

PATERNAL INCARCERATION AND CHILDREN'S RISK OF BEING CHARGED BY EARLY ADULTHOOD: EVIDENCE FROM A DANISH POLICY SHOCK*

CHRISTOPHER WILDEMAN^{1,2} and SIGNE HALD ANDERSEN²

¹Department of Policy Analysis and Management, Cornell University

²Rockwool Foundation Research Unit

KEYWORDS: paternal incarceration, mass imprisonment, intergenerational transmission of crime, register data, Denmark

In this article, we exploit a Danish criminal justice reform that dramatically decreased the risk of incarceration for individuals convicted of some types of crimes to isolate how having a father who was eligible for a noncustodial sentence under the reform affected a child's risk of ever subsequently being charged with a crime. Specifically, we use a difference-in-differences framework to compare all Danish children 12–18 years of age whose fathers were eligible for a noncustodial sentence instead of incarceration under the reform [N = 1,546] with a reference group of children whose fathers were convicted of similar crimes but were ineligible [N = 1,852] in the 2 years surrounding when the reform was enacted [July 1, 2000] as a way of testing the effects of the reform on children's risk of ever being charged with a crime by 22–28 years of age. Our estimates indicate that having a father sentenced under the reform sharply decreased the risk of being charged in the next 10 years for boys but not for girls. Taken together, these results indicate that both paternal criminality and paternal incarceration promote the criminal justice contact of male children and, hence, that paternal incarceration is not solely a symptom of criminality but also a cause of it.

As the U.S. incarceration rate has increased, criminologists have developed an acute interest in how incarceration affects current and former inmates (e.g., Massoglia, 2008; Pager, 2003; Western, 2006), those tied to them (e.g., Clear, 2007; Comfort, 2008; Wildeman, Schnittker, and Turney, 2012), and social inequality (e.g., Foster and Hagan, 2015; Wakefield and Uggen, 2010; Wildeman and Muller, 2012). Because of strong interest in the intergenerational transmission of crime and criminal justice contact (e.g., Fergusson, Horwood, and Nagin, 2000; Robins, West, and Herjanic, 1975; West and Farrington, 1977), research on the consequences of paternal incarceration for child

* Additional supporting information can be found in the listing for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2017.55.issue-1/issuetoc>.

The authors thank Lars H. Andersen, Peter Fallesen, Maria Fitzpatrick, Mike Lovenheim, Sara Wakefield, the four anonymous reviewers along with Editor-in-Chief D. Wayne Osgood at *Criminology* for providing helpful comments, as well as Danielle Zucker for providing excellent research assistance throughout the course of this project. We also thank the Rockwool Foundation and the Institute for the Social Sciences at Cornell University for funding this research.

Direct correspondence to Christopher Wildeman, 137 Martha Van Rensselaer Hall, Department of Policy Analysis and Management, Cornell University, Ithaca, NY 14853 (e-mail: christopher.wildeman@cornell.edu).

well-being has been an especially lively area. By using a range of data sets and methods, research in this area has consistently tied paternal incarceration to a whole host of poor outcomes through childhood (e.g., Geller et al., 2012; Wakefield and Wildeman, 2011; Wildeman, 2010) and into adolescence (e.g., Foster and Hagan, 2007; Hagan and Foster, 2012a; Murray, Loeber, and Pardini, 2012) and even adulthood (e.g., Murray and Farrington, 2008; Murray et al., 2014).

Of course, it may be the case that the poor outcomes of children of incarcerated fathers are driven not by paternal incarceration but instead by paternal criminality and a host of other factors that led their fathers to end up incarcerated (Johnson and Easterling, 2012; Sampson, 2011; Wildeman, Wakefield, and Turney, 2013). This concern is especially troubling in regard to children's criminal justice contact as 1) there are theoretical reasons to expect the paternal incarceration–children's criminal justice contact relationship to be spurious and 2) the range of methods used to consider the effects of paternal incarceration on children's criminal justice contact (e.g., Murray and Farrington, 2005; Porter and King, 2015; Roettger and Swisher, 2011) do less to assuage concerns regarding spuriousness than do those used to consider other outcomes (e.g., Andersen and Wildeman, 2014; Geller et al., 2012; Wakefield and Wildeman, 2014).

In this article, we provide the first test of how an exogenous shock in the risk of paternal incarceration affects children's risk of being charged with a crime by 22–28 years of age using register data and a Danish policy shock that led to a dramatic increase in the use of community service (rather than in incarceration) for Danish adults sentenced to spend less than a year in a correctional facility for a select group of crimes. This study makes a contribution for two reasons, although it is important to note that it provides only *indirect* insight into the effects of paternal incarceration on individual children because of the focus on a specific policy shock. First, this study is a uniquely strong test in this subfield as it is the first to link an exogenous change in the risk of paternal incarceration¹ with children's risk of criminal justice contact. Second, the policy shock the study exploits provides estimates not of exogenous variation in sentence length, as previous work on the effects of incarceration has done (e.g., Green and Winik, 2010; Kling, 2006; Loeffler, 2013),² but on exogenous variation in spending time incarcerated relative to receiving some other noncustodial sentence. As such, it provides insight into how alternatives to incarceration such as community service could affect the criminal justice contact of both fathers and their children.

PATERNAL INCARCERATION AND CHILDREN'S CRIMINAL JUSTICE CONTACT

Research on the intergenerational transmission of criminality has a great history in criminology as there has long been intense interest in the factors that cause the

-
1. We focus on paternal incarceration because research on maternal incarceration has 1) produced unstable estimates in Denmark as few Danish mothers experience this event (Wildeman et al., 2014) and 2) found inconsistent effects in the United States (e.g., Hagan and Foster, 2012b; Huebner and Gustafson, 2007; Wildeman and Turney, 2014).
 2. We lump Loeffler (2013) with these studies because all individuals in his sample had been detained during the trial, meaning that all of them, whether “treated” with a prison sentence or not, had been exposed to a jail stay.

concentration of crime in a few families (e.g., Fergusson, Horwood, and Nagin, 2000; Robins, West, and Herjanic, 1975; West and Farrington, 1977). This article seeks to provide a strong test of a new—although, unfortunately, largely disconnected—branch of this research that attempts to estimate the effects of paternal incarceration on children's outcomes, including criminality and criminal justice contact.

Although a large literature on paternal incarceration and child well-being exists, some of which have used stringent tests in an attempt to tease out how paternal incarceration affects children (Foster and Hagan, 2015; Johnson and Easterling, 2012; Wildeman, Wakefield, and Turney, 2013), little research has so far provided a strong test of how paternal incarceration affects children's involvement in crime and the criminal justice system. And, indeed, few of the methods criminologists use to test for effects—covariate adjustment, matching, fixed effects, synthetic regressions, difference-in-differences estimation, and instrumental variables estimation—have been exploited in studies on the intergenerational transmission of criminal justice contact. Most research in this area has relied primarily on covariate adjustment to deal with selection into incarceration (Besemer et al., 2011; Murray and Farrington, 2005; Murray, Janson, and Farrington, 2007; Roettger and Swisher, 2011; van de Rakt, Murray, and Nieuwebeerta, 2012), although one recent study used an ingenious variant of synthetic regressions to deal with selection in a different way (Porter and King, 2015). In general, research in this area has found fairly consistent associations between paternal incarceration and boys' delinquency, criminality, and criminal justice contact, with associations especially strong in the United States and the United Kingdom and somewhat weaker in other developed democracies. For girls, the association between paternal incarceration and these outcomes has been fairly inconsistent.

Unfortunately, of the five more rigorous methods discussed earlier, only matching would be easy to add to this list using only the data used for the studies mentioned. This is the case for two reasons. First, neither fixed effects models, which rely on within-individual change, nor synthetic regressions, which often key in on individuals who have not yet experienced the event but will in the future, can easily be applied to the data researchers in this area have used to date. This is because changes in paternal incarceration—especially first incarcerations—are unlikely to happen close to the time of children's criminal justice contact, leading to reference cells that are very small. Consider Porter and King's (2015) analysis, for instance. In their study, all the future first paternal incarcerations that provide the placebo group happened *after* the child was 11–19 years of age, which means that all the fathers in this group experienced their *first* incarceration between ages 36 and 44 (assuming the average father in the study was 25 when the child was born). In this instance, 14.7 percent of their sample experienced incarceration at some point; only 2.3 percent experienced their first incarceration after the child was 11–19 (Porter and King, 2015: 427).³ The same issue applies to fixed effects models, although in this instance, fathers cycling through the criminal justice system when their children are teenagers likely generate more within-individual change.

Second, both difference-in-difference methods and instrumental variable methods exploit exogenous sources of variation in the risk or duration of incarceration to isolate

3. This is why we will later present estimates for both all paternal incarcerations and first paternal incarcerations.

the causal effects of this event. Unfortunately, with some exceptions, such studies would usually require linked administrative data on fathers and their children, in addition to some exogenous source of variation in the risk or duration of incarceration, and such data tend to be sorely lacking. In large part because of these data limitations, we know of no studies on paternal incarceration and children's delinquency, criminality, and criminal justice contact that have to date been able to exploit a source of exogenous variation in paternal incarceration and use either of these methods.

