Relaxing Charging Restrictions in Narcotics Cases

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Abstract

This paper studies the effects of prosecutorial charging decisions in Cook County. Felony narcotics cases cannot ordinarily be reduced to misdemeanors but can when combined with other charges. In such cases, misdemeanors rise by 5.3 to 7.1 percentage points. Defendants shift over both the no charges-misdemeanor margin and the misdemeanor-felony margin. The combined effect of these shifts is a modest fall in recidivism. Relative to others, Black defendants are more likely both to be on the felony-misdemeanor margin and to reduce recidivism. These results approximate a policy that permits misdemeanor narcotics charges without disallowing felony charges. JEL: C21; K14

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1 Introduction

The rigorous study of criminal justice policy requires navigating multi-dimensional choice problems. Trial judges assign defendants to non-conviction, probation, or prison. Bail judges must choose between detainment in jail, supervised release, and unsupervised release. Prosecutors making charging decisions, the focus of this paper, can bring serious felony charges, less serious misdemeanor charges, or no charges at all. Random variation in the judge or prosecutor assigned to each case can provide some insight into these decisions and their consequences, but judge IV methods struggle to handle a decision with more than two options. They require either additional instruments, strong assumptions, or partial identification (Mountjoy 2022, Rivera 2023, Humphries et al. 2023, Kamat et al. 2023).

This paper considers an supplementary inference strategy: variation in the *ability* to assign defendants to a particular state. The criminal justice system runs on complex rules that attorneys and judges must adhere to. The interaction of these rules may sometimes open or close options for particular defendants who are not otherwise special. I consider one such case in the criminal justice system of Chicago, derive an estimator with a clear interpretation in terms of underlying complier groups, and generate new insights about the recidivism effects of moving defendants over both the no charges-misdemeanor margin and the misdemeanor-felony margin.

The policy at the heart of this research is the intentional omission of misdemeanor narcotics charges from a state's criminal code. This forces prosecutors to either charge narcotics defendants with a felony or dismiss the charges entirely. This provision persists today in 26 states including Illinois (Drug Policy Alliance). Adherence to this policy is clear in the data. In court data from Cook County covering 2000-2014, just 0.5% of felony narcotics cases ended in a misdemeanor conviction. Other felony drug cases, primarily cannabis, are not subject to this restriction. They ended in a misdemeanor conviction 13.9% of the time.

Variation in this restriction permits research on two important question. First, how many defendants would be charged with a misdemeanor if doing so was permitted, and from which groups would they be drawn? The latter part of this question is especially important. Of the narcotics defendants currently charged with a felony, some would instead face a misdemeanor if the charging restriction was relaxed. Likewise, a subset of the defendants who currently have their charges dropped would also pivot to misdemeanors. Understanding the size and composition of these groups can teach us which charging margins defendants tend to sit on, and whether this varies by the characteristics of the defendant. It is also directly important to any policymakers contemplating relaxing the restriction. It gives insight about whether that policy would make the criminal justice system as a whole more or less punitive, and whether certain types of defendants are particularly likely to be harmed or helped by this policy.

The second question to consider is how the observed shifts across these margins affect future offending. Copious research recovers effects of incarceration (Green and Winik 2010; Loeffler 2013; Nagin and Snodgrass 2013; Aizer and Doyle 2015; Mueller-Smith 2015; Harding et al. 2017; Bhuller et al. 2020; Rose and Shem-Tov 2021; Jordan et al. 2022), but less is known about the effects of prosecutorial charging decisions. This paper recovers a combination of the recidivism effect of moving from no charges to misdemeanor prosecution (also studied by Agan, Doleac, and Harvey 2022 using random assignment to prosecutors) and the recidivism effect of moving from felony charges to misdemeanor prosecution. I cannot separately identify these two effects with the variation available to me, but evidence from demographic groups with different rates of each type of complier provide suggestive evidence about their different effects.

To answer these questions I first derive the estimator that arises from comparing two otherwise identical populations, one of which is subject to the charging restriction and one of which is not. Using a simple model of the prosecutor's decision in each environment, I break this population into four principal strata: Always Felony defendants who are charged with a felony regardless of the restriction; Always No Charges defendants who are let go regardless of the restriction; Felony Compliers who are charged with a felony in the restricted environment and with a

misdemeanor in the unrestricted environment; and No Charges Compliers who are let go in the restricted environment and charged with a misdemeanor in the unrestricted environment. The first question posed in this paper, the effect of relaxing the charging restriction on actual charges, can be found by calculating the size of these principal strata, which is straightforward given (hypothetical) data that assigns defendants at random between the restricted and unrestricted environment.

To address the second question on how this translates to recidivism behavior, I derive a LATE-like estimator that combines Felony Compliers and No Charges Compliers. Given only one change in the data, the ability of prosecutors to bring misdemeanor charges, I cannot separately identify LATEs for each group of compliers. While this joint LATE does not allow for counterfactuals involving just one complier group, it does directly address an important policy question: the total expected effect of a policy that relaxes the narcotics charging restriction.

This paper also requires some empirical innovation. I do not observe experimental variation in the ability to charge a misdemeanor in narcotics cases. I rely instead on variation in the initial bundle of charges present at the outset of a case. Some defendants are initially charged with felony narcotics violations alone. This leaves no opportunity for a downgrade to misdemeanor. Other defendants face initial charges of both felony narcotics and some other misdemeanor. In these situations, the prosecutor has the power to dismiss the narcotics charges and pursue only the misdemeanor. I find in my data that while misdemeanor outcomes are almost non-existent in cases involving just initial narcotics charges, they are present (though still uncommon) in cases with both felony narcotics charges and misdemeanor charges. I will refer to such initial misdemeanor charges as "auxiliary" charges.

The key comparison in this study is therefore between defendants initially charged with felony narcotics alone and defendants initially charged with both felony narcotics and an auxiliary charge. For example: a defendant who is arrested with cocaine in their right pocket versus a defendant who is arrested with cocaine in their right pocket and cannabis in their left. The former defendant must be either charged with a felony or let go. The latter can also be charged with a misdemeanor.

In order to account for the possibility that defendants facing auxiliary misdemeanor charges are unobservably different from those who are not, I will throughout the paper use a design akin to Difference-in-Differences to study the effect of these auxiliary charges in narcotics cases relative to their effects in non-narcotics cases. This design first restricts attention to either narcotics defendants or non-narcotics defendants. Then it estimates the relationship between auxiliary charges and my outcome of interest: either how a prosecutor charged the case or 4-year felony recidivism. Finally, it compares these apparent effects of auxiliary charges across the samples of narcotics and non-narcotics defendants. This design relies on the assumption that the fundamental relationship between auxiliary charges and my outcomes of interest is the same in both narcotics and non-narcotics cases, meaning that any residual differences are driven by the unique way that auxiliary charges expand the prosecutor's choice set only when the main charges are for narcotics.

Applying this design to 14 years of data from Cook County, I find that charging decisions in narcotics cases respond significantly to the presence of auxiliary charges. Rates of misdemeanor charges rise by 5.3 to 7.1 percentage points when auxiliary charges are present. Conversely, rates of both felony arraignments and dropped cases fall. When prosecutors are given the flexibility to charge a misdemeanor in narcotics cases, they draw marginal defendants from both the group they would have otherwise charged with a felony (Felony Compliers) and from the group they would have otherwise let go with no charges (No Charges Compliers). This means that the theoretical point discussed above has empirical bite: relaxing the charging restriction does not uniformly reduce severity.

When I use the same design to address my second question, I find that felony recidivism falls modestly for particular groups, and I can reject overall increases in recidivism of greater than 0.5 percentage points. Recidivism reductions are largest for defendants with no prior felony convictions (2.3 pp) and defendants facing the least serious class of

felony charges (2.5 pp). The recidivism reductions are primarily in new drug crime. Violent crime is unaffected, and property crime may rise slightly for some subgroups.

Perhaps the starkest dimension of heterogeneity I study is the distinction between Black defendants and non-Black defendants. I find that Felony Compliers are over-represented among Black defendants, and No Charges Compliers are over-represented among non-Black defendants. That is, relaxing the charging restriction would largely shift Black defendants from felonies to misdemeanors and largely shift non-Black defendants from no charges to misdemeanors. Recidivism patterns are similarly split. Black defendants reduce felony recidivism by 1.6 percentage points while non-Black defendants are flat overall and even increase recidivism for misdemeanor and property crime. These findings have important implications for racial equity in the Cook County criminal justice system. My estimates of charging and recidivism responses by race suggest that relaxing charging restrictions could close the covariate-adjusted racial gap in felony arraignments for narcotics defendants by approximately $1/3$ and the racial gap in felony recidivism by approximately $1/5$. Furthermore, while I cannot say definitively that the difference in recidivism patterns is driven by the different rates of Felony and No Charges Compliers in these two groups, my findings are suggestive evidence that more lenient charging reduces recidivism on both the misdemeanor-felony and misdemeanor-no charges margin.

The first main contribution of this paper is a method for considering the consequences of adding an option to a decision maker's choice set. The estimators presented here have a clear interpretation and make no assumptions about the decision maker's utility or rationality, aside from Independence of Irrelevant Alternatives. The interpretation of the estimates foregrounds the discretion of the decision maker, which is important when considering policy implications. The decision maker completely determines the complier sets, so policies that directly induce compliance (e.g. by forbidding felony charges in certain cases) may have different results from those measured here. Stated in my setting, prosecutors get to select which defendants are shifted to a misdemeanor charge when one is available, and selecting defendants according to a different rule may produce different outcomes.

This paper also makes a contribution to the empirical literature on charging decisions and their consequences. It directly addresses policy relevant questions in the ongoing debate about whether and how to "defelonize" parts of the US criminal justice system. This paper also provides suggestive evidence that shifting defendants to more lenient charges, no matter the margin or the outcome of those charges, can reduce recidivism. Furthermore, its insight about how auxiliary charges change the prosecutor's effective choice set, or similar ideas about the charging restrictions imposed by law on prosecutors, can be applied in other contexts to further probe the effects of charging decisions.

The remainder of this paper begins with a literature review in Section 2 that discusses previous studies that have explored the margins between felony charges, misdemeanors, and no charges. Section 3 describes my empirical strategy in detail. Section 4 discusses the relevant aspects of the Cook County criminal justice system. Section 5 describes and summarizes my data. Section 6 presents estimates of the effects of relaxing the charging restriction on what charges prosecutors bring and on recidivism outcomes. Section 7 concludes.

2 Literature Review

This paper relates to previous work that studies the effects of shifting defendants between different forms of prosecution, including a dismissal of charges. Agan, Doleac, and Harvey (2023) find that defendants on the margin between misdemeanor prosecution and dismissal have much lower recidivism rates if released. Hogan (2022) and Agan, Doleac, and Harvey (2022) consider the effects of reformist prosecutors across the U.S. who pledge to dismiss certain categories of charges as a matter of policy. They find an increase in homicide rates and no clear effect, respectively. Zarkin et al. (2012), Mueller-Smith and Schnepel (2021), and Augustine et al. (2022) uncover significant benefits of deferred prosecution programs, which leave a defendant without a criminal record following their cooperation with

supervision or a treatment program. Mueller-Smith, Pyle, and Walker (2023) find social benefits to dismissing more cases predicted to be at the margin of felony prosecution.

This paper also relates to a literature on California's Proposition 47 reform in 2014. This reform redefined many drug and property crimes (including simple drug possession) to misdemeanors. To be precise, many of these charges had formerly been "wobblers," meaning that the prosecutor had discretion to charge either a felony or a misdemeanor. The reform changed them to plain misdemeanors. This is different from the variation considered in this paper, which is essentially the conversion of a plain felony to a "wobbler." Dominguez-Rivera, Lofstrom, and Raphael (2019) study the effect of Prop 47 on overall crime rates, finding little consistent evidence that the reform increased violent crime but some evidence that it increased property crime. Bird, Nguyen, and Grattet (2020) find that the reform reduced rates of recidivism for affected defendants, primarily for further drug crime. Lofstrom, Martin, and Raphael (2020) and Macdonald and Raphael (2020) find that the reform reduced racial disparities in the criminal justice system. Additional "defelonization" across the U.S. is often proposed by criminal justice reformers (Moreno and Subramanian 2014, Durnan and Elderbroom 2018). This paper provides a basis for considering a form of defelonization different from that carried out in California, which restores the prosecutor's ability to charge a misdemeanor without taking away their ability to charge a felony.

This paper complements many of the findings in these literatures in a new setting with a new mechanism. I too find reductions in drug recidivism and in racial disparities when defendants are shifted towards misdemeanor prosecutions (which in Cook County have many of the characteristics of deferred prosecution programs). Defendants in my design are shifted from both felony prosecution and non-prosecution towards misdemeanors. My work therefore supplements the findings in Agan, Doleac, and Harvey (2023), which deal exclusively with defendants on the margin between misdemeanor prosecution and non-prosecution.

