, 2000), but they also have low levels of neighborhood resources and social services (Sampson, Morenoff, and Gannon-Rowley, 2002; Wilson, 1987). In addition, as Sampson and Laub (1993: 295) have noted, "counties characterized by. . .a large concentration of the 'underclass'. . .are more likely than other counties to be perceived as containing offensive and threatening populations" and so are more likely to be more heavily policed. Kubrin and Stewart (2006) hypothesized and found higher levels of recidivism among felons released to community supervision in places with higher levels of CD. For these reasons, we might expect that those individuals

who are labeled as convicted felons would see "deficits and disadvantages"—specifically recidivism—"pile up" more often in places that have higher levels of CD.

Whatever the value of taking account of the negative consequences of CD, Sampson, Morenoff, and Gannon-Rowley (2002: 446) have suggested that the common tactic of focusing on concentrated disadvantage may ". . .obscure the potential protective effects of affluent neighborhoods." Morenoff, Sampson, and Raudenbush (2001: 528) have also noted that "the resources that affluent neighborhoods can mobilize are theoretically relevant to understanding the activation of social control . . . ." They found that a measure of concentrated affluence relative to poverty (index of concentrated extremes, ICE) was related to lower levels of homicide. Consistent with that, Kubrin and Stewart (2006) hypothesized and showed that ICE was negatively related to the likelihood of recidivism for ex-offenders in Portland, Oregon. On that basis, we might expect that the negative effects of a felony convict label could be diminished in places with higher levels of ICE.

However, the ICE is really a measure of "relative inequality" (Kubrin and Stewart, 2006: 177) and not of "relative affluence" as operationalized by Sampson, Morenoff, and Earls (1999). It is reasonable to imagine that with extremes of poverty and affluence coexisting in the same county, the "protective effects of affluent neighborhoods" (Sampson, Morenoff, and Gannon-Rowley, 2002) could lead to "the activation of social control" (Morenoff, Sampson, and Raudenbush, 2001) in ways that result in more aggressive policing and prosecution, especially in poorer sections of the county. In that context, one might expect higher levels of ICE to result in higher levels of formal social control and recidivism generally and in an amplification of the effects of being labeled as a convicted felon.

We also expect that convicted felons will be more likely to recidivate in places with higher index crime rates and with a stronger law enforcement presence. The rationale for the former expectation is that places with higher levels of crime, which will also likely have increased levels of CD, have more criminal opportunities, learning environments, and role models (Akers, 1998; Anderson, 1999; Warr, 2001). Individuals who are labeled as convicted felons and who may experience social exclusions not experienced by their nonlabeled counterparts could find it easier to resume criminal activity in such a context, rather than in places with lower levels of crime. This occurrence may explain why, for example, Smith and Paternoster (1990) found that the likelihood of recidivism for those individuals who had been referred to juvenile court in Florida was higher in counties with higher crime rates.

The expectation concerning higher recidivism for convicted felons in places with a stronger police presence is predicated on Lofland's (1969)

concept of "prevalent sensitivity." His argument is that labels—in this case, recidivating felon—are more likely to be "imputed" in places where a higher prevalent sensitivity to misbehavior exists because more "specialists" are deployed to detect and respond to it. In his terms: "as the number of imputational specialists increases . . . it is likely that the number of people imputed as deviant will also increase" (1969: 136). So if individuals labeled as convicted felons resume criminal activity, it seems more likely that they would be caught and officially recidivate where more police officers are present.

As a final conceptual issue, it can be noted that every adult facing a felony conviction, whether or not adjudication is withheld, has been arrested at least once and prosecuted. Assuming that those experiences constitute labeling events, the ideal situation described by Bernburg and Krohn (2003) of comparing samples from the general (unlabeled) population to assess "absolute labeling" effects is not available for the label of convicted felon. However, when it comes to generating the structural impediments that can accrue from labeling, the event of felony conviction, although "relative," may nonetheless be relatively more important than any other for adults. Certainly, police contact, arrest, and prosecution do not cause individuals to lose their civil rights or to be legally excluded from the great variety of occupational opportunities denied to those individuals who are convicted felons. By controlling for prior convictions, both adult and juvenile, an analysis of the consequences of having adjudication withheld or applied for convicted felons affords an estimate of the independent effect of "being labeled or not" in a relative labeling context that is most critical.

LABELING EFFECTS: PRIOR RESEARCH

Several systematic reviews of labeling effects research (Barrick, 2005; Bernburg, 2002; Palamara, Cullen, and Gertsten, 1986; Paternoster and Iovanni, 1989) underscore several things. First, much evidence that can be inferred as relating to labeling is actually sanction effects research, which is often primarily concerned with deterrence. More important, extraordinary methodological variation exists in this research, in terms of both the specification of independent and dependent variables and the elaboration of contingent or subsample analyses. This variation makes a brief summary of any research results or any patterns of support or nonsupport for labeling hypotheses all but impossible. Thus, we limit our discussion of prior research here to that which is most relevant to the analyses that we have undertaken. Specifically, we review studies that have examined "conviction" as the key labeling event, and we review some contingent results

of labeling research that anticipate the kinds of analyses that we describe below.

CONVICTION EFFECTS RESEARCH

We are aware of five studies that isolate conviction as a key labeling event. Two studies address misdemeanor charges of domestic violence (Thistlethwaite, Wooldredge, and Gibbs, 1998; Ventura and Davis, 2005), another deals with drunk driving (Taxman and Piquero, 1998), and two are concerned with the "conviction" of juveniles for serious offenses (Fagan, Kupchik, and Liberman, 2003; Hagan and Palloni, 1990). To our knowledge, no research to date has examined the labeling effect of a felony conviction for adults, which is the focus of the current study.

