Drinking Alone? The Effect of an Alcohol Treatment Program on Relationship Stability for Convicted Drunk Drivers in Denmark

By SIGNE HALD ANDERSEN

This article tests whether an alcohol treatment program for drunk drivers in Denmark increased the stability of their relationships with spouses or cohabiting partners. The treatment program, implemented in 1990, allowed a group of offenders to avoid prison and participate in a rehabilitation program. I use it here as a natural experiment, exploiting a rich administrative dataset to show that the program marginally increases offenders' relationship stability. I also test whether increased relationship stability observed among the treated offenders results from their pardon from prison or from their participation in the rehabilitation program. Results suggest that the rehabilitation program drives the effect. These findings contribute to the literature on what alternative sanctions could be offered to offenders to improve their long-term social outcomes.

Keywords: drunk driving; IV models; relationship stability

Because the informal consequences of incarceration concern not only the (ex)inmates but also their families, the link between incarceration and relationship stability warrants investigation. A divorce—or a comparable ending of a less formalized romantic relationship—is the final signal that a particular family unit is dysfunctional, and if there is a link between incarceration and divorce it will be a strong indicator of the extent and power of the informal consequences of incarceration. Previous studies argue that having a criminal spouse poses a serious threat to marriage; research demonstrates the high break-up rates among ex-inmates and their partners (Barnes et al. 2014; Hagan and Dinovitzer 1999; Lopoo and Signe Hald Andersen is a senior researcher at the Rockwool Foundation Research Unit in Copenhagen, Denmark. Her recent research on inequality, the intergenerational transmission of disadvantages, crime, and the child welfare system has been published in Demography, Social Forces, American Journal of Public Health and Journal of Marriage and Family.

Correspondence: sha@rff.dk

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Western 2005; Massoglia, Remster, and King 2011; Siennick, Stewart, and Staff 2014; Turney 2015; Western and McLanahan 2000; Western and Pettit 2010; Western and Wildeman 2009).

The separation of partners because of incarceration may explain a large part of this finding; incarceration limits the emotional interactions that nourish romantic relationships (Geller, Garfinkel, and Western 2011), and the absence of one partner from the household for a period of time is likely to distort the partners’ postincarceration division of labor and power balance (Rindfuss and Stephen 1990). Also, the separation causes the two people to build separate experiences (Western and McLanahan 2000; Geller, Garfinkel, and Western 2011; Turney and Wildeman 2013), which increases the likelihood that they will grow apart and end up divorcing. In addition, the 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 others around them learn that they have spent time in prison (Apel et al. 2010; Western and McLanahan 2000; Wildeman and Western 2010). The only way that the partners of inmates can escape such prejudice is to end the relationship.

What is important, though, is that there is a likely correlation between the probability of experiencing incarceration and having specific personality traits that contribute to relationship instability. As discussed in the seminal work of Gottfredson and Hirschi (1990), many offenders suffer from a lack of self-control, and this trait is likely to reduce their probability of entering and maintaining a stable relationship. This explanation implies that ex-inmates would fall victim to relationship instability even in the counterfactual situation where they do not experience incarceration.

A few studies have attempted to separate the causal effects of incarceration from other effects. Charles and Luoh (2010) show how an increase in the share of incarcerated men within a geographical area causally reduces the share of married women in that area. 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, these studies have limited policy implications for how to deter (ex)offenders from future crimes. Therefore, we need evidence as well for the causal effects of noncustodial alternatives to prison.

To contribute to this literature, this study tested how a noncustodial alternative to prison introduced to individuals convicted of drunk driving in Denmark in 1990 affected 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 of at least one year. This noncustodial alternative to prison is interesting not only because it exempts offenders from prison but also because it offers a “replacement” punishment (i.e., the participation in the program)—a punishment that addresses and treats a root cause of the crime. Such alcohol treatment programs are possible alternatives to the damaging prison sentences for drunk drivers and may inspire the introduction of similar treatment programs for other offender groups.
My study contributes to the literature by providing a causal estimate of the effect of “escaping” prison and considering the causal effect of a program aimed at treating a root cause of the crime committed. I find that the program has a nonnegligible, 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 allows only 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 treatment 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 up a committee tasked with 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 two-thirds of all drunk drivers consumed so much alcohol that they required treatment. This finding inspired the search for alternative forms of punishment for criminals suffering from alcoholism, and part of the agenda was to provide a punishment that would also reduce recidivism among this particular group of offenders by addressing and treating the characteristics that caused them to offend.

