

# Drinking alone?

The effect of alcohol treatment programs  
for drunk drivers on relationship stability

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**Abstract**

This paper tests whether the introduction of an alcohol treatment program for drunk drivers, which allowed this groups of offenders to avoid prison and participate in a rehabilitation program instead, increased their relationship stability. Using the natural experiment that occurred with the implementation of the program in 1990 and exploiting the rich Danish administrative data, my study has two contributions. First, it shows that the introduction of the program causally increases offenders' relationship stability by 3 percentage points (from 33 to 36 percentage points). Second, it contributes to the discussion on what alternative sanction forms we should offer offenders, to secure their long term outcomes, by testing whether the increased relationship stability observed among the treated offenders resort from their pardon from prison or from their participation in the rehabilitation program. The test suggests that the rehabilitation program drives the effect.

Keywords: Drunk driving, IV models, Relationship stability

## **Introduction**

With the focus on how the informal consequence of incarceration also concerns the families of (ex-)inmates, we have ample reason to study the link between incarceration and relationship stability. A divorce – or comparable endings of less formalized romantic relationships – is the final signal that a particular family constellation is dysfunctional, and if incarceration accounts, at least partly, for the break-up, it is a strong indicator of the extent and power of the informal consequences. To address this question, previous studies argue that having a criminal spouse poses a serious threat to marriage and they demonstrate the high break-up rates among ex-inmates and their partners (Barnes et al, 2014; Hagan & Dinovitzer, 1999; Lopoo & Western, 2005; Massoglia et al, 2011; Siennick et al., 2014; Turney, forthcoming; Western & McLanahan, 2000; Western & Pettit, 2010; Western & Wildeman, 2009).

The separation between the partners that result from the incarceration may explain a large part of this finding; incarceration limits the emotional interactions nourishing romantic relationships (Geller et al, 2011) and the absence of one partner from the household for a shorter and longer period of time is likely to distort the partners' post-incarceration division of labor and power balance (Rindfuss & Stephen, 1990). Also, the separation causes the two parts to build separate experiences (Western & McLanahan, 2000; Geller et al, 2011; Turney & Wildeman, 2013), which increases the likelihood that they grow apart and end up divorcing. In addition, the mere stigma of having an incarcerated partner matters. Ex-inmates as well as their partners are likely to be exposed to a number of prejudices that are activated when their surroundings learn that they have spent time in prison (Apel et al, 2010; Western & McLanahan, 2000; Wildeman & Western, 2010). The only way that the partners of inmates can escape such prejudice is to end the relationship.

Importantly though, there is a likely correlation between the probability of experiencing incarceration and of having specific personality traits that may also influence the probability that offenders experience relationship instability. As discussed in the seminal work of Gottfredson and Hirschi (1990) many offenders suffer from lack of self-control and this personality trait is likely to also reduce and their probability of

entering and maintaining a stable relationship. This explanation implies that ex-inmates would fall victims of relationship instability also in the counterfactual situation where they do not experience incarceration.

A few studies have attempted to separate the causal effect of the incarceration from other effects – such as the effect of e.g. specific personality traits. Here Charles & Luoh, (2010) shows how an increase in the share of incarcerated men within a geographical area causally reduces the share of married women in that area, and Robert Apel and coauthors (2010) use propensity score matching to document increased divorce probabilities among ex-inmates. Such causal studies represent important milestones in our understanding of the detrimental effects of incarceration, however, they have only limited policy implications if we wish to punish offenders – e.g. to deter them of future crime. Instead we need evidence of the causal effect of non-custodial alternatives to prison.

To contribute to this part of the literature, my study tests how a non-custodial alternative to prison introduced to drunk drivers in Denmark in 1990 affects the relationship stability of this offender group. With this alternative, drunk drivers were pardoned from prison provided that they participated in and completed an alcohol treatment program with the duration of at least one year. This non-custodial alternative to prison is interesting because it not only exempts offenders from prison, it also offers a replacement which represents an actual punishment (i.e. the participation in the program), a punishment that also addresses and treats the roots of the crime. Such alcohol treatment programs represent possible, policy relevant alternatives to the damaging prison sentences for drunk drivers, and may inspire the introduction of similar custom made treatment programs for other offender groups.

With this setup, my study contributes first, by providing a causal estimate of the effect of escaping prison for drunk drivers and second, by considering also the causal effect of a program aimed at treating the criminal nature of the offenders. I find that the program has a non-negligible, positive effect on the relationship stability of drunk drivers; while only 33 percent of the controls experience relationship stability following their conviction, this applies to 36 percent of the treated. My empirical setup only allows for an estimation of the intent-to-treat (ITT) parameter, however, back-of-the-envelope-calculations suggest that relationship

stability is improved by as much as 7.5 percent among those drunk drivers who actually follow and complete the program. Further analyses imply that the effect does not differ by sentence length. Thus, the program works, not by removing incarceration as a source of separation between the spouses, but by rehabilitating the person and hopefully relieving him of his alcohol problem.

