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ABSTRACT

Using quasi-random assignment of criminal cases to judges, we estimate large incarceration spillovers in criminal and brother networks. When a defendant is sent to prison, there are 51 and 32 percentage point reductions in the probability his criminal network members and younger brothers will be charged with a crime, respectively, over the ensuing four years. Correlational evidence misleadingly finds small positive effects. These spillovers are of first order importance for policy, as the network reductions in future crimes committed are larger than the direct effect on the incarcerated defendant.

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I. Introduction

The long-run consequences of incarceration depend not only on an inmate's recidivism, but also on any spillover effects they have on an inmate's criminal and family networks. Capturing these peer effects are important for evaluating criminal justice policy and designing effective prison systems, as the societal costs and benefits of incarceration could be magnified or muted once network effects are taken into account. Spillover effects are particularly relevant from a policy perspective, as incarceration rates in both the U.S. and other OECD countries are currently near all-time highs (World Prison Brief, 2016).

Social scientists and policymakers have long been interested in understanding the effect of peers on the behavior of other members in a network.[1] In the area of crime, there are several studies which document positive associations in criminal activity in neighborhoods and families, and a growing literature which looks at peer effects when neighborhoods, schools or cellmates are quasi-randomly assigned. There is also an emerging literature on spillover effects within families which focuses on intergenerational links.[2] Our study contributes to the literature by providing the first quasi-experimental evidence for incarceration spillovers within pre-existing, endogenously chosen criminal networks. We also provide new evidence for brothers, adding to recent work on incarceration spillovers between parents and children.

Estimating the spillover effects of incarceration is difficult for two reasons: data availability and bias from either correlated unobservables or simultaneity. The data requirements to estimate spillover effects are daunting, as the researcher not only needs to be able to identify and link criminal groups and brothers, but also follow these network members over time. Correlated unobservables creates a bias because common environmental or demographic factors are likely to drive both higher incarceration and criminal activity within a network,

[1] For example, peer effects have been studied in the context of college achievement (Carrell, Fullerton and West 2009, Sacerdote 2001), paternity leave (Dahl, Løken and Mogstad 2014), DI takeup (Dahl, Kostøl and Mogstad 2014), workplace productivity (Cornelissen, Dustmann and Schonberg 2017, Mas and Moretti 2009, Falk and Ichino 2006), financial decisions (Bursztyn, Ederer, Ferman and Yuchtman 2014, Duflo and Saez 2002), consumption (Kuhn, Kooreman, Soetevent and Kapteyn 2011) and technology adoption (Bandiera and Rasul 2006).

[2] For correlational evidence, see for example, Akee, Copeland, Keeler, Angold and Costello (2010), Case and Katz (1991), Duncan, Kalil, Mayer, Tepper and Payne (2005), Hjalmarsson and Lindquist (2012), Meghir, Palme and Schnabel (2012) and Thorbherry (2009). For quasi-experimental estimates of random assignment to a peer group, see Bayer, Hjalmarsson and Pozen (2009), Billings, Ross and Demming (2016), Billings and Hoekstra (2018), Billings and Schnepel (2017), Damm and Dustmann (2014), Ludwig, Duncan and Hirschfield (2001), Kling, Ludwig and Katz (2005) and Rotger and Galster (2017). For spillover effects from parents to children and among siblings, see Bhuller, Dahl, Løken and Mogstad (2018), Billings (2018), Dobbie, Grönqvist, Niknami, Palme and Priks (2017), Norris, Pecenco and Weaver (2018), and Wildeman and Andersen (2017). For effects among criminal groups, see Phillipe (2017), which uses fixed effects, and Lindquist and Zenou (2014), which uses intransitive triads and variation in the number of network links as instruments for key network players.

and simultaneity bias arises because it is difficult to identify who is affecting who in a network.

Our paper overcomes these challenges in the context of Norway's criminal justice system. First, we merge several administrative data sources to construct a census of all crimes, criminal court cases and incarceration spells in Norway for the period 2005 to 2016. We then link these records for all defendants in Norway to similarly rich panel data for criminal network members and brothers. To identify causal effects, we take advantage of the random assignment of criminal cases to judges who systematically vary in how likely they are to send a defendant to prison. We utilize this exogenous variation to examine whether a defendant's incarceration affects the probability their other network members will be charged with a crime in the future.[3]

Our measure of judge stringency is the average incarceration rate in all other cases a judge handles. This stringency measure serves as an instrument for the defendant's incarceration since it is highly predictive of the judge's decision in the current case, but as we document, uncorrelated with observable case characteristics of the defendant and the characteristics of other members in their network. We define criminal groups based on the existence of a network link up to third order for joint criminal charges in the past (excluding the defendant's current case), although the results are robust to restricting peers to only first order links (i.e., direct co-offenders in a prior criminal case).

Our paper offers three main empirical results. First, using judge stringency as an instrument, we find large network spillover effects. When a criminal network member is incarcerated, their peer's probability of being charged with a future crime *decreases* by 51 percentage points over the next 4 years. Likewise, having an older brother incarcerated *reduces* the probability his younger brother will be charged with a crime by 32 percentage points over the next 4 years.

Second, these peer effects are concentrated in networks where the links between individuals are likely to be active and salient. The criminal network spillovers are driven by peers with strong potential ties to the incarcerated defendant, defined as living geographically close and having network ties for crimes committed recently. Peers who are less likely to be in contact with each other are unaffected. For the brother network, the spillover passes only from older to younger brothers, and not the other way around. More generally, we find no spillover effects for other family members such as sisters, children and spouses.[4]

[3] Random judge designs have been used in other contexts as well; for example, see Autor, Mogstad and Kostøl (2015), Belloni, Chen, Chernozhukov and Hansen (2012), Dahl, Kostøl and Mogstad (2014), Dobbie and Song (2015), Doyle (2007, 2008), French and Song (2014) and Maestas, Mullen and Strand (2013).

[4] In a completely different context, Dahl, Løken and Mogstad (2014) find similar patterns for more versus less salient peer networks: strong peer effects in paternity leave take-up for co-workers and brothers, but not for neighbors or other family members.