What is therefore needed is a test of the relationship between paternal incarceration and children's criminal justice contact that uses a strong design to provide more definitive insight into whether this relationship is indeed plausibly causal or may be merely spurious.

THE DANISH CONTEXT

CRIME AND PUNISHMENT IN DENMARK AND THE UNITED STATES

Although this article provides just such a test, its applicability to the United States, where the incarceration rate is six times as high (e.g., Walmsley, 2013)⁴ and the welfare state is less generous to families (e.g., Esping-Andersen, 1990), is unclear. And, as such, we first provide an extensive discussion of the Danish context before providing an extensive discussion of the policy shock we rely on.

Although levels of criminal activity in Denmark do not differ greatly from those in the United States for less serious crimes, levels of serious criminal activity are dramatically lower in Denmark than they are in the United States (United Nations Office of Drugs and Crime, 2015). The Danish homicide rate (.7 per 100,000), for example, is only approximately one fifth that of the United States (3.8 per 100,000).

The Danish criminal justice system also differs from the U.S. one in at least six important regards. First, the Danish criminal justice system has far harsher sentences for driving under the influence and other serious traffic offenses than the United States does, although this is among the handful of sentences where the Danish sanctions are harsher than the U.S. ones. Second, among those sentenced to serve time, sentences tend to be far shorter in Denmark than in the United States. A life sentence in Denmark, for instance, is 16 years. Third, nearly all Danish inmates are released after they have served roughly two thirds of their sentences, with it being possible to serve an even smaller proportion of the sentence. U.S. inmates serve a far larger proportion of their sentences. Fourth, noncustodial alternatives to incarceration—ranging from electronic monitoring to community service—are used at a higher rate and for a wider range of crimes in Denmark than they are in the United States. Fifth, many Danish prisons are open—meaning that inmates are free to leave to pursue approved activities such as work or education. Such prisons do not exist in the United States. Finally, in the United States, sentences of less than 1 year are generally served in local jails, meaning that inmates with short sentences tend to be close to their homes. In Denmark, even sentences of only a couple months may be served in facilities that are far away.

The Danish and U.S. contexts, thus, differ greatly, which is important to remember when we consider the generalizability of our results to contexts such as the United

4. Wildeman (2009) and Wildeman and Andersen (2015) estimated paternal incarceration for U.S. and Danish children.

States (see Andersen, 2015; Andersen and Wildeman, 2014; see also Pratt, 2008, for a broader discussion). Different as they are, research on the consequences of paternal incarceration for children in Denmark has been highly consistent with recent research in the United States. Danish research has shown, for instance, that paternal incarceration increases the risk of foster care placement (Andersen and Wildeman, 2014) and child mortality (Wildeman et al., 2014). Although these are not oft-considered associations in the U.S. context (but see Wildeman, 2012), the broader association between paternal incarceration and well-being from childhood to early adulthood has been tested extensively in the U.S. context, with results consistent with what has been found in the Danish context (e.g., Foster and Hagan, 2015). Thus, a Danish study on the effect of paternal incarceration on children's criminal justice contact can facilitate vital insights into the U.S. context, in particular, because no research on paternal incarceration in the United States has been able to rely on an exogenous shock in the risk of paternal incarceration.

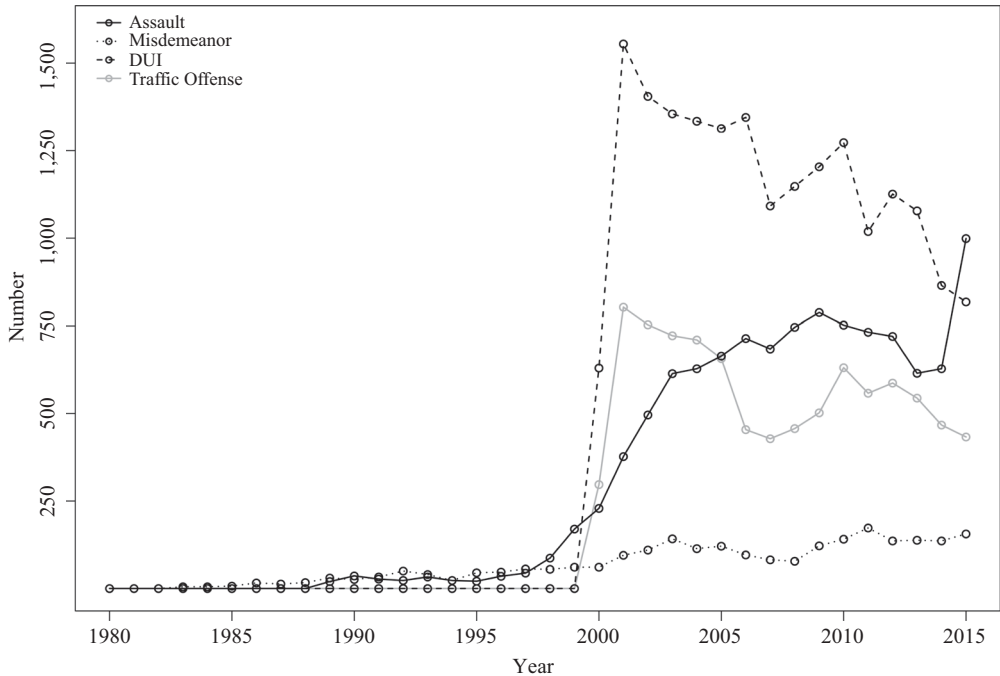
COMMUNITY SERVICE IN DENMARK

For an exogenous shock in the risk of paternal incarceration, we rely on a reform that expanded the use of community service in Denmark in 2000 and, in so doing, decreased the risk of incarceration for individuals convicted of some types of crimes. In Denmark, community service comprises 30 to 300 hours of work that contributes to broader society, can be completed either at a public institution (such as a public library) or at a nonprofit institution (such as a thrift store), and can be used in addition to probation as an alternative sentence to incarceration. There is no direct scale of conversion between the length of the prison term that the offender would have received and the number of community service hours the offender has to perform, although more severe crimes tend to somewhat increase the required number of community service hours.

Community service was first introduced in Denmark in 1992. At this initial stage, community service was meant to replace prison sentences of up to a year for crimes such as misdemeanors and simple assault, but judges initially were hesitant to rely on this alternative to incarceration, leading to the small uptick in community service shown in 1992 (figure 1). A second reform, which was announced in April 2000 and implemented on July 1, 2000, however, dramatically expanded the use of community service, allowing judges also to sentence offenders of drunk driving and other serious traffic offenses to community service instead of incarceration. The 2000 reform further mandated that all social inquiry reports, which are prepared prior to the conviction and used by judges to decide on the sanction type, contain explicit statements from criminal justice officials concerning whether the offender was suited for community service.

Both the explicit expansion of the use of community service as well as the new use of the social inquiry reports drove the rise in the use of community service after the reform shown in figure 1, with 1,000 Danes serving their time in community service in 1999 and 3,500 doing so in 2001. This rise was driven primarily by Danes sentenced for drunk driving, but there were also increases for other types of sentences, including assault. The increase is especially interesting for individuals convicted of misdemeanors and assault as these groups were eligible for community service before the 2000 reform (through the 1992 reform) and for whom we might not expect to see a reform effect in 2000. Because only individuals sentenced to incarceration could receive community

Figure 1. Number of Individuals in Denmark Sentenced to Community Service for Four Specific Offenses, 1980–2015



service, each individual receiving community service after the reform corresponds with one less individual serving time in prison than would have been the case absent the reform.

Because of how sharp a change in the use of community service the 2000 reform led to, it is perhaps unsurprising that some research has considered the consequences of this reform both for the offenders it affected (Andersen, 2015) and their families (Andersen and Wildeman, 2014). These earlier analyses of the effects of this policy shock provided support for two conclusions. First, being sentenced to community service instead of incarceration increases earnings and decreases welfare dependency (Andersen, 2015); it also cuts the probability of having a child placed in foster care (Andersen and Wildeman, 2014). Second, the overall effects of community service on recidivism are weak. Although these studies made an important methodological and substantive contribution to research on how alternatives to incarceration affect offenders and their families, they provided limited insight into the long-run consequences of paternal incarceration for children. And, as such, the current study relies on the same policy shock but greatly extends research in this area by considering the long-run consequences of paternal incarceration for children's risk of ever being charged by 22–28 years of age. Nonetheless, because of the important effects of family income and foster care placement on children's subsequent risk of criminal justice contact, it would be reasonable to expect on the basis of these studies that experiencing paternal community service instead of incarceration might decrease children's criminal justice contact.