## **Background**

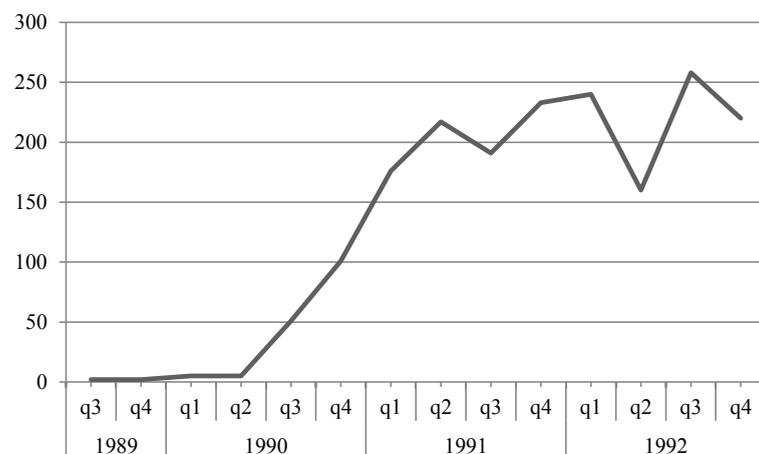
In 1986 the Danish Prison Service set down a committee with the purpose of finding a way to reduce alcohol abuse among criminal Danes. As background material to this work, the committee surveyed the extent of alcohol use among inmates imprisoned for drunk driving, and they found that as many as 2/3 of all drunk drivers consumed so much alcohol that they required treatment. This finding inspired the search for alternative forms of punishment and part of the agenda was to provide a punishment which would also reduce recidivism among this particular group of offenders by addressing and treating the characteristics causing their offending.

As a consequence, the Prison Service piloted an alcohol treatment program among a small group of convicted drunk drivers in 1989. This program allowed offenders to either follow an alcohol treatment program while imprisoned or, to avoid prison altogether, had they already initiated treatment. This last option paved the way for the 1990 scheme. In July 15<sup>th</sup>, 1990, Danish politicians implemented an alcohol treatment and pardon scheme of drunk drivers. This scheme implied that offenders sentenced to prison for 1 to 40 days due to drunk driving, could apply for pardon if they participated in and completed a comprehensive 1-year alcohol treatment program and did not recidivate to crime within a two-year period after the sentence. The program consisted in group therapy and a controlled intake of the medication Antabus, which is known for making its users extremely sick when consuming alcohol. The program only applied to drunk drivers who had not caused injury on other human beings as a result of their drunk driving.

In this initial phase program participation was not assigned by the judges sentencing the drunk drivers, rather, after their conviction, the drunk drivers could apply for program participation at the Prison Service. Thus, far from all eligible drunk drivers participated in the program and those that actually participated

program increased from 20 to 40 percent, which meant that a substantial share of the targeted group entered the program (Clausen Nielsen & Kyvsgaard, 2007; Clausen Nielsen, 2007). Figure 1 below shows the influx into the program from the 3<sup>rd</sup> quarter of 1989 till the 4<sup>th</sup> quarter of 1992. The figure illustrates the low number of participants in the first pilot scheme tested before July 15 1990, and the rapidly increasing influx from that date onwards.

Figure 1: Quarterly influx of participants into the alcohol treatment program



Note: The figure is based on data from Direktoratet for Kriminalforsorgen (1994)

In April 1994 the Prison Service expanded the program to also include drunk drivers with sentences of between 40 and 60 days, and in 2000 decision makers decided to convert the program from its administrative status into an actual sentence option for judges to choose. Today, the alcohol treatment program is an established part of the pamphlet of non-custodial sentences in Denmark that also includes sentence types such as community services and electronic monitoring. Of these, only the alcohol treatment program aims at rehabilitating the offender by fixing the roots of his or her criminal behavior. Thus evaluating the consequences of this policy change for the relationship stability of those convicted of drunk driving will

provide us with important knowledge, not only on the effect of being exempt from prison, but also on the effect of replacing a prison sentence with another type of punishment.

### ***Drunk drivers***

Admittedly, drunk drivers represent a very specific group of criminals: Drunk driving rarely constitutes the back bone of a criminal career, since this type of crime does not produce any monetary or tradable material gains. Also, drunk drivers differ from other criminals, not only by the nature of their crime, but also by their personality traits and socio-economic resources (Hubicka et al, 2010; Jornet-Gibert et al, 2013; Maxwell, 2012; Portman et al, 2013). Despite, or maybe even because of this, their reactions to a new type of non-custodial punishment, merits general criminological attention.