Third, bias due to selection on unobservables matters. OLS yields modest and positively-signed spillover effects for both networks, even after including an extensive set of controls. Taken at face value, OLS leads to the erroneous conclusion that incarceration either has no effect or slightly increases future crime within networks, whereas the IV estimates which control for selection bias finds that incarceration of a defendant has a strong preventative effect on their network peers.

Turning to possible mechanisms, it is important to understand the effect of incarceration on the defendant. In our data, mean prison time served is 4.8 months, with approximately 90% of defendants serving less than one year. So while the incapacitive effect of removing the defendant from their network can help explain a short term effect, it cannot explain the effects we observe up to four years later. The reduction in crime is not driven by a reduced probability of committing a new crime together (which happens only rarely), suggesting it is the positive peer influence of the defendant more generally.

These empirical findings have important implications for policy. In Bhuller, Dahl, Løken, and Mogstad (2016), we document that defendants sent to prison commit fewer crimes, participate in job training programs at a higher rate, and increase their employment post release. This rehabilitative effect is likely due in part to investments in education and training programs, but also through the extensive use of "open prisons" in which prisoners are housed in low-security surroundings, given their own rooms for safety and privacy, and allowed frequent family visits. In comparison, in many other countries rehabilitation has taken a back seat in favor of prison policies emphasizing punishment and incapacitation. In the current paper, we show that incarceration positively changes the behavior not only of the defendant, but also of members of their network. A policy simulation which increases average judge stringency by 1 standard deviation illustrates the policy relevance of these spillover effets. This simulation makes clear that failing to account for incarceration spillover effects will provide misleading projections of total policy impact and post-reform recidivism rates, as the network reductions in future crimes committed are larger than the direct effect on the incarcerated defendant.

The remainder of the paper proceeds as follows. Section II describes our research design, setting and data. In Section III, we assess the instrument and interpret the treatment. Section IV presents and interprets our main findings on network spillover effects. The final section offers some concluding remarks on the importance of network spillovers for public policy.

II. Research Design, Setting and Data

In Bhuller, Dahl, Løken, and Mogstad (2016, hereafter BDLM), we take advantage of a similar research design and data to estimate the effect of incarceration on the defendant. That paper serves as the backdrop to our current paper, as it is important to know the effect of incarceration on the defendant to help interpret any spillover effects on their network members. This section describes our research design and data, copying some of the most relevant information from BDLM. While further details can be found there, here we highlight how the quasi-random assignment of judges, combined with the richness of our data, can be used to estimate network spillover effects. We also provide a brief background on prison conditions in Norway, with a comparison to other Western European countries and the U.S.

II.A. Setting

We study spillover effects within the criminal justice system of Norway. If the police suspect an individual of a crime, they file a formal report. A public prosecutor then decides whether the individual should be charged with a crime as well as whether the case should proceed to a court trial. About half of police reports lead to a formal criminal charge. Of these charged cases, the public prosecutor advances approximately 40% of them to a trial. The other charged cases are either dismissed, directly assigned a fine, or sent to mediation by the public prosecutor.

Of the cases which proceed to trial, approximately 60% are non-confession cases, while the remaining are cases where the defendant has confessed to the charges filed by the public prosecutor.[5] We focus on non-confession cases in this paper. Once a case proceeds to trial, it is assigned to a judge. If the judge finds the accused guilty, he or she can assign a combination of possible punishments which are not necessarily mutually exclusive. Slightly over half of cases result in incarceration, with probation, community service and fines combined accounting for 44% of outcomes. In a small fraction of cases (5%), the defendant is found not guilty. If multiple individuals are charged in the same case, they take part in the same trial, but can have different charges and different sentences depending on their role in the crime.

The law in Norway dictates that cases are assigned to judges according to the principle of randomization (Bohn, 2000; NOU, 2002). There are a few exceptions, such as for especially severe crimes or cases involving juveniles, which we exclude from our sample. To have a sample of randomly assigned cases for the same pool of judges, we limit our sample to regular judges handling non-confession cases. Regular judges are permanent civil servants (versus

[5] A defendant chooses whether to confess prior to knowing who their assigned judge will be. The absence of plea bargaining makes the interpretation of our IV estimates easier (see Dobbie, Goldin and Yang 2018).

deputy judges who generally serve for a limited 3 year term).[6]

We measure the strictness of a judge based on their incarceration rate for all other cases they have handled between 2005 and 2014. There are 597 judges, each of whom have presided over an average of 238 randomly assigned court cases. To construct our judge stringency measure, we calculate the leave-out mean judge incarceration rate conditional on fully interacted court and year fixed effects to account for the fact that randomization occurs within the pool of available judges.

II.B. Data

We use several administrative datasets which can be linked using individual identifiers. Information on all court cases between 2005 and 2014 comes from the Norwegian Courts Administration. We link this information with administrative data that contain complete records up to 2016 for all criminal charges, including the type of crime and date of a crime. We merge these datasets with administrative registers containing demographic information from Statistics Norway for every resident.

A key advantage of our register data is that we can link past (suspected or convicted) co-offenders to each other and brothers to each other. We define criminal networks based on prior co-offender links up to third order. This means that two co-offenders who were jointly charged in the same criminal case in the past (first order link) are defined to be in the same network, as are co-offenders of co-offenders (second order) and co-offenders of co-offenders of co-offenders (third order). To focus on networks which are likely to be active, our baseline sample limits the network to individuals living within a five mile radius of each other and who have a link which is less than 3 years old at the time of the court case. We further exclude co-offenders in the defendant's current case, so as to avoid network peers being directly affected by the strictness of the judge to which a defendant is assigned. In our baseline sample, 34% of the links within the criminal network are first order, while the remaining are second or third.[7]

Appendix Table A.1 reports descriptive statistics for defendants accused of a crime and brought to trial. There are a total of 53,855 randomly assigned non-confession criminal cases during 2005-2012, with 34,799 unique defendants. Of these, 6,967 cases have individuals who can be matched to a criminal network which is likely to be active and 29,871 cases can be paired to a brother.

[6] We further restrict the dataset to judges who handle at least 50 randomly assigned cases and to courts which have at least two regular judges in a given year. Our regression samples are limited to cases between 2005 and 2012 so that each defendant can be followed for four years.