The two domestic violence studies that examine conviction effects involved midwestern cities and used rearrest within 1 year for another domestic violence charge as a measure of recidivism. Multivariate logistic analyses in both studies showed that conviction significantly *reduced* the likelihood of recidivism, thus supporting a deterrent effect more than a labeling effect (Thistlethwaite, Wooldredge, and Gibbs, 1998: 394; Ventura and Davis, 2005: 270).

Taxman and Piquero (1998) estimated the consequences of more than 3,500 "drunk driving convictions" in Maryland between 1985 and 1993. They measured recidivism by reconviction for drunk driving within 3 years. Their primary concern was to distinguish between the effects of punitive as opposed to rehabilitative responses by the court. But they also made an important distinction—from a labeling perspective—between those who received a "guilty disposition" and those who received "probation before judgement" (PBJ). The latter seems to be similar to the "adjudication withheld" option that we examine for felony convictions, inasmuch as getting PBJ meant that one's conviction was "stayed" and effectively expunged from the public record if probation was successfully completed (Taxman and Piquero, 1998: 133).

The authors report that for all offenders, conviction increased the risk of recidivism by 12 percent but that effect was not significant with other controls included. Separate analyses for first-time offenders showed that a guilty disposition increased the risk of recidivism by 27 percent, and this was the only significant punishment or rehabilitation effect for such offenders (Taxman and Piquero, 1998: 135–7). This finding supports an interpretation that conviction for drunk driving had a labeling effect that was contingent on prior record, which is an issue that we discuss more fully in the next section.

Hagan and Palloni (1990) reanalyzed data from the *Cambridge Study in*

Delinquency Development that were originally collected by West and Farrington (1973). The primary dependent variables were self-reports of delinquency and crime (38 items) that were generated by interviews with randomly chosen London youth. The interviews were conducted over a period of years when the boys were between 8 and 24 years of age. For those who were interviewed, official data from the Criminal Record Office were used to create matched samples of youth who had been "convicted" by the age of 15 years and those who had not (Farrington, 1977: 114).[5]

In looking at self-reported delinquency subsequent to the experience of conviction, Hagan and Palloni (1990) controlled for individual and family attributes that could be taken as indicators of "characterological and cultural" traits that might predict delinquent involvement. They also controlled for prior reported delinquency. A key finding was that being convicted before the age of 15 years significantly increased the likelihood of delinquency at ages 16–17, 18–19, and 21–22 years independent of the effects of all other controls. This result clearly supports the expectation that the labeling experience of conviction contributes to subsequent delinquent and criminal involvement (1990: 278–9).[6]

Fagan, Kupchick, and Liberman (2003) compared the consequences for juveniles of being convicted in an adult criminal court with those of being convicted in a juvenile court for felony assault, robbery, or burglary. To do this, they matched counties from northern New Jersey and from New York, which were all part of the same New York metro SMSA. From the New Jersey counties, they examined the consequences of juvenile court conviction, and from the New York counties, the same was done for criminal court conviction. They tracked rearrest and reincarceration outcomes for at least 2 years after the completion of punishment for those convicted.

An outcome called "any court action," which the authors describe as "analogous to conviction," was found to significantly increase the chances of rearrest for violent, property, and weapons crimes (not drugs) for youth convicted in either the juvenile or the criminal courts. However, it was also found that adolescent offenders convicted in criminal court were rearrested and reincarcerated more often and more quickly for violent, property, and weapons offenses than those convicted in juvenile court. The reverse was true for drug crimes (Fagan, Kupchick, and Liberman, 2003: 69). In sum, the available evidence on whether conviction is related to

5. Farrington (1977: 114) describes the conviction data in this manner: "searches of the Criminal Record Office" uncovered a number of youth who "had been found guilty in court. . . ." The crimes for which these youth had been convicted "were mainly burglaries, thefts and unauthorized takings of motor vehicles."

6. The authors also report a significant interactive effect of parent and son conviction on subsequent delinquency, which was the primary theoretical focus of their study (Hagan and Palloni, 1990: 278–9).

recidivism is limited to juveniles, misdemeanor domestic violence offenders, and first-offending drunk drivers. How felony conviction plays out in the life course of adults remains to be seen.

CONTINGENT LABELING EFFECTS RESEARCH

It should be noted that some studies reporting what we call contingent labeling effects are not labeling studies per se. In fact, several of them are primarily concerned with specific (as distinct from general) deterrence, but if they report a relationship with recidivism subsequent to a sanctioning experience, we designate that as a potential labeling effect.

One consistent contingency seems to be prior record. As noted, Taxman and Piquero's study (1998: 135–7) of drunk driving convictions found substantially stronger labeling effects (increased recidivism) for first-time offenders than for their full sample. Similarly, Fagan, Kupchick, and Liberman (2003: 41) report that the relatively stronger labeling effects (recidivism) for youth convicted in criminal as opposed to juvenile court are especially pronounced for those with no record of prior arrests. Horwitz and Wasserman (1979) found that more severe sanctions were related to higher rates of rearrest for juveniles in Newark, but that effect held only for those identified as first-time offenders on the basis of arrest records. DeJong (1997) analyzed the rearrest likelihood of those either sentenced to jail or not for misdemeanors[7] in New York City. She reported that the experience of incarceration increased recidivism risk but only for first-time arrestees (1997: 571). The pattern of observed labeling effects being concentrated among first offenders is consistent with the suggestion by Paternoster and Iovanni (1989) that incremental labeling events may be less consequential than those that occur earlier in the life course of an individual.