First, this offender group faces the same type of punishment as other offenders, including prison sentences. In Denmark, whether drunk driving triggers a fine or a prison sentence depends on the driver's blood alcohol content (BAC) and the number of previous convictions of drunk driving. In general, driving with a BAC of 0.05 (5 grams of alcohol in 10 liters of blood) is considered drunk driving, and a BAC of 0.2 or higher always releases a prison sentence. This compares to a US practice where most states consider driving with a BAC of 0.08 drunk driving and a BAC of 0.15 releases a prison sentence (though this level varies by state). Second, drunk drivers make up between 5 and 15 percent of the total offender group (depending on who constitutes the gross group), which means that their responses to punishment will impact our overall assessment of the consequences of punishment. Third, with lower initial criminal propensities than other convicted offenders and fewer expectations of experiencing incarceration at some point their lives, this group is likely to suffer more from the incarceration than other offenders (Lapham & England-Kennedy, 2012). As a consequence, the negative effects of incarceration will show up quite clearly in this offender group.

Fourth, and most importantly, the immediate roots of the offense are more visible among drunk drivers than among other offender groups. It is their urge to drink, their lack of self-control and their inability to foresee the consequences of their actions when drunk which causes the crime. And these roots are treatable - at least to some degree. One may train the ability to exert self-control in specific situations, and Antabus may help

fight the urge to drink. Thus with the option to treat the origins of drunk driving, we have a valid alternative to sending this offender group off to prison. Focusing on this offender group in the right empirical setup, we may improve our understanding of not only the implications of escaping prison, but also of getting rid of the personality traits causing the crime in the first place. Thus drunk drivers represent an interesting case for studying the consequences of alternative punishment forms, which includes a treatment element. And given that Gottfredson and Hirschi are right in claiming that the roots of most criminal acts is lack of self-control, knowledge on how to improve self-control among one group of offenders (the drunk drivers) may inspire programs improving the self-control among other offender groups.

### **Method**

To test the effect of the alcohol treatment program on relationship stability, one would ideally want to conduct an experiment that randomizes the alcohol treatment program among a large group of drunk drivers and test differences in outcomes between those that got the program – the treated - and those that did not get the program – the controls. Given the law of large numbers, this randomization implies the treated and the controls are identical on all observed and unobserved characteristics and the only thing causing potential differences in their outcomes would result from the differences in their treatment status. This would then give us the causal effect of the treatment on a given outcome.

In this study, I exploit the natural experiment which occurred with the introduction of the alcohol treatment program in 1990. With this program, drunk drivers' chances of participating in an alcohol treatment program and avoiding prison increased quite dramatically (cf. figure 1), and provided that individuals convicted of drunk driving before and after July 15<sup>th</sup> 1990 are identical, any differences in their outcomes reflect their different chances of participating in the program. In the absence of a fully controlled, randomized experiment as described above, this natural experiment provides me with a nice and useful setup for studying the effect of the program.

Importantly though, we have no individual level registration of drunk drivers participating in and completing the alcohol treatment program in its early years, we only know the aggregated numbers that I used for

constructing figure 1. This lack of information means that I cannot estimate the effect of the program for those who actually participated in it. Also, as mentioned, the considerable self-selection of offenders into the program during this period poses a serious challenge to finding a suitable control group for the treated.

We do, however, know that during the first years, the participation rate among drunk drivers fulfilling the formal requirements of the program lies at a substantial level of between 20 and 40 percent. Thus, rather than estimating the average treatment effect for the treated (which is impossible due to the lack of information on who gets the treatment) and facing the problems of identifying a useful control group, I estimate the Intent To Treat parameter (the ITT). This strategy requires a simple model, where I just compare the outcomes of those who would never get the chance of participating in the program (i.e. those convicted before the introduction of the program) with those that had that chance - because they were convicted after the introduction of the program – and of which 20 to 40 percent actually participated in and completed the program. Assuming that those convicted of drunk driving before and after July 15 1990 are identical on all observed and unobserved parameters, the ITT represents a causal estimate.

I refer to all those who had the option to participate – even if they did not actually participate – as the treated and all those who did not have the option as the controls. Hence my treatment group includes all drunk drivers who receive their conviction after the introduction of the program July 15, 1990. I estimate the ITT using a standard OLS model. Here relationship stability is my outcome variable and treatment status is my main dependent variable. I furthermore include a range of controls, which I describe below.