[7] When we examine network spillovers separately by order of the link, the estimates remain large and statistically significant, but are not statistically different from each other.

Regardless of the sample, defendants facing potential prison time are a disadvantaged group: they have little education, low earnings, and high unemployment. Serial offenders are common, with almost 40% of defendants having been charged for a different crime in the previous year. There is also a mix of crime types. Around one fourth of cases involve violent crime, while property, economic, and drug crime each comprise a little more than 10 percent of crimes. Drunk driving, other traffic offenses and miscellaneous crime make up the remainder. Appendix Table A.1 also reports on peer group characteristics. On average, each defendant has just over 4 members in their criminal network (conditional on having at least one network member) and 1.7 brothers (conditional on having a at least one brother).

II.C. Prison Conditions and Prisoner Characteristics in Norway

To help with interpretation, we briefly describe prison conditions in Norway. Low-level offenders go directly to open prisons, which have minimal security, as well as more freedoms and responsibilities. Physically, these open prisons resemble dormitories rather than rows of cells with bars. More serious offenders are sent to closed prisons with heightened security. Norway has a strict policy of one prisoner per cell and tries to place prisoners close to home so that they can maintain links with the families. This means that there is often a waiting list for non-violent individuals before they can serve their prison time; in our data the average wait time is 5 months. To help with rehabilitation, prisons offer education, mental health, substance abuse and job training programs; if not enrolled in one of these programs, a prisoner is required to work within the prison. After release, there is an emphasis on helping offenders reintegrate into society and the labor market.

Comparing Norway to other countries reveals both similarities and differences.[8] One difference is the amount of money spent on prisoners, which is higher in Norway compared to the average for Western Europe and even higher compared to the average for the U.S. But these averages mask substantial heterogeneity both across countries and across U.S. states, in large part due to differences in labor costs (which are roughly two-thirds of prison budgets). Another difference is sentence length. Average prison spells are just over 6 months in Norway, and almost 90% are less than 1 year. This is considerably shorter compared to the average prison time of 2.9 years for the U.S., and fairly similar to the median of 6.8 months in other Western European countries. Because of this disparity in sentence lengths, the average cost per prisoner spell in Norway and Europe is smaller compared to the U.S., even though the cost per prisoner per year is higher.

[8] For cost estimates, see Aebi et al. (2015), Vera Institute for Justice (2012) and NYC Independent Budget Office (2013). For average prison spells see Pew Center (2011) and Aebi et al. (2015). For details on criminal populations, see Bureau of Justice Statistics (2015), Raphael and Stoll (2013), Kristoffersen (2014) and Aebi et al. (2015). More details can be found in BDLM.

Norway's prison population looks broadly similar to other Western European nations and the U.S., both in terms of demographics and the types of crimes committed. And while it shares some commonalities with the U.S., the U.S. is an international outlier in incarceration. Norway's rate of 72 per 100,000 population is slightly lower compared to the average for European countries of 102 per 100,000. In sharp contrast, the U.S. has a rate of almost 700 per 100,00. A majority of this gap is due to longer prison sentences in the U.S., particularly for minor crimes (Raphael and Stoll, 2013).

III. Assessing the Instrument and Interpreting the Treatment

III.A. IV Model

Our goal is to causally estimate spillover effects in criminal and family networks. We label a defendant facing possible incarceration with the subscript *1* and their peer with the subscript *2*. The influence of a defendant being incarcerated after being accused of a crime, I, on their peer's probability of being charged with a crime in the future, C, can be modeled as:

$$C_2 = \alpha_2 + \beta_2 I_1 + \delta_2 X + \varepsilon_2 \tag{1}$$

where X includes a full set of interacted year-of-case registration by court dummy variables as well as a set of control variables for both the defendant's and their peer group's characteristics. Identifying these types of spillover effects is challenging due to the well-known problems of correlated unobservables, reflection and endogenous group membership. The third challenge is not an issue in our setting, since the endogenous groups are formed prior to the defendant's incarceration decision. We overcome the first two challenges in an IV setting using the conditional random assignment of cases to judges, which gives rise to exogenous variation in the probability a defendant is incarcerated. The first stage of our model is:

$$I_1 = \theta Z_1 + \pi X + v_1 \tag{2}$$

where Z_1 denotes the stringency measure for the judge assigned to the defendant's case. We also report reduced form estimates of Z_1 on C_2 conditional on $X-$ which do not require assumptions about instrument exclusion or monotonicity. These estimates capture the effect of the defendant's judge stringency on the criminal behavior of his peers.

III.B. Instrument Relevance and Validity

Our instrument is the average judge incarceration rate in other cases a judge has handled, including the judge's past and future cases that may fall outside of our estimation sample. The mean of the instrument is 0.46 with a standard deviation of 0.07. The histogram appearing in Appendix Figure A.1 reveals a wide spread in a judge's tendency to incarcerate; a judge at the 90th percentile incarcerates about 54% of cases as compared to approximately 37% for a judge at the 10th percentile. The appendix figure also plots the probability a defendant is sent to prison in the current case as a function of whether he is assigned to a strict or lenient judge. The likelihood of receiving a prison sentence is monotonically increasing in the judge stringency instrument, and is close to linear.

The first panel of Table A.3 reports corresponding first stage estimates for the samples of all defendants, criminal network defendants and brother defendants. We regress a dummy for whether a defendant is incarcerated in the current case on our judge stringency instrument and find that being assigned to a judge with a 10 percentage point higher overall incarceration rate increases the probability of receiving a prison sentence by between 4 to 5 percentage points.

Appendix Table A.2 verifies that judges in all cases, as well as the criminal network and brother subsamples we will be focusing on, are randomly assigned to cases. The first column regresses incarceration on a variety of variables measured before the court decision for the sample of all defendants. It reveals that defendant characteristics, type of crime and defendant past work and criminal history variables are highly predictive of incarceration, with most coefficients being individually significant. In columns 3 and 5 we repeat this exercise for the co-offender and brother subsamples, but add in peer group characteristics as well. For these subsamples, we likewise find these pre-determined variables significantly predict a defendant's incarceration.