This paper also relates to a recent literature dealing with multi-dimensional decisions in the criminal justice system. This is difficult when variation comes from differences in the attitudes of randomly assigned judges or prosecutors. Allowing those attitudes to vary arbitrarily allows defendants to move across multiple margins as assignment changes. Previous work has addressed this with a strong single-index assumption (Rivera 2023), searching for additional instruments (Mountjoy 2022, Humphries et al. 2023), and partial identification (Kamat et al. 2023). This paper uses variation in ability rather than attitudes. This avoids problems of monotonicity across assigned decision makers and shuts down some nuisance margins across which defendants might move. It also focuses on a policy question where the recidivism effect of the total motion of defendants towards misdemeanor charges, rather than the particular effect on each margin, is of direct policy interest. Unfortunately, I do not observe prosecutor identities in my court data, so it is not possible to combine this variation in ability with variation in attitudes, but this could be a fruitful avenue for future work.

The findings in this paper stem not from an official change of policy at the level of a state legislature or county prosecutor, but rather from discretion among individual prosecutors. Research into the criminal justice system has long emphasized the importance of prosecutorial discretion, sometimes finding that it exacerbates racial disparities (Rehavi and Starr 2014, Tuttle 2021, Sloan 2020, Sloan 2022). However, recent research suggests that prosecutors can have a moderating, or at least neutral, influence on the racial disparities they inherent from actors earlier in the system (Harrington and Shaffer 2022, Shaffer 2022).

3 Empirical Strategy

To better understand what can be learned from a change in the prosecutor's choice set, consider a simple model of the charging decision. A prosecutor must, for each case, choose a charge type: either felony, F, misdemeanor, M, or no

charges, N. The unrestricted choice set $\mathcal{C} = \{F, M, N\}$ contains these three charge types. Each case also involves a vector of characteristics of the alleged crime and defendant, X_i. The prosecutor then maximizes for each i:

$$\max_{c \in \mathcal{C}} \quad U(c, X_i)$$

I leave the form of the prosecutor's utility function unspecified, so the solution to this problem is simply to choose the largest element of $\{U(F, X_i), U(M, X_i), U(N, X_i)\}$.

Now consider the prosecutor's restricted choice when misdemeanor charges are not possible:

$$\max_{\tilde{c} \in \mathcal{C} \setminus M} \quad U(\tilde{c}, X_i)$$

The solution to this restricted problem should only differ from the unrestricted problem when $c_i^* = M$.[1] There is therefore a set of just 4 possible pairs $(c_i^*, \tilde{c}_i^*)$:

- $c_i^* = \tilde{c}_i^* = F$: The prosecutor's optimal choice is felony charges, even under the unrestricted choice set. Call this set of defendants Always Felonies.
- $c_i^* = \tilde{c}_i^* = N$: The prosecutor's optimal choice is no charges, even under the unrestricted choice set. Call this set of defendants Always No Charges.
- $c_i^* = M$ and $\tilde{c}_i^* = F$: The prosecutor will charge a felony under the restricted choice set, but if the restriction is relaxed, the prosecutor prefers to charge a misdemeanor instead. Call this set of defendants Felony Compliers.
- $c_i^* = M$ and $\tilde{c}_i^* = N$: The prosecutor will drop charges under the restricted choice set, but if the restriction is relaxed, the prosecutor prefers to charge a misdemeanor instead. Call this set of defendants No Charges Compliers.

Therefore, the number of defendants who would be made better off by an expansion of the prosecutor's choice set is given by the count of Felony Compliers. The number of defendants who would be made worse off by that expansion is the count of No Charges Compliers. Given random variation in which cases are subject to the charging restriction, the size of these groups can be estimated by comparing the rates of felony and no charges across cases with and without the restriction:

$$Pr\{c_i^* = M,\ \tilde{c}_i^* = F\} = Pr\{\tilde{c}_i^* = F\} - Pr\{c_i^* = F\}$$

$$Pr\{c_i^* = M,\ \tilde{c}_i^* = N\} = Pr\{\tilde{c}_i^* = N\} - Pr\{c_i^* = N\}$$

Estimating these objects addresses the first key question: how the charges faced by defendants would be changed by a relaxation from the restricted choice set to the unrestricted choice set.

To consider the second key question posed above, the anticipated effect on recidivism, I add an outcome to the model, $Y(c_i, X_i) = Y_i(c_i)$. This outcome depends on the charges brought according to a potential outcomes framework:

$$Y_i(c_i) = 1\{c_i = F\} Y_i^F + 1\{c_i = M\} Y_i^M + 1\{c_i = N\} Y_i^N$$

Relying momentarily again on the fiction of a random division of cases between the restricted and unrestricted choice sets, it is possible to evaluate the average counterfactual change in this outcome induced by relaxing the restricted choice set into the unrestricted choice set:

[1] This is not necessarily true if Independence of Irrelevant Alternatives does not hold for the prosecutor. My specification of the utility function implicitly makes this assumption.

$$
\begin{aligned}
&E\left[Y_i\left(c_i\right)|c_i = c_i^*\right] - E\left[Y_i\left(c_i\right)|c_i = \tilde{c}_i^*\right] = \\
&E\left[Y_i^F - Y_i^F | c_i^* = F, \tilde{c}_i^* = F\right] Pr\left\{c_i^* = F, \tilde{c}_i^* = F\right\} + \\
&E\left[Y_i^N - Y_i^N | c_i^* = N, \tilde{c}_i^* = N\right] Pr\left\{c_i^* = N, \tilde{c}_i^* = N\right\} + \\
&E\left[Y_i^M - Y_i^F | c_i^* = M, \tilde{c}_i^* = F\right] Pr\left\{c_i^* = M, \tilde{c}_i^* = F\right\} + \\
&E\left[Y_i^M - Y_i^N | c_i^* = M, \tilde{c}_i^* = N\right] Pr\left\{c_i^* = M, \tilde{c}_i^* = N\right\}
\end{aligned}
$$

Note that the first two terms are 0, leaving only the effects for Felony Compliers and No Charge Compliers. This estimator then has the flavor of a Local Average Treatment Effect (LATE) in that only a subset of the possible counterfactual scenarios contribute to the aggregate measure (Imbens and Angrist 1994). If, for example, one wished to know the effect of filing no charges in a case with $c_i^* = F$ and $\tilde{c}_i^* = F$, this expression would be uninformative.

This estimator differs from the standard LATE, however, in that it averages two effects, one for Felony Compliers and one for No Charges Compliers, each weighted by the how common that group is. Recent work has discussed the separate identification of treatment effects in environments with three or more treatments (Mountjoy 2022, Humphries et al. 2023, Kamat et al. 2023). These strategies rely on partial identification or combining multiple instruments. The strategy in this paper allows a different trade-off. On the one hand, my setting features promising patterns of shifts across charging treatments. Going from the restricted to the unrestricted choice set can only induce movements $N \rightarrow M$ or $F \rightarrow M$. Movement out of M is impossible because M was not an option in the restricted choice set. Movement between F and N is ruled out by the prosecutor's choice model (or, more generally, Independence of Irrelevant Alternatives). These patterns satisfy, for example, the Unordered Partial Monotonicity assumption of Humphries et al. (2023). However, separate identification is ultimately prevented by the fact that I only observe one dimension of variation. Unlike a setting with multiple instruments or decisionmakers of varying stringency, I cannot change one margin while holding the other fixed. Felony Compliers and No Charges Compliers are either both "turned on" or both "turned off." Therefore, the estimates I present below will necessarily combine the treatment effect for Felony Compliers with the treatment effect for No Charges Compliers.

Another aspect of the problem clarified by this estimator is the role of prosecutor discretion. The composition of the Felony and No Charges Compliers sets are determined exclusively by the prosecutor. This is potentially a very narrow subset of defendants, and one selected by prosecutors who observe more than both policymakers and researchers. Selecting defendants into misdemeanor charging by some other rule may produce different results.

This estimator also provides a way of thinking about the alignment of the prosecutor's goals with society. In an extreme scenario where prosecutors select charges in order to minimize recidivism, affording them more discretion only gives them more options in pursuit of their goal. Relaxing the charging restriction could never increase recidivism. Prosecutors may, however, have other goals that influence their decisions, and giving them more freedom in those decisions may negatively affect society insofar as the prosecutor's personal goals are out of step with those of society. This possibility motivates the direct study of recidivism outcomes.

3.1 Auxiliary Charges

The primary difficulty with estimating the objects considered above is that I do not observe random variation between the restricted and unrestricted choice sets. Instead, I will exploit the fact that some cases are comprised exclusively of felony narcotics charges, which cannot be downgraded to misdemeanors, and some cases *combine* felony narcotics

charges with misdemeanors.[2] I will call these non-felony-narcotics charges "auxiliary charges." Prosecutors in cases with auxiliary charges still cannot obtain a misdemeanor conviction specifically for the associated felony narcotics charge, but they are able to obtain a misdemeanor conviction in the case overall, and the details of the felony narcotics charge may still play a role in whether or not they wish to.

An illustrative example to keep in mind is comparing two hypothetical defendants. The first defendant is arrested and brought into the police station with cocaine in their right pocket. This defendant can be charged with felony narcotics possession but nothing else. The second defendant arrives at the police station with cocaine in their right pocket and cannabis in their left. This second defendant can still be charged with felony narcotics possession, but the prosecutor also has the ability to charge them with misdemeanor cannabis possession. This is not exactly the same as granting the ability to charge the defendant for misdemeanor cocaine possession, but it still allows the prosecutor to charge the case or defendant "as a whole" with a misdemeanor.

There is reason to be concerned, however, that comparing felony narcotics cases with and without auxiliary charges is not a perfect experiment. Relative to a similar case with just an initial felony narcotics charges, a case with an auxiliary charge differs in two ways. First, the prosecutor now has the option to charge a misdemeanor on the full case. But second, the defendant is now alleged to have engaged in more extensive criminal activity. The defendant with both cocaine and cannabis may be different from the defendant with just cocaine. Thus cases or defendants with auxiliary charges may be materially different from those without, and this prevents me from recovering only the effect of expanding the prosecutor's choice set.

To formalize this, define $\hat{X}_i$ as equivalent to X_i with the addition of an auxiliary charge, then the design estimates:

$$\begin{aligned} E\left[Y\left(c_i, \hat{X}_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = \tilde{c}_i^*\right] = \\ \left(E\left[Y\left(c_i, X_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = \tilde{c}_i^*\right]\right) + \\ E\left[Y\left(c_i, \hat{X}_i\right) - Y\left(c_i, X_i\right)|c_i = c_i^*\right] \end{aligned}$$

where the first term on the right hand side is the estimator discussed above and the second term captures the bias introduced by comparing defendants with an auxiliary charge to those without.

To correct for this bias, I will use an approach similar to a difference in difference estimator, but this estimator will have no time dimension. The idea is to compare the effect of auxiliary charges in felony narcotics cases, where they relax charging restrictions, to the effect of auxiliary charges in *other* felony cases, where they do not. The first term, calculated within felony narcotics cases, is the same as before:

$$E\left[Y\left(c_i, \hat{X}_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = \tilde{c}_i^*\right]$$

But the second term, calculated within felony non-narcotics cases, allows the prosecutor to charge c_i^* regardless of the presence of an auxiliary misdemeanor:

$$E\left[Y\left(c_i, \hat{X}_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = c_i^*\right]$$

The difference between the two terms therefore returns my object of interest:

[2]Fewer than 0.7% of cases in my analysis sample combine felony narcotics charges with felony charges of some other kind. In these cases, the non-narcotics charges are typically more serious, and the presence of narcotics charges does not constrain the prosecutor's ability to reduce the other felony. I treat such cases as part of my comparison sample categorized under the non-narcotic charge.

$$\left(E\left[Y\left(c_i, \hat{X}_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = \tilde{c}_i^*\right]\right) -$$
$$\left(E\left[Y\left(c_i, \hat{X}_i\right)|c_i = c_i^*\right] - E\left[Y\left(c_i, X_i\right)|c_i = c_i^*\right]\right) =$$
$$E\left[Y_i\left(c_i\right)|c_i = c_i^*\right] - E\left[Y_i\left(c_i\right)|c_i = \tilde{c}_i^*\right]$$

I take a similar approach for estimating the size of each complier group.