Since the ITT parameter reflects the average outcomes of those who participated in the program and those who had the chance of participating but who did not apply for it, it will always represent a lower bound estimate of the true effect: Assuming that my hypothesis is true, that the program enhances relationship stability, the effect size achieved through ITT will reflect a weighted average of the larger share of stable relationships among the actual participants and the lower share of stable relationships among the non-participants (the last share should ideally correspond to the share of stable relationships among the controls).

## Data and variables

To study the effect of the alcohol treatment program on the relationship stability of the convicted drunk drivers, I use data from Statistics Denmark. In Denmark, all residents have a unique personal number that identifies the resident in a great many transactions, such as interactions with the welfare system, schooling and work status. Statistics Denmark makes these data available for statistical and research purposes, and the panel goes as far back as 1980. These data provide information on criminal offences, convictions, relationship status and a range of other background characteristics such as age, number of children, educational levels, income etc. With their time range and type of information, these data are highly suited for testing how the introduction of the alcohol treatment program affected the relationship stability of those convicted of drunk driving around 1990.

From this data, I select all individuals convicted of drunk driving from a year before the introduction of the alcohol treatment program July 15, 1990 till a year after the introduction. In accordance with the program specifications, I exclude those sentenced to more than 40 days of prison, and those whose drunk driving caused injury to other human beings. This gives me a sample of 5,504 individuals. Of these, 2731 individuals (49.62 percent) receive their conviction before the introduction of the program and the rest, 2773 individuals receive their conviction after the introduction. Due to the experimental nature of my treatment, this procedure for sample selection secures treatment and control groups which are identical on all observed and unobserved characteristics (as also demonstrated in table 1). I could also have chosen a narrower or wider time frame for my sample (e.g. 6 months or 2 years before and after the introduction of the program), however with the 1 year time frame I cover potential seasonal variation in drunk driving, but reduce the influence from too much macro level variation.

To test the effect of the alcohol treatment program on relationship stability I use two binary outcomes variables. The first variable takes the value one when the offender has the same partner the year of the conviction and the first year after the conviction. 1894 of the 5504 drunk drivers (34.4 percent) has the same partner in these two years. The second variable takes the value one when the offender has the same partner

the year of the conviction and the first as well as the second year after the conviction. 1682 of the 5504 drunk drivers has the same partner in these three years (30.6 percent). Note that I do not distinguish between cohabitation and marriage. This is because in Denmark, as in other Scandinavian countries, stable cohabitation is in many ways considered equal to marriage, at the legal as well as at the individual and emotional level (Duvander, 1999). Thus distinguishing between the two states is not necessarily fruitful for understanding relationship stability among drunk drivers.

As shown in table 1, there is a substantial, significant difference between the number of stable relationships in the control group and the treatment group: The 4 percent difference is in favor of the treatment group, and indicates that drunk drivers who got the option to avoid prison and get the alcohol treatment program were more likely to have a stable relationship compared to drunk drivers who did not get this option.

## Control variables

I also include a range of control variables. First there is a group of standard demographic indicators such as gender, children, ethnicity, income and education. Table 1 shows descriptive statistics by group membership, and we learn that most individuals in both groups are men (with only 10 respectively 11 percent women in both groups). The offenders are on average in their late 30's and only few are immigrants. Approximately a third has children, 43 percent have not pursued education beyond elementary school, and average yearly income the year before the conviction in the control group is 170.000 DKK (~26.000 USD) and 164.000 DKK (~ 25.000 USD) in the treatment group. This large difference in yearly income between the treatment and the control group is significant at a 10 percent level which is a cause for concern, as it may reflect further unobserved differences between the two groups. Importantly, the difference results from a few high earners in the control group, and removing these individuals from the sample does not alter my overall conclusions in any way.

My second group of variables consists of measurements of relationship status prior to the conviction. I include these measurements to ensure that the two groups have had identical relationship experiences, so that potential post conviction differences in relationship stability between the two groups it is not merely a

reminiscent of different pre-conviction relationship statuses. Thus I include measures of whether or not the offender was single the year before the conviction, and whether or not the offender has been in a stable relationship before the conviction (i.e. having had the same partner 1 to 3 years prior to the conviction).

There are no differences between the two groups on these variables.