IDENTIFICATION STRATEGY

Our study exploits the expansion illustrated in figure 1 in the use of community service as an alternative to incarceration to identify the effect of paternal incarceration on children's risk of being charged by 22–28 years of age. Because of the magnitude of the change, we can confidently expect changes in the share of individuals sentenced to incarceration who ultimately serve their time in prison or in the community before and after the reform to be uncorrelated with offender traits. Thus, even if there is selection of offenders with specific characteristics into community service (which is likely), the change in the use of this sentence type caused by the reform will push offenders at the margin from incarceration into community service, and it will introduce exogenous variation into who ultimately serves a prison sentence and who does community service.

This reform expanding the use of community service represents an experimental setup that we may exploit for making causal inference of the effect of community service on child charges using the difference-in-differences (DID) estimator. The major advantage of this estimator in a setup like ours is that it does not only rely on before–after reform comparisons of children's risk of being charged when estimating the reform effect. Because time trends other than the reform we consider may drive the results in such comparisons, the DID furthermore relies on a carefully selected reference group that one may reasonably assume is subject to the same time trends as our reform sample (children of fathers eligible for community service). By subtracting changes in outcomes across the reform year for the reference group from changes in outcomes in our reform sample, the DID factors out other time trends in this specific time period and prevents them from driving the results (for discussion, see Wooldridge, 2002; Greene, 2002).

Because the reference group is so essential to the DID setup, we discuss the treatment and reference groups in detail here. As we mentioned, our treatment group consists of all children whose fathers were sentenced to incarceration, probation, or community service before and after the reform for a series of crimes covered under the reform, including assaults, drunk driving, other serious traffic offenses, or other misdemeanors, as individuals receiving sentences of these types for these specific crime types were covered under the reform (table 1). We restrict our attention only to individuals sentenced to a punishment other than fines because the reform did not target individuals receiving fines as punishments for these crimes and, hence, they do not logically fit within the treatment group. Thus, the roughly 80 percent of individuals convicted of these crimes who received only fines are excluded from the treatment group because they were not eligible for the reform by virtue of their sentence.⁵ For the reference group, we construct a sample of fathers convicted of offenses similar in severity to those covered under the reform that were not covered under the reform. Because individuals convicted of these crimes were not made eligible for community service regardless of the type of conviction they received under the 2000 reform we focus on, we include all individuals sentenced to these crimes in the reference group.

5. The exclusion of these individuals also makes it clear that Denmark does not have penalties as harsh for driving under the influence (DUI) and other serious traffic offenses as table 1 would indicate if it represented all individuals convicted of these offenses.

Table 1. Crime Type, Number, Percentage, and Sentence Types for the Treatment and Reference Groups

Crime Type	N	% ^{b,c}	% Receiving a Specific Sentence Type ^a			
			Fines	Probation	Incarceration	Community Service ^d
Treatment Group						
Simple assault	377	23		49	44	5
Assault	54	3		4	91	5
Stealing from property	58	4		71	17	12
Driving under the influence	1,005	61		23	58	19
Traffic offenses ^e	163	10		2	62	36
Reference Group						
Sexual assaults	87	4	44	30	23	3
Assaults against officers	84	4	26	29	43	2
Threats, negligence, and harm	100	5	50	24	25	1
Forgery	41	2	7	62	26	5
Arson	11	1		27	55	18
Breaking and entering	857	42	93	4	3	
Stealing from vehicle	14	1	50	36	14	
Stealing vehicle	72	4	61	15	21	3
Fraud and malfeasance	243	12	19	55	7	19
Receiving stolen goods	157	8	56	32	8	3
Robbery	8			25	75	
Tax fraud	8			25	50	25
Vandalism	19	1	42	42	16	
Usury	16	1	94		6	
False testimony	10	1			100	
Counterfeiting	8		38	13	25	25
Violating warnings	35	2	86	9	3	3
Illegal eavesdropping	15	1	30	13	27	
Drug law violations	251	12	74	8	16	2

^aBlank cells indicate that no offenders convicted of the specified offense received the specific sentence type. Individuals convicted of the crimes covered in the treatment group but sentenced to fines are excluded from the treatment group as they were not eligible for the reform. This represents roughly 80 percent of individuals sentenced for these crimes.

^bPercentages are based on the treatment or reference group.

^cBlank cells indicate that fewer than 1 percent of the treatment or reference group were convicted of those offenses.

^dAlthough some other offenses were eligible for community service before, the reform only affected crimes in the treatment group.

^eMost traffic offenses involved driving with a suspended license.

Table 1 provides a list of the reference group offenses. Although the reference group is more heterogeneous than the treatment group, the offenses map onto each other well in terms of severity within both the U.S. and the Danish criminal justice systems. Of course, as we will discuss, to mesh well with the DID framework, the two groups must be similar not only in terms of offense severity but also in terms of compositional shifts happening around the time of the reform and must have common trends in the dependent variable prior to the policy shock.

Now that we have a sense of what the treatment and reference groups will consist of, it is worth formally writing out the model. Equation 1 shows the DID. Here, $\bar{y}_{A,1}$ denotes the mean number of children in the reference group with charges by 22–28 years of age during the prereform year; $\bar{y}_{A,2}$ denotes the mean number of children in the reference group with charges by 22–28 years of age during the postreform year. Similarly, $\bar{y}_{B,1}$ and

$\bar{y}_{B,2}$ denote the pre- and postreform year means in child outcomes in our treatment group (children of fathers eligible for community service):

$$\text{reform effect}_1 = (\bar{y}_{B,2} - \bar{y}_{B,1}) - (\bar{y}_{A,2} - \bar{y}_{A,1}) \quad (1)$$

The equation shows how we estimate the reform effect by simply subtracting the mean differences in outcomes across the reform years in the reference group from the mean differences in outcomes across the reform years in the treatment group. Note also that in our case, the DID estimates the intent-to-treat parameter (ITT), rather than the actual treatment effect, as $\bar{y}_{B,2}$ reflects the average outcome for all children with fathers eligible for community service in the postreform sample, rather than just those with fathers who are sentenced to community service.

In our empirical analyses, where we also wish to include control variables (as we will discuss), we use a linear probability model⁶ to estimate the DID parameter as specified in equation 2. Here α_0 is the intercept, T is a dummy variable that takes the value 1 in the postreform year, and S is a dummy variable that takes the value 1 if an individual belongs to our treatment group. In this model, the interaction effect of T and S identifies our reform effect, which means that β_3 is similar to the reform effect specified in equation 1. Notice how the model also allows for the inclusion of a range of control variables, here illustrated as the vector of controls $\beta_4 C$:

$$y = \alpha_0 + \beta_1 T + \beta_2 S + \beta_3 (T * S) + \beta_4 C + \varepsilon \quad (2)$$

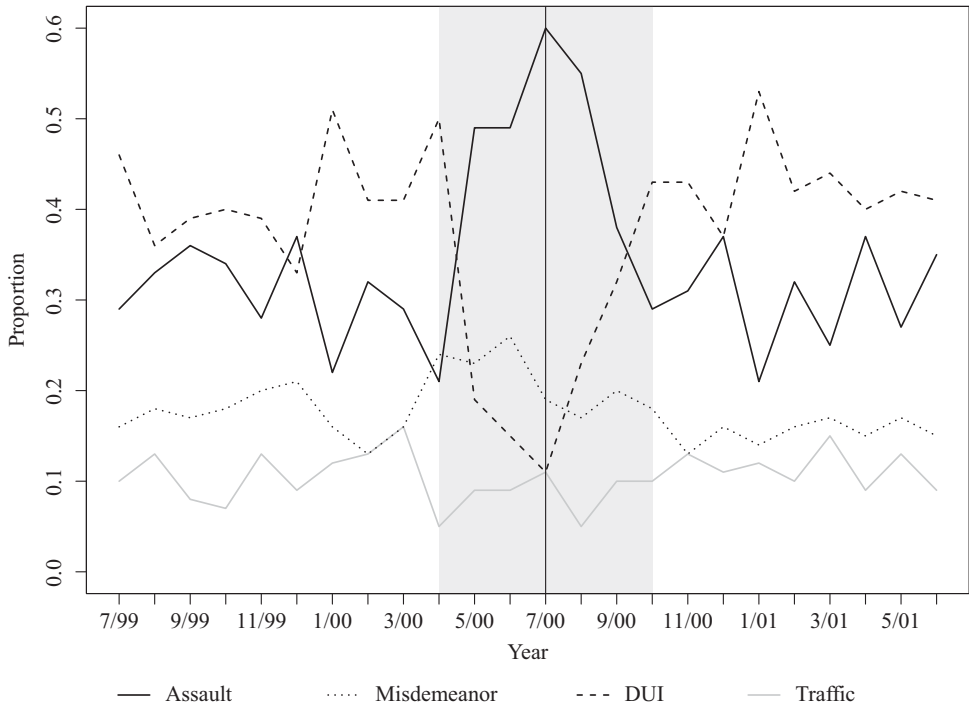
Importantly, the DID relies on the common trend assumption; only if the treatment and reference groups are exposed to the same time trends does the model produce unbiased results. Although we can never fully test this assumption, we can test whether our treatment and reference groups experience common trends in outcomes prior to the implementation of the reform; the presence of common trends prior to the reform will substantiate the claim that the groups would also have experienced common trends in these outcomes across the reform year in the absence of the reform. We furthermore present evidence that the composition of the treatment and reference groups does not change greatly across the reform year to ensure that differences in outcomes between the groups reflect the reform effect, rather than the effects driven by other group dynamics.