To continue with the illustrative example from above, we may be concerned that the defendant with cocaine and cannabis differs in some fundamental way from the defendant with just cocaine, so the difference in their outcomes will be a combination of the effect of relaxing the narcotics charging restriction, $E\left[Y_i\left(c_i\right)|c_i = c_i^*\right] - E\left[Y_i\left(c_i\right)|c_i = \tilde{c}_i^*\right]$, and whatever else is fundamentally different about the defendants, $E\left[Y\left(c_i, \hat{X}_i\right) - Y\left(c_i, X_i\right)|c_i = c_i^*\right]$. My proposed solution to this problem is to introduce two additional defendants. One is brought into the police station with a stolen phone in their right pocket. They can be charged with felony theft, but the prosecutor also has the option to charge misdemeanor theft. The other arrives with a stolen phone in their right pocket and cannabis in their left. Relative to the defendant who just stole a phone, the cannabis does nothing to expand the legal options available to the prosecutor, though it may still change the fundamentals of the case. Comparing the outcomes of these two defendants should simply capture the fundamental difference signaled by the fact that one has cannabis. This difference can then be removed from the total difference in the defendants holding cocaine, leaving just the charge restriction-relaxing effect of the cannabis.

Design Validity

The primary identifying assumption I rely on in this design is that, conditional on observable case and defendant characteristics, the effect of an auxiliary misdemeanor charge on both the prosecutor's *unconstrained* charging decision and on expected recidivism is the same for narcotics cases as it is for non-narcotics cases. Comparing the typical defendant with just cocaine to the typical defendant with cocaine and cannabis changes as much about the defendant, the circumstances of the case, and the prosecutor's views on c_i^* as the comparison from stolen phone to stolen phone and cannabis.

The following confounding scenario is of particular concern. Suppose that female defendants are found exclusively among narcotics defendants with auxiliary charges. Then, in the absence of controls, the direct effect of gender on outcomes will load onto auxiliary charges, and because the pattern is different in non-narcotics cases, the D-I-D-like design will not correct the problem. Of course, gender is observed, so it is possible to control for its effect by interacting it with charge type. My primary results are from models where all covariates are interacted in this way. However, there may still be some concern about unobservable characteristics, such as defendant income or drug addiction. The benefit of the D-I-D-like design is that unobservables only pose a threat to identification if they are unbalanced across both the narcotics/non-narcotics and the auxiliary charge/no auxiliary charge dimensions. Below, I will investigate the extent of this balance with respect to observable case characteristics. In the absence of random assignment, I do find that observable covariates and auxiliary charges are sometimes correlated. However, the extent of this correlation in narcotics is not unusual relative to other crime categories.

Another way to probe the identifying assumption is to note that narcotics cases should be unique in encouraging less severe charges in the presence of auxiliary charges. If, for example, auxiliary charges make felony arraignments less likely in theft cases relative to others, this suggests some mechanism other than relaxation of charging restrictions that observables do not properly control for. I conduct this placebo test in Section 6 below and find only limited

evidence that other felony charge types react to auxiliary charges. This establishes that auxiliary charges have a similar direct impact across different types of non-narcotics case, but it cannot rule out the possibility that they have a distinct effect in narcotics cases over and above the way that they relax charging restrictions.

3.2 The Nuances of Defelonization Policy

Finally, it is worth carefully considering how this choice set framework described here maps onto commonly proposed defelonization policies. First, note that prosecutors tend to have broad discretion in pursuing "lesser included charges" of those charges enumerated on the charging document. For example, if the prosecutor can prove that a defendant stole $2,000, a felony, then this also implies that the defendant stole $400, a misdemeanor. Evidence demonstrating theft of $2,000 can thus be applied, at the prosecutor's discretion, to either a felony or misdemeanor charge. Therefore, the existence of a lesser included charge that is a misdemeanor will tend to ensure that the prosecutor's choice set includes misdemeanor charges even when the initial charges are to a felony. In some places, as in California, there may even be an explicit list of crimes where prosecutors have discretion to bring either a misdemeanor or a felony for exactly the same conduct.

Therefore, defelonization will not always expand the prosecutor's choice set. When defelonization involves taking some scenarios that *could* formerly have lead to felony charges and mandating that they can now lead *only* to misdemeanor charges, then the prosecutor's choice set is not strictly expanding. For example, a policy in Illinois that made narcotics possession of less than 1 gram a misdemeanor would *add* the option to charge a misdemeanor for any defendants possessing more than 1 gram (due to there now being a lesser included charge), but it would *replace* the option to charge a felony for defendants possessing less than 1 gram. This local contraction of the prosecutor's choice set may motivate political opposition to defelonization proposals from those who are concerned that the policy would be "soft on crime."

An alternative policy, one that only expands the prosecutor's choice set, would be to explicitly give the prosecutor discretion to charge either a misdemeanor or a felony, as was done in California prior to Prop 47. This is the policy that most directly approximates the variation studied in this paper.

4 Institutional Background

4.1 Charging

The data from this project come from Cook County, IL. In Illinois, possession of less than 15 grams of any controlled substance without intent to distribute is a Class 4 felony. This is the least serious class of felony charge, carrying an incarceration sentence of 1-3 years that is frequently converted to probation. There is no misdemeanor narcotics possession. The only narcotics-related misdemeanor charge that occurs with any frequency is possession of drug paraphernalia. This can, and does, appear as an auxiliary charge in a felony narcotics case, but narcotics possession cannot be converted into a drug paraphernalia case unless there is some evidence that the defendant possessed drug paraphernalia in addition to the narcotic.

The decision of what charges to write in the initial charging document is separate from the decision of which charges to pursue at court. All non-narcotics cases must pass felony review, where a dedicated first-line prosecutor decides on the validity of the charges and drafts the charging document. Cases that are rejected in felony review are not part of my estimation sample. Narcotics cases are instead charged directly by the arresting police officer and are not subject to felony review. In both types of case, the initial charging document is then passed to a pretrial prosecutor who

may decide to drop the case, pursue a misdemeanor (if available), or seek a felony arraignment. This is the decision my theoretical model focuses on.

This path for charges ensures that the person who decides whether the initial charging document includes a line for e.g. cannabis possession is different from the person who could decide to leverage that cannabis possession charge into a misdemeanor case. This supports my empirical strategy, which takes the presence or absence of an auxiliary charge as given, rather than something the primary prosecutor also chooses. However, it must be noted that non-narcotics and narcotics cases are initially charged by different parties (felony review prosecutors for the former and police officers for the latter) and subject to different levels of screening before they enter my analysis dataset. These procedural asymmetries will drive a violation of my identifying assumption if they cause different types of defendants to be charged with auxiliary charges across the narcotics and non-narcotics samples. I address this below by considering an alternative subset of auxiliary charges that are driven by contraband (cannabis and weapons possession) and so are less subjective.

4.2 Misdemeanors

The primary distinction between a misdemeanor and a felony is that a misdemeanor conviction can lead to no more than 1 year of incarceration, which must occur in the county jail. In practice, incarceration terms for misdemeanors are rare in Cook County. 6% of misdemeanor defendants in my data serve jail time. Defendants formally convicted of misdemeanors are more likely to be assigned to supervised release.

Even this, however, is a rare outcome of misdemeanor cases in Cook County. Misdemeanor defendants are often most interested in avoiding a formal conviction. To encourage cooperation from defendants, prosecutors will offer the opportunity to avoid a formal conviction in return for an informal commitment to a program like community service or drug treatment. This is implemented through "deferred prosecution" wherein the prosecutor "strikes off [the charge with] leave [to reinstate]" (SOL) or dismisses the suit without prejudice. The threat of a true conviction should the case be reinstated incentivizes cooperation with the terms of the agreement. I will refer to these outcomes as "structured nonconvictions" and endeavor to treat them as misdemeanor prosecutions in my analysis.

In cases where only misdemeanor charges are initially brought, it is possible to gauge the magnitude of structured nonconvictions. Here, the mere existence of the case signals the prosecutor's intention to pursue a misdemeanor. In these cases, the formal conviction rate is only 16.9%.[3] That is far lower than the formal conviction rate in arraigned felonies, which is approximately 89.7%. This suggests that the vast majority of misdemeanor cases were bargained to a structured non-conviction.

I am primarily interested in felony cases that become misdemeanors, and this complicates matters. The initial bringing of both felony and misdemeanor charges does not clearly signal an intention to charge a misdemeanor, nor does the later dismissal of both kinds of charge clearly signal an intention not to prosecute. In particular, prosecutors may negotiate the terms of a structured nonconviction while both felony and misdmeanor charges remain on the docket, then drop the whole case once an agreement has been reached. It is therefore unclear whether cases that are dropped in my data should be categorized as misdemeanors or no charges. I discuss my approach to this measurement problem in Section 5.2 below.

[3]To calculate this misdemeanor conviction rate, I first restrict attention to cases where only misdemeanor charges are brought. This is a distinct sample from my main analysis sample, which requires at least one felony charge. I then, impose the sample selection rules described below and calculate the rate at which defendants plead or are found guilty.

4.3 Felonies

Felonies are a more serious charge than misdemeanors and may be punished with either probation supervision or incarceration in state prison. In either case, the minimum sentence is 1 year. The court process for felonies differs from the process for misdemeanors in several ways. Felony cases have much lower pretrial release rates than misdemeanors, so felony defendants are much more likely to be held in jail to await trial. The key step in charging a felony is arraignment, which occurs after the prosecutor has established probable cause and marks the formal bringing of felony charges. I treat arraignment as a clear marker of the prosecutor's intention to pursue felony charges. Once a case passes felony arraignment in Cook County, charge bargaining down to a misdemeanor is rare. Among felony arraignments that ended in conviction in my sample period, only 3.6% were amended to a misdemeanor. Even when this happens, much of the punishment for defendants on the felony-misdemeanor margin has already been assessed, such as time spent in jail awaiting trial.

Defendants arraigned for felonies, if convicted, are sentenced to either probation or incarceration. 58% of arraigned defendants in my analysis sample are sentenced to incarceration with a median sentence of 2 years (75th percentile 4 years). Due to credits for pretrial detainment and day-for-day good time credits, this is an expected sentence of less than 1 year. 32% of defendants are sentenced to probation also with a median length of 2 years (75th percentile also 2 years). The remaining 10% are not convicted. Felony convictions can produce substantial collateral consequences, such as employment stigma, enhanced charges in future criminal cases, and loss of access to public benefits. Some of these collateral consequences arise from formal misdemeanor convictions as well, but as discussed above, misdemeanor cases are much more likely to end in a way that does not leave a criminal record.

Both incarceration and probation may affect opportunities for and measurement of recidivism. Incarcerated defendants are incapacitated in the sense that they cannot commit new crimes when in prison.[4] Defendants who are on probation are under greater supervision and scrutiny than ordinary people, which could increase the rates at which criminal offending is detected. Defendants on probation may also return to the court for technical violations of that probation that would not be criminal behavior for someone outside the system. I do not count these as recidivism. Furthermore, defendants sentenced to incarceration must submit to at least 1 year of Mandatory Supervised Release, which is similar to probation but run by the state instead of the county. These dynamics pose a problem for my analysis because they may change the rates of observed offending behavior as compared to actual offending behavior in the counterfactual where the defendant is released unconditionally. This paper measures the first, while the prosecutor (and society at large) presumably cares more about the second. I address this below by considering an alternative "donut" measure of recidivism that excludes the first two years post-conviction, when defendants convicted of a felony are most likely to be incarcerated or on probation.

5 Data

My data link together Chicago Police Department (CPD) arrest records, municipal court records, and circuit court records. The useful overlap between my arrest and court data begins in June of 2000 and runs to June of 2019. Using these data, I can observe the original charges filed in each case. I also observe some information about how that case concludes, the limitations of which are discussed in Section 5.2. Defendants are identified with a fingerprint-based ID number that I use to track defendants across cases. I use this ID to both construct criminal histories and detect recidivism events.

[4] My measure of recidivism is based on Cook County court data, and there are no state prisons in Cook County. Therefore, even insofar as prisoners *are* committing new crimes while incarcerated, they will not be picked up by my outcome measure.

5.1 Sample Selection

I begin with 468,334 felony cases. I drop 9,454 court cases linked to multiple arrests and 17,967 arrests linked to multiple court cases. Both these scenarios potentially confuse the relationship between alleged behavior at time of arrest and charging decisions. I also drop 5,996 cases where key information about the defendant or charge is missing. I drop 5,634 cases that were processed outside of Chicago's central bond court. I restrict attention to defendants born after 1968, which ensures they are younger than 16 when I begin observing felony proceedings in 1984, so none of their adult criminal history falls outside my historical data.[5] This excludes 142,792 cases (30.5% of the starting sample). Finally, I drop 52,136 cases that begin after 2014 to provide 4 years of followup time for my recidivism measure. I retain a primary analysis sample of 234,355 cases, 50% of my starting data.

5.2 Definitions of Key Variables

Treatment

The primary treatment decision made by the prosecutor is how to charge a case. Crucially, this is distinct from how a case concludes. For example, a case that is charged as a felony may still end in a non-conviction if the defendant is acquitted at trial. Felony charging is easy to measure in my data. I mark any case with a felony arraignment as a charged felony.