Table 1: Descriptive statistics

Variable	Control group Mean (std. dev.)	Treatment group Mean (std. dev.)	Difference (std. err)
Same partner, 2 years	0.33 (0.47)	0.36 (0.48)	0.04 (0.01)**
Same partner, 3 years	0.29 (0.45)	0.32 (0.47)	0.04 (0.01)**
Gender (1=female)	0.10 (0.30)	0.11 (0.31)	0.01 (0.01)
Age	37.91 (10.80)	38.41 (10.66)	0.49 (0.29)
Immigrant	0.04 (0.20)	0.04 (0.20)	0.00 (0.01)
Any children	0.29 (0.45)	0.30 (0.46)	0.01 (0.01)
Income (in 100.000 DKK), t-1	1.70 (1.35)	1.64 (1.06)	-0.06 (0.04)†
Education (only elementary school)	0.43 (0.50)	0.43 (0.50)	-0.00 (0.01)
Single, 1 year before conviction	0.53 (0.50)	0.53 (0.50)	-0.00 (0.01)
Same partner, 1-3 years before the conviction	0.68 (0.47)	0.67 (0.47)	-0.01 (0.01)
Prison 1 year before conviction	0.05 (0.23)	0.05 (0.22)	-0.00 (0.01)
Prison 2 years before conviction	0.04 (0.22)	0.05 (0.23)	0.00 (0.01)
Conditional sentence, 1 year before conviction	0.01 (0.11)	0.01 (0.10)	-0.00 (0.00)
Conditional sentence, 2 years before conviction	0.01 (0.12)	0.01 (0.11)	-0.00 (0.00)
Any crime, 1 year before conviction	0.20 (0.47)	0.19 (0.47)	-0.01 (0.01)
Any crime, 2 years before conviction	0.20 (0.48)	0.19 (0.48)	-0.01 (0.01)
Sentence length	18.17 (7.11)	17.76 (6.77)	-0.42 (0.19)*
No. of observations	2,731	2,773	

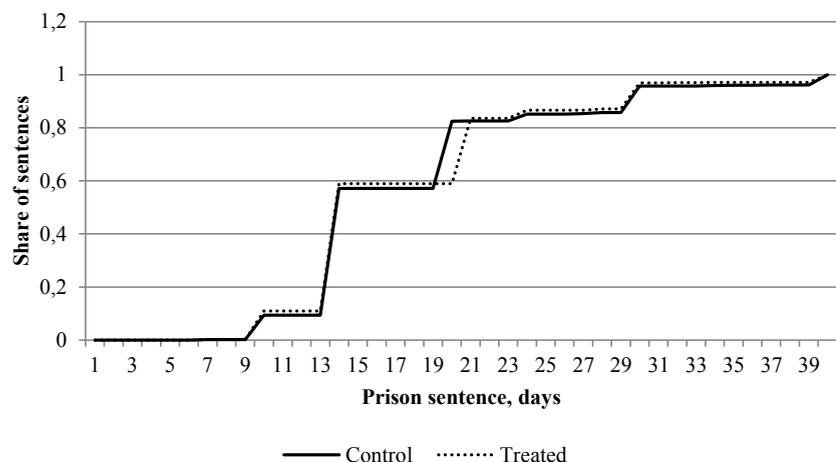
\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

My third group of variables measures criminal activities prior to the current conviction. Again this is to ensure that the two groups are identical on any characteristics that may affect their relationship statuses and here, criminal history seems relevant. Thus I include indicators of imprisonment, conditional sentences and any convictions 1 respectively 2 years prior to the conviction. None of these indicators differ significantly across the groups.

Last, I include the number of days each offender is sentenced to prison. Using sentence length as an indicator of the severity of the crime committed, this is to ensure an even distribution of degree of criminality in my two groups. Unfortunately there is a small difference between the two groups on this indicator – my control group receives sentences that are approximately half a day longer than what I observe in the treatment group. This is not much, but again it may be a proxy of other unobserved differences between the two groups, and one may fear that the introduction of the alcohol treatment program has somehow changed judges' sentencing behavior: say that judges wished to promote the use of the program they could simply decide to give sentences of 40 days or less.

To investigate this even further, figure 2 illustrates the differences between the two groups. The figure shows that a larger share of controls receives 20 days prison sentences, which is driven by a tendency among the judges to give sentences of 20 rather than 21 days before the introduction of the program. Also, the figure shows that the sentence length distribution of the treated is just slightly skewed to the right throughout its range. It is unlikely, however, that the share of offenders with specific sentence lengths differs between the groups – except for at 20/21 days. This is evidence against the concern that the program changed judges sentencing behavior; we should observe the major difference between the groups at the cut-off point of eligibility, had that been the case. Including this variable in the model will control for differences related to sentence lengths between the treated and the controls.

Figure 2: Cumulative distribution of sentence length, by treatment status



Note: The figure is based on data from Direktoratet for Kriminalforsorgen (1994)

## Results

With the substantial difference presented in table 1, between treated and controls in their score on the two outcome variables, it is not surprising that also the estimated effect of the treatment – of being convicted after July 15<sup>th</sup> 1990 – is positive and significant for both outcomes. Table 2 show results from my main model, where, in addition to estimating the effect of my treatment, I also include my control variables. According to these models, my treatment group is 3 percentage points more likely to stay with the same partner for both 2 and 3 years after their conviction. Hence, having had the option to participate in an alcohol treatment program instead of going to prison stabilizes one’s relationship.