When relying on a reform for causal inference, it is also important that we clarify whether the announcement of the reform disturbs the timing of sentences. This could happen if the announcement of the reform comes before its implementation, and if actors in the system have an interest in having certain offenders convicted before (or after) the reform. In that case, offenders convicted before and after the reform may differ in a way that undermines our test. We may detect the extent of this problem by assessing the stability of sentence types in the months immediately between the announcement (April 2000) and when the reform kicked in (July 2000).

As figure 2 shows, there is good reason to be concerned about an announcement effect contaminating our DID results. In April 2000, which was when the reform was

6. We present the model only for the linear probability model here in the interest of simplifying this section.

Figure 2. Proportion of all Sentences by Offense Type, July 1999–June 2001



announced, the monthly proportion of offenders in the four groups affected by the reform shifted dramatically, with the proportion convicted of misdemeanors roughly doubling in the immediate lead-in to the reform, while the proportion convicted of driving under the influence was cut roughly in half. Thus, if we did not remove the 3 months before and after the reform from the analysis, our estimates would likely be contaminated by an announcement effect. All analyses presented in this article, thus, drop the 3 months immediately around when the reform was implemented.

DATA

CONSTRUCTING TREATMENT AND REFERENCE GROUPS FROM REGISTRY DATA

To study the effect of this exogenous shock in the risk of paternal incarceration on children, we rely on administrative data from Statistics Denmark. All residents in Denmark have a unique personal identification number that is used to identify them in all major interactions with public authorities as well as with a range of private institutions (such as banks, for example). By using this unique personal identification number, Statistics Denmark collects information on each Dane and makes these data available for statistical and research purposes. These registers are available as a yearly panel dating

back to 1980, and they contain information on everything, such as dealings with the welfare system, marital status, and criminal justice contact, including incarceration.

The Danish administrative registers are well suited for the study that we undertake in this article for several reasons. First, with the registers we may link parents and children, and we observe the criminal histories of both generations, as well as a range of other individual and family characteristics. Second, there is no attrition in the registers, which is a large advantage when studying vulnerable populations like households that are involved with the criminal justice system in at least one—and maybe many—generations. Obviously, the data are limited to information that is recorded by the personal identification number, and by definition suffer from underreporting in areas such as actual criminality, as the data contain complete information only on crimes that come to the attention of the authorities and are linked with specific individuals.

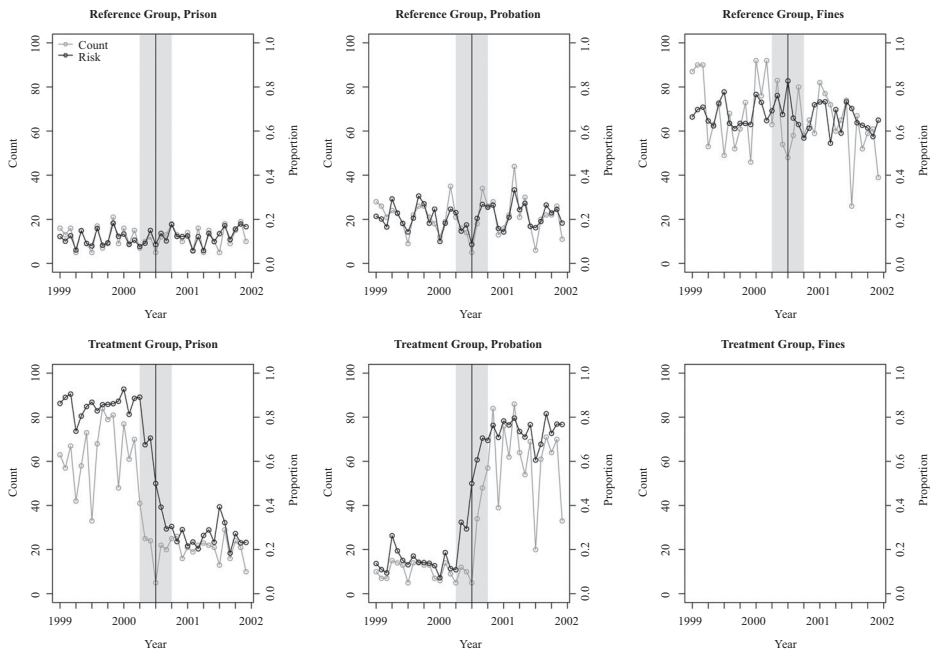
From these data, we construct our treatment and reference groups. To have sufficient data but reduce the influences from macro-level trends that are not dealt with in our DID framework, we include all children of fathers convicted April 1999–April 2000 and September 2000–September 2001. We remove children whose fathers were sentenced in the 3 months before the reform was enacted (but after it was announced) and in the 3 months after it was enacted.

We delimit our treatment and reference groups in four key ways. First, we only include children 12–18 years of age at the time of the conviction. We use this limitation to ensure that the children are not so old that they are no longer susceptible to parental influence or so young that we do not have sufficient data to study their criminal justice contact over the 10 years that we follow them. Second, to prevent instances where the same child appears twice in our data (because the father has more than one conviction in the specified time period) and potentially appears in both the prereform and postreform groups, we only use a child's first appearance in the data. Third, we restrict the sample to children of fathers who did not experience any other convictions during the year prior to their appearance in either group. Finally, as the traits of the immigrant children in our sample vary across the reform years, we exclude these children from our analytic sample. This leaves us with 1,546 children (786 boys and 760 girls) in the treatment group and 1,852 in the reference group (952 boys and 900 girls). We consider girls and boys separately because research has tended to do so (e.g., Besemer et al., 2011; Murray, Janson, and Farrington, 2007), or has focused exclusively on boys (e.g., Murray and Farrington, 2005), and because research including both boys and girls has tended to find qualitatively different effects for these groups.

In addition to constructing a basic reference group, we also use propensity score matching and 1-to-1 matching to construct a second reference group based on the first reference group. Our matched reference group consists of the children in the original reference group that are also comparable with the children in the treatment group on a range of background characteristics, as defined by their propensity score. Table S.1 in the online supporting information⁷ shows the balancing properties of matched samples, by child gender. As with our treatment group, this reference group has 1,546 observations (786 boys and 760 girls).

7. Additional supporting information can be found in the listing for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2017.55.issue-1/issuetoc>.

Figure 3. Number and Proportion of Individuals in the Treatment and Control Group by Sentence Type in the Period Around the Reform



NOTE: The graph for the fines treatment group is intentionally blank to indicate that zero individuals exist in that cell group.

Figure 3 shows that the reform did indeed differentially affect the sentences of the fathers who were convicted in the treatment and reference groups. For fathers in the reference group, there was no discernible effect of the policy shock on their risk of serving time in prison, being placed on probation, or being assessed a fine. The same could hardly be further from the truth for the treatment group. In the period before the reform, children in the treatment group had roughly a 70 percent chance of having their father incarcerated—although there are large monthly fluctuations in this risk because of the small counts. After the reform, however, their probability of having their father incarcerated declined to approximately 30 percent. This change was mirrored by a parallel shift in the probability of having a father sentenced to probation after the reform was implemented. Thus, figure 3 suggests that the reform had a dramatic effect on the probability of experiencing paternal incarceration for children in the treatment group but not in the control group.

Of course, a non-negligible share of the children in our treatment and reference groups is likely to have experienced paternal incarceration prior to the relevant conviction in our analysis, and this may affect our results. On the one hand, children of previously incarcerated fathers will have already been exposed to the detrimental effects of this sentence type and experiencing yet another spell of paternal incarceration may matter little. In contrast, for children of fathers who have no previous experiences of incarceration, whether the father gets sentenced to community service or jail may greatly affect

their subsequent lives. Thus, the effects may vary between these two groups of children, and potentially, the reform effect may be more pronounced in a sample of children with no previous experiences of paternal incarceration. We test this by selecting a subsample of children from our treatment and reference groups who have never experienced paternal incarceration, and we report results based on this sample, along with our other results.

To sum up, our DID analyses rely on four reference groups, all consisting of children of fathers convicted of the offenses described earlier in table 1. The first reference group contains all children whose fathers were convicted of comparably serious crimes not covered under the 2000 reform; the second contains only those that can be matched to children in our treatment group on background traits; the third contains all children from the first reference group that have never previously experienced paternal incarceration; and the fourth contains all children from the matched second reference group who have never experienced paternal incarceration.

DEPENDENT VARIABLE

In the analysis, our dependent variable indicates whether a child has been charged with a crime within 10 years after his or her father's conviction. We use this long time period to address the fact that many children in our sample are younger than the age of criminal liability (which was 16 for most of the analysis) at the time of their father's conviction and younger than the age at which criminal activities peak (e.g., Hirschi and Gottfredson, 1983). The outcome variable is a binary indicator, which takes the value 1 if the child was ever charged in the period and 0 if he or she was not.