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 either to follow an alcohol treatment program while imprisoned or to avoid prison altogether, had they already initiated treatment. This second option paved the way for the 1990 scheme. On July 15, 1990, Danish politicians implemented an alcohol treatment and pardon scheme for drunk drivers. This scheme allowed offenders sentenced to prison for 1 to 40 days due to drunk driving to apply for pardon if they participated in and completed a comprehensive one-year alcohol treatment program and did not recidivate to crime within a two-year period after the sentence. The program consisted of group therapy and the controlled intake of Antabus, a medication known for making its users extremely sick if they were to consume alcohol. The program applied only to drunk drivers who had not caused injury to others as a result of their drunk driving.

In the 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 who actually participated probably represented a very select group of offenders. Still, within a few months, the participation rate for the program increased from 20 to 40 percent, which
meant that a substantial share of the targeted group had entered the program (Clausen Nielsen and Kyvsgaard 2007; Clausen Nielsen 2007). Figure 1 shows the influx into the program from the third quarter of 1989 until the fourth 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 onward.

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 converted the program into a sentence option from which judges could choose. Today, the alcohol treatment program is an established part of the pamphlet of noncustodial sentences in Denmark that also includes sentence types such as community services and electronic monitoring. Of these, only the alcohol treatment program aims to rehabilitate the offender by fixing the roots of his or her criminal behavior. Evaluating the consequences of this policy change for the relationship stability of those convicted of drunk driving, then, will provide us with important knowledge not only about the effect of being exempt from prison, but also on the effect of replacing prison with another type of punishment.

**Drunk drivers**

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

This offender group faces the same punishments 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 results in a prison sentence. This compares to the U.S. practice, where most states consider driving with a BAC of 0.08 drunk driving and a BAC of 0.15 results in a prison sentence (though this level varies by state). 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 the overall assessment of the consequences of punishment. With lower initial criminal propensities than other convicted offenders and fewer expectations of experiencing incarceration, this group is likely to suffer more from incarceration than other offenders (Lapham and England-Kennedy 2012). Therefore, the negative effects of incarceration will show up quite clearly in this offender group.

The immediate roots of the offense are also more visible among drunk drivers than among other offender groups. Combinations of their urge to drink, their lack of self-control, and their inability to foresee the consequences of their actions are the root of the crime. And these roots are treatable—at least to some degree. One may learn to exert self-control in specific situations, and Antabus may help to fight the urge to drink. By 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 repressing the personality traits that cause the crime in the first place. Thus drunk drivers represent an interesting case for studying the consequences of a form of alternative punishment that includes a treatment element. Given Gottfredson and Hirschi’s (1990) claim that the root 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 to improve 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 availability of alcohol treatment among a large group of drunk drivers and test differences in outcomes between those who were treated and those who were not—the control group. With a sufficiently large number of enrollees, randomization implies that the treatment and the control groups are identical on all observed and unobserved characteristics, and the only thing causing potential differences in their outcomes would be treatment status.

In this study, I exploit a natural experiment that occurred in Denmark with the introduction of an alcohol treatment program in 1990. With this program, drunk
drivers’ chances of participating in alcohol treatment and avoiding prison increased dramatically (cf. Figure 1). Provided that individuals convicted of drunk driving before and after July 15, 1990, are identical, any differences in their outcomes reflect their differing chances of participating in the program. In the absence of a fully controlled, randomized experiment as described above, this natural experiment provides a useful setup for studying the effect of the program.

There was no individual-level registration of drunk drivers either participating in or completing the alcohol treatment program in its early years; we know only the aggregated number that I use here to construct Figure 1. This lack of information means that I cannot accurately estimate the effect of the program on those who actually participated in it. Also, the considerable self-selection of offenders into the program during this period poses a serious challenge to finding a suitable control group.

We do, however, know that during the initial years of the program, the participation rate among drunk drivers was between 20 and 40 percent. Thus, rather than estimating treatment effects (which is impossible due to lack of information), I estimate the intent to treat (ITT). With this strategy, I compare the outcomes of those who did not get the chance to participate in the program (i.e., those convicted before the introduction of the program) with those who did have the chance (i.e., those convicted after the introduction of the program, which includes the 20 to 40 percent who actually participated in 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 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 were convicted after the introduction of the program in July 15, 1990. I estimate the ITT using a standard ordinary least squares model. Here, relationship stability is my outcome variable and treatment status is my main dependent variable. I also 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 nonparticipants (the last share should ideally correspond to the share of stable relationships among the controls).