As described previously, this is an ITT parameter, which reflects the combined effect for those convicted after July 15<sup>th</sup> 1990 who receive the program (approximately 40 percent) and those convicted after that date who go to prison. If we assume that the introduction of the program does not affect the incarcerated part of the treatment group, a back-of-the-envelope calculation suggests that the program improves the relationship

stability by as much as 7.5 percentage points among the actual program participants (0.3/0.4). This is a substantial, non-negligible effect.

Table 2: Main models

Variable	Same partner, 2 years	Same partner, 3 years
Treatment: convicted after July 15 <sup>th</sup> 1990	0.03 (0.01)**	0.03 (0.01)**
Gender (1=female)	-0.05 (0.02)**	-0.06 (0.02)***
Age	0.01 (0.00)***	0.01 (0.00)***
Immigrant	0.01 (0.03)	0.04 (0.03)
Any children	0.20 (0.01)***	0.18 (0.01)***
Prison 1 year before conviction	0.03 (0.02)	0.04 (0.02)
Prison 2 years before conviction	0.04 (0.02)†	0.03 (0.02)
Conditioned sentence, 1 year before conviction	0.01 (0.05)	0.04 (0.05)
Conditioned sentence, 2 years before conviction	0.06 (0.04)	0.03 (0.01)
Any crime, 1 year before conviction	-0.01 (0.01)	-0.01 (0.01)
Any crime, 2 years before conviction	0.00 (0.01)	-0.00 (0.01)
Income (in 100.000 DKK )	0.02 (0.00)**	0.02 (0.00)***
Education (only elementary school)	-0.01 (0.01)	0.00 (0.01)
Single, 1 year before conviction	-0.52 (0.01)***	-0.48 (0.01)***
Same partner, 1-3 years before the conviction	0.12 (0.01)***	0.14 (0.01)***
Sentence length	-0.00 (0.00)	-0.00 (0.00)*
Intercept	0.30 (0.03)***	0.23 (0.03)***
F-test/R <sup>2</sup>	307.68***/0.48	269.92***/0.44

\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

From table 2, we furthermore learn that female drunk drivers are less likely to stay with the same partner, but that higher income increases this probability. Also – and as could be expected – having children increases relationship stability. Offenders who were single the year before their conviction are also much less likely to experience relationship stability after their conviction, just as offenders with stable pre-conviction

relationships are more likely to experience relationship stability after their conviction. In contrast to what I expected, previous crime does not seem to destabilize relationships.

Table 2 furthermore shows that relationship stability increases by age. This could be expected since older offenders are probably more likely to form part of a stable relationship at the time of the conviction compared to younger offenders, simply because the probability of being in a relationship and appreciating its benefits increases by age. This also implies that older offenders drive the effect that I see in my main models. I split the sample into four age groups to test this relation further. As shown in table 3, it is in fact the older offenders that drive the effect, particularly with regards to my first outcome: while there is no effect of the treatment on offenders aged 15-29 and 30-44 years of age, I get significant effects for offenders in the age groups 45-59 and 60-74. Note however that the effect of the treatment on my second outcome for offenders aged 60-74 is only marginally significant, indicating that the age pattern is less pronounced for this outcome.

Table 3: Results by age group

Age group	Same partner, 2 years	Same partner, 3 years
15-29 years	0.03 (0.02)	0.03 (0.02)
30-44 years	0.01 (0.01)	0.02 (0.01)
45-59 years	0.04 (0.02)*	0.04 (0.02)*
60-74 years	0.12 (0.05)*	0.10 (0.06)†

\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

In a similar vein, based on the findings in Western & McLanahan (2000) which implies that incarceration is more likely to destabilize weak relationships, it is relevant to also test the stability of the effects among subgroups as defined by their relationship history. Table 4 shows treatment estimates of two re-specifications: in the first re-specification I only estimate the model based on the 2307 offenders who were in a relationship at the time of their conviction. They should be the ones driving the effects found in the main models. I retrieve the treatment effects found in the main models, but while the effect sizes are similar across the two specifications, the standard errors are larger in the new models. This is an indication that the

reduction in number of observations in my models reduces the precision with which I estimate the coefficients of the re-specified models.

In the second re-specification I estimate the models separately by subgroups as defined by whether or not they were in a stable relationship at the time of their conviction (these are relationships lasting for more than three years). From this second re-specification we see that offenders who were not in a stable relationship at the time of their conviction drive the effects found in the main models. Their treatment effect is similar to what I found in the main models presented in table 2, while the treatment effect for offenders in stable relationships at the time of the conviction is small and insignificant. This finding makes theoretical sense, as it seems likely that new relationships are more vulnerable to the stress produced when a partner is absent due to incarceration or to the continued stress and discomfort resulting from the partner's untreated alcoholism. This finding is in accordance with the findings of Western & McLanahan (2000).