As indicated in table 2, 60 percent of the boys in the treatment group and 25 percent of the girls in the treatment group are charged with a crime in the period we consider. Furthermore, as table 3 demonstrates, 64 percent of boys whose fathers were convicted before the reform were charged in the next 10 years; 57 percent of the boys whose fathers were convicted after the reform were charged in the same period. The difference is statistically significant and substantial, corresponding to a 7 percent decline in the risk of being charged. For girls, the overall pre-/postdifference in charges is 2 percent, a difference that is not statistically significant, although it still represents a decline in the risk of being charged. As indicated in table 3, there are no statistically significant pre- and postreform differences on the outcome variable for either the matched or the unmatched reference group.⁸

CONTROL VARIABLES

We use controls in three core ways. First, it is crucial to demonstrate that no differential compositional shifts occur between our treatment and reference groups. For this purpose, we compare descriptive statistics on relevant parameters across the reform year for the treatment and reference groups. Ideally, the composition of each group is constant across the reform, but we may accept compositional changes provided they are similar across the groups because such similarities in changes are compatible with the common trend

8. Tables S.2 and S.3 in the online supporting information show these statistics for children with no prior experiences of paternal incarceration.

Table 2. Descriptive Statistics for the Full Sample

	Unmatched				Matched			
	Reference Group		Treatment Group		Reference Group		Treatment Group	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Boys								
Child charges	.60	.49	.60	.49	.59	.49	.60	.49
Child age	14.62	1.99	14.77	2.01	14.71	2.00	14.78	2.01
Father's logged income	11.79	1.40	11.95	1.48	11.84	1.33	11.95	1.49
Father's unemployment	.10	.30	.10	.30	.10	.30	.10	.30
Father's education	2.18	1.38	2.20	1.29	2.20	1.40	2.20	1.29
Father is single	.54	.50	.47	.50	.47	.50	.47	.50
Father convicted in last 5 years	.54	.50	.54	.50	.53	.50	.54	.50
Father's age	41.60	6.47	41.80	6.00	41.83	6.47	41.80	6.00
Year of current conviction	2000	.83	2000	.83	2000	.82	2000	.83
Month of current conviction	6.28	3.57	6.40	3.55	6.33	3.57	6.40	3.55
# obs.	952		786		786		786	
Girls								
Child charges	.26	.44	.25	.43	.25	.43	.25	.43
Child age	14.80	2.00	14.90	1.99	14.85	2.03	14.90	1.99
Father's logged income	11.82	1.32	12.01	1.26	11.93	1.04	12.01	1.26
Father's unemployment	.11	.31	.08	.27	.08	.27	.08	.27
Father's education	2.15	1.40	2.17	1.30	2.19	1.42	2.17	1.30
Father is single	.54	.50	.45	.50	.48	.50	.45	.50
Father convicted in last 5 years	.53	.50	.55	.50	.53	.50	.55	.50
Father's age	41.99	6.24	41.43	5.59	41.69	6.12	41.43	5.59
Year of current conviction	2000	.82	2000	.86	2000	.83	2000	.86
Month of current conviction	6.08	3.49	6.55	3.48	6.40	3.46	6.55	3.48
# obs.	900		760		760		760	

ABBREVIATIONS: # obs. = number of observations; SD = standard deviation.

Table 3. Pre- and Postreform Descriptive Statistics for the Full Sample

	Treatment Group				Reference Group			
	Prereform		Postreform		Prereform		Postreform	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Unmatched Sample								
Boys								
Child charges	.64	.48	.57	.50	.58	.49	.62	.49
Child age	14.88	2.05	14.68	1.97	14.66	2.01	14.60	1.97
Father's logged income	11.98	1.37	11.92	1.59	11.85	1.10	11.74	1.63
Father's unemployment	.10	.30	.09	.29	.10	.29	.10	.30
Father's education	2.20	1.26	2.21	1.31	2.25	1.37	2.11	1.39
Father is single	.46	.50	.48	.50	.56	.50	.53	.50
Father convicted last 5 years	.57	.50	.52	.50	.53	.50	.54	.50
Father's age	41.73	5.90	41.86	6.10	41.25	6.43	41.93	6.50
Year of current conviction	1999	.47	2001	.44	1999	.48	2001	.43
Month of current conviction	6.42	3.57	6.39	3.53	6.36	3.61	6.21	3.53
Father incarcerated	0.88	0.33	0.23	0.42	0.11	0.31	0.13	0.34
# obs.	369		417		498		454	
Girls								
Child charges	.26	.44	.24	.43	.25	.44	.26	.44
Child age	14.91	1.96	14.89	2.02	14.84	2.05	14.75	1.95
Father's logged income	11.97	1.34	12.05	1.18	11.77	1.46	11.86	1.15
Father's unemployment	.09	.29	.06	.25	.12	.32	.10	.30
Father's education	2.28	1.39	2.08	1.21	2.12	1.37	2.18	1.44
Father is single	.41	.49	.48	.50	.54	.50	.54	.50
Father convicted last 5 years	.55	.50	.54	.50	.53	.50	.53	.50
Father's age	41.49	5.64	41.38	5.55	42.08	6.26	41.88	6.21
Year of current conviction	1999	.45	2001	.43	1999	.49	2001	.42
Month of current conviction	6.86	3.50	6.27	3.46	6.03	3.49	6.13	3.48
Father incarcerated	0.86	0.35	0.25	0.44	0.12	0.33	0.11	0.31
# obs.	359		401		471		429	

(Continued)

Table 3. Continued

	Treatment Group				Reference Group			
	Prereform		Postreform		Prereform		Postreform	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Matched Sample								
								<i>t</i> Test
Boys								
Child charges	.64	.48	.57	.50	.57	.50	.60	.49
Child age	14.88	2.05	14.68	1.97	14.74	2.00	14.68	1.99
Father's logged income	11.98	1.37	11.92	1.59	11.86	1.20	11.82	1.43
Father's unemployment	.10	.30	.09	.29	.11	.31	.09	.29
Father's education	2.20	1.26	2.21	1.31	2.27	1.40	2.13	1.40
Father is single	.46	.50	.48	.50	.49	.50	.45	.50
Father convicted last 5 years	.57	.50	.52	.50	.52	.50	.53	.50
Father's age	41.73	5.90	41.86	6.10	41.54	6.45	42.08	6.49
Year of current conviction	1999	.47	2001	.44	1999	.48	2001	.44
Month of current conviction	6.42	3.57	6.39	3.53	6.39	3.57	6.27	3.57
Father incarcerated	0.88	0.33	0.23	0.42	0.11	0.32	0.14	0.35
# obs.	369		417		364		422	
Girls								
Child charges	.26	.44	.24	.43	.23	.42	.27	.44
Child age	14.91	1.96	14.89	2.02	14.94	1.05	14.80	1.97
Father's logged income	11.97	1.34	12.05	1.18	11.94	1.05	11.92	1.03
Father's unemployment	.09	.29	.06	.25	.09	.29	.07	.26
Father's education	2.28	1.39	2.08	1.21	2.20	1.40	2.19	1.44
Father is single	.41	.49	.48	.50	.46	.50	.50	.50
Father convicted last 5 years	.55	.50	.54	.50	.52	.50	.54	.50
Father's age	41.49	5.64	41.38	5.55	41.69	6.10	41.69	6.14
Year of current conviction	1999	.45	2001	.43	1999	.48	2001	.43
Month of current conviction	6.86	3.50	6.27	3.46	6.32	3.52	6.47	3.40
Father incarcerated	0.86	0.35	0.25	0.43	0.11	0.31	0.11	0.31
# obs.	359		401		380		380	

ABBREVIATIONS: # obs. = number of observations; SD = standard deviation.

assumption. Second, we use controls to calculate the propensity score we use to match children. Third, we adjust for observed differences between the treatment and reference groups in all analyses by using the unmatched sample.

For these purposes, we include measures of socioeconomic characteristics, family characteristics, and previous paternal criminal justice involvement. We measure all indicators during the year prior to the conviction. Table 2 shows the descriptive statistics of these variables for the treatment and reference groups (unmatched and matched) by child gender. The two samples differ only by a little on these characteristics, although there are some significant differences for both boys and girls when we focus just on the unmatched reference group. But as we would expect, we find no significant differences between the samples when we use the matched reference group. Also, because we compare 11 different characteristics for each child gender in two different samples (matched and unmatched), we should adjust our threshold for acceptable significance levels accordingly. Use of the Bonferroni correction implies that we operate with a significance level of .005 ($.05 / 11 = .0045$), rather than with the standard .05 level, which corresponds to a t value of 2.85. In light of this correction, we get a great deal fewer significant differences in the unmatched samples—and we still get no significant differences in the matched samples.