In columns 2, 4 and 6 we examine whether judge stringency can be predicted by these same sets of variables, and find no statistically significant relationship for any of the samples. The estimates are close to zero, and the number of significant coefficients is not more than would reasonably be expected due to chance. The coefficients are also not jointly significant, providing strong evidence for conditional randomization. In BDLM, we provide additional tests for conditional independence, the exclusion restriction and monotonicity, and find strong support for the validity of the judge stringency instrument.

III.C. Interpreting the Treatment

To understand the peer effects results which follow, it is important to first understand the effect of incarceration on the defendant. Panel 2 of Appendix Table A.3 reports estimates for the probability a defendant will be charged with a crime within four years after their court decision. OLS estimates a positive and significant effect of incarceration on future criminal charges for each of our samples. But as we found in BDLM, these OLS results are misleading. For example, for all defendants, IV yields reductions in future crime of 24 percentage points relative to an average of 67 percent. The crime reducing effects are somewhat larger for the co-offender sample and slightly smaller for the brother sample.

In our data, mean prison time served is 4.8 months, with almost 90 percent of defendants serving less than one year. As shown in BDLM, defendants sent to prison reduce their crime both during and after their release, pointing to both incapacitation and rehabilitation as important channels. As we argue in BDLM, the rehabilitative effect is likely due in part to investments in education and training programs (which result in increased employment), high quality and safe prison conditions (see Section II.C.) and post-release support programs.

IV. Network Spillovers

IV.A. Criminal Network

Baseline estimates. We being our investigation of incarceration spillovers by examining effects within criminal networks. As a reminder, our baseline definition of criminal networks is based on prior co-offender links up to third order which are close in both time (less than 3 years) and space (within a 5 mile radius).

The top panel of Table I reports first stage estimates for our criminal network sample of the defendant's probability of being sent to prison using our judge stringency instrument. Since we are interested in estimating spillover effects, the unit of observation is a defendant-peer pair. This means that defendants can appear multiple times in the sample if they have more than one peer in their criminal network[9]. On average, each defendant has 4 peers. The first stage effect of the instrument is strong and statistically significant, showing that if a defendant is assigned to a judge with a 10 percentage point higher overall incarceration rate the probability of receiving a prison sentence rises by about 4 percentage points.[10]

The bottom panel of Table I reports estimates of the probability a criminal network peer has ever been charged with a crime. Separate estimates are reported for year 1 (the period when the defendant is likely to be incapacitated in prison), years 2-4 (the period post release), and years 1-4. In all cases, OLS estimates small, positive spillover effects when a defendant is incarcerated. In sharp contrast, we find large and opposite-signed results when using our judge stringency instrument. For example, the effect of a defendant being assigned a judge who is 10 percentage points more stringent is a reduction in their peer's crime by 2 percentage

[9] To account for correlation across peers, we cluster on the network in the statistical inference.

[10] Note the first stage coefficient need not be one, unless the following conditions hold: (i) the sample of cases used to calculate the stringency measure is exactly the same as estimation sample, (ii) there are no covariates, and (iii) there are a large number of cases per judge. In our setting, there is no reason to expect a coefficient of one. In particular, the full set of interacted court, judge type, case type and year dummies breaks this mechanical relationship.

points over the ensuing 4 years. When scaled, this yields an IV estimate of a 51 percentage point drop in criminal charges for the peers of an incarcerated defendant.

Looking at columns 1 and 2, we see a reduction in peer crime when the defendant is likely to be in prison as well as after their release. We find a 31 percentage point drop in peer crime in year 1 and a 49 percentage point drop in years 2-4, which are large effects relative to the dependent means. So while the incapacitive effect of removing the defendant from their network can help explain a short term effect, it cannot explain the effects we observe in the longer term. Interestingly, the reduction in crime is not driven by a reduced probability of committing a new crime together (which happens only rarely), suggesting it is the positive peer influence of the defendant more generally.

Alternative definitions of criminal networks. Our baseline definition of criminal networks includes links up to third order. Column 2 in Appendix Table A.4 reveals qualitatively similar RF and IV estimates compared to our baseline for first order links. While the estimate remains statistically significant, the standard error increases by more than 50% given the smaller sample size. In column 3, we look at criminal networks defined as including only 2nd and 3rd order links (and not 1st order links). These estimates are again qualitatively similar and statistically significant, but with larger standard errors. What we take away from this exercise is that it would be too restrictive to define criminal networks as being comprised of only first order links.

In Table II, we explore the importance of closeness in a criminal network. We first try an alternative definition of closeness in space, using criminal network links occurring in the same municipality instead of within a five mile radius (but keeping closeness in time of 3 years or less). The IV estimate in column 2 of -0.42 is qualitatively similar to the baseline estimate of -0.51 in column 1. In columns 3 and 4, we look at the complement of these two distance conditions. When the link is greater than 5 miles or in a different municipality, the point estimates are small and close to zero. The close versus distant estimates are statistically different when comparing columns 1 and 3 (p-value = .03) and close to statistically different when comparing columns 2 and 4 (p-value = .11).

We next try an alternative definition of closeness in time, using criminal network links from more than three years ago (but keeping closeness in distance). When the links are distant in time, the IV estimates are small and close to zero. The close versus distant estimates are statistically different when comparing columns 1 and 5 (p-value = .02) as well as columns 2 and 6 (p-value = .06).

Table II makes clear that spillovers occur only for criminal networks which are likely to be active. Peers with links which are not geographically close are unlikely to interact with each other on a frequent basis. And peers without recent criminal activity are unlikely to be current members of the same criminal group. These results highlight the usefulness of our rich panel data, allowing us to focus on networks which are likely to be active.

IV.B. Brother Network

We now turn to brother and broader family networks. In Table III, we present results for the younger brother of a defendant in the top panel and results for older brothers in the bottom panel. We estimate a 32 percentage point reduction over a four year period in the probability a younger brother will be charged with a crime if his older brother is incarcerated. This effect is somewhat smaller in year 1 (-0.135) compared to the estimate for years 2-4 (-0.338), although it should be noted that the two estimates are not directly comparable as they cover different lengths of time. The time pattern indicates the reduction in a younger brother's crime extends past the time when his older brother is in jail.