Stakes in conformity have also been studied in relation to sanction effects. In general, what has been found is that apparent labeling effects are stronger for those who have a low stake in conformity or weak ties to conventional society. DeJong (1997) reported that incarceration increased the likelihood of rearrest especially for those with "low stakes" as indicated by having at least two of these traits: unmarried, unemployed, and not a high-school graduate. Sherman et al. (1992: 686) found that formal arrest for domestic violence significantly increased the likelihood of rearrest in Milwaukee among those individuals who had a low stake in conformity as measured by marital status and employment. Two additional

7. Her sample was taken from "criminal court" as opposed to "Supreme Court" cases, which in New York involves primarily misdemeanors or felony cases that are negotiated down. Felonies that are not negotiated down are tried in the Supreme Court.

domestic violence studies focused on those individuals with "high" stakes and concluded that sanctions for these people are associated with less recidivism, which the authors conclude is a stronger deterrent effect (Pate and Hamilton, 1992; Thistlethwaite, Wooldredge, and Gibbs, 1998). The negative relationship between stakes and recidivism for those sanctioned at least implies that those with lower stakes would experience higher recidivism (labeling effect) as reported by Dejong (1997) and Sherman et al. (1992).

A third contingency that has received some attention is race. Several early studies found that labeling was more consequential for white juveniles than for others. Ageton and Elliott (1974) reported the race-specific effect for police contact and self-reported "delinquency orientation" among California youth. Harris (1975) found that New Jersey youth who had been incarcerated for longer periods expressed a higher "relative expected value of criminal choice" but that relationship held only for whites. Harris (1975: 97) hypothesized that the label might mean more to white youth because it came from a system "dominated by white . . . authorities," whereas minority youth might perceive the label as "created and applied by 'outsiders.'" In contrast, the most recent and sophisticated labeling work with juveniles has shown that juvenile justice intervention increased the chances of subsequent adult criminality only for blacks (Bernburg and Krohn, 2003: 1314).

Two domestic violence intervention studies report conflicting results concerning race in relation to the effects of arrest. Sherman et al. (1992: 680) report that race did not condition the effect of arrest on subsequent domestic violence in Milwaukee. However, Berk et al. (1992: 705) found that for pooled data from four cities, the labeling effect of arrest on subsequent offending was 11 percent higher for blacks than for whites and 47 percent higher for blacks who were unemployed than for employed whites.

Two reviews of the recidivism literature conclude that males are consistently more likely than females to resume criminal activity after formal sanctioning (Baumer, 1997; Gendreau, Little, and Goggin, 1996). Consistent with this generalization, Giordano, Cernkovich, and Lowery (2004) reported longitudinal data from Ohio showing that previously institutionalized women were much less likely to recidivate than their male counterparts. Similarly, Smith and Paternoster (1990) found that male youth who had been formally processed by the juvenile justice system in Florida were significantly more likely to recidivate. However, Taxman and Piquero's (1998) study of drunk driving convictions, Sung's (1993) analysis of sentenced drug offenders, and Thistlethwaite, Wooldredge, and Gibbs's (1998) research on individuals arrested for domestic violence each found that sex was not a significant predictor of recidivism.

On the basis of conceptual considerations that have been raised and prior research that has been reported, there are reasonably consistent expectations that we can bring to our analysis concerning the relevance of prior record and stakes in conformity for potential labeling effects. But the same cannot be said for sex or race. Specifically, both conceptual and empirical support exists for expecting stronger labeling effects for less experienced offenders and for those with lower stakes in conformity. Concerning the sex of offenders, the conceptual arguments favor greater labeling consequences for women than for men, but the empirical evidence suggests otherwise. Finally, expectations concerning the relative effect of labeling by race are mixed on both conceptual and empirical grounds.[8]

RESEARCH METHODOLOGY

In this study, we assess the consequence for recidivism of having adjudication applied or withheld for 71,548 male offenders and 24,371 female offenders found guilty of a felony and sentenced to probation in Florida between 2000 and 2002. To the best of our knowledge, this is the first labeling research to examine the effect of adult felony conviction, and the first to examine whether community-level characteristics, such as rates of crime and CD, influence the relationship between the felony conviction and recidivism.

Our dependent variable, recidivism, is operationalized by whether (yes = 1) a felony probationer is convicted of another felony within 2 years of being sentenced. This variable involves a new offense for which an individual is sentenced to prison, probation, or jail, and it does not include technical violations of the terms of probation. This measure of recidivism only captures those actually found guilty of another offense, and not those who may have been arrested and charged but had charges dropped or were found not guilty. Overall, 19 percent of our sample recidivated within the 2-year follow-up period.[9]

8. It should be noted that several studies have examined recidivism among felony probationers, and with relative consistency, they report that recidivism is more likely for males, minorities, and persons with a more extensive prior record (Benedict and Huff-Corzine, 1997; Kruttschnitt, Uggen, and Shelton, 2000; Petersilia, 1985; Spohn and Holleran, 2002; Whitehead, 1991). However, these were not studies of labeling effects and the contingencies thereof, because they involve only individuals who have been labeled. Our concern is whether these factors, as well as stakes in conformity, have a conditional consequence for observed labeling effects.

9. Arrest as an alternative measure of recidivism was considered, but those data are not available to us. The inclusion of probation revocation was also considered but rejected because the vast majority of those involved technical violations and not the commission of a new crime. Thus, the most complete and homogeneous measure available to us in these data is reconviction.

LEVEL 1 PREDICTORS

Our primary independent variable is whether (yes = 1) a person being sentenced to probation for a felony has adjudication formally applied. This outcome certifies the felony convict label. A total of 40 percent of our felony probationers were convicted in this manner.

Among the other factors used to model recidivism are demographic attributes of the defendant, his or her crime, and prior record. The means and standard deviations of the level 1 variables used are described in table 1, which also shows the bivariate correlation matrix for all variables of interest. At the individual level, the Florida Sentencing Guidelines database was used to identify offenders as either non-Hispanic blacks (yes = 1) or Hispanic of whatever race (yes = 1). The surnames of all offenders who were not identified as Hispanic from the Guidelines data were checked against the U.S. Census list of Hispanic surnames (Word and Perkins, 1996), and any individual whose name matched one of those on the list was coded as an Hispanic defendant. The use of the surname list afforded a more comprehensive identification of Hispanics, which raises their proportion in our sample from 11.5 percent to 14.4 percent. Unfortunately, we cannot know how many females are identified as Hispanic or not by virtue of marriage. Also included as a predictor was the defendant's age in years at sentencing.