Table 4: Results by relationship status

Specification	Same partner, 2 years	Same partner, 3 years
Only offenders in a relationship at the time of the conviction	0.03 (0.01)*	0.03 (0.02)†
Only offenders in unstable relationship at the time of the conviction (<3 years)	0.04 (0.01)**	0.03 (0.01)*
Only offenders in stable relationship at the time of the conviction (> 3 years)	0.01 (0.01)	0.01 (0.01)

\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

### *Mechanisms*

As described, the treatment administered to the drunk drivers in my sample has – at least – two components. On the one hand the offenders are obliged to participate in a program that addresses the roots of their criminal behavior. But on the other hand, they also avoid going to prison. This means that two mechanisms may explain my findings – the lack of exposure to the incapacitation/deterrence effect of prison and the

rehabilitation effect of the program. There is no straightforward way of separating the two effects, since none of the offender groups – or subgroups - are exposed to only one mechanism. We may, however, test variations in the effect across prison sentences. If the treatment effect does not vary by the length of the prison sentence that the alcohol treatment program replaces, it is an indication that the separation between the two partners resulting from the incapacitation is irrelevant for relationship stability and that only the rehabilitation caused by program participation matters. If, on the other hand, the treatment effect varies by length of the prison sentence it is an indication that the rehabilitation aspect of the program is not the main explanation.

For this purpose, I first test differences in the effect between offenders who receive prison sentence of 14 day or less or 15 days or more. Second, I test differences in effects between offenders sentenced to 21 days or less and offenders sentenced to 22 days or more (note that I hereby circumvent the problem described earlier and illustrated in figure 2, pertaining to judges switching from 20 to 21 days of prison). With these thresholds I get two (four) relatively large groups, thus ensuring that any lack of significance does not result from problems pertaining to small groups.

I conduct the two tests by constructing two new dummy variables: the first dummy variable takes the value 1 when the offender has received a sentence of more than 14 days and the second takes the value 2 when the offender has received a sentence of more than 21 days. I interact these dummy variables with treatment status and include both the new dummy variable and the interaction term in my models. With this setup, the interaction term will test whether the treatment effect varies across sentence length, and a significant coefficient will indicate that that is indeed the case.

Table 5 shows the results from these tests, and as can be seen, the interaction term is insignificant in all models. This is empirical evidence that the treatment effect does not vary across sentence length. This test is then a strong indication that the program works by rehabilitating the offender, rather than by preventing longer periods of separation between partners, due to incarceration.

Table 5: Results by sentence length

Specification	Same partner, 2 years	Same partner, 3 years
Treatment: convicted after July 15 <sup>th</sup> 1990	0.04 (0.01)**	0.03 (0.01)**
Prison sentences>14 days	-0.01 (0.01)	-0.01 (0.02)
Interaction term	-0.02 (0.02)	0.00 (0.02)
Treatment: convicted after July 15 <sup>th</sup> 1990	0.03 (0.01)**	0.04 (0.01)**
Prison sentences>21 days	-0.02 (0.02)	-0.02 (0.01)
Interaction term	-0.01 (0.02)	-0.02 (0.02)

\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

### **Robustness check**

Importantly though, we need to ensure that my effects do not resort from macro level changes in relationship stability that co-occur with the implementation of the alcohol treatment program. If, for reasons not related to the changed sentencing practice of drunk drivers, drunk drivers become more likely to experience stable relationships at the beginning of the 1990'ies, this will show up as a significant effect of the treatment and lead us to falsely conclude that the program works.

To further investigate this possibility I conduct two robustness checks. First, I test differences in the relationship stability of drunk drivers sentenced from a year before till a year after the implementation of the program, but who got prison sentences of more than 40 days – which meant that they were not eligible for the program. Second, I test difference in relationship stability of drunk drivers sentenced from year before till a year after the implementation of the program, but whose sentence only included a fine or a conditional sentence. Again this is a group of drunk drivers not eligible for the program. Even though the nature of their offences differs between these groups and the group of drunk drivers in focus in my study – as indicated by their different punishments – it is my claim that these groups represent the closest possible comparison groups, that are most likely to experience the same macro trends in relationship stability as the drunk drivers eligible for the program. If I find a significant effect of being sentenced before and after the program for

these two groups, it is an indication that something, besides the implementation of the program, affects the relationship stability of drunk drivers during those particular years.