Turning to older brothers, we find no spillovers when the younger brother is incarcerated. The point estimates in all three columns are close to zero and statistically insignificant. Moreover, the 4 year estimates for the younger versus older brothers in the top and bottom panels are statistically different from each other (p-value = .03). This finding is consistent with evidence that suggests that older siblings are influential on younger sibling's behavior for risky behaviors such as smoking and drinking (Averett, Argys and Rees, 2011; Oettinger 2000).

What about other family ties? In Bhuller, Dahl, Løken, and Mogstad (2018), we found no systematic evidence for intergenerational spillovers from parent to child. Consistent with this, when we look at all other family members besides brothers (i.e., spouses, sisters and children), we find no evidence of a spillover effect of incarceration, as reported in Table IV. The IV estimate for these other family members combined is 0.053 (s.e. = .051), and is statistically different from the estimate for younger brothers (p-value = 0.01). So while younger brothers are heavily influenced by their older brother, there is no evidence of a link for other family ties. These results highlight the importance of separating out different types of family ties when examining spillover effects.

V. Policy Relevance of Incarceration Spillovers

Our results show that the consequences of incarceration depend not only on an inmate's recidivism, but also on the effect it has on their broader criminal and family networks. These findings have important implications for criminal justice policy and the design of effective prison systems, as the societal costs and benefits of incarceration are magnified once network effects are taken into account. To illustrate this point, we simulate the total reduction in crime from a hypothetical policy which makes judges more stringent.

There are two components from this type of policy change: the direct effect on defendants, and the spillover effects on network members. We consider an increase in judge stringency of 1 standard deviation, which translates into making judges 7.23 percentage points more likely to incarcerate a defendant on average. To calculate how this change in the instrument affects criminal network members and younger brothers, we multiply the reduced form estimates from Tables I and III by .0723 and by the number of members in the relevant network. The direct effect is calculated based on the reduced form estimates for defendants in each of the networks (Appendix Table A.3). To ease interpretation, the predicted reduction in criminal charges is reported as a fraction of the total number of defendants and network members ever charged with a crime during the 4 year sample window.

In Figure I, we graph the direct, spillover and total effects of the policy simulation. In the first year after the court decision, making judges 1 standard deviation stricter on average is predicted to reduce the recidivism rate of defendants and their younger brothers by 1.7 percent. Most of this initial reduction can be attributed to the direct effect on the defendants, likely due to an incapacitation effect. Over time, however, the spillover effect on younger brothers increases: by year 4, the total reduction in the recidivism rate is 2.6 percent, of which the spillover effect accounts for 61 percent of the drop. In comparison, the spillover component explains even more of the reduction in recidivism in the criminal network. Relatively large spillover effects arise within the criminal network because there are more criminal peers compared to younger brothers per defendant. Taken together, the simulation results highlight that spillovers are of first order importance for policy, as the network reductions in future crime are larger than the direct effect on the incarcerated defendant.

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Table 1. Effect of Incarceration on the Defendant's Criminal Network.

	Year 1	Years 2-4	Years 1-4
	(1)	(2)	(3)
	Dependent Variable:		
	Pr(Defendant Incarcerated)		
FS: Judge Stringency	0.384***	0.384***	0.384***
	(0.125)	(0.125)	(0.125)
Dependent Mean	0.519	0.519	0.519
No. Observations	26,671	26,671	26,671
	Dependent Variable:		
	Pr(Criminal Network Member Ever Charged)		
OLS: Incarcerated	0.004	0.016*	0.014*
	(0.008)	(0.008)	(0.008)
RF: Judge Stringency	-0.119*	-0.189***	-0.196***
	(0.064)	(0.049)	(0.047)
IV: Incarcerated	-0.309*	-0.491***	-0.510***
	(0.166)	(0.188)	(0.178)
Dependent Mean	0.467	0.659	0.736
No. Peers Per Defendant	4.03	4.03	4.03
No. Observations	26,671	26,671	26,671

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. Specifications include all variables listed in Table A.2 as controls, plus court x court entry year FEs. Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.
**p<0.1, **p<0.05, ***p<0.01

Table III. Effect of Incarceration on the Defendant's Brothers.

	Year 1	Years 2-4	Years 1-4
	(1)	(2)	(3)
	Dependent Variable:		
	Pr(Defendant Incarcerated)		
FS: Judge Stringency	0.556***	0.556***	0.556***
	(0.087)	(0.087)	(0.087)
Dependent Mean	0.549	0.549	0.549
No. Observations	24,954	24,954	24,954
	Dependent Variable:		
	Pr(Younger Brother Ever Charged)		
OLS: Incarcerated	0.010*	0.011*	0.017**
	(0.005)	(0.007)	(0.007)
RF: Judge Stringency	-0.075*	-0.188***	-0.179***
	(0.043)	(0.064)	(0.060)
IV: Incarcerated	-0.135	-0.338**	-0.323**
	(0.083)	(0.134)	(0.126)
Dependent Mean	0.141	0.260	0.302
No. Peers Per Defendant	1.41	1.41	1.41
No. Observations	24,954	24,954	24,954
	Dependent Variable:		
	Pr(Defendant Incarcerated)		
FS: Judge Stringency	0.462***	0.462***	0.462***
	(0.084)	(0.084)	(0.084)
Dependent Mean	0.549	0.549	0.549
No. Observations	21,266	21,266	21,266
	Dependent Variable:		
	Pr(Older Brother Ever Charged)		
OLS: Incarcerated	0.007	0.010	0.018**
	(0.005)	(0.007)	(0.007)
RF: Judge Stringency	-0.003	-0.040	0.032
	(0.052)	(0.052)	(0.057)
IV: Incarcerated	-0.006	-0.086	0.069
	(0.112)	(0.114)	(0.125)
Dependent Mean	0.129	0.220	0.263
No. Peers Per Defendant	1.36	1.36	1.36
No. Observations	21,266	21,266	21,266

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. Specifications include all variables listed in Table A.2 as controls, plus court x court entry year FEs. Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.
**p<0.1, **p<0.05, ***p<0.01

Table IV. Effect of Incarceration on the Defendant's Other Family Members.