Table 3 shows the extent to which the characteristics differ within groups across the reform year. For boys, none of the control variables change significantly as a result of the reform. The same is true for girls if we operate with the Bonferroni-adjusted significance levels.⁹

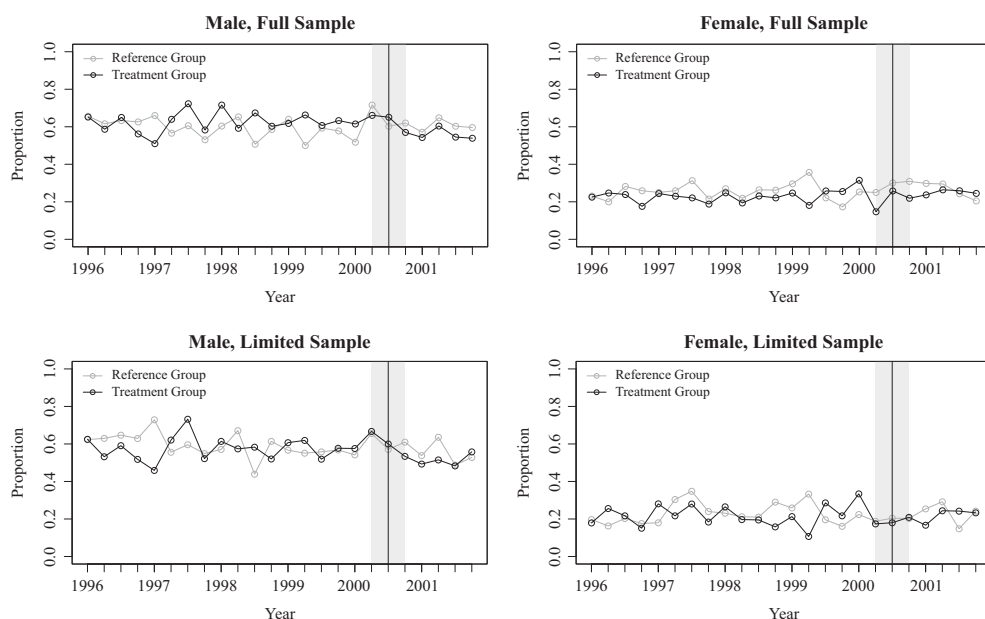
COMMON TRENDS

In the prior section, we showed a marked degree of stability across the reform year in sentence types and controls for our treatment and reference groups. These tests show that compositional changes or changed sentence structures in the two groups are unlikely to drive our results.

A final, but equally important, test to validate our choice of reference group is to show that time trends in the dependent variable are similar across the treatment and reference groups. This test will substantiate our claim that the common trend assumption holds with our reference group. We explore the parallelism by mapping variation on the dependent variable over a longer period prior to the reform date. For this purpose, we expand the treatment and reference groups to include all children who meet the criteria from January 1995 through December 2001. We then compare quarterly levels of child charges for children of fathers convicted of offenses that are and are not (or, in the case of the prereform time periods, would be or would not be) eligible for community service under the reform. Figure 4 displays these trends. As can be observed, fluctuations in the treatment and reference groups occur between the first quarter of 1995 until the time of the reform (the third quarter of 2000). Importantly, however, no group stands out by demonstrating dramatically changing levels of criminal justice contact over this time period, and the observed fluctuations have similar characteristics across groups.

9. For information on changes in the limited sample, see tables S.2 and S.3 in the online supporting information.

Figure 4. Proportion of Male and Female Children Ever Being Charged by Age 22–28 in the Treatment and Control Groups Before and After the Reform



RESULTS

MAIN RESULTS

The goal of this article is to extend research on the effects of paternal incarceration on children by using a Danish policy shock, Danish registry data, and a difference-in-differences framework to test how a policy shock that markedly decreased the risk of paternal incarceration for Danish children also influenced their risk of being charged with a crime at any point in the next 10 years.

Table 4 presents our main results in this regard as it estimates the ITT effects of exposure to the policy shock on children's risk of ever being charged in the next 10 years by gender, and by our four different versions of the reference group. For all of the results we present in this article, we present results from both ordinary least-squares (OLS) regression models and logistic regression models. This step is especially important for considering the differential effects of paternal incarceration by child sex as it would be unrealistic to expect a comparably large absolute shift in the risk of ever being charged for boys and girls given their much different baseline prevalences of this event.

For boys, the estimated effect from the OLS models ranges from $-.09$ to $-.10$, depending on the reference group, and all estimated effects are statistically significant at the .05 level. As the baseline prevalence of having ever been charged was roughly .60 for boys, this .09 to .10 drop in the prevalence of having ever been charged corresponds with roughly a 15 percent decrease in the cumulative prevalence of having ever been charged—a substantial effect of the policy shock.

Table 4. Results From Difference-in-Difference Models Estimating the Effects of Becoming Eligible for Community Service (Relative to Incarceration) on the Basis of the 2000 Community Service Policy Shock, Full and Limited Samples^a

	Matched Reference Group						Unmatched Reference Group					
	OLS			Logistic Regression			OLS			Logistic Regression		
	Full Sample		Limited Sample	Full Sample		Limited Sample	Full Sample		Limited Sample	Full Sample		Limited Sample
	<i>B</i>	SE	<i>B</i>	SE	<i>B</i>	SE	<i>B</i>	SE	<i>B</i>	SE	<i>B</i>	SE
Boys												
Coefficient	-.10	.05*	-.10	.06*	-.43	.21*	-.42	.25†	-.10	.06†	-.40	.20*
# obs.	1,588		1,054		1,588		1,054		1,738		1,738	
Girls												
Coefficient	-.03	.04	-.07	.05	-.16	.24	-.39	.30	-.05	.05	-.13	.23
# obs.	1,504		1,034		1,504		1,054		1,660		1,660	

ABBREVIATIONS: # obs. = number of observations; OLS = ordinary least squares; SE = standard error.

^aThe limited sample includes only fathers who had not even previously been incarcerated before the reform.

† $p < .10$; * $p < .05$.

The estimated effects for boys in models estimated using a logistic regression model were substantial as well. As with the OLS results, the estimates for the logistic regression models were stable—and were significant at either the .05 level (twice) or the .10 level (twice)—and correspond with a 33 percent ($e^{-.40}$) to a 35 percent ($e^{-.43}$) decrease in the odds of ever being charged. Thus, the results for boys using four different reference groups and two different types of models paint a coherent portrait of the policy shock decreasing the risk of ever being charged for affected boys.

The results are similarly clear-cut for girls, although in a different way. In the OLS models, the coefficients for being exposed to the policy shock for girls range from a low of $-.02$ to a high of $-.07$, but they are statistically insignificant across all four models. Results for girls using logistic regression models are likewise statistically insignificant across all four models, although some coefficients are substantial. In the limited sample of girls whose fathers had never been incarcerated before the focal conviction, the point estimates using both the matched ($-.39$ to $-.42$) and unmatched ($-.28$ to $-.40$) reference groups were similar for girls and boys. Nonetheless, none of the results for girls attain significance in any of the eight models we present here, meaning we cannot infer much from the substantial but noisy estimates for girls.

PLACEBO RESULTS

Although the analytic strategy used in this article is far stronger than the designs used by most previous research in this area and yields interesting and important results, at least one concern must still be addressed: that our results merely reflect a difference between children of fathers sentenced to community service and those who are not and that this difference rather than anything having to do with the policy shock is the true underlying driver of our results. To test this concern, we therefore run a series of placebo tests where we artificially shift our reform date back and forth to test whether our results reflect a policy shock or something else entirely.

For each of the eight OLS¹⁰ model specifications shown in table 4, we rerun the analysis 12 times, meaning we run an additional 96 analyses in this stage to provide as stringent a placebo test as possible. Specifically, we move the reform date back 180 days six times—to 180, 360, 540, 720, 900, and 1,080 days before the reform took place—and forward 180 days six times—to 180, 360, 540, 720, 900, and 1,080 days after the reform took place.