The defendant's crime was coded as either property (yes = 1) or drug related (yes = 1), with violent crime as the reference category.[10] This choice was made because violent offenders were the least likely to recidivate[11] and we expected the other types of crime to be more consequential in our recidivism models. The seriousness of the crime is indicated by a score generated by the Florida Sentencing Guidelines. Points ranging from 4 to 116 are assigned to the primary offense, and additional points are assigned for additional offenses and a variety of other factors, including victim injury or the use of a firearm.[12] A related variable, which partially reflects crime seriousness, is length of supervision in months that is specified by the probation sentence.

Prior record is indicated by points that are assigned by the Florida

10. "Other" crimes was such an eclectic category that we have not included it in the analysis. The most common of these crimes were Traffic (53 percent); Weapons, Possession (16 percent); Escape (15 percent); and DUI, No Injury (4 percent).
11. The relative frequency of recidivism was 25 percent for property offenders, 23 percent for drug offenders, and 15 percent for violent offenders.
12. Additional points can be assigned on the basis of legal status violations, community sanction violations, prior serious felony points, violation of the Law Enforcement Protection Act, drug trafficking, grand theft of a motor vehicle, commission of the primary offense by a street gang member, and involvement of domestic violence.

Table 1. Correlations, Means, and Standard Deviations among Study Variables

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1. Reconviction	1																		
2. Adjudicated	.112*	1																	
3. Male	.067*	.082*	1																
4. Hispanic	–.035*	–.044*	.077*	1															
5. Black	.102*	.097*	–.031*	–.309*	1														
6. White	–.073*	–.062*	–.024*	–.407*	–.743*	1													
7. Age	–.103*	.145*	–.043*	–.025*	–.089*	.103*	1												
8. Violent offense	–.083*	–.014*	.035*	.062*	–.032*	–.013*	.048*	1											
9. Property offense	.031*	–.039*	–.099*	.038*	–.041*	.012*	–.099*	–.422*	1										
10. Drug offense	.038*	.051*	.070*	–.091*	.068*	–.001	.059*	–.412*	–.653*	1									
11. Prior supervision violation	.154*	.427*	.039*	–.089*	.142*	–.073*	.137*	–.095*	–.036*	.115*	1								
12. Prior record points (Ln)	.174*	.487*	.116*	–.128*	.135*	–.040*	.149*	–.093*	–.064*	.142*	.486*	1							
13. Crime seriousness (Ln)	–.002	.068*	.111*	.056*	–.011*	–.029*	–.047*	.461*	–.280*	–.104*	–.025*	–.050*	1						
14. Supervision length (Ln)	–.011*	.196*	.021*	–.046*	–.035*	.066*	.006*	.110*	.039*	–.131*	.071*	.077*	.302*	1					
15. Inverse Mills ratio	–.167*	–.580*	–.149*	.067*	–.154*	.101*	–.260*	–.001	.074*	–.074*	–.677*	–.887*	–.160*	–.187*	1				
16. ICE	–.004	–.037*	.001	–.116*	–.053*	.133*	.019*	–.056*	–.037*	.083*	.035*	.035*	–.046*	.018*	.090*	1			
17. Disadvantage	.015*	.004	.016*	.201*	.166*	–.300*	–.014*	.060*	.032*	–.083*	–.006	–.066*	.076*	–.107*	–.024*	–.684*	1		
18. Crime rate	.027*	–.005	.014*	.235*	.111*	–.272*	–.014*	.041*	.041*	–.076*	.010*	–.040*	.057*	–.137*	–.010*	–.323*	.641*	1	
19. Police presence	.004	–.003	.030*	.202*	.067*	–.206*	.022*	.036*	–.007*	–.023*	–.006	–.049*	.059*	–.083*	.003	–.218*	.412*	.595*	1
Mean	0.19	0.40	0.75	0.14	0.36	0.49	30.96	0.21	0.40	0.38	0.22	–2.46	2.99	3.21	1.08	–0.04	0.01	5661	231.5
SD	0.39	0.49	0.44	0.35	0.48	0.50	10.55	0.41	0.49	0.49	0.41	3.99	0.68	0.48	0.56	0.03	0.88	1562	31.69

*$p < .05$.

NOTE: $N = 95{,}919$ within county; $N = 67$ between counties.

Guidelines to offense-specific prior convictions for a felony or misdemeanor as an adult or juvenile by a state, federal, military, or foreign court. As with other research using Guidelines data, this affords an unusually comprehensive measure of an individual's prior criminal record. Offenses that occurred more than 10 years before the current offense are not scored if the individual has been conviction free for that time. Also included among our predictors of recidivism is whether the individual had previously violated the terms of supervision while on probation or community control (yes = 1), which can be regarded as another measure of prior record. To correct for skewness, we used natural log values for crime seriousness, prior record, and supervision length.

It is possible that being adjudicated or not may be the consequence of uncontrolled factors that also predict an outcome of interest such as recidivism. In such a situation, the judge's decision to adjudicate the offender is not independent of the residual terms in the equation for recidivism, which would result in biased estimates. To control for this "treatment effect" (Moffitt, 1999), which is somewhat analogous to "selection bias,"[13] we computed inverse Mills ratios for inclusion in each model estimating recidivism.