	Spouse	Sisters	Children	Spouse, Sisters & Children
	(1)	(2)	(3)	(4)
Dependent Variable:				
FS: Judge Stringency	0.450***	0.502***	0.360***	0.447***
	(0.08)	(0.071)	(0.109)	(0.069)
Dependent Mean	0.542	0.550	0.555	0.550
No. Observations	13,796	44,255	22,119	80,170
Dependent Variable:				
OLS: Incarcerated	-0.003	0.005	0.009	0.004
	(0.007)	(0.003)	(0.007)	(0.003)
RF: Judge Stringency	0.064	0.022	-0.007	0.024
	(0.050)	(0.024)	(0.058)	(0.022)
IV: Incarcerated	0.142	0.043	-0.018	0.053
	(0.117)	(0.048)	(0.160)	(0.051)
Dependent Mean	0.139	0.078	0.207	0.124
No. Peers Per Defendant	1.00	1.60	1.87	2.27
No. Observations	13,796	44,255	22,119	80,170

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. Specifications include all variables listed in Table A.2 as controls, plus court x court entry year FEs. Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.

**p<0.1, **p<0.05, ***p<0.01.

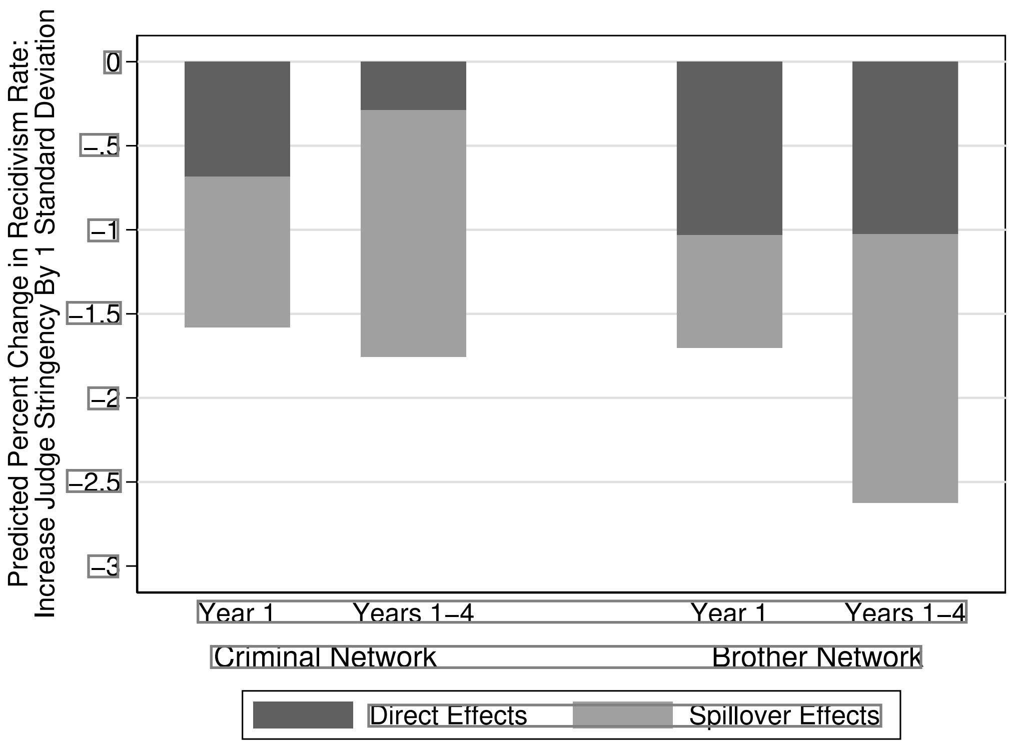

Figure 1. Direct and Spillover Effects of Increased Judge Stringency.

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. The vertical bars show the predicted percent change in the fraction of ever charged network members following a simulated increase in average judge stringency by 1 standard deviation (7.23 percentage points). The predicted reductions are reported as a fraction of the observed number of defendants and network members ever charged with a crime by the end of year 4.

Appendix Tables and Figures

Table A.1. Descriptive Statistics.

	A. All Defendants		B. Defendants Linked To Criminal Network		C. Male Defendants With Brothers	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Defendant Characteristics:						
Age	32.997	(11.534)	25.392	(7.824)	31.821	(10.789)
Female	0.109	(0.312)	0.054	(0.226)	-	-
Foreign born	0.139	(0.346)	0.021	(0.144)	0.080	(0.271)
Married	0.108	(0.310)	0.152	(0.359)	0.078	(0.269)
Number of children	0.789	(1.246)	0.077	(0.267)	0.695	(1.162)
High school	0.171	(0.376)	0.007	(0.083)	0.168	(0.374)
Some college	0.048	(0.213)	0.012	(0.108)	0.035	(0.184)
Missing Xs	0.031	(0.172)	0.010	(0.098)	0.006	(0.075)
Type of Crime:						
Violent crime	0.267	(0.442)	0.330	(0.470)	0.280	(0.449)
Property crime	0.136	(0.343)	0.194	(0.395)	0.140	(0.347)
Economic crime	0.108	(0.311)	0.037	(0.189)	0.087	(0.282)
Drug related	0.127	(0.332)	0.142	(0.349)	0.124	(0.329)
Drunk driving	0.072	(0.259)	0.047	(0.212)	0.074	(0.261)
Other traffic	0.076	(0.265)	0.054	(0.226)	0.080	(0.271)
Defendant's Past Crime and Work History:						
Ever Employed, t-1	0.350	(0.477)	0.212	(0.409)	0.356	(0.479)
Ever Employed, t-2 to t-5	0.468	(0.499)	0.281	(0.450)	0.474	(0.499)
Ever Charged, t-1	0.465	(0.499)	0.722	(0.448)	0.504	(0.500)
Ever Charged, t-2 to t-5	0.639	(0.480)	0.866	(0.341)	0.690	(0.462)
Ever Incarcerated, t-1	0.138	(0.344)	0.255	(0.436)	0.158	(0.365)
Ever Incarcerated, t-2 to t-5	0.288	(0.453)	0.417	(0.493)	0.333	(0.471)
Peer Group Characteristics:						
No. Peers	-	-	4.115	(6.058)	1.659	(0.929)
No. Peers Ever Employed, t-1	-	-	0.513	(0.947)	0.689	(0.799)
No. Peers Ever Employed, t-2 to t-5	-	-	0.632	(1.111)	0.783	(0.852)
No. Peers Ever Charged, t-1	-	-	2.560	(4.186)	0.224	(0.491)
No. Peers Ever Charged, t-2 to t-5	-	-	3.473	(5.340)	0.459	(0.690)
No. Peers Ever Incarcerated, t-1	-	-	0.886	(1.883)	0.061	(0.256)
No. Peers Ever Incarcerated, t-2 to t-5	-	-	1.048	(2.262)	0.141	(0.394)
No. Peers Former Cooffenders	-	-	0.807	(1.168)	0.051	(0.247)
No. Observations	53,855		6,967		29,871	