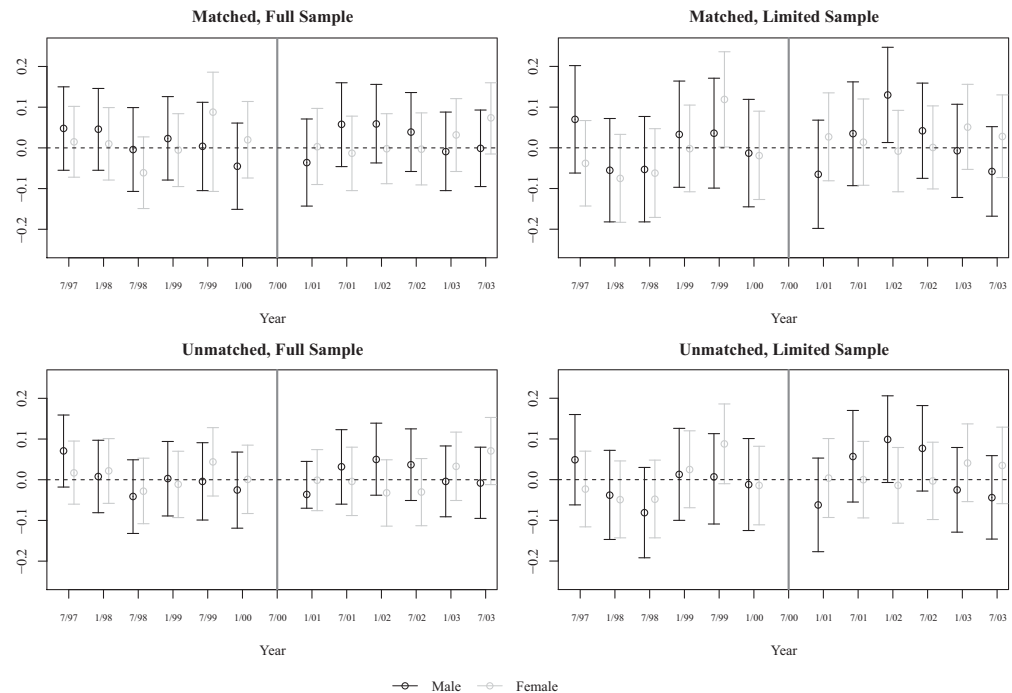
Figure 5 shows the point estimates and 95 percent confidence intervals for these analyses. For boys, 2 of the 48 point estimates are significant at the .10 level—even less than we would expect as a result of chance—and only 1 of the 48 estimates is significant at the .05 level—again even less than we would expect as a result of chance. Even absent a Bonferroni correction, therefore, the results for boys further buttress our claim that there is a real effect of the policy shock on the risk of ever being convicted for Danish boys affected by this change. The results for girls are similar.

DISCUSSION AND CONCLUSION

Although researchers have used a wide array of tests and data sets to consider the consequences of paternal incarceration for child well-being, existing research has yet

10. We present only results from OLS models in this stage to present the results more easily.

Figure 5. Results from Synthetic Regression Models



to exploit an exogenous shock in the risk of paternal incarceration to isolate the causal effects of experiencing this event on children’s criminal justice contact. The lack of such a study is problematic for two reasons, one empirical and the other theoretical. On the empirical side, this omission is problematic because isolating an exogenous shock in the risk of experiencing some event, although certainly no silver bullet, is often thought to provide the strongest possible causal test, even though it also often provides a narrow one. Of course, absent a theoretical account of how paternal criminality (or family background factors) rather than paternal incarceration could drive the paternal incarceration–child criminal justice contact association, the lack of a study such as ours would be less problematic. But given strong reasons to suspect that factors other than incarceration could be driving this relationship (e.g., Sampson, 2011), this omission is especially problematic.

The goal of this article was to fill this gap by using a uniquely strong empirical setup and high-quality data to test the effects of a policy shock that dramatically decreased the risk of paternal incarceration for affected children on their risk of ever being charged with a crime in the next 10 years. By using a Danish criminal justice reform that was introduced in the year 2000 and a difference-in-differences framework, we provide a compelling test of how having a father incarcerated (relative to sentenced to probation with community service) affects children’s risk of ever being charged by early adulthood. Although providing the strongest causal test of this relationship to date is the core contribution of this article, it is also worth noting that a secondary contribution is driven by the type of variation in criminal justice contact we key off. Most studies on the effects of incarceration have focused on shifting the length of time incarcerated rather than on the risk of

experiencing the event (e.g., Green and Winik, 2010; Kling, 2006). Although isolating exogenous variation in sentence length is obviously of the utmost importance, our study provides insight into how children being exposed to substantively different forms of criminal justice contact—probation (with community service) and incarceration—affects their well-being. And, as such, we believe that it offers an even more substantively interesting treatment.

The results provide support for three conclusions. First, boys whose fathers committed crimes covered under the reform were significantly less likely to have ever been charged with a crime in the next 10 years than were boys whose fathers committed comparably serious crimes not covered under the reform. And this effect persists across a host of robustness checks, including ones that drop the months immediately before and after the reform's implementation. Furthermore, carefully constructed synthetic regressions in which we test for implausible effects also indicate that the effects we find in our model may indeed represent real effects. Second, the effects we uncover for boys are not only statistically significant but also substantial. The decrease in children's risk of being charged by 22–28 years of age as a result of the policy ranged from 9 to 10 percentage points depending on the model, which corresponds with an approximate 15 percent decline in the risk of ever being charged by early adulthood. Third, although the point estimates for a policy effect for girls were substantial for some models, none of the results for girls indicated a statistically significant policy effect, leading us to be unsure whether the policy had any causal effect on girls' subsequent risk of ever being charged in the next 10 years. Taken together, our results suggest that paternal incarceration has an independent effect on boys' (but maybe not on girls') risk of criminal justice contact in the next 10 years and that these effects are substantial.

These findings have important implications for how we as criminologists think about the intergenerational transmission of crime in an era of mass incarceration as they imply that at least some of the paternal incarceration–children's criminality and criminal justice contact associations shown in earlier research—is indeed causal for boys. Given the many reasons to expect that this association was not a result of a causal effect of paternal incarceration but of other factors, these findings suggest that future research on the intergenerational transmission of criminality must consider not only the effect of paternal criminality on children's criminal justice contact but also the effect of paternal incarceration on these outcomes. On a broader level, these findings suggest that when we model the incarceration–crime relationship, we should test not only the immediate effects of incarceration on crime, which most research has suggested prompts non-negligible decreases in crime (but see Johnson and Raphael, 2012; Travis, Western, and Redburn, 2014), but also the long-term spillover effects of incarceration on crime as the findings from this article suggest that at the least, there will be spillover effects on the children of incarcerated fathers.

Yet this article nonetheless also has limitations, three of which we highlight here. First, it is unclear how the results generalize to other developed democracies and, most importantly given the United States's very high rates of incarceration, the U.S. context. Although generalizing across contexts is always difficult, we expect that the effects in Denmark represent a lower bound of the U.S.-specific paternal incarceration effect as the more generous Danish social safety net could possibly offset some consequences of paternal incarceration for children. Future research in this area might thus seek to

identify exogenous variation in the risk of paternal incarceration in the United States to provide greater insight into how paternal incarceration affects U.S. children.

Second, we provided no test of the mechanisms through which paternal incarceration increases children's risk of being charged by early adulthood. It may be the case, for instance, that increases in material hardship in households as a result of paternal incarceration drive the effect (e.g., Schwartz-Soicher, Geller, and Garfinkel, 2011). Or the increases in harsh maternal parenting behaviors as a result of paternal incarceration (e.g., Turney, 2014),¹¹ the trauma of having a parent forcibly removed from the household (e.g., Comfort, 2008), or the stigma of having a father incarcerated (e.g., Braman, 2004) may instead be driving the effects that we find. But on the basis of the current analysis, we simply do not know what the driver is. This is not merely an academic concern as, absent changes in the incarceration rate, the most direct way to minimize the consequences of paternal incarceration for children is to design interventions that address the mechanisms driving these consequences. As such, future research in this area should seek to understand better the consequences through which paternal incarceration affects children.

Third, the current study considered only the consequences of paternal incarceration for one type of outcome—children's criminal justice contact—and, hence, it remains unclear whether many of the other well-documented paternal incarceration–child well-being associations in fact represent causal relationships or merely correlations. For some outcomes that have received much attention in the literature, such as children's externalizing and aggressive behaviors (e.g., Geller et al., 2012; Wakefield and Wildeman, 2011), a causal effect seems likely given the strong link between childhood aggression and subsequent criminal justice contact. Yet for other outcomes, the importance of providing a strong causal test is acute. Future research in this area must thus seek to provide stronger causal tests across a range of child outcomes.

Despite these limitations, this article nonetheless makes an important contribution not only to research on the consequences of paternal incarceration for children but also to research on the intergenerational transmission of criminality as it shows using exogenous variation in the risk of paternal incarceration introduced by a Danish policy reform that paternal incarceration increases boys' (but not girls') risk of ever being charged with a crime by early adulthood.

REFERENCES

- Andersen, Signe Hald. 2015. Serving time or serving the community? Exploiting a policy reform to assess the causal effects of community service on income, social benefit dependency and recidivism. *Journal of Quantitative Criminology* 31:537–63.
- Andersen, Signe Hald, and Christopher Wildeman. 2014. The effect of paternal incarceration on children's risk of foster care placement. *Social Forces* 93:269–98.
- Besemer, Syske, Victor Van der Geest, Joseph Murray, Catrien C.J.H. Bijleveld, and David P. Farrington. 2011. The relationship between parental imprisonment and offspring offending in England and the Netherlands. *British Journal of Criminology* 51:413–37.

11. Research has found minimal consequences of paternal incarceration for the positive parenting behaviors of mothers (Turney and Wildeman 2013), so the decrease in positive parenting is not a plausible mechanism in the U.S. context.