Data and Variables

In Denmark, all residents have a unique personal number that identifies their interactions with the welfare system, schooling, and workforce. Statistics Denmark makes these data available for statistical and research purposes. The panel data go as far back as 1980, providing information on criminal offences,
convictions, relationship status, age, number of children, educational levels, income, and so on. 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 these data, I select all individuals convicted of drunk driving from a year before the introduction of the alcohol treatment program (July 15, 1989) to a year after the introduction (July 15, 1991). 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 others. This gave me a sample of 5,504 individuals. Of these, 2,731 individuals (49.62 percent) received their conviction before the introduction of the program and the remainder, 2,773 individuals, received their conviction after the introduction. This procedure for sample selection secures treatment and control groups that are identical on all observed and unobserved characteristics (as demonstrated in Table 1). I could have chosen a narrower (e.g., six months) or wider (e.g., two years) time frame for my sample, however with the one-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 outcome variables. The first variable accounts for whether the offender has the same partner the year of the conviction and the first year after the conviction ($1 = \text{yes}$). Of the 5,504 drunk drivers, 1,894 (34.4 percent) had the same partner in these two years. The second variable accounts for whether the offender has the same partner the year of the conviction and both the first and second year after the conviction ($1 = \text{yes}$). Of the 5,504 drunk drivers, 1,682 had the same partner in these three years (30.6 percent). Note that I do not distinguish between cohabitation and marriage. 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 levels (Duvander 1999).

As Table 1 shows, 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 were given the option to avoid prison and participate in the alcohol treatment program were more likely to have a stable relationship compared with drunk drivers who did not get this option.

Control variables

I also include a range of control variables. First I include a group of standard demographic indicators such as gender, whether the offenders have 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 and 11 percent women in both groups, respectively). The offenders are on average in their late thirties, and only a few are immigrants. Approximately one-third has children, 43 percent have not pursued education beyond elementary school, and the average yearly income the year before conviction in the control group is 170,000 DKK (~26,000 USD) and 164,000 DKK (~25,000 USD).
USD) in the treatment group. This large difference in yearly income between the treatment and the control groups is significant at the 10 percent level, which is a cause for concern, as it may reflect further unobserved differences between the two groups. However, it is important to note that removing these individuals from the sample does not alter my overall findings.

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 postconviction differences in relationship stability between the two groups is not merely a remnant of different preconviction relationship statuses. Thus, I include measures of whether the offender was single the year before the conviction and whether the offender had been in a stable relationship before the conviction (i.e., having had the same partner one to three years prior to the conviction). There are no differences between the two groups on these variables.

**TABLE 1**
Descriptive Statistics

| Variable                                      | Control group Mean (SD) | Treatment group Mean (SD) | Difference (SD) |
|-----------------------------------------------|-------------------------|---------------------------|-----------------|
| Same partner, 2 years                         | .33 (.47)               | .36 (.48)                 | .04 (.01)**     |
| Same partner, 3 years                         | .29 (.45)               | .32 (.47)                 | .04 (.01)**     |
| Gender (1 = female)                           | .10 (.30)               | .11 (.31)                 | .01 (.01)       |
| Age                                           | 37.91 (10.80)           | 38.41 (10.66)             | .49 (.29)       |
| Immigrant                                     | .04 (.20)               | .04 (.20)                 | .00 (.01)       |
| Any children                                  | .29 (.45)               | .30 (.46)                 | .01 (.01)       |
| Income (in 100,000 DKK), t-1                  | 1.70 (1.35)             | 1.64 (1.06)               | -.06 (.04)†     |
| Education (only elementary school)            | .43 (.50)               | .43 (.50)                 | -.00 (.01)      |
| Single, 1 year before conviction              | .53 (.50)               | .53 (.50)                 | -.00 (.01)      |
| Same partner, 1–3 years before the conviction | .68 (.47)               | .67 (.47)                 | -.01 (.01)      |
| Prison 1 year before conviction               | .05 (.23)               | .05 (.22)                 | -.00 (.01)      |
| Prison 2 years before conviction              | .04 (.22)               | .05 (.23)                 | .00 (.01)       |
| Conditional sentence, 1 year before conviction| .01 (.11)               | .01 (.10)                 | -.00 (.00)      |
| Conditional sentence, 2 years before conviction| .01 (.12)               | .01 (.11)                 | -.00 (.00)      |
| Any crime, 1 year before conviction           | .20 (.47)               | .19 (.47)                 | -.01 (.01)      |
| Any crime, 2 years before conviction           | .20 (.48)               | .19 (.48)                 | -.01 (.01)      |
| Sentence length                               | 18.17 (7.11)            | 17.76 (6.77)              | -.42 (.19)*     |

No. of observations: 2,731 2,773

**p < .01. *p < .05. †p < .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 any other imprisonment, conditional sentences, and convictions both one and two years prior to the current conviction. None of these indicators differ significantly across the groups.