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. The omitted category for education is “Less than high school, year t-1” and the omitted category for type of crime is “Other crimes”.

Table A.2. Testing for Random Assignment of Criminal Cases to Judges.

	A. All Defendants		B. Defendants Linked To Criminal Network		C. Male Defendants With Brothers	
	Incarcerated	Judge Stringency	Incarcerated	Judge Stringency	Incarcerated	Judge Stringency
	(1)	(2)	(3)	(4)	(5)	(6)
Defendant Characteristics:						
Age	0.004***	0.000	0.008***	-0.000	0.004***	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Female	-0.058***	-0.001*	-0.114***	-0.003	-	-
	(0.006)	(0.001)	(0.027)	(0.003)		
Foreign Born	0.003	0.001	0.046**	0.004	-0.005	0.000
	(0.005)	(0.000)	(0.019)	(0.003)	(0.015)	(0.002)
Married	-0.016*	-0.001	0.030	-0.005	-0.015	-0.001
	(0.009)	(0.001)	(0.048)	(0.007)	(0.014)	(0.001)
Number of Children	-0.003	0.000	-0.014	0.000	0.005	0.000
	(0.003)	(0.000)	(0.012)	(0.001)	(0.004)	(0.000)
High School	-0.000	0.001	0.018	-0.002	-0.007	0.001
	(0.006)	(0.001)	(0.027)	(0.003)	(0.010)	(0.001)
Some College	-0.055***	0.000	-0.105**	-0.005	-0.079***	0.003
	(0.010)	(0.001)	(0.047)	(0.006)	(0.018)	(0.002)
Missing Xs	-0.398***	0.012	-0.793**	-0.055	-0.760***	0.027
	(0.098)	(0.012)	(0.354)	(0.048)	(0.199)	(0.020)
Type of Crime:						
Violent Crime	0.085***	0.000	0.052***	-0.003	0.086***	-0.000
	(0.007)	(0.001)	(0.017)	(0.002)	(0.009)	(0.001)
Property Crime	-0.043***	0.000	-0.065***	0.001	-0.049***	-0.001
	(0.009)	(0.001)	(0.020)	(0.002)	(0.011)	(0.001)
Economic Crime	-0.057***	0.001	-0.068*	0.009*	-0.040***	0.003**
	(0.009)	(0.001)	(0.038)	(0.005)	(0.014)	(0.002)
Drug Related Crime	-0.058***	-0.001	-0.061***	0.000	-0.053***	-0.003**
	(0.009)	(0.001)	(0.021)	(0.003)	(0.011)	(0.001)
Drunk Driving	0.067***	-0.001	-0.006	0.002	0.072***	-0.001
	(0.010)	(0.001)	(0.030)	(0.003)	(0.014)	(0.001)
Other Traffic Crime	-0.061***	-0.000	-0.040	-0.005*	-0.037***	-0.001
	(0.011)	(0.001)	(0.029)	(0.003)	(0.014)	(0.001)
Defendant's Past Crime and Work History:						
Employed, t-1	0.018***	-0.000	0.036*	-0.002	0.027***	-0.000
	(0.006)	(0.001)	(0.020)	(0.002)	(0.008)	(0.001)
Employed, t-2 to t-5	0.006	-0.000	0.004	-0.002	0.006	-0.001
	(0.007)	(0.001)	(0.017)	(0.002)	(0.009)	(0.001)
Charged, t-1	0.048***	-0.001	0.030*	-0.000	0.042***	0.000
	(0.006)	(0.001)	(0.016)	(0.002)	(0.007)	(0.001)
Charged, t-2 to t-5	0.041***	0.000	0.029	0.004*	0.044***	-0.001
	(0.006)	(0.001)	(0.022)	(0.002)	(0.008)	(0.001)
Incarcerated, t-1	0.142***	0.000	0.175***	0.000	0.130***	-0.001
	(0.008)	(0.001)	(0.017)	(0.002)	(0.010)	(0.001)
Incarcerated, t-2 to t-5	0.171***	0.001	0.155***	-0.002	0.164***	0.002**
	(0.007)	(0.001)	(0.018)	(0.002)	(0.010)	(0.001)
Peer Group Characteristics:						
No. Peers	-	-	0.008**	-0.000	0.009*	0.000
			(0.003)	(0.000)	(0.005)	(0.000)
No. Peers Employed, t-1	-	-	0.026**	-0.002	-0.004	-0.000
			(0.010)	(0.001)	(0.008)	(0.001)
No. Peers Employed, t-2 to t-5	-	-	-0.007	0.001	-0.011	0.001
			(0.009)	(0.001)	(0.007)	(0.001)
No. Peers Charged, t-1	-	-	-0.013***	0.001	0.008	-0.001
			(0.004)	(0.001)	(0.008)	(0.001)
No. Peers Charged, t-2 to t-5	-	-	-0.006	0.000	-0.011	-0.000
			(0.004)	(0.001)	(0.007)	(0.001)
No. Peers Incarcerated, t-1	-	-	0.019**	-0.003*	-0.006	0.001
			(0.008)	(0.001)	(0.015)	(0.002)
No. Peers Incarcerated, t-2 to t-5	-	-	0.001	0.001	0.020*	-0.001
			(0.006)	(0.001)	(0.010)	(0.001)
No. Peers Former Cooffenders	-	-	-0.019***	0.000	0.034**	0.001
			(0.007)	(0.001)	(0.014)	(0.001)
Dependent mean	0.521	0.459	0.574	0.464	0.547	0.458
Joint F-statistic	145.3	0.665	23.52	0.910	55.35	1.134
(p-value)	(.000)	(.862)	(.000)	(.601)	(.000)	(.293)
No. Observations	53,855		6,967		29,871	