- Braman, Donald. 2004. *Doing Time on the Outside: Incarceration and Family Life in Urban America*. Ann Arbor: University of Michigan Press.
- Clear, Todd R. 2007. *Imprisoning Communities: How Mass Incarceration Makes Disadvantaged Communities Worse*. New York: Oxford University Press.
- Comfort, Megan. 2008. *Doing Time Together: Love and Family in the Shadow of the Prison*. Chicago, IL: University of Chicago Press.
- Esping-Andersen, Gosta. 1990. *The Three Worlds of Welfare Capitalism*. New York: Wiley.
- Fergusson, David M., L. John Horwood, and Daniel S. Nagin. 2000. Offending trajectories in a New Zealand birth cohort. *Criminology* 38:525–52.
- Foster, Holly, and John Hagan. 2007. Incarceration and intergenerational social exclusion. *Social Problems* 54:399–433.
- Foster, Holly, and John Hagan. 2015. Punishment regimes and multilevel effects of parental incarceration: Intergenerational, intersectional, and interinstitutional models of social inequality and systemic exclusion. *Annual Review of Sociology* 41:135–58.
- Geller, Amanda, Carey E. Cooper, Irwin Garfinkel, Ofira Schwartz-Soicher, and Ronald B. Mincy. 2012. Beyond absenteeism: Father incarceration and child development. *Demography* 49:49–76.
- Green, Donald P., and Daniel Winik. 2010. Using random judge assignments to estimate the effects of incarceration and probation on recidivism among drug offenders. *Criminology* 48:357–87.
- Greene, William H. 2002. *Econometric Analysis*. Upper Saddle River, NJ: Prentice Hall.
- Hagan, John, and Holly Foster. 2012a. Intergenerational educational effects of mass imprisonment in America. *Sociology of Education* 85:259–86.
- Hagan, John, and Holly Foster. 2012b. Children of the American prison generation: Student and school spillover effects of incarcerating mothers. *Law & Society Review* 46:37–69.
- Hirschi, Travis, and Michael Gottfredson. 1983. Age and the explanation of crime. *American Journal of Sociology* 89:552–84.
- Huebner, Beth M., and Regan Gustafson. 2007. The effect of maternal incarceration on adult offspring involvement in the criminal justice system. *Journal of Criminal Justice* 35:283–96.
- Johnson, Elizabeth I., and Beth Easterling. 2012. Understanding unique effects of parental incarceration on children: Challenges, progress, and recommendations. *Journal of Marriage and Family* 74:342–56.
- Johnson, Rucker, and Steven Raphael. 2012. How much crime reduction does the marginal prisoner buy? *Journal of Law and Economics* 55:275–310.
- Kling, Jeffrey R. 2006. Incarceration length, employment, and earnings. *American Economic Review* 96:863–76.
- Loeffler, Charles E. 2013. Does imprisonment alter the life course? Evidence on crime and employment from a natural experiment. *Criminology* 51:137–66.
- Massoglia, Michael. 2008. Incarceration as exposure: The prison, infectious disease, and other stress-related illnesses. *Journal of Health and Social Behavior* 49:56–71.
- Murray, Joseph, Catrien C.J.H. Bijleveld, David P. Farrington, and Rolf Loeber. 2014. *Effects of Parental Incarceration on Children: Cross-National Comparative Studies*. Washington, DC: American Psychological Association.

- Murray, Joseph, and David P. Farrington. 2005. Parental imprisonment: Effects on boys' antisocial behaviour and delinquency through the life-course. *Journal of Child Psychology and Psychiatry* 46:1269–78.
- Murray, Joseph, and David P. Farrington. 2008. Parental imprisonment: Long-lasting effects on boys' internalizing problems through the life-course. *Development and Psychopathology* 20:273–90.
- Murray, Joseph, Carl-Gunner Janson, and David P. Farrington. 2007. Crime in adult offspring of prisoners: A cross-national comparison of two longitudinal samples. *Criminal Justice and Behavior* 34:133–49.
- Murray, Joseph, Rolf Loeber, and Dustin Pardini. 2012. Parental involvement in the criminal justice system and the development of youth theft, marijuana use, depression, and poor academic performance. *Criminology* 50:255–302.
- Pager, Devah. 2003. The mark of a criminal record. *American Journal of Sociology* 108:937–75.
- Porter, Lauren C., and Ryan D. King. 2015. Absent fathers or absent variables? A new look at paternal incarceration and delinquency. *Journal of Research in Crime and Delinquency* 52:414–43.
- Pratt, John. 2008. Scandinavian exceptionalism in an era of penal excess—Part I: The nature and roots of Scandinavian exceptionalism. *British Journal of Criminology* 48: 119–37.
- Robins, Lee N., Patricia A. West, and Barbara L. Herjanic. 1975. Arrests and delinquency in two generations: A study of Black urban families and their children. *Journal of Child Psychology and Psychiatry* 16:125–40.
- Roettger, Michael E., and Raymond R. Swisher. 2011. Associations of fathers' history of incarceration with sons' delinquency and arrest among Black, White, and Hispanic males in the United States. *Criminology* 49:1109–47.
- Sampson, Robert J. 2011. The incarceration ledger. *Criminology & Public Policy* 10: 819–28.
- Schwartz-Soicher, Ofira, Amanda Geller, and Irwin Garfinkel. 2011. The effect of paternal incarceration on material hardship. *Social Service Review* 85:447–73.
- Travis, Jeremy, Bruce Western, and Steve Redburn (eds.). 2014. *The Growth of Incarceration in the United States: Exploring Causes and Consequences*. Washington, DC: National Academies Press.
- Turney, Kristin. 2014. The consequences of paternal incarceration for maternal neglect and harsh parenting. *Social Forces* 92:1607–36.
- Turney, Kristin, and Christopher Wildeman. 2013. Redefining relationships explaining the countervailing consequences of paternal incarceration for parenting. *American Sociological Review* 78:949–79.
- United Nations Office of Drugs and Crime. 2015. *UNODC Statistics*. <https://data.unodc.org/>.
- van de Rakt, Marieke, Joseph Murray, and Paul Nieuwbeerta. 2012. The long-term effects of paternal imprisonment on criminal trajectories of children. *Journal of Research in Crime and Delinquency* 49:81–108.
- Wakefield, Sara, and Christopher Uggen. 2010. Incarceration and stratification. *Annual Review of Sociology* 36:387–406.
- Wakefield, Sara, and Christopher Wildeman. 2011. Mass imprisonment and racial disparities in childhood behavioral problems. *Criminology & Public Policy* 10:793–817.

- Wakefield, Sara, and Christopher Wildeman. 2014. *Children of the Prison Boom: Mass Incarceration and the Future of American Inequality*. New York: Oxford University Press.
- Walmsley, Roy. 2013. *World Prison Population List*. 10th ed. London, U.K.: Home Office.
- West, Donald James, and David P. Farrington. 1977. *The Delinquent Way of Life: Third Report of the Cambridge Study in Delinquent Development*. London, U.K.: Heinemann Educational Books.
- Western, Bruce. 2006. *Punishment and Inequality in America*. New York, NY: Russell Sage Foundation.
- Wildeman, Christopher. 2009. Parental imprisonment, the prison boom, and the concentration of childhood disadvantage. *Demography* 46:265–80.
- Wildeman, Christopher. 2010. Paternal incarceration and children's physically aggressive behaviors: Evidence from the fragile families and child wellbeing study. *Social Forces* 89:285–309.
- Wildeman, Christopher. 2012. Imprisonment and infant mortality. *Social Problems* 59:228–57.
- Wildeman, Christopher, and Lars H. Andersen. 2015. Cumulative risks of paternal and maternal incarceration in Denmark and the United States. *Demographic Research* 32:1567–80.
- Wildeman, Christopher, Signe Hald Andersen, Hedwig Lee, and Kristian Bernt Karlson. 2014. Parental incarceration and child mortality in Denmark. *American Journal of Public Health* 104:428–33.
- Wildeman, Christopher, and Christopher Muller. 2012. Mass imprisonment and inequality in health and family life. *Annual Review of Law and Social Science* 8:11–30.
- Wildeman, Christopher, Jason Schnittker, and Kristin Turney. 2012. Despair by association? The mental health of mothers with children by recently incarcerated fathers. *American Sociological Review* 77:216–43.
- Wildeman, Christopher and Kristin Turney. 2014. Positive, negative, or null? The effects of maternal incarceration on children's behavioral problems. *Demography* 51:1041–1068.
- Wildeman, Christopher, Sara Wakefield, and Kristin Turney. 2013. Misidentifying the effects of parental incarceration? A comment on Johnson and Easterling (2013). *Journal of Marriage and Family* 75:252–8.
- Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Sectional and Panel Data*. Cambridge, MA: MIT Press.

Christopher Wildeman is an associate professor of policy analysis and management at Cornell University in Ithaca, New York, and a senior researcher at the Rockwool Foundation Research Unit in Copenhagen, Denmark. His research considers the prevalence, causes, and consequences of parental incarceration and child welfare contact in the United States and Denmark.

Signe Hald Andersen is the director of research at the Rockwool Foundation Research Unit in Copenhagen, Denmark. Her current research interests include the intergenerational transmission of risks, with particular focus on crime and foster care.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Table S.1. Balancing Properties of the Matching Models

Table S.2. Descriptive Statistics for the Limited Sample

Table S.3. Pre- and Postreform Descriptive Statistics for the Limited Sample