The inverse Mills ratios were computed through a two-stage process. First, we estimated a probit model to predict adjudication, including all level 1 independent variables shown in table 1. We also included exclusion restrictions, which are "variables that affect the selection process but not the substantive equation of interest" (Bushway, Johnson, and Slocum, 2007: 152). For this purpose, whether the offender went to trial (1 = yes) is used to predict adjudication but not recidivism, because this variable is arguably relevant for adjudication and not recidivism. In addition, since our data are drawn from 67 counties, we included 66 county dummy variables and only kept those significant county dummy variables for a reduced probit model. The second step calculated the inverse Mills ratio using the predicted value for each offender in the probit equation for adjudication from the first step, $Z\delta$. Following Bushway, Johnson and Slocum

13. Selection bias (Berk, 1983; Heckman, 1979) refers to a situation wherein not all members of a population are represented inasmuch as some have been "selected" in terms of variables that need to be accounted for when predicting subsequent outcomes. This process applies, for example, to people who are sentenced to prison (as opposed to probation) when one is interested in the factors that would predict differential sentence length. In other instances, all members of a population are represented, but some are accorded one "treatment" as opposed to another (Moffitt, 1999). This process applies to a situation such as our own in which all persons found guilty of a felony are either adjudicated or have adjudication withheld. The object of controlling for "treatment effect" is to account for the influence of variables that may predict both the treatment experienced and its presumed consequence—in this case, recidivism.

(2007: 161), the inverse Mills ratio was estimated for each case by dividing the normal density function evaluated at $-Z\delta$ by 1 minus the normal cumulative distribution function estimated at $-Z\delta$.

One primary objective of this research is to determine whether "larger social contexts" (Sampson and Laub, 1997) play a role in shaping the potential labeling effects of felony conviction. The social contexts that we examine are characteristics of the county in which offenders experienced their probation supervision. We would prefer to have a lower level of aggregation, but data limitations relative to the situating of offenders by place preclude that possibility. Our second-level variables include total crime rate per 100,000 residents and a measure of police presence, which is the number of police per 100,000 residents. Both of these measures are for the year 2000 (Florida Department of Law Enforcement, 2005). Our measure of CD uses 2000 Census data for percent black, percent of families receiving public assistance, percentage of persons living below the poverty level, and the percentage of families headed by a single mother. Factor loadings were used to weight the variables included in the CD index. Several other variables failed to load on this factor, including unemployment rate, percent Hispanic, and percent of the population under 18 years of age.

To operationalize the ICE, we follow Kubrin and Stewart (2006) and Massey (2001). For a given county, the ICE index is computed as [(number of affluent families – number of poor families) / total number of families]. In this context, "affluent" is defined as families that are 2 standard deviations above the mean family income and "poor" is defined as families below the officially designated poverty level. We expect each of the second level variables to potentially interact with felony conviction in the specification of labeling effects. The means and standard deviations for these variables, as well as their bivariate correlations with first-level and second-level predictors are described in table 1.

ANALYTIC STRATEGY

HLM represents an improvement over traditional regression techniques when analyzing "nested" or multilevel data. According to Raudenbush and Bryk (2002), multilevel analysis using traditional regression modeling risks violation of two regression assumptions, the independence of error terms and homoscedasticity. Individuals nested within second-level contexts—in our case, counties—will no longer represent independent observations because they may share many of the same characteristics. This analysis results in correlated error terms and biased estimates of standard errors. HLM corrects for this problem "by incorporating into the statistical model a unique random effect for each organizational [county level] equation" (Bryk and Raudenbush, 1992: 84). Second, tests of significance for

second-level variables are accomplished in HLM by adjusting the degrees of freedom to represent the number of second-level units in the analysis. HLM also allows researchers to test the cross-level effects of second-level factors on individual-level relationships. The improved estimation of independent effects and cross-level interactions are of primary interest in the current research.

As discussed by Raudenbush and Bryk (2002), the use of traditional HLM with a dichotomous dependent variable such as recidivism would not be appropriate for two reasons. First, the assumption that error terms are normally distributed is violated with binary outcome data. Also violated is the assumption of linearity in the relationship between the independent and the dependent variables and the assumption that the variance of the error terms be distributed equally across all values of the dependent variable. To overcome these problems, hierarchical generalized linear modeling (HGLM) is used with these data. All level 1 and level 2 variables in the analysis are grand mean centered with the exception of dichotomous measures. Both Raudenbush and Bryk (2002) and Hox (2002) observe that grand mean centering as opposed to group mean centering facilitates the interpretation of coefficients, especially at level 1 and that, without serious theoretical justification for doing otherwise, is the preferred choice. Hox further notes that not using grand mean centering in models with cross-level interactions can lead to "serious interpretation problems" (2002: 56). In all analyses, unit-specific models with robust standard errors are used. Nonsignificant variance components are specified as fixed.

RESEARCH FINDINGS

UNCONDITIONAL MODEL

The unconditional model is estimated primarily to measure the magnitude of variation in recidivism across counties. Our variance component estimate, .037, is significant ($p < .001$), which is a common indicator that additional modeling using second-level predictors is warranted. However, the intraclass correlation of .006 suggests that although between county variation is significant, it is clearly not substantial. Nonetheless, we will proceed with second-level analyses.

LEVEL 1 RESULTS

The second stage of analysis estimates recidivism as a function of individual-level predictors. Results of the level 1 conditional modeling are described in table 2, which shows the effect of individual-level predictors on recidivism and the degree to which those effects vary across counties

(variance component). All individual-level predictors are significantly related to recidivism except for crime seriousness.

In table 2, we see that odds of recidivism for property offenders are 72 percent greater than violent offenders and that the odds that black probationers will fail are 36 percent higher than for whites. Those who had violated the terms of a previous probation have odds of recidivism that are 22 percent greater than for those who had not, and males are 38 percent more likely to fail than females. The odds of recidivism are lower for Hispanics than for non-Hispanic whites and are reduced for those with longer terms of probation supervision. Most importantly, table 2 shows that independent of the effects of all other predictors, having been convicted of a felony increases the odds of recidivism by 17 percent when compared with those who had adjudication withheld.