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. All specifications include controls for court x court entry year FEs. Reported F-statistic refers to a joint test of the null hypothesis for all variables except the FEs. The omitted category for education is "Less than high school, year t-1" and the omitted category for type of crime is "Other crimes". Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.
**p<0.1, **p<0.05, ***p<0.01

Table A.3. Effect of Incarceration on the Defendant.

	A. Defendants	B. Defendants Linked To Criminal Network	C. Male Defendants With Brothers
	(1)	(2)	(3)
		Dependent Variable: ***Pr(Defendant Incarcerated)***	
FS: Judge Stringency	0.398***	0.407***	0.508***
	(0.051)	(0.116)	(0.066)
Dependent Mean	0.516	0.571	0.544
No. Observations	50,911	6,612	28,150
		Dependent Variable: ***Pr(Defendant Ever Charged)***	
OLS: Incarcerated	0.048***	0.023**	0.045***
	(0.005)	(0.011)	(0.006)
RF: Judge Stringency	-0.097**	-0.153**	-0.087*
	(0.038)	(0.078)	(0.049)
IV: Incarcerated	-0.243**	-0.376*	-0.172*
	(0.101)	(0.225)	(0.101)
Dependent Mean	0.672	0.877	0.719
No. Observations	50,911	6,612	28,150

Note: Sample consists of randomly-assigned non-confession criminal cases processed 2005-2012. Specifications include all variables listed in Table A.2 as controls, plus court x court entry year FEs. Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.
**p<0.1, **p<0.05, ***p<0.01

Table A.4. Effect of Incarceration by Order of Network Link.

	A. 1st, 2nd & 3rd Order Links	B. 1st Order Link	C. 2nd & 3rd Order Links
	(1)	**(2)**	**(3)**
		Dependent Variable: Pr(Criminal Network Member Ever Charged)	
OLS: Incarcerated	0.014*	0.017	0.003
	(0.008)	(0.013)	(0.009)
RF: Judge Stringency	-0.196***	-0.252**	-0.189***
	(0.047)	(0.099)	(0.053)
IV: Incarcerated	-0.510**	-0.585**	-0.592**
	(0.178)	(0.275)	(0.254)
Dependent Mean	0.736	0.741	0.734
No. Observations	26,671	8,979	17,692
Fraction of Network	100%	33.6%	66.4%

Note: Sample consists of 53,855 randomly-assigned non-confession criminal cases processed 2005-2012. Specifications include all variables listed in Table A.2 as controls, plus court x court entry year FEs. Standard errors are heteroskedasticity robust and two-way clustered on judge ID and defendant ID.
**p<0.1, **p<0.05, ***p<0.01

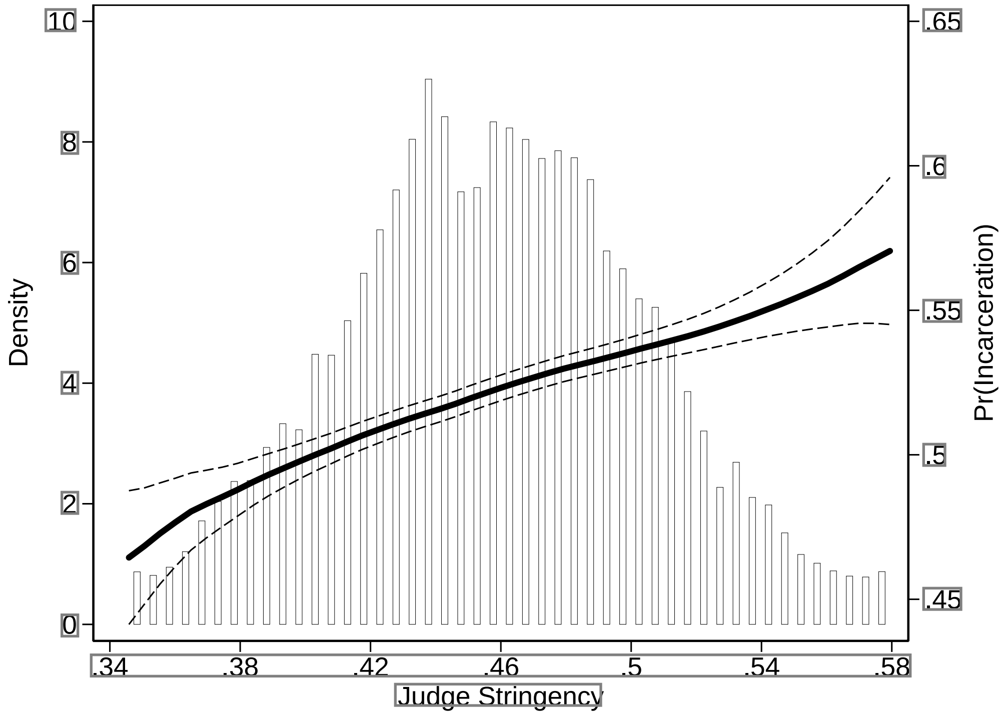

Figure A.1. First Stage Graph of Incarceration on Judge Stringency.

Note: Sample consists of randomly assigned non-confession criminal cases processed 2005-2012. Probability of incarceration is plotted on the right y-axis against leave-out mean judge stringency of the assigned judge shown along the x-axis. The plotted values are mean-standardized residuals from regressions on court x court entry year interacted fixed effects and all variables listed in Table A.2. The solid line shows a local linear regression of incarceration on judge stringency. Dashed lines show 90% confidence intervals. The histogram shows the density of judge stringency along the left y-axis for each sample (top and bottom 2% excluded).