Last, I include the number of days each offender was sentenced to prison. Using sentence length as an indicator of the severity of the crime committed ensures 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 an indication of other unobserved differences between the two groups. Or, perhaps, the introduction of the alcohol treatment program somehow changed judges’ sentencing behaviors: they may have wished to promote participation in the treatment program and, therefore, decided to give sentences of 40 days or fewer.

Figure 2 illustrates the differences between the two groups. The figure shows that a larger share of controls received 20-day 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. It is unlikely, however, that the share of offenders with specific sentence lengths differs between the groups—except at 20/21 days. This provides evidence to the contrary that the program changed judges’ sentencing behavior; if it had, we would observe the major difference between the groups at the cutoff point of eligibility. Including this variable in the model will control for differences in sentence lengths between the treated and the controls.

Results

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

As described previously, this is an ITT parameter, which reflects the combined effect for those convicted after July 15, 1990, who received drug treatment (approximately 40 percent) and those who went 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, nonnegligible effect.

From Table 2, we learn that female drunk drivers are less likely to stay with the same partner. However, their higher income increases the probability that they stay with the same partner, just as having children may increase relationship stability. Offenders who were single the year before their conviction are much less likely to experience relationship stability after their conviction, just as offenders with stable preconviction relationships are more likely to experience relationship stability after their conviction. In contrast to what I expected, previous criminal behavior does not seem to destabilize relationships.

Table 2 further shows that relationship stability increases by age. This could be because older offenders are more likely than younger offenders to be in a stable relationship at the time of the conviction. 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 who drive the effect, particularly with regard to my first outcome: while there is no effect of the treatment on offenders aged 15 to 29 and 30 to 44 years old, 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.

Based on the findings in Western and McLanahan (2000), which indicate that incarceration is more likely to destabilize weak relationships, I also test the effects among subgroups as defined by their relationship history. Table 4 shows treatment estimates of two respecifications: in the first respecification, I estimate
the model based only on the 2,307 offenders who were in a relationship at the time of their conviction. They should drive 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 because there are fewer observations in my models, the precision with which I estimate the coefficients of the respecified models is reduced.
In the second respecification, I estimate the models separately by subgroups as defined by whether the offenders were in a stable relationship at the time of their conviction (these are relationships lasting for more than three years). From this second respecification, 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 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 in Western and McLanahan (2000).

### Mechanisms

The treatment administered to the drunk drivers in my sample has at least two components. First, offenders participate in a program that addresses the roots of their criminal behavior. Second, they also avoid going to prison. Two mechanisms, then, 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—is exposed to only one mechanism. We may, however, test variations in the effects 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, the rehabilitation aspect of the program is not the main explanation. I first test differences in the effect between offenders who receive prison sentences of 14 days or fewer, and 15 days or more. Second, I test differences in effects between offenders sentenced to

| Specification                                           | Same partner, 2 years | Same partner, 3 years |
|---------------------------------------------------------|-----------------------|-----------------------|
| Only offenders in a relationship at the time of the conviction | .03 (.01)*           | .03 (.02)†           |
| Only offenders in unstable relationship at the time of the conviction (< 3 years) | .04 (.01)**          | .03 (.01)*           |
| Only offenders in stable relationship at the time of the conviction (> 3 years) | .01 (.01)            | .01 (.01)            |

**p < .01. *p < .05. †p < .1.
21 days or fewer, and offenders sentenced to 22 days or more (note that here I 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 of 1 when the offender has received a sentence of more than 14 days, and the second takes the value of 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 tests whether the treatment effect varies across sentence length, and a significant coefficient indicates that that is indeed the case.

Table 5 shows the results from these tests. 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, then, is a strong indication that the program works by rehabilitating the offender rather than by preventing longer periods of separation between partners due to incarceration.

Robustness check

Importantly, we need to ensure that my effects do not result 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 practices, drunk drivers become more likely to experience stable relationships at the beginning of the 1990s, 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 who were sentenced from a year before through a year after the implementation of the program, but who received prison sentences of more than 40 days; that is, those who were not eligible for the treatment program. Second, I test difference in relationship stability of drunk drivers who were sentenced from a year before through to a year after the implementation of the program, but whose sentence only included a fine or a conditional sentence. This group is also not eligible to participate in the program. Even though the nature of their offences differs between these groups and the group of drunk drivers in my study, 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 who were eligible for the treatment 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, affected the relationship stability of drunk drivers during those particular years.

Table 6 shows the results from these tests, showing 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, the coefficients are 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 suggest that the change in relationship stability observed among the drunk drivers who became eligible for the alcohol treatment program in July 1990 is a result of the change in sentencing practices.