Table 2. Level 1 Conditional HGLM Model of Recidivism

Independent Variable	Coefficient	SE	Odds Ratio	Variance Component
Intercept	–2.158	.061	.081***	.092***
Adjudicated	.155	.032	1.167***	.016*
Male	.320	.035	1.377***	.028***
Age at sentencing	–.044	.002	.957***	.000*
Hispanic	–.082	.041	.921*	[a]
Black	.308	.034	1.360***	.020**
Property	.543	.037	1.721***	.018*
Drug	.386	.046	1.470***	.042***
Prior supervision violation	.199	.049	1.220***	.033*
Crime seriousness (Ln)	.037	.027	1.037	[a]
Prior record points (Ln)	.087	.022	1.091***	.007*
Supervision length (Ln)	–.218	.031	.804***	.017**
Inverse Mills ratio	–.472	.107	.624***	.156**

*$p < .05$; **$p < .01$; ***$p < .001$.

[a] Nonsignificant variance components have been fixed.

NOTE: $N = 95{,}919$ within county; $N = 67$ between counties.

As noted, both theory and prior research (sometimes contradictory) have raised the possibility that labeling effects may vary by a person's race, sex, prior record, and stakes in conformity. To assess this possibility, we repeat the level 1 modeling described above with interaction terms involving adjudication status and the aforementioned individual attributes. Typically, stakes in conformity is operationalized in terms of things that an individual could potentially lose, such as a marital bond or employment as

the result of criminal behavior. Unfortunately, those particular data (as well as educational attainment) are not available for Florida probationers.

However, a theoretical analog to stakes in conformity is Hirschi's (1969) "commitment to conventional lines of action." It is presumed to work the same way as stakes in conformity, inasmuch as it provides an individual with "something to lose" in the event of delinquent or criminal behavior. It seems reasonable to suggest that individuals who have achieved the age of 30 years and have avoided a criminal or delinquent record (up to the current instance) have established more of a commitment to conventional behavior than those who are younger with a criminal record. As such, they have acquired a stake in conformity that threatens at least the loss of reputation and status as a consequence of criminal behavior. Thus, we measure "commitment" or stakes in conformity in terms of age and prior record. For our purpose, "high stakes" individuals are those who are 30+ years of age without a prior conviction (felony or misdemeanor) as an adult or juvenile.

Table 3 shows the recidivism model with all level 1 estimators and with the addition of four interaction terms involving adjudication and offender characteristics. All level 1 predictors, except Hispanic, and crime seriousness in three of four models, have a significant relationship with recidivism. The effect of adjudication on recidivism is significantly higher for females than for males. Table 3 also shows that the likelihood of recidivism, given adjudication is significantly higher for white offenders than for others. Prior record (yes/no) has no significant interaction with adjudication status in predicting recidivism. With regard to our measure of high stakes in conformity, table 3 shows that probationers who had achieved the age of 30 years without a criminal or delinquent conviction are significantly more likely to recidivate given adjudication than those with a prior record before the age of 30 years.[14]

SECOND-LEVEL RESULTS

In this stage of our analysis, the slope for the relationship between recidivism and adjudication is modeled using county-level contextual variables in conjunction with individual-level predictors. Strong collinearity problems are encountered when the ICE and CD are included in the same

14. In separate models (not shown, results available on request), the effect of adjudication is significant for men as well as for women; for Hispanics, blacks, and whites; for those with and without a prior record; and for those with and without high stakes. This finding suggests a robustness in the consequences of adjudication for recidivism, notwithstanding the significance of interaction terms in table 3 showing the effects of adjudication to be stronger for women, whites, and those with high stakes.

Table 3. Level 1 Conditional HGLM Model with Adjudication and Offender Characteristic Interactions

	Sex		Prior Record		Stakes in Conformity		Race	
Independent Variable	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio[a]**	**Coefficient**	**Odds Ratio**
Intercept	–2.575	.076***	–2.546	.078***	–2.477	.084***	–2.543	.079***
Adjudicated	.285	1.330***	.152	1.164***	.067	1.069	.057	1.059
Male	.408	1.503***	.316	1.372***	.266	1.305***	.317	1.372***
Age at sentencing	–.044	.957***	–.045	.956***	–.059	.943***	–.044	.957***
Hispanic	–.079	.924	–.081	.923	–.029	.972	–.018	.982
Black	.309	1.362***	.316	1.372***	.358	1.430***	.380	1.462***
Property	.546	1.726***	.564	1.758***	.528	1.695***	.545	1.725***
Drug	.387	1.472***	.408	1.504***	.390	1.477***	.390	1.477***
Prior supervision violation	.218	1.244***	.180	1.198**	.361	1.435***	.217	1.242***
Crime seriousness (Ln)	.043	1.044	.036	1.037	.120	1.127***	.040	1.041
Prior record points (Ln)	.095	1.100***	.060	1.061**	.102	1.107**	.093	1.097***
Supervision length (Ln)	–.218	.804***	–.246	.782***	–.260	.771***	–.216	.806***
Inverse Mills ratio	–.450	.638***	–.591	.554***	–.152	.859	–.456	.634***
Male * Adj	–.204	.816***						
No prior pts * Adj			.020	1.020				
High stakes * Adj					.203	1.225**		
White * Adj							.145	1.156***

*$p < .05$; **$p < .01$; ***$p < .001$.

[a]These results are from a reduced sample including only those with high or low stakes in conformity ($N = 41{,}627$).