Distinguishing misdemeanor cases from dropped cases is more difficult due to the problem of structured nonconvictions. I address this problem by first estimating the effect of auxiliary charges on what I can reliably measure. I divide cases into three mutually exclusive and exhaustive categories: cases with a felony arraignment, cases with no felony arraignment and a formal misdemeanor conviction, and cases with neither a formal misdemeanor conviction nor a felony arraignment. The first of these categories is unambiguously the set of cases where the prosecutor intends to pursue felony charges. The second is a subset of those cases where the prosecutor pursued misdemeanor charges. The third is all cases where the prosecutor pursued no charges, plus a fraction of those actually prosecuted as misdemeanors. I then calculate the true prevalence of misdemeanor charges and non-charges under various assumptions on the formal misdemeanor conviction rate in the sample of felony cases with auxiliary charges that are prosecuted as misdemeanors.

Among these hypotheticals, I give particular priority to the formal conviction rate in misdemeanor-only cases, 16.9%. This relies on the assumption that misdemeanor-only cases and felony cases which become misdemeanors are not different in their formal conviction rates. This should also be thought of as a lower bound, as some of the misdemeanor-only cases used to calculate that 16.9% rate may have been dropped by prosecutors without any attempt made to prosecute the misdemeanor case. Accounting for this could increase the formal misdemeanor conviction rate to as much as 21% under the assumption that prosecutors drop cases about as often in misdemeanor-only cases as they do in felony cases.[6] Below I will present my main estimates of charging impacts for a variety of assumptions about the formal misdemeanor conviction rate.

[5] My measure of criminal record is based on just felonies in order to limit the impact this restriction has on my data. My sample begins in 2000, so the youngest defendant dropped by this rule is 32 years old. A comparable measure of criminal history that included misdemeanor charges is unlikely to be informative anyway, as 87.3% of defendants in my analysis sample have at least one prior misdemeanor charge even within the limited historical data I have.

[6] Assume that misdemeanor-only and felony cases share a No Charges rate of 19%. Then the formal conviction rate among cases where prosecutors actually pursue charges in misdemeanor only cases is $\frac{16.9\%}{81\%} = 20.8\%$. Plugging this back into the number of apparent No Charges one expects to be generated by misdemeanor prosecutions in the felony sample, given an overall rate of observed formal misdemeanor convictions of 3.4% as in Table 2, yields 14.3%. Subtracting this from the number of observed cases with no conviction or arraignment, 33.4% according to Table 2, gives a No Charge rate of 18.9% in the felony sample. This confirms the original assumption.

Auxiliary Charges

I mark a case as having an auxiliary misdemeanor charge if it includes any misdemeanor among the charges initially filed when the case entered municipal court. Table 1 collects misdemeanor auxiliary charges observed in my analysis sample into 10 coarse categories. It lists the frequency of the charge category, then the percentage of those charges that are associated specifically with felony narcotics cases. The most common auxiliary misdemeanor is cannabis possession, followed by unlicensed firearms. Both cannabis and drug paraphernalia charges are more likely to be associated with felony narcotics cases, but other misdemeanors less directly associated with drug use, like soliciting prostitution and trespassing, also commonly accompany felony narcotics charges.

Outcome

My outcome variable of interest is an indicator for recidivism, with my main outcome being a new felony charge observed in the Cook County data within the 4 years after disposition of the initial case. I date the recidivism horizon from the end of the initial case and not the end of any resulting incarceration to ensure that defendants who are the same age during their case are also the same age during the period when their recidivism outcomes are observed.

5.3 Estimating Equation

I use estimating equation below:

$$Y_{itd} = \beta Covars_{itd} + \gamma_N Narc_{itd} + \beta_N Narc \times Covars_{itd} + \gamma_A Aux_{itd} + \gamma Narc \times Aux_{itd} + \delta_d + \tau_t + \epsilon_{itd} \quad (1)$$

where i indexes cases, t time, and d the police district where the arrest was made. Y_{itd} is my outcome of interest, either charging treatment (felony, misdemeanor, or no charges) or an indicator for recidivism. When estimating the former model, I use a multinomial logit specification for the three possible treatments. $Covars_{itd}$ is a vector of covariates: indicator for black defendant; indicator for male defendant; vector of defendant age effects; indicators for no priors and 1-2 priors; vector of dummies for felony class (describes severity of the charge); a dummy for more than one felony charge in this case; and a vector of dummies for felony category (e.g. robbery, theft, murder). δ_d is a vector of district-of-arrest dummies that I also interact with the indicator for narcotics charges. τ_t is a vector of year and month dummies that I interact with the indicator for narcotics charges.

The coefficient on Aux_{itd}, γ_A, captures the difference between cases with and without auxiliary charges. My object of interest is γ, which captures the difference induced by auxiliary charges for narcotic felonies over and above their effect on non-narcotic felonies.

5.4 Summary Statistics

Table 2 presents summary statistics for my main analysis sample. 74% of defendants are Black, much higher than the rate of 26% for the population of Cook County. 87.7% of defendants are male, and defendants have an average age of 26.9. Defendants are divided roughly equally between having no prior charges, having 1-2 prior charges, and having 3 or more. 46.4% of defendants will be charged with another felony in the next 4 years. A majority of defendants are charged with Class 4 felonies, the least serious.

Defendants in narcotics and non-narcotics cases are broadly similar in their demographics, though narcotics defendants are less likely to be male, more likely to be Black, and tend more towards long criminal histories. Narcotics

charges are much more concentrated in Class 4, indicating possession of a small amount of drugs, and narcotics defendants face fewer charges overall. Narcotics charges more serious than this are generally for trafficking rather than possession. Table 2 also summarizes the characteristics of the cases with and without auxiliary charges. Defendants facing auxiliary charges are more likely to be male, to have no criminal history, and to be charged with less serious felonies. The last column of Table 2 shows summary statistics for my population of particular interest: defendants charged with both a narcotics felony and an auxiliary misdemeanor. These defendants have the highest rate of Class 4 felony charges among any of the samples considered.

The bottom rows of Table 2 summarize charging treatments. Formal misdemeanor convictions are rare overall but especially rare in narcotics cases, comprising less than 1% of the sample. Narcotics charges are also substantially less likely to progress to a felony arraignment, which is likely driven by the fact that narcotics charges are not subject to the felony review process by which the Cook County State's Attorney screens felony charges before they appear in these data. Relative to narcotics defendants overall, defendants facing both a narcotics charge and an auxiliary charge have more than three times the rate of formal misdemeanor convictions.

6 Results

6.1 Specification Checks

Before going into the details of how prosecutor charging decisions respond to auxiliary charges in narcotics cases relative to non-narcotics cases, I will investigate the identifying assumption of my analysis: that the effect of an auxiliary misdemeanor charge on both the prosecutor's *unconstrained* charging decision and on expected recidivism is the same for narcotics cases as it is for non-narcotics cases. First, I investigate balance by observable covariates. Second, I conduct the placebo test discussed in Section 3.1 to verify that only narcotics cases have a significant effect on *constrainted* charging decisions.

Covariate Predictions of Auxiliary Charge

In an ideal dataset for this design, auxiliary charges would be assigned at random, so no covariate (observed or unobserved) would be correlated with them. Given that auxiliary charges are not randomly assigned in my data, this balance condition does not hold. Certain types of defendants are clearly more likely to engage in the types of behavior that produce a combination of misdemeanor and felony charges. For example, black defendants are 1 percentage points less likely to have an auxiliary charge, and men are 5 percentage points more likely. Fortunately, this design does not require this strict balance condition because it compares not just across auxiliary misdemeanors but across narcotics and non-narcotics charges.

Figure 2 investigates the question of whether the correlations between observables and auxiliary charges are unusual in narcotics cases relative to other types of cases. It estimates a series of regressions of the form:

$$Aux_{itd} = \beta Covars_{itd} + \gamma_N Narc_{itd} + \sum_{c=1}^{8} \left(\gamma_c Cat_{itd}^c + \gamma_c^W Cat_{itd}^c \times W_{itd}\right) + \delta_d + \tau_t + \epsilon_{itd}$$

where each Cat_{itd}^c is one of the non-narcotics crime categories (murder, sexual assault, robbery, assault, burglary, theft, weapons, and other), and W_{itd} is an observable covariate. Each γ_c^W thus captures the difference in the ability of W_{itd} to predict Aux_{itd} in category c relative to the omitted category of narcotics. Figure 2 plots γ_c^W in rows defined by W_{itd}. It shows that while the γ_c^W are occasionally large, they are typically distributed both below and above 0, meaning that they are both below and above the correlation in narcotics cases. This indicates that there are

real differences in the correlations between observables and auxiliary charges within crime categories, but that the narcotics category is not unusual in the extent or direction of these differences. The results presented here control for imbalances in these observables, so the primary question is whether there is remaining concern over the influence of unobservables. It is impossible to measure this directly, but the results here do not indicate that narcotics charges are "special" in a way that might drive strong correlations with unobservables.

The one covariate that challenges this conclusion is the indicator for a class 4 felony charge. While the γ_c^W are still distributed around 0, they are larger in magnitude than with other W_{itd}. This is because, in narcotics cases, felony class is highly correlated with whether the charge is for possession or trafficking, and drug trafficking charges are much less likely to be accompanied by auxiliary misdemeanors. Robustness checks below will consider a specification that removes trafficking charges and focuses on felony classes 1 and 4 where nearly all possession charges reside.

Cross Effects by Charge Category

I next conduct the placebo test discussed in Section 3.1 above. The idea of this test is that if my design is only capturing the effect of relaxing charging restrictions, it should not see prosecutors reduce felony arraignments when a non-narcotics case has auxiliary charges.To test this, I estimate a multinomial logit in my three observable treatments: felony arraignment, misdemeanor conviction, and neither conviction nor arraignment. This multinomial logit follows the specification in Equation 1 with each charge category of interest taking the place of narcotics. For non-narcotic categories, I remove narcotics charges from the estimation sample. This is because I already have reason to think that narcotics charges have a special response to auxiliary charges, so the model would estimate the opposite response when comparing other charge categories to narcotics cases if they were left in. I then estimate the marginal response in these treatment outcomes to the interaction term with auxiliary charges, γ, restricting attention to just charges of the type of interest. I focus on the change in felony arraignments because this is not complicated by the problem of structured nonconvictions.

Table 3 I find that felony arraignments in narcotics cases decline by 3.3 percentage points when auxiliary charges are present. Among the 8 other charge types I consider, I primarily find either an increase in felony arraignments or no statistically significant change. The only other charge type with a large response is sexual assault, where felony arraignments decline by 12 percentage points when auxiliary charges are present. I will not speculate here why sexual assault cases apparently respond to auxiliary charges except to note that sexual assault cases are unusual in many dimensions, including the complexity of charging rules and the importance of post-conviction collateral consequences such as sex offender registries. Below I will show that my main results are robust to excluding sexual assault cases from the comparison sample.

6.2 Charging Treatment Response

To estimate the effect of relaxing the narcotics charging restriction on the charging treatments selected by prosecutors, I repeat the procedure used to calculate cross effects by charge categories above, but this time I follow the specification in Equation 1 exactly. The columns labeled "Raw" in Table 4 present the estimated marginal responses of the three charge options within the sample of narcotics defendants.

At first glance, the raw columns for the full analysis sample indicate that adding auxiliary misdemeanors to narcotics cases primarily shifts prosecution decisions from felony charges, which decline by 3.3 percentage points, to no charges, which increase by 2.3 percentage points. The increase in misdemeanor charges is comparatively modest at 1 percentage point. However, observed cases with no arraignment and no conviction are primarily misdemeanor cases with a structured nonconviction. For a hypothetical formal misdemeanor conviction rate of a, the true change in

the misdemeanor charging rate is $\frac{0.01}{a}$, and the true change in cases with no charges is $0.023 - \left(\frac{1-a}{a}\right) 0.01$. Felony charging rates are unaffected.

Figure 1 plots the adjusted changes in treatment for various hypothetical values of a. The observed formal conviction rate in misdemeanor-only cases, 0.169, is marked with the leftmost vertical line. At this conviction rate, the change in cases with no charges is in the expected direction. This is also true when $a = 0.21$, the rightmost vertical line, which assumes prosecutors drop charges as frequently in misdemeanor-only cases as in felony cases.

Returning to Table 4, the columns labeled "Adjusted" report confidence intervals for these adjusted values.[7] I find an increase in misdemeanor charges of 5.3 to 7.1 percentage points. This is driven by a 2.3-4.3 percentage point fall in felony charges and a 1.1-4.6 percentage point fall in no charges. Therefore, both types of compliers are present in at least some amount, though it is not clear which group is larger.