Table 6 shows the results from these tests, and we learn that neither drunk drivers sentenced to more than 40 days of prison nor drunk drivers sentenced to fines or conditional sentences experience any significant change in their relationship stability across the reform years. In all cases are the coefficients insignificant (note however, that the number of offenders who get prison sentences of more than 40 days is quite low, which may explain the model's large standard error and thus the insignificant result). These findings provide evidence that the change in relationship stability observed among the drunk drivers who become eligible for the alcohol treatment program on July 15<sup>th</sup> 1990 is a result of the changed sentencing practice.

Table 6: Robustness checks

Offender groups	Same partner, 2 years	Same partner, 3 years
Drunk drivers, prison sentences 40<	0.06 (0.04)	0.04 (0.04)
Drunk drivers, fines only	0.01 (0.02)	0.02 (0.02)

\*\*\*: p<0.001, \*\*: p<0.01, \*: p<0.05, †: p<0.1

Note: Models for drunk drivers sentenced to more than 40 days of prison is based on 301 offenders. Models for drunk drivers sentenced to fines only are based on 1059 offenders.

## Conclusion

This paper extends existing research on incarceration and relationship instability in two ways. First it estimates the causal effect of a non-custodial sentence introduced to drunk drivers in 1990. Drunk drivers participating in the program could avoid prison provided that they participated in and completed an alcohol treatment program. My estimates show that drunk drivers who had the option to avoid prison by participating in this program – and of whom as many as 40 percent participated – experienced higher relationship stability compared to drunk drivers who did not have the option to participate in the program. The ITT-parameter

corresponds to a treatment effect of 3 percentage points, however as explained, this type of parameter almost automatically expresses only a lower bound effect. Subsequent back-of-the-envelope calculations suggest that the treatment effect for those who actually enter and complete the program is as high as 7.5 percentage points. According to these findings the informal consequences of incarceration are substantial and that they do indeed also concern the families of (ex-)inmates. My findings corresponds well with the findings presented in Apel (this issue), who uses the NLSY and multiple-event, discrete time hazard models to estimate the short- and long-term effects of incarceration on cohabitational and marital status. He finds that respondents who was recently released from jail or prison were 2.45 times more likely than other respondents to experience a change in cohabitational/marital status. Using data from the Fragile Families and Child Wellbeing Study and fixed effects models, Wildeman et al (this issue) additionally demonstrate how relationship instability is equally likely across facility types (local jail, state prison, federal prison, and unknown facility types), even if the ethnographic evidence presented by Comfort (this issue) indicates that short term confinement is particularly troublesome for the offender's intimate partner and other family members.

The second contribution of my paper consists in discussing and attempting to identify the effect of the different components of the program to increase our understanding of how to satisfy the need for punishing offenders in a situation where we do not wish to expose them to the negative consequences of prison. Here, the alcohol treatment program is unique by replacing classic principles of punishment – incapacitation and deterrence - with rehabilitation. My empirical setup for this test is not as clean as one could have wanted because the program contains two treatment components – the absence of prison and the rehabilitation. Still, my analyses indicate the program effect does no vary across offenders with different sentence lengths, which suggest that is the actual alcohol treatment (the rehabilitation component), rather than the direct effect of incarceration, which drive my results. We may tentatively interpret this result as a support to Gottfredson and Hirschi's (1990) claim that it is personality traits, such as lack of self-control, which explains the link between incarceration and relationship instability, rather than separation or stigma.

My findings suggest that if we wish to reduce the informal consequences of incarceration, we cannot just pardon offenders' prison sentence but must also address and relieve them of the personality traits causing them to offend. It is not only important that we do not send offenders to prison, it is also important what we offer instead. Thus today, when we witness the much needed drive towards more lenient sanctions against offenders in e.g. the US, it is crucial that we consider new alternatives and their informal consequences carefully. And even if the present analysis rests on a Danish case – with a criminal justice system that is very different from the one found in the US – we have little reason to suspect that the mechanisms identified in Denmark would be substantially different from what would be at stake in other countries. The individual propensity for drunk driving may very well reflect the same personality traits across contexts, and rehabilitation programs which are found to improve these personality traits in one context are likely to also improve such personality traits in another context.

These contributions aside, one could ask why we should care about the relationship stability of drunk drivers – is the relationship status of a select group of offenders important enough to justify our attention? It is, and it is so for a number of reasons. First, and most importantly, stable relationships are a resource for vulnerable individuals and their children (Western & McLanahan, 2000); if a prison sentence causes divorce, it may be the beginning – or mark the acceleration – of an actual social deroute, something which is costly both socially and individually. Second, and related, we as a society can only be interested in securing the stability of offenders' lives, not the least to prevent them from reoffending and causing additional harm to themselves and others. By helping offenders maintain or initiate stable lives after they have served their sentence, it is likely that their reoffending rates drop.

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