NOTES: Nonsignificant variance components have been fixed; $N = 95{,}919$ within county; $N = 67$ between counties.

models. Like Kubrin and Stewart (2006), we resolve that issue by developing estimates that keep these variables separate. However, whereas Kubrin and Stewart only examined those level 2 predictors, our estimates also include overall crime rates and police presence. These models originally include all level 1 and level 2 predictors. The latter are introduced sequentially in two different estimates in order to keep ICE and CD separate. The sequence for introduction is as follows: ICE, crime rate, and police presence and, alternatively, CD, crime rate, and police presence. Following the criteria articulated by Raudenbush and Bryk (2002: 268), we first added the level 2 variables sequentially to the intercept model and retained only those that were significant. Then we sequentially added the level 2 variables to the adjudicated equation and again only retained significant variables.[15]

Table 4a. Combined Level 1 and Level 2 HGLM Model of Recidivism (ICE)

Independent Variable	Coefficient	SE	Odds Ratio
Intercept	–2.546	.061	.078***
ICE	1.298	.517	3.662*
Adjudicated	.154	.032	1.167***
Male	.314	.035	1.369***
Age at sentencing	–.045	.002	.956***
Hispanic	–.083	.041	.921*
Black	.312	.034	1.366***
Property	.552	.036	1.738***
Drug	.390	.046	1.477***
Prior supervision violation	.184	.054	1.202**
Crime seriousness (Ln)	.030	.028	1.031
Prior record points (Ln)	.078	.024	1.082**
Supervision length (Ln)	–.216	.032	.806***
Inverse Mills ratio	–.509	.118	.601***

*$p < .05$; **$p < .01$; ***$p < .001$.

NOTES: Nonsignificant variance components have been fixed; $N = 95{,}919$ within county; $N = 67$ between counties.

15. Raudenbush and Bryk note that "the analyst will usually want to develop a tentative model for the intercept . . . before proceeding to fit models for the random slopes" (2002: 267) and then, "the most direct evidence of whether a level-2 predictor should be included is the magnitude of its estimated effect and related *t*-ratio. Predictors with *t*-ratios near or less than 1 are obvious candidates for exclusion from the model" (2002: 268).

Table 4b. Combined Level 1 and Level 2 HGLM Model of Recidivism (CD)

Independent Variable	Coefficient	SE	Odds Ratio
Intercept	–2.530	.061	.080***
Concentrated Disadvantage	–.060	.026	.942*
Adjudicated	.157	.031	1.170***
Male	.311	.036	1.364***
Age at sentencing	–.044	.002	.957***
Hispanic	–.082	.042	.921*
Black	.314	.035	1.369***
Property	.549	.036	1.731***
Drug	.389	.046	1.475***
Prior supervision violation	.192	.051	1.211**
Crime seriousness (Ln)	.035	.026	1.036
Prior record points (Ln)	.083	.022	1.087**
Supervision length (Ln)	–.218	.031	.804***
Inverse Mills ratio	–.486	.107	.615***

*$p < .05$; **$p < .01$; ***$p < .001$.

NOTES: Nonsignificant variance components have been fixed; $N = 95{,}919$ within county; $N = 67$ between counties.

Tables 4a and b show the results of estimating recidivism using all level 1 predictors, including adjudication status and any significant level 2 predictors. We are concerned with the direct effects of county-level contexts on recidivism (intercept model) and more importantly, with any possible interactions of level 2 variables and adjudication. Crime rates and police presence have no significant relationships either directly with recidivism or interactively with adjudication status. Neither the ICE nor CD interacts significantly with adjudication in the prediction of recidivism. However, concentrated extremes of affluence and poverty are associated with higher levels of recidivism across Florida counties, whereas CD is associated with slightly lower levels of recidivism.

The lack of interactive effects involving adjudication and level 2 predictors suggests that being convicted as a felon increases the likelihood of recidivism regardless of the characteristics of counties in which probationers are supervised. As with the individual-level traits discussed above (table 3), this further illustrates the apparent robustness of the effects of a felony convict label inasmuch as those effects are not conditioned either by individual or "larger social contexts." In addition, although not a focus of this research, our data for Florida counties show opposite direct effects of ICE and CD on recidivism than what Kubrin and Stewart (2006) report

for Oregon census tracts.[16]

DISCUSSION

As noted, this study is the first one to examine the effects of being labeled a convicted felon on the recidivism of adults. The option of withholding adjudication of guilt and the label of convicted felon for those who are legally guilty provides a unique opportunity to explore the consequences of labeling within the criminal justice system. The decision to withhold adjudication involves a situation wherein legally equivalent adults—in terms of guilt—are either labeled as a felon or not. In terms of social and legal significance, a more consequential label is not available in the criminal court system. Our principal findings can be briefly summarized.

1. Being adjudicated guilty as a felon significantly and substantially increases the likelihood of recidivism in comparison with those who have adjudication withheld.

2. The effect of being adjudicated guilty on recidivism is stronger for whites than for blacks and Hispanics and for females as opposed to males. It is also stronger for those who reach the age of 30 years without any prior convictions compared with those with priors before turning 30 years old.

3. The community-level contexts that we examined have no consequence for the relationship between adjudication of guilt and recidivism.

At a practical level, these results underscore the potential benefits that

16. There are several methodological differences between the Kubrin and Stewart (2006) study and our own. Their study was performed in Portland, Oregon and ours in Florida. The proportion of non-Hispanic whites in Multnomah County (Portland) was substantially higher (79.4 percent) in the 2000 Census than in Florida (66.5 percent). Their sample, which included misdemeanants as well as felons, was 68 percent white, 25 percent black, 4 percent Hispanic, and 3 percent other. Our sample is 49 percent white, 36 percent black, and 14 percent Hispanic (others excluded). Their measure of recidivism is arrest, whereas ours is reconviction for a felony. More important, perhaps, their data for ICE and CD were aggregated at the census tract level and ours at the county level. At the lower level of aggregation, it makes sense to expect the relative presence of affluence to provide for both social capital and collective efficacy in ways that might reduce recidivism overall. But at the county level, the affluent and poor are likely not living near one another and the potential benefits of having affluence nearby may not work in the same way. In fact, the social and physical distance that may characterize the affluent and poor in a county may work in ways that afford the affluent the opportunity and means to more vigorously police poor neighborhoods and prosecute those who may be apprehended. This would not only mitigate the recidivism retarding effects of affluence noted by Kubrin and Stewart, but it may also aggravate the prospects of criminal activity resulting in a formal charge and conviction—our measure of recidivism.

can accrue to individuals and to society—in this case, from less criminal activity—when a policy of minimizing harm (Clear, 1995; Rubin, 1999) is implemented in the administration of justice. The withholding of adjudication for those who have been found guilty of a felony is a policy that these data suggest is directly related to lower levels of recidivism than are found among those who are formally labeled as a convicted felon. In this case, minimizing harm at the individual level has consequences for reducing harm in the broader community.