The remaining rows of Table 4 repeat this analysis for different subsamples.[8] Focusing on the estimate for Black defendants, the estimated raw change in felony arraignment rises to 5.6 percentage points. The raw estimate for changes in no compliers becomes even more positive, such that the adjusted confidence interval now includes zero. This indicates that Black defendants are much more likely to be Felony Compliers, and I cannot dismiss the possibility that no Black defendants are No Charges Compliers.

The estimates for non-Black defendants are the opposite. The raw difference in felony arraignments is positive with a confidence interval that includes 0, and the raw estimate for the change in no compliers is -0.21 with an adjusted confidence interval that excludes 0. This indicates that non-Black defendants are much more likely to be No Charges Compliers, and I cannot dismiss the possibility that no non-Black defendants are Felony Compliers.

There is a similar, though less pronounced, pattern among defendants with and without a prior felony. Defendants with a prior felony are skewed towards being Felony Compliers while defendants without a prior felony are skewed towards being No Charges Compliers. This makes intuitive sense. Defendants with criminal histories are more likely to face harsh charges, and so they are more concentrated on the misdemeanor-felony margin, whereas defendants without criminal histories lie more on the no charges-misdemeanor margin.

From a policy perspective, these results suggest that relaxing the charging restriction on narcotics cases would benefit some defendants and harm others. Some who would have faced felony charges instead face misdemeanors, but some who would have seen their charges dismissed instead find themselves in court. There is important heterogeneity though, and the benefits of relaxing charges would be concentrated among Black defendants and defendants with criminal histories while the detriments would be concentrated among non-Black defendants and defendants without criminal histories.

6.3 Recidivism Response

I now estimate the effect of relaxing the narcotics charging restriction on the recidivism outcomes of defendants. I do this using a linear probability version of Equation 1 with 4-year felony recidivism as the outcome. Table 5 presents my main results. Each column considers a different subsample of the data, and I will discuss each in turn below, but I begin by first discussing the main effect of auxiliary charges.

[7] For felonies, this is a simple 95% confidence interval around the raw estimate. For misdemeanors, I calculate the 95% confidence interval around the raw estimate and divide both bounds by a. For no charges, I start with the 95% confidence interval around the raw estimate. From the lower bound, I subtract the upper bound of the misdemeanor CI multiplied by $\frac{1-a}{a}$. From the upper bound, I subtract the *lower* bound of the misdemeanor CI multiplied by $\frac{1-a}{a}$.

[8] I calculate my adjustment for structured nonconvictions using the formal misdemeanor conviction rate calculated within the set of misdemeanor-only cases belonging to that subsample. I will later present results that divide the sample of felony cases by the class of the most serious charge. I do not consider these samples at this stage because there is no equivalent formal conviction rate in misdemeanor-only cases to use in the adjustment below.

The first row of Table 5 presents γ_A, the correlation between auxiliary charges and recidivism in the sample of non-narcotics cases. This is generally positive, meaning that an auxiliary charge tends to predict future felony recidivism, at least among defendants for whom it does not affect the prosecutor's sentencing options. This is not unreasonable. Whether or not an auxiliary misdemeanor changes the sentencing outcome in a case, it may reflect a greater extent of criminal activity on the part of the defendant. The correlation between auxiliary charges and recidivism in the sample of *narcotics* cases can then be backed out from the sum of the coefficients in the first and second rows. For all columns, this difference is small. On their face, these results say that auxiliary charges have an imperceptible effect on recidivism in narcotics cases. However, that interpretation does not account for the baseline positive correlation between auxiliary charges and recidivism.[9]

The interaction coefficients, γ, in the second row of Table 5 do make this adjustment. The full-sample result in the first column shows that felony recidivism is lower for narcotics cases with an auxiliary charge attached, but the estimated coefficient is not statistically significant. This null result is itself policy relevant because it suggests that felony charging restrictions can be relaxed without fear of sparking greater felony recidivism. The 95% confidence interval of my estimate admits increases in felony recidivism of no greater than half a percentage point.

This null result, however, masks significant and important heterogeneity. The remaining columns in Table 5 estimate the same model for subsamples of the data. Column 2 shows that Black defendants have a statistically significant recidivism reduction of 1.7 percentage points. Meanwhile, column 3 shows the interaction effect for non-Black defendants is positive and statistically insignificant. It is useful to interpret these findings in light of the above results on how treatments change in these two groups. The recidivism reductions are concentrated among Black defendants, who are also the group with a high concentration of Felony Compliers, while non-Black defendants, with a high concentration of No Charges Compliers, see no reduction in recidivism.

These findings suggest, but do not prove, that the observed recidivism reduction is driven by shifting defendants into more lenient forms of prosecution rather than the reverse. However, it may also be driven by unobserved characteristics of the racial groups considered. For example, defendants facing their first felony charges are, like non-Black defendants, skewed towards No Charges Compliers, but they experienced relatively large recidivism reductions. This could be because their estimate was dominated by a strong effect from the Felony Compliers who were in the unique position of avoiding collateral consequences as a result of the reduction to misdemeanor charges. Similar, though less intuitive, phenomena could be affecting the Black and non-Black estimates.

This finding also has important implications for racial equity in the Cook County courts. Relaxing narcotics charging restrictions would improve both the charging and recidivism outcomes for Black defendants relative to non-Black defendants. Both gaps were exceptionally large over this period. In raw data, black narcotics defendants were 25.9 percentage points more likely to be arraigned for a felony and 24.6 percentage points more likely to face a felony recidivism charge within 4 years. Even my full suite of controls only shrinks the felony arraignment gap to 15.1 percentage points and the recidivism gap to 15.8 percentage points. Relaxing narcotics charging restrictions would close these adjusted gaps by approximately 5 and 3 percentage points, respectively, or $1/3$ of the charging gap and $1/5$ of the recidivism gap.

Returning to Table 5, I find in columns 4 and 5 that the recidivism reduction is concentrated in defendants with no prior felony conviction, who see a 2.4 percentage point reduction in recidivism. The collateral consequences of a felony conviction are particularly salient for these defendants, so an opportunity for a misdemeanor prosecution instead may be especially important.

In columns 6 and 7 of Table 5, I find that the recidivism reduction is concentrated in defendants whose most serious

[9]Importantly, this baseline positive correlation is not a violation of my identifying assumption that auxiliary charges have, leaving aside how they relax charging restrictions, the same effect in both narcotics and non-narcotics cases. This result simply says that this baseline effect is to make recidivism more likely. Identification fails only if the effect is somehow different for narcotics cases relative to non-narcotics cases.

charge is a Class 4 felony. These defendants have a 3 percentage point reduction in recidivism, while the recidivism of defendants charged with a more serious crime is flat. One possible explanation for this finding is that defendants facing the least serious class of felony are more likely to be marginal to the prosecutor's charging decision.

6.4 Alternative Definitions of Recidivism

In Table 6, I explore alternative definitions of the recidivism outcome. Each panel in this table replicates Table 5 with a different definition of recidivism on the left hand side of Equation 1.

Panel A of Table 6 considers a "donut" measure of recidivism that excludes the first two years, when defendants are more likely to be incarcerated or under some form of supervision as a consequence of the case at hand. The resulting estimates are somewhat attenuated, though I can still reject increases in recidivism across the whole sample of less than 0.5 percentage points. The recidivism reduction for Black defendants remains negative and significant. These results may simply reflect complex dynamics of recidivism over time, though they also suggest that concerns about the interaction between supervision intensity and measured recidivism are not unfounded.

Panel B of Table 6 focuses specifically on misdemeanor, rather than felony, recidivism over the 4-year horizon. The measured effect of felony charge relaxation on recidivism increases is now positive in the full sample and most subamples. These results are mostly, but not entirely, wiped out in Panel C, which considers a "donut" misdemeanor recidivism measure comparable to the one in Panel A. Only non-Black defendants continue to show a positive effect. The change in observed misdemeanor recidivism induced by relaxing felony narcotics charging is therefore either concentrated in the first two years or driven by the greater supervision experienced by defendants after a (formal or informal) conviction.

Lastly, Panels D-F of Table 6 divide felony recidivism into drug crime, violent crime, and property crime. I find that only drug recidivism responds strongly to auxiliary charges in narcotics cases. Panel D follows a similar pattern to Table 5 but with larger point estimates. In contrast, Panels E and F show almost no significant changes either way in violent and property recidivism. Only two responses, property crime for non-Black defendants and defendants with a prior conviction are marginally significant. These estimated null effects are reasonably precise. I can rule out overall changes greater than 1 percentage point in either direction for the overall sample, and 2 percentage points for most subsamples. These results suggest that relaxing narcotics charging restrictions would primarily affect future drug crime. The effects, if any, on other kinds of crime were too small for me to detect.

6.5 Alternative Definitions of Auxiliary Misdemeanor

In Table 7, I consider alternative definitions of auxiliary charges. Each panel in this table replicates Table 5 with a different definition of auxiliary charges. In each panel, cases with auxiliary charges that do not meet this definition are dropped from the estimation sample. This ensures that the regressions continue to compare a set of cases with auxiliary charges and a set of cases definitively without auxiliary charges.

In panels A and B of Table 7, I focus on only drug auxiliary charges (cannabis and drug paraphernalia charges) and non-drug auxiliary charges, respectively. In Panel A, I find a similar pattern of results to Table 5 with generally more negative effects, but standard errors are also larger, so no recidivism estimate is statistically significant. Panel B looks at non-drug auxiliary charges instead. Most coefficient estimates are now closer to zero, but I find a 3.4 percentage point drop specifically for defendants charged with the least severe felonies. From these tables, I conclude that neither drug nor non-drug auxiliary charges alone are responsible for the effects I find in the main results.

In Panels C and D of Table 7, I contrast contraband auxiliary charges with non-contraband. The goal of this exercise is to see how my estimates change when focusing on a subset of auxiliary charges that are less subjective because

they depend on the defendant possessing a particular item in particular circumstances. I define contraband auxiliary charges as cannabis possession, possession of an illegal firearm, and firearm possession without a license.[10] I find particularly strong recidivism effects when focusing on these contraband auxiliary charges. My full sample estimate of the recidivism reduction is 4.3 percentage points, which goes as high as 6.5 percentage points for defendants without prior convictions. Surprisingly, even non-Black defendants exhibit a significant reduction in recidivism when only contraband auxiliary charges are considered. This is a significant contrast from Panel D, where the observed recidivism changes are generally null but strongly positive for nonblack defendants. Overall, the strong recidivism reductions I observe when restricting attention to auxiliary charges that are not easily manipulated gives me confidence that relaxing felony charging restrictions would result in similar reductions.

6.6 Robustness

Tables A1-A4 recreate Tables 4-7 with a version of Equation 1 that omits the full interaction with narcotics charges. These results can help to give a sense of the impact these covariate interactions have on my estimates. The largest shift is in prosecutor charging decisions, Table A1, which shows lower rates of No Charges Compliers and higher rates of Felony Compliers.. The recidivism responses in Tables A2-A4 all follow a similar pattern to my main results with generally larger effects. For example, I find that recidivism falls by a statistically significant 1.6 percentage points in the full sample, which persists even into the "donut" measure of recidivism that leaves out the first 2 years.

Tables A5-A8 recreate Tables 4-7 with a sample that is more responsive to the issues raised in Section 6.1. This means removing trafficking charges, class X, 2, and 3 narcotics charges, and sexual assault cases from the sample.The estimates of charging responses and recidivism responses in are very similar to my main estimates and, if anything, show larger responses to auxiliary charges. From this, I conclude that my main results are not significantly biased by the inclusion these cases and provide some comfort that correlations of auxiliary charging patterns with unobservables have a modest effect on my findings.

6.7 Discussion

Taken as a whole, these results indicate that the most robust decrease in recidivism is short term drug crime. This accounts for the weaker and sometimes positive coefficients when focusing on misdemeanor recidivism and "donut" measures. Violent and property crime recidivism are unlikely to fall substantially, and may even rise a small amount (less than 1 percentage point). If one assigns a small social value to curtailing the drug abuse of individuals, it's possible to read these results as finding no important effects on recidivism one way or the other. This is underlined by the fact that prosecutors infrequently chose to charge narcotics crimes as misdemeanors, even when auxiliary charges gave them the option to do so.

Such an interpretation of the results, however, still has important implications for policy. Beyond the costs associated with changing the laws themselves, giving prosecutors more flexibility in narcotics charging would not require a significant investment of resources. These results show that even at the low rate that prosecutors currently use that flexibility, such a policy could substantially improve the racial equity of felony charging and conviction. There may furthermore be benefits for the overall fairness or efficiency of the courts that are harder to measure. Recall that in the theoretical model describing prosecutors' decisions, giving those prosecutors more choices necessarily improved their utility, and hence social welfare insofar as the two are alligned. While one might ordinarily ask "why relax charging restrictions?", in this case it may be reasonable to ask "why *not* relax charging restrictions?". From this perspective,

[10]These are a subset of the broader weapons category, which also includes some illegal *uses* of firearms.

results on recidivism that are at worst null are useful information for setting policy because they show that an important potential downside of relaxing charging restrictions is not a significant threat.