**Conclusion**

This article extends existing research on incarceration and relationship instability in two ways. First, it estimates the causal effect of a noncustodial sentence introduced to those convicted of driving drunk 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 experienced higher relationship stability compared with drunk drivers who did not have the option to participate in the program. The ITT parameter corresponds to a treatment effect

| Specification | Same partner, 2 years | Same partner, 3 years |
|---------------|----------------------|----------------------|
| Treatment: convicted after July 15th 1990 | .04 (.01)** | .03 (.01)** |
| Prison sentences >14 days | –.01 (.01) | –.01 (.02) |
| Interaction term | –.02 (.02) | –.02 (.02) |
| Treatment: convicted after July 15th 1990 | .03 (.01)** | .04 (.01)** |
| Prison sentences >21 days | –.02 (.02) | –.02 (.01) |
| Interaction term | –.01 (.02) | –.02 (.02) |

**p < .01.

| Offender groups | Same partner, 2 years | Same partner, 3 years |
|----------------|----------------------|----------------------|
| Drunk drivers, prison sentences > 40 days | .06 (.04) | .04 (.04) |
| Drunk drivers, fines only | .01 (.02) | .02 (.02) |

NOTE: Models for drunk drivers sentenced to more than 40 days of prison are based on 301 offenders. Models for drunk drivers sentenced to fines only are based on 1,059 offenders.
of 3 percentage points. This is almost certainly a low estimate, though, because
the data do not allow us to isolate a “treatment” group of offenders who partici-
pated in the alcohol treatment program—only those who were not incarcerated.
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 percent-
age points. According to these findings, the informal consequences of incarce-
ration are substantial for (ex)inmates as well as their families.

My findings correspond well with the findings presented in Apel (this vol-
ume), who uses the National Longitudinal Study on Youth and multiple-event,
discrete time hazard models to estimate the short- and long-term effects of incar-
ceration on cohabitation and marital status. He finds that respondents who were
recently released from jail or prison were 2.45 times more likely than other
respondents to experience a change in cohabitation/marital status. Using data
from the Fragile Families and Child Wellbeing Study and fixed effects models,
Wildeman, Turney, and Yi (this volume) also demonstrate how relationship insta-
bility is equally likely across facility types (local jail, state prison, federal prison,
and unknown facility types), even though the ethnographic evidence presented
by Comfort (this volume) indicates that short-term confinement is particularly
troublesome for the offender’s intimate partner and other family members.

This article also attempts to increase our understanding of how to punish
offenders without exposing them to the negative consequences of prison. In my
study, the alcohol treatment program replaced classic principles of punishment—
incapacitation and deterrence—with rehabilitation. This test is not as clean as one
would want it to be, because the program contains two treatment components—
the absence of prison and the rehabilitation. Still, my analyses indicate the pro-
gram effect does not vary across offenders with different sentence lengths, which
suggests that it is the actual alcohol treatment (the rehabilitation component),
rather than the direct effect of incarceration, that drives my results. We may inter-
pret this result as supporting Gottfredson and Hirschi’s (1990) claim that it is
personality traits, such as lack of self-control, that explain the link between incar-
ceration and relationship instability, rather than separation or stigma.

My findings suggest that if we wish to reduce the consequences of incarcera-
tion, we cannot simply pardon offenders from prison; we must also address the
psychology behind why they offend. The type of punishment that we offer as an
alternative to prison is also important. Thus, as we witness the drive toward more
lenient sanctions against offenders in the United States, for example, we must
consider the informal consequences of alternatives to prison carefully. Although
the present analysis rests on a Danish case—with a criminal justice system that is
very different from the one found in the United States—we have little reason to
suspect that the mechanisms identified in Denmark would be substantially dif-
ferent in other countries. The individual propensity for drunk driving may very
well reflect the same psychological traits across contexts, and rehabilitation pro-
grams that improve these traits in one context are likely to also improve such
traits in other contexts.

Why should we care about the relationship stability of those convicted of
drunk driving? Is the relationship status of a select group of offenders important
enough to justify our attention? It is, and for a number of reasons. First, and most importantly, stable relationships are a resource for vulnerable individuals and their children (Western and McLanahan 2000). If a prison sentence causes divorce, it may be the beginning—or mark the acceleration—of an actual social deroute, something that is costly both socially and individually. Second, and relatedly, we as a society should be interested in securing the stability of offenders’ lives, not only to prevent them from reoffending but also to prevent them from causing harm to themselves and others. Their reoffending rates may drop if offenders maintain or initiate stable relationships after they have served their sentences.

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