At a theoretical level, the pattern of findings suggests that the consequences of labeling are greater for those who would otherwise be considered less likely to recidivate. As noted, research has generally shown that recidivism is more likely for males, minorities, and persons with a more extensive prior record (Benedict and Huff-Corzine, 1997; Kruttschnitt, Uggen, and Shelton, 2000; Spohn and Holleran, 2002; Whitehead, 1991). These data suggest that those who are generally more likely to recidivate are less disadvantaged by a formal label than are women, whites, and those without an early prior record. It could also be argued that women, whites, and those without an early prior record have more to lose from criminal stigma because crime is more often associated with males, minorities, and those with extensive criminal experience.

It also happens that the pattern of results reported here is consistent with some of the theoretical expectations raised earlier in this article. Specifically, the diminished relevance of the felony convict label for black and Hispanic offenders was anticipated, even though prior research is both limited and inconsistent in this regard. For example, Harris (1976: 433) hypothesized that the effects of official labeling would be least consequential for those most excluded from "ascriptive membership" in dominant groups, whereas Paternoster and Iovanni (1989: 382) hypothesized that those who "grant less legitimacy to the legal order and its agents"—presumably including racial and ethnic minorities—could be "more impervious to labeling effects." Assuming that, on average, whites are less disadvantaged than blacks, our results run counter to the expectation raised by Sampson and Laub's (1997: 153) that deficits would "pile up faster" for those who are already disadvantaged. These results also diverge from those of Bernburg and Krohn (2003) who found that labeling effects were stronger for African-American youth than for others. It is possible that the race-specific effects of formal labeling are different for adults than for juveniles, which is an issue that future research should carefully explore.

The fact that a felony convict label more strongly predicts recidivism for females than for males is consistent with the scant theorizing that has been done about labeling effects for women, but it is inconsistent with a limited body of empirical evidence showing that labeling effects are more consequential for males. Messerschmidt (1993: 4) hypothesized that because

men exercise greater power in society than women, "men should . . . have greater opportunity to counteract official labeling." Giordano, Cernkovich, and Lowery (2004: 189) hypothesized that the greater social stigma attached to "antisocial" behavior by females could "be more limiting to life chances/opportunities for a return to conventional roles" and thus more conducive to recidivism. Although our results are consistent with those expectations, the process whereby labeling comes to have more consequence for women than for men cannot be known from these data.

The finding that labeling effects are stronger for those who, in Hirschi's (1969) terms, have established a more substantial "commitment to conventional action" by having reached at least 30 years of age without a prior record is consistent with the "greater vulnerability" labeling hypothesis (Sherman et al., 1992). In that view, it is expected that labeling would be more consequential for those with a higher stake in conformity and who, on that account, presumably "care more about the opinions of conventional society" (1992: 682). Another way to interpret this result is that, as Paternoster and Iovanni (1989) hypothesized, labeling experiences have diminishing consequences for those persons who have been formally labeled earlier in life.

Although not part of our original theoretical framework, it is worth noting that finding less recidivism for those having adjudication withheld is consistent with expectations from a "reintegrative shaming" perspective. As described by Braithwaite (1989: 55), reintegrative shaming combines "expressions of community disapproval" for a behavior with "gestures of reacceptance into the community of law-abiding citizens." One who has been found guilty of a felony and sentenced to probation has clearly received an expression of community disapproval. At the same time, not being labeled a convicted felon (having adjudication withheld) reduces the prospect of formal and informal exclusions from the community of law-abiding citizens that traditionally accompany the label of convicted felon.

In fact, Braithwaite (1989: 101) juxtaposes and contrasts reintegrative shaming with the central labeling concept of "stigmatization," which he describes as "disintegrative shaming in which no effort is made to reconcile the offender with the community." What the decision to impose or withhold adjudication of guilt may amount to is a choice between stigmatization and reintegration—or at least diminished disintegration. The avoidance of the label in this instance not only reduces the potentially stigmatizing effects of labeling, but it may at the same time increase the reintegrative potential of the punitive response for the individual involved.

The lack of an interaction between second-level variables and adjudication status in these analyses could mean one of three things. On the one hand, it could indicate that the effect of adjudication on recidivism is sufficiently robust that it holds in a variety of community contexts. Thus, it is

seemingly unaffected by levels of crime, police presence, CD, and an ICE of affluence and poverty. Alternatively, as the intraclass correlation suggests, there may be insufficient variation in recidivism across counties to matter for these purposes. Finally, it may be the case that community context is inadequately operationalized with data that are aggregated at the county level. Ideally, an assessment of the impact of community characteristics on the relationship between adjudication as a felon and recidivism would make use of neighborhood-level data, aggregated at the census tract or block level. Unfortunately, we cannot locate our probationers more precisely than by the county in which their probation supervision took place.

Another limitation of the current research is the potential for omitted variable biases at both levels of analysis. At the individual level, it would have been helpful if available data included socioeconomic status as well as three additional indicators of stakes in conformity— employment, marital status, and educational attainment. At the second level of aggregation, measures of probation caseload could have relevance for the likelihood of recidivism, as could measures of social capital within different social contexts.

In sum, this research has shown that when adults are adjudicated guilty of a felony, a significant and substantial increase occurs in the likelihood of reconviction within 2 years. When combined with the recent results reported by Bernburg and his colleagues (Bernburg and Krohn, 2003; Bernburg, Krohn, and Rivera, 2006) involving the labeling of juveniles, these data provide additional encouragement for the renewed empirical assessment of labeling theory. Moreover, research moving forward in this area should pay particular attention to whether and how labeling effects vary for different kinds of individuals and for broader social contexts that may be operationalized more finely than was possible in the current research.

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