7 Conclusion

This study finds that, were it not for the charging restrictions currently in place, there would be narcotics defendants on both the no charges-misdemeanor margin and the misdemeanor-felony margin. Furthermore, it finds suggestive evidence that defendants recidivate less when placed on the more lenient side of each margin. Black defendants sit primarily on the misdemeanor-felony margin, so restoring the ability to charge a misdemeanor primarily moved them to less severe charges. They also contributed disproportionately to the observed reduction in recidivism. Defendants of other races sit primarily on the no charges-misdemeanor margin, so restoring the ability to charge a misdemeanor tended to increase the severity of their charges. They were also only group to show consistent potential for *increased* recidivism. However, these patterns could have been driven by unobservable characteristics not accounted for in my analysis.

The findings in this study also have important policy implications. The variation considered here approximates a policy that gives prosecutors the ability to charge a misdemeanor in narcotics cases without taking away the ability to charge a felony. This research therefore suggests that such a policy would produce a small reduction in felony recidivism while closing racial gaps in felony charge and recidivism rates. However, I also note three important caveats. First, the circumstances that can currently generate a relaxation of the charging restriction are unpredictable and non-obvious. An explicit policy change that enabled misdemeanor charging for narcotics possession may erode the deterrence value of the law as currently written. People who know that a more lenient punishment is on the table may be more willing to violate narcotics laws.

Second, this study is not experimental and may be affected by omitted variable bias, albeit only of a very particular form. If certain unobservable types of defendants have strong recidivism tendencies and are not balanced across narcotics/non-narcotics and auxiliary charge/no auxiliary charge, my estimates of the effect of relaxing charging restrictions will be biased. Using observable covariates and outcomes as a proxy for what I cannot measure, I find mixed evidence for the idea that narcotics cases are typical in when and how auxiliary misdemeanors are attached to felonies. That evidence suggests that the influence of unobservables is likely modest, but it is never possible to know for sure, even when observables are perfectly balanced.

Third, these results rely on prosecutorial discretion. 17.7% of narcotics defendants are charged with an auxiliary misdemeanor, but I find a shift towards misdemeanor charges of at most 7.1 percentage points. Even when the prosecutor has the option to charge a misdemeanor, they decline to exercise it more often than not. The defendants who do see a change in their charge status were selected by prosecutors for that treatment according to some criteria unobserved by policymakers. This selection may skew towards defendants who are likely to reduce their recidivism if charged with a misdemeanor. A policy that selects defendants into misdemeanor prosecution according to some coarser metric like drug weight may not achieve the same results.

Ultimately, the evidence presented here should not be treated as definitive. It may be prudent to organize a randomized control trial to test these findings more directly. In that event, I would advocate for a design that continues to involve prosecutor discretion as a key step in the assignment of charges. A procedure that directly assigns charges to defendants would estimate treatment effects for different complier sets than those relevant to an actual policy that relaxes narcotics charging restrictions. Furthermore, charging defendants facing only felony narcotics charges with a misdemeanor remains illegal in the relevant states, so some measure of legislative approval would be necessary to do so, even on an experimental basis. The design in this study suggests a way to work around this. By focusing on felony

narcotics defendants with auxiliary charges and randomizing some to a treatment where prosecutors cannot charge misdemeanors even though the auxiliary charge permits it, it is possible to estimate a clean effect of the narcotics charging restrictions, albeit only for the population of defendants who enter court facing an auxiliary charge.

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Figures and Tables

Table 1: Auxiliary Charge Categories

Category	Count of Charges	Felony Narcotics Rate
Cannabis	15258	.653
Firearms	11639	.112
Assault/Battery	5985	.248
Resisting/Obstructing Arrest	5003	.415
Trespassing	4937	.585
Vehicles/Traffic	4836	.38
Drug Paraphernalia	4115	.754
Public Disorder	3557	.502
Soliciting	1783	.779
Theft	828	.234

Notes: This table separates auxiliary misdemeanor charges in the main analysis sample by category. The felony narcotics rate is the percentage of misdemeanor charges in that category which were auxillary to a felony narcotics charge. Individual charges with fewer than 100 occurences as charges auxillary to felony narcotics are not categorized and so excluded from these aggregates. Charges do not match one-to-one with cases because some defendants have multiple qualifying auxiliary charges.

Table 2: Summary Statistics

Variable	All	Narcotic	Non-Narcotic	Auxiliary Charge	No Auxiliary Charge	Narcotics and Auxiliary
Count	234355	131347	103008	49027	185328	23247
Black	73.97%	76.59%	70.61%	73.05%	74.21%	72.8%
Male	87.72%	86.69%	89.03%	90.36%	87.02%	87.18%
Age (Years)	26.9	27.2	26.5	26.5	27	27.3
No Prior Charges	37.09%	35%	39.76%	39.6%	36.43%	37.27%
1–2 Prior Charges	31.71%	31.58%	31.89%	31.95%	31.65%	31.75%
4-year Felony Recidivism	46.4%	52.11%	39.12%	44.07%	47.02%	49.72%
Count of Charges	1.9	1.6	2.2	3.1	1.6	2.7
Class X	5.93%	4.77%	7.41%	4.31%	6.36%	4.28%
Class 1	10%	12.62%	6.65%	8.39%	10.42%	12.19%
Class 2	14.88%	9.09%	22.28%	12.94%	15.4%	3.88%
Class 3	10.14%	0.56%	22.35%	13.19%	9.33%	0.62%
Class 4	59.06%	72.97%	41.32%	61.17%	58.5%	79.03%
Auxiliary Charge	20.92%	17.7%	25.03%	100%	00%	100%
No Conviction or Arraignment	33.44%	45.53%	18.03%	30.93%	34.1%	49.45%
Misdemeanor Conviction	3.35%	0.42%	7.08%	4.39%	3.07%	1.57%
Felony Arraignment	63.21%	54.05%	74.89%	64.68%	62.82%	48.98%

Notes: This table presents summary statistics of the main analysis sample. The first column includes the entire sample. The second and third columns subset to narcotics and non-narcotics cases, respectively. The fourth and fifth columns subset to cases with and without auxiliary charges, respectively. The sixth column is cases that include both a felony narcotics charge and an auxiliary charge.

Table 3: Auxiliary Charge Effects Interacted With Felony Case Types

Charge Category	Narcotics			
Change in Felonies	-.0333			
SE	(.0051)			
Charge Category	Murder	Sexual Assault	Assault	Robbery
Change in Felonies	-.0441	-.1206	-.0275	-.0074
SE	(.0521)	(.0452)	(.0143)	(.0135)
Charge Category	Burglary	Theft	Weapons	Other
Change in Felonies	-.0003	-.006	.0204	-.0012
SE	(.012)	(.0103)	(.0072)	(.0091)

Notes: This table presents the change in observable charging outcomes due to auxiliary misdemeanor charge interacted with various felony case types. This begins with a multinomial logit estimation of three mutually exclusive and exhaustive categories: No arraignment or conviction (N); Misdemeanor Conviction (M); and Felony Arraignment (F). This estimation controls for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. Except when the case type under consideration is narcotics charges, narcotics charges are excluded from the estimation sample. The estimate for each case type also includes an interaction between that case type and having an auxilliary misdemeanor term. The raw estimates presented are the marginal effect of the interaction term on felony arraignment within the sample of defendants with that case type.

Table 4: Charge Outcomes

Sample	Raw Change in N	Adjusted Change in N	Raw Change in M	Adjusted Change in M	Raw Change in F	Adjusted Change in F
All	.023	[-.046, -.011]	.01	[.053, .071]	-.033	[-.043, -.023]
(SE)	(.005)		(.001)		(.005)	
Black	.045	[-.03, .011]	.011	[.055, .076]	-.056	[-.068, -.044]
(SE)	(.006)		(.001)		(.006)	
Nonblack	-.021	[-.098, -.033]	.009	[.036, .071]	.012	[-.006, .03]
(SE)	(.009)		(.001)		(.009)	
Priors	.032	[-.041, .003]	.011	[.052, .073]	-.043	[-.056, -.03]
(SE)	(.007)		(.001)		(.007)	
No Priors	.006	[-.072, -.015]	.009	[.043, .073]	-.015	[-.03, 0]
(SE)	(.008)		(.001)		(.008)	

Notes: This table presents the change in observable charging outcomes due to an auxiliary misdemeanor charge interacted with a felony narcotics charge. This begins with a multinomial logit estimation of three mutually exclusive and exhaustive categories: No arraignment or conviction (N); Misdemeanor Conviction (M); and Felony Arraignment (F). This estimation controls for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. It also includes an interaction between the case being a nacrotic drug case and having an auxilliary misdemeanor term. The raw estimates presented are the marginal effect of the interaction term on each charging outcome within the sample of narcotics defendants. The adjusted estimates account for structured nonconvictions in the misdemeanor courts under the assumption that formal conviction rates in cases that begin as felonies are equal to formal conviction rates (in the same sample) for all-misdemeanor cases.

Table 5: D-I-D Results: Subsamples

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0100**	0.0131**	0.00179	0.00286	0.0198***	0.0230*	0.00848*
	(0.00363)	(0.00436)	(0.00651)	(0.00481)	(0.00541)	(0.0108)	(0.00386)
NarcoticxAux Misd.	-0.00886	-0.0163**	0.0107	-0.000177	-0.0229**	-0.0245*	0.00311
	(0.00505)	(0.00602)	(0.00928)	(0.00657)	(0.00785)	(0.0115)	(0.00846)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. All standard errors are clustered at the defendant level.

Table 6: D-I-D Results: Recidivism Definition

Panel A: Felony Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00850**	0.0119**	0.00118	0.00578	0.0130**	0.0107	0.00874**
	(0.00320)	(0.00393)	(0.00528)	(0.00444)	(0.00433)	(0.00989)	(0.00336)
NarcoticxAux Misd.	-0.00745	-0.0124*	0.00710	-0.00324	-0.0138*	-0.00830	-0.0120
	(0.00455)	(0.00561)	(0.00742)	(0.00623)	(0.00633)	(0.0105)	(0.00759)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: 4-Year Misdemeanor Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0128***	0.0107*	0.0126	0.00401	0.0242***	-0.00912	0.0156***
	(0.00369)	(0.00437)	(0.00684)	(0.00476)	(0.00576)	(0.0106)	(0.00396)
NarcoticxAux Misd.	0.0168***	0.0111	0.0307**	0.0265***	0.000294	0.0413***	0.00494
	(0.00508)	(0.00598)	(0.00955)	(0.00649)	(0.00806)	(0.0113)	(0.00844)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Misdemeanor Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00712*	0.0102*	-0.00276	0.00649	0.00745	-0.00357	0.00905*
	(0.00349)	(0.00425)	(0.00594)	(0.00465)	(0.00517)	(0.0104)	(0.00371)
NarcoticxAux Misd.	0.00774	0.00236	0.0226**	0.00960	0.00395	0.0213	-0.00446
	(0.00485)	(0.00592)	(0.00814)	(0.00641)	(0.00725)	(0.0111)	(0.00809)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: 4-Year Drug Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00508	0.00665	0.00572	0.00333	0.00791*	0.0208*	0.00307
	(0.00287)	(0.00359)	(0.00440)	(0.00399)	(0.00392)	(0.01000)	(0.00297)
NarcoticxAux Misd.	-0.0154***	-0.0211***	-0.00266	-0.0113	-0.0228***	-0.0343**	-0.00175
	(0.00449)	(0.00550)	(0.00742)	(0.00604)	(0.00654)	(0.0107)	(0.00782)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel E: 4-Year Violent Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00139	0.00350	-0.00376	-0.00240	0.00824*	0.000184	0.00102
	(0.00238)	(0.00296)	(0.00379)	(0.00319)	(0.00352)	(0.00711)	(0.00254)
NarcoticxAux Misd.	0.00278	0.000135	0.00828	0.00783	-0.00621	0.00378	0.00372
	(0.00318)	(0.00398)	(0.00483)	(0.00429)	(0.00459)	(0.00752)	(0.00517)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel F: 4-Year Property Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	-0.000569	-0.000150	-0.00349	-0.00460	0.00290	0.00990	-0.00199
	(0.00210)	(0.00246)	(0.00407)	(0.00287)	(0.00291)	(0.00543)	(0.00228)
NarcoticxAux Misd.	0.00441	0.00182	0.0117*	0.00751*	0.000859	-0.00616	0.00634
	(0.00281)	(0.00326)	(0.00553)	(0.00381)	(0.00390)	(0.00583)	(0.00404)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different recidivism outcome variable. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. All standard errors are clustered at the defendant level.

Table 7: D-I-D Results: Auxiliary Charge Type

Panel A: Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00637	0.00120	0.0284*	0.0135	-0.00183	-0.00403	0.00792
	(0.00654)	(0.00746)	(0.0136)	(0.00852)	(0.0101)	(0.0137)	(0.00747)
NarcoticxAux Misd.	-0.0141	-0.0170	-0.0139	-0.0171	-0.0136	-0.00788	0.00472
	(0.00802)	(0.00927)	(0.0160)	(0.0104)	(0.0124)	(0.0147)	(0.0123)
Observations	203850	151391	52459	129178	74672	94063	109787

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: Non-Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0124**	0.0178***	-0.00127	0.00307	0.0248***	0.0431***	0.00966*
	(0.00391)	(0.00473)	(0.00691)	(0.00522)	(0.00578)	(0.0129)	(0.00411)
NarcoticxAux Misd.	-0.00146	-0.00928	0.0165	0.00534	-0.0117	-0.0344*	0.00509
	(0.00600)	(0.00715)	(0.0111)	(0.00774)	(0.00950)	(0.0138)	(0.0109)
Observations	219087	161948	57139	138011	81076	94519	124568

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Contraband

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00744	0.00317	0.0183	-0.00502	0.0212**	-0.0148	0.0121*
	(0.00532)	(0.00606)	(0.0111)	(0.00708)	(0.00797)	(0.0134)	(0.00580)
NarcoticxAux Misd.	-0.0426***	-0.0391***	-0.0476**	-0.0249**	-0.0652***	-0.0300*	-0.0114
	(0.00729)	(0.00836)	(0.0149)	(0.00962)	(0.0111)	(0.0145)	(0.0115)
Observations	207905	155256	52649	130784	77121	92304	115601

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: Non-Contraband

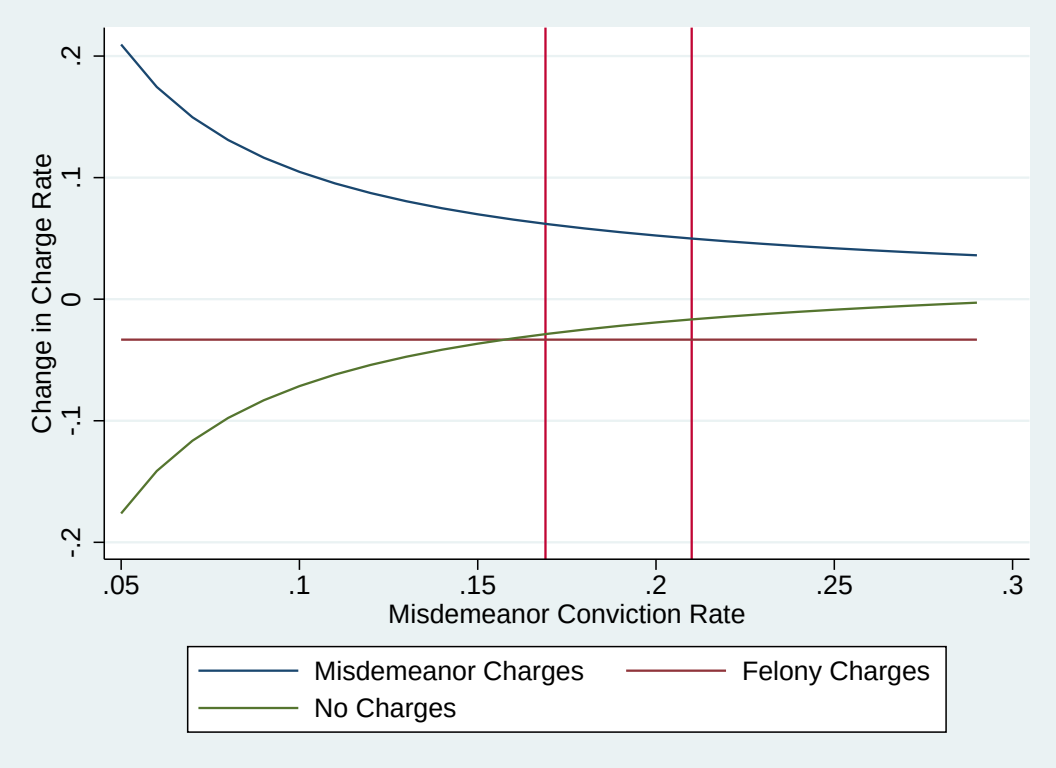
	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0124**	0.0200***	-0.00306	0.00774	0.0201**	0.0473***	0.00859*
	(0.00414)	(0.00508)	(0.00711)	(0.00551)	(0.00615)	(0.0134)	(0.00437)
NarcoticxAux Misd.	0.0112	-0.000734	0.0343**	0.0128	0.00664	-0.0244	0.0128
	(0.00598)	(0.00725)	(0.0105)	(0.00772)	(0.00941)	(0.0142)	(0.0108)
Observations	216025	158802	57223	136855	79170	96096	119929

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

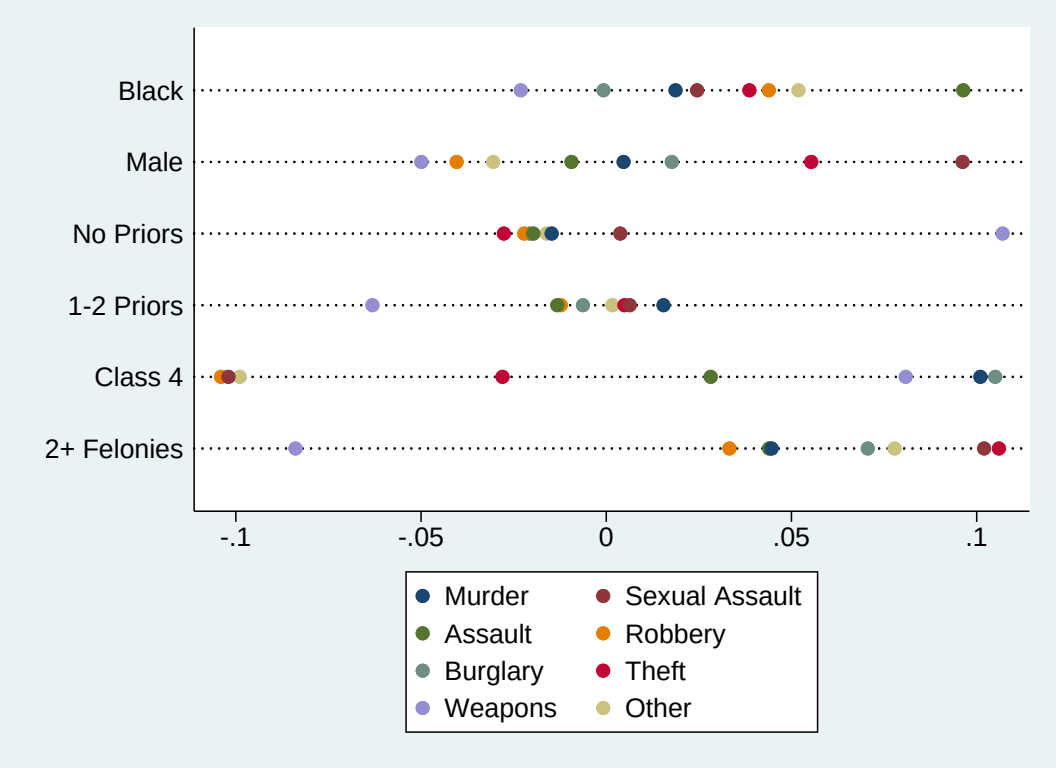
Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different definition of auxiliary charge. Auxiliary charges that do not match the definition in a given panel are dropped. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. All standard errors are clustered at the defendant level.

Figure 1: Charge Outcomes by Misdemeanor Conviction Rate



Notes: This figure combines the results from Table 4 with assumptions about the misdemeanor conviction rate among cases prosecuted as misdemeanors which began with at least one felony charge. For each hypothetical value of the misdemeanor conviction rate, it calculates the change in each of the three charging treatments induced by an auxiliary misdemeanor in narcotics cases relative to non-narcotics cases. The observed formal conviction rate in misdemeanor-only cases is shown by the leftmost vertical line. The rightmost vertical line adjusts this to a hypothetical where prosecutors drop misdemeanor cases as often as they do felony cases.

Figure 2: Covariate-Crime Category Interactions in Auxiliary Charge Predictions



Notes: This figure depicts the coefficients on interaction terms between covariates (specified by y-axis labels) and crime category (specified by dot color) in a regression that predicts the presence of auxiliary charge. They therefore capture the crime-category-specific ability of each covariate to predict an auxiliary charge. Each line summarizes a separate regression. In each regression, narcotics is the omitted category, so dots should be interpreted as the difference between narcotics and the listed crime category. Coefficients less than -0.1 and greater than 0.1 have been bottom- and topcoded, respectively.

Appendix A: Additional Results

Table A1: No Interactions: Charge Outcomes

Sample	Raw Change in N	Adjusted Change in N	Raw Change in M	Adjusted Change in M	Raw Change in F	Adjusted Change in F
All	.053	[-.02, .016]	.011	[.058, .075]	-.065	[-.075, -.054]
(SE)	(.005)		(.001)		(.005)	
Black	.076	[-.005, .038]	.012	[.061, .082]	-.088	[-.101, -.076]
(SE)	(.006)		(.001)		(.006)	
Nonblack	-.008	[-.084, -.019]	.008	[.035, .068]	0	[-.019, .018]
(SE)	(.01)		(.001)		(.009)	
Priors	.06	[-.021, .025]	.013	[.059, .082]	-.072	[-.086, -.059]
(SE)	(.007)		(.001)		(.007)	
No Priors	.033	[-.046, .012]	.009	[.043, .073]	-.042	[-.058, -.026]
(SE)	(.008)		(.001)		(.008)	

Notes: This table presents the change in observable charging outcomes due to an auxiliary misdemeanor charge interacted with a felony narcotics charge. This begins with a multinomial logit estimation of three mutually exclusive and exhaustive categories: No arraignment or conviction (N); Misdemeanor Conviction (M); and Felony Arraignment (F). This estimation controls for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district. It also includes an interaction between the case being a nacrotic drug case and having an auxilliary misdemeanor term. The raw estimates presented are the marginal effect of the interaction term on each charging outcome within the sample of narcotics defendants.. The adjusted estimates account for structured nonconvictions in the misdemeanor courts under the assumption that formal conviction rates in cases that begin as felonies are equal to formal conviction rates (in the same sample) for all-misdemeanor cases.

Table A2: No Interactions: Subsamples

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0302***	0.0318***	0.0183**	0.0250***	0.0342***	0.0338**	0.0254***
	(0.00378)	(0.00468)	(0.00661)	(0.00510)	(0.00557)	(0.0108)	(0.00406)
NarcoticxAux Misd.	-0.0155**	-0.0234***	0.00807	-0.00712	-0.0277***	-0.0175	0.000929
	(0.00501)	(0.00596)	(0.00923)	(0.00652)	(0.00780)	(0.0113)	(0.00835)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district. All standard errors are clustered at the defendant level.

Table A3: No Interactions: Recidivism Definition

Panel A: Felony Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0198***	0.0269***	0.00836	0.0210***	0.0192***	0.0157	0.0184***
	(0.00332)	(0.00421)	(0.00536)	(0.00469)	(0.00445)	(0.00987)	(0.00353)
NarcoticxAux Misd.	-0.0130**	-0.0196***	0.00527	-0.0105	-0.0166**	-0.00425	-0.0162*
	(0.00451)	(0.00556)	(0.00737)	(0.00617)	(0.00630)	(0.0103)	(0.00748)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: 4-Year Misdemeanor Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0479***	0.0445***	0.0409***	0.0387***	0.0554***	0.0173	0.0480***
	(0.00385)	(0.00471)	(0.00694)	(0.00507)	(0.00591)	(0.0105)	(0.00417)
NarcoticxAux Misd.	0.0105*	0.00510	0.0240*	0.0227***	-0.00788	0.0456***	0.0104
	(0.00504)	(0.00593)	(0.00949)	(0.00644)	(0.00800)	(0.0111)	(0.00832)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Misdemeanor Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0277***	0.0339***	0.0128*	0.0306***	0.0233***	0.0126	0.0278***
	(0.00361)	(0.00455)	(0.00599)	(0.00490)	(0.00527)	(0.0104)	(0.00388)
NarcoticxAux Misd.	0.00326	-0.00344	0.0207*	0.00550	-0.000839	0.0249*	-0.00474
	(0.00481)	(0.00587)	(0.00809)	(0.00636)	(0.00720)	(0.0109)	(0.00798)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: 4-Year Drug Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0185***	0.0231***	0.0154***	0.0199***	0.0164***	0.0308**	0.0126***
	(0.00302)	(0.00390)	(0.00450)	(0.00426)	(0.00405)	(0.0100)	(0.00313)
NarcoticxAux Misd.	-0.0187***	-0.0238***	-0.00249	-0.0142*	-0.0247***	-0.0289**	-0.00476
	(0.00445)	(0.00544)	(0.00738)	(0.00599)	(0.00649)	(0.0106)	(0.00770)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel E: 4-Year Violent Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00336	0.00369	-0.000259	-0.000793	0.0105**	-0.000847	0.00309
	(0.00245)	(0.00314)	(0.00382)	(0.00334)	(0.00355)	(0.00708)	(0.00263)
NarcoticxAux Misd.	0.00325	0.00124	0.00784	0.00957*	-0.00748	0.00565	0.00706
	(0.00316)	(0.00395)	(0.00483)	(0.00426)	(0.00457)	(0.00739)	(0.00509)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel F: 4-Year Property Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0109***	0.0119***	0.00469	0.0105***	0.00870**	0.0200***	0.00878***
	(0.00216)	(0.00259)	(0.00408)	(0.00300)	(0.00295)	(0.00545)	(0.00236)
NarcoticxAux Misd.	-0.000202	-0.00443	0.00941	0.00110	-0.00104	-0.00710	0.00287
	(0.00278)	(0.00322)	(0.00548)	(0.00377)	(0.00387)	(0.00577)	(0.00402)
Observations	234355	173341	61014	147427	86928	104367	129988

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different recidivism outcome variable. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district. All standard errors are clustered at the defendant level.

Table A4: No Interactions: Auxiliary Charge Type

Panel A: Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0230***	0.0178*	0.0384**	0.0317***	0.00936	0.00796	0.0232**
	(0.00663)	(0.00770)	(0.0136)	(0.00870)	(0.0102)	(0.0138)	(0.00757)
NarcoticxAux Misd.	-0.0169*	-0.0227*	-0.00854	-0.0198	-0.0140	-0.00161	0.00238
	(0.00798)	(0.00923)	(0.0160)	(0.0104)	(0.0123)	(0.0145)	(0.0122)
Observations	203850	151391	52459	129178	74672	94063	109787

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: Non-Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0356***	0.0395***	0.0175*	0.0286***	0.0417***	0.0573***	0.0293***
	(0.00409)	(0.00511)	(0.00704)	(0.00555)	(0.00597)	(0.0127)	(0.00435)
NarcoticxAux Misd.	-0.00759	-0.0150*	0.0128	-0.00127	-0.0173	-0.0289*	0.00772
	(0.00596)	(0.00708)	(0.0110)	(0.00767)	(0.00945)	(0.0134)	(0.0108)
Observations	219087	161948	57139	138011	81076	94519	124568

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Contraband

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0283***	0.0218***	0.0369***	0.0180*	0.0360***	0.00270	0.0279***
	(0.00545)	(0.00637)	(0.0112)	(0.00735)	(0.00811)	(0.0134)	(0.00597)
NarcoticxAux Misd.	-0.0490***	-0.0478***	-0.0512***	-0.0322***	-0.0677***	-0.0294*	-0.0115
	(0.00723)	(0.00828)	(0.0148)	(0.00958)	(0.0110)	(0.0144)	(0.0114)
Observations	207905	155256	52649	130784	77121	92304	115601

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: Non-Contraband

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0354***	0.0419***	0.0145*	0.0336***	0.0364***	0.0596***	0.0287***
	(0.00432)	(0.00546)	(0.00723)	(0.00583)	(0.00633)	(0.0131)	(0.00460)
NarcoticxAux Misd.	0.00435	-0.00672	0.0325**	0.00536	0.000692	-0.0165	0.0120
	(0.00593)	(0.00719)	(0.0105)	(0.00765)	(0.00937)	(0.0137)	(0.0106)
Observations	216025	158802	57223	136855	79170	96096	119929

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different definition of auxiliary charge. Auxiliary charges that do not match the definition in a given panel are dropped. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district. All standard errors are clustered at the defendant level.

Table A5: No Sexual Assault; Possession Only: Charge Outcomes

Sample	Raw Change in N	Adjusted Change in N	Raw Change in M	Adjusted Change in M	Raw Change in F	Adjusted Change in F
All	.011	[-.066, -.028]	.012	[.06, .081]	-.023	[-.034, -.013]
(SE)	(.005)		(.001)		(.005)	
Black	.031	[-.055, -.009]	.013	[.063, .088]	-.044	[-.057, -.031]
(SE)	(.007)		(.001)		(.007)	
Nonblack	-.024	[-.105, -.038]	.009	[.038, .075]	.015	[-.003, .033]
(SE)	(.01)		(.002)		(.009)	
Priors	.022	[-.062, -.013]	.013	[.06, .086]	-.036	[-.05, -.022]
(SE)	(.007)		(.001)		(.007)	
No Priors	-.007	[-.09, -.029]	.01	[.045, .079]	-.003	[-.019, .013]
(SE)	(.008)		(.001)		(.008)	

Notes: This table presents the change in observable charging outcomes due to an auxiliary misdemeanor charge interacted with a felony narcotics charge. This begins with a multinomial logit estimation of three mutually exclusive and exhaustive categories: No arraignment or conviction (N); Misdemeanor Conviction (M); and Felony Arraignment (F). This estimation controls for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. It also includes an interaction between the case being a nacrotic drug case and having an auxilliary misdemeanor term. Non-possession cases are dropped from the narcotics sample, and sexual assault cases are dropped from the comparison sample. The raw estimates presented are the marginal effect of the interaction term on each charging outcome within the sample of narcotics defendants. The adjusted estimates account for structured nonconvictions in the misdemeanor courts under the assumption that formal conviction rates in cases that begin as felonies are equal to formal conviction rates (in the same sample) for all-misdemeanor cases.

Table A6: No Sexual Assault; Possession Only: Subsamples

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0118**	0.0145***	0.00457	0.00409	0.0218***	0.0235*	0.0102**
	(0.00365)	(0.00438)	(0.00654)	(0.00483)	(0.00543)	(0.0109)	(0.00387)
NarcoticxAux Misd.	-0.0122*	-0.0196**	0.00637	-0.00230	-0.0270***	-0.0246*	0.00192
	(0.00528)	(0.00633)	(0.00954)	(0.00690)	(0.00813)	(0.0116)	(0.0135)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. Non-possession cases are dropped from the narcotics sample, and sexual assault cases are dropped from the comparison sample. All standard errors are clustered at the defendant level.

Table A7: No Sexual Assault; Possession Only: Recidivism Definition

Panel A: Felony Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00949**	0.0125**	0.00303	0.00655	0.0141**	0.0113	0.00972**
	(0.00321)	(0.00394)	(0.00531)	(0.00445)	(0.00435)	(0.00991)	(0.00338)
NarcoticxAux Misd.	-0.00666	-0.0108	0.00651	-0.000796	-0.0150*	-0.00893	0.00160
	(0.00476)	(0.00592)	(0.00763)	(0.00657)	(0.00654)	(0.0105)	(0.0118)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: 4-Year Misdemeanor Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0144***	0.0118**	0.0154*	0.00502	0.0260***	-0.00935	0.0172***
	(0.00370)	(0.00439)	(0.00687)	(0.00478)	(0.00578)	(0.0106)	(0.00397)
NarcoticxAux Misd.	0.0187***	0.0130*	0.0299**	0.0278***	0.00392	0.0418***	0.0213
	(0.00531)	(0.00629)	(0.00983)	(0.00682)	(0.00836)	(0.0113)	(0.0134)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Misdemeanor Recidivism Years 3-4

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00847*	0.0113**	-0.000675	0.00776	0.00864	-0.00328	0.0104**
	(0.00350)	(0.00426)	(0.00598)	(0.00467)	(0.00519)	(0.0105)	(0.00373)
NarcoticxAux Misd.	0.00974	0.00439	0.0234**	0.0121	0.00568	0.0221*	-0.00108
	(0.00506)	(0.00624)	(0.00838)	(0.00675)	(0.00751)	(0.0111)	(0.0126)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: 4-Year Drug Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00581*	0.00723*	0.00711	0.00378	0.00875*	0.0210*	0.00386
	(0.00289)	(0.00360)	(0.00443)	(0.00401)	(0.00393)	(0.0100)	(0.00298)
NarcoticxAux Misd.	-0.0179***	-0.0229***	-0.00618	-0.0138*	-0.0240***	-0.0341**	0.00113
	(0.00472)	(0.00584)	(0.00765)	(0.00641)	(0.00680)	(0.0107)	(0.0124)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel E: 4-Year Violent Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00317	0.00490	-0.00108	-0.000670	0.00975**	0.000676	0.00283
	(0.00239)	(0.00296)	(0.00379)	(0.00319)	(0.00352)	(0.00713)	(0.00254)
NarcoticxAux Misd.	0.000344	-0.00239	0.00541	0.00536	-0.00810	0.00260	0.00275
	(0.00330)	(0.00418)	(0.00490)	(0.00449)	(0.00469)	(0.00753)	(0.00795)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel F: 4-Year Property Felony Recidivism

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	-0.000798	-0.000362	-0.00388	-0.00493	0.00282	0.00966	-0.00222
	(0.00211)	(0.00246)	(0.00410)	(0.00289)	(0.00292)	(0.00543)	(0.00229)
NarcoticxAux Misd.	0.00487	0.00183	0.0116*	0.00823*	0.000879	-0.00624	0.0126*
	(0.00296)	(0.00346)	(0.00569)	(0.00406)	(0.00404)	(0.00584)	(0.00627)
Observations	202374	145018	57356	123912	78462	102783	99591

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different recidivism outcome variable. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. Non-possession cases are dropped from the narcotics sample, and sexual assault cases are dropped from the comparison sample. All standard errors are clustered at the defendant level.

Table A8: No Sexual Assault; Possession Only: Auxiliary Charge Type

Panel A: Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00632	0.00103	0.0287*	0.0132	-0.00136	-0.00397	0.00763
	(0.00654)	(0.00746)	(0.0136)	(0.00853)	(0.0101)	(0.0137)	(0.00748)
NarcoticxAux Misd.	-0.0169*	-0.0210*	-0.0143	-0.0191	-0.0173	-0.00863	0.0116
	(0.00827)	(0.00962)	(0.0163)	(0.0108)	(0.0127)	(0.0147)	(0.0182)
Observations	173459	124393	49066	106804	66655	92583	80876

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel B: Non-Drug

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0145***	0.0196***	0.00162	0.00471	0.0269***	0.0440***	0.0117**
	(0.00393)	(0.00475)	(0.00694)	(0.00524)	(0.00580)	(0.0129)	(0.00413)
NarcoticxAux Misd.	-0.00389	-0.0105	0.0111	0.00377	-0.0136	-0.0341*	0.00662
	(0.00629)	(0.00755)	(0.0114)	(0.00815)	(0.00987)	(0.0139)	(0.0172)
Observations	188795	134957	53838	115662	73133	93089	95706

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel C: Contraband

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.00854	0.00409	0.0198	-0.00411	0.0220**	-0.0142	0.0133*
	(0.00532)	(0.00606)	(0.0111)	(0.00708)	(0.00798)	(0.0134)	(0.00580)
NarcoticxAux Misd.	-0.0484***	-0.0460***	-0.0487**	-0.0304**	-0.0704***	-0.0314*	0.00109
	(0.00762)	(0.00879)	(0.0153)	(0.0101)	(0.0115)	(0.0146)	(0.0172)
Observations	177739	128381	49358	108597	69142	90895	86844

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel D: Non-Contraband

	Full Sample	Black	Nonblack	Prior	No Prior	Class 4	Not Class 4
Aux Misd.	0.0141***	0.0215***	-0.000423	0.00909	0.0220***	0.0482***	0.0102*
	(0.00416)	(0.00510)	(0.00715)	(0.00554)	(0.00617)	(0.0134)	(0.00439)
NarcoticxAux Misd.	0.00975	-0.000982	0.0298**	0.0124	0.00476	-0.0239	0.00936
	(0.00624)	(0.00762)	(0.0108)	(0.00809)	(0.00973)	(0.0142)	(0.0176)
Observations	185485	131687	53798	114320	71165	94599	90886

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates the effect of an auxiliary charge on 4-year felony recidivism among narcotics cases relative to non-narcotics cases for various subsamples. Each panel uses a different definition of auxiliary charge. Auxiliary charges that do not match the definition in a given panel are dropped. The first row is the main effect of auxiliary charges for non-narcotics defendants. The second row is the effect of auxiliary charges for narcotics defendants relative to the effect in the first row. All models control for year and month fixed effects, race, sex, age, prior felony charges, indicator for more than 1 felony charge, felony class and category, and police district, all interacted with narcotics case. Non-possession cases are dropped from the narcotics sample, and sexual assault cases are dropped from the comparison sample. All standard errors are clustered at the defendant level.