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The effect of plant closure on crime

Abstract:
We estimate the effect of exposure to plant closure on crime using an individual-level panel data set containing criminal charges for all unmarried and employed Norwegian men below the age of 40. Men originally employed in plants that subsequently closed are 14 percent more likely to be charged of a crime than comparable men in stable plants. There is no difference in charge rates prior to closure, supporting a causal interpretation of our result. Within crime categories, we find no effect of plant closure on property crime, perhaps because closure has a small and insignificant effect on subsequent earnings. We estimate an effect of plant closure on categories of non-acquisitive crime, suggesting a role for mental distress or idleness. A role for idleness is supported by evidence that the effects of plant closure on crime tend to be more pronounced for crimes committed during the week than on weekends.

Keywords: crime, plant closure, plant downsizing, displacement

JEL classification: J12, J63, J65

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1. Introduction

The costs to society of crime are substantial. Estimates suggest that the costs of crime may exceed 10 percent of GDP in developed countries (Entorf and Spengler 2002, p. 91). Besides material damage and other tangible costs, victims and potential victims of crime also incur psychological costs. As a consequence, crime prevention initiatives have great priority in many countries. In addition to traditional enforcement, these initiatives often consist of educational and job assistance programs, motivated by the notion that crime is closely linked with employment opportunities. Public and political interest in possible links between labor market conditions and crime has risen in the face of the ongoing worldwide recession (Hauser and Baker 2008).

There is a rich economic literature exploring the link between labor market conditions and crime.[1] Much of the empirical work in the literature draws on US data sources to estimate the relationship between area (usually state) unemployment rates and crime, with the general finding that unemployment has a modest but statistically significant positive effect on property crime rates, with little or no effect on violent crime rates.[2] These findings are consistent with traditional economic rational choice theories of crime, which predict that a reduction in licit earnings opportunities increases the allocation of time towards crime for profit (Ehrlich 1973, Becker 1968). However, there is limited empirical evidence investigating the mechanisms through which labor market conditions may affect criminal behavior.

We contribute to the existing literature by bringing longitudinal individual-level data to bear on the relationship between labor market experiences and crime behavior, specifically, by investigating the impact of plant closures on the subsequent crime behavior of affected workers. Workers suffering involuntary job loss represent an important subset of individuals through which weakening labor markets might affect crime rates. Focusing on plant closure allows us to investigate the impact of involuntary job loss while circumventing the most obvious forms of omitted variable bias. In addition, employing micro-level data allows us, to some degree, to explore different mechanisms that have been theorized to explain the link between job loss and criminal behavior.

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[1] Freeman (1995) surveys earlier research in this area. More recent contributions to this literature include Lin (2008), Oster and Angell (2007), Edmark (2005), Gould, Weinberg and Mustard (2002), Raphael and Winter-Ebmer (2001), Machin and Meghir (2004), and Grogger (1998).

[2] Employing instrumental variable methods to address measurement error problems and the endogeneity of state unemployment rates, Raphael and Winter-Ebmer (2001) and Lin (2008) find that a one percentage point increase in unemployment raises property crime rates 4-6 percent. Studies that fail to address the endogeneity of state unemployment rates have generally produced estimates a third as large. Gould, Weinberg and Mustard (2002) employ IV methods similar to Raphael and Winter-Ebmer (2001) and Lin (2008). They estimate smaller effects of state unemployment rates (calculated over non-college educated men) in models that simultaneously estimate the effect of local wages.

Our analysis draws on two Norwegian data sources maintained by Statistics Norway. The first comprises detailed register data on the criminal charges brought against any resident from 1992 through 2004. A second longitudinal register database known as the *FD-trygd* provides a rich array of socioeconomic and demographic variables for the entire resident population during that same period. Employment spell records in the *FD-trygd* allow us to calculate employment counts by plant and year, used to identify workers employed in a plant in 1995 that subsequently closed or was stable.

Our main analytic sample consists of over 44,000 unmarried men, 18-40 years old, who were employed with at least one year tenure in 1995, and who worked in plants that either remained stable or closed over the following five years.[3] Our effect estimate is based on comparisons of covariate-adjusted criminal charge rates across young adult male workers originally employed in closing and stable plants. The crucial identifying assumption is that unobserved determinants of crime are uncorrelated with the likelihood that an individual worker's plant closes. Importantly, if this assumption does not hold, we would expect workers in closing plants to have higher (or lower) rates of criminal behavior prior to 1995, which we can directly test.

Our empirical results show that young unmarried men's exposure to plant closure has a substantial effect on criminal behavior. The likelihood of any charge being brought against men originally employed in plants that subsequently closed is about 14 percent higher than for men in stable plants. Notably, charge rates *prior to* 1995 are modestly lower for men originally employed in stable plants compared to men in closing plants, with virtually no difference in covariate-adjusted rates. This supports the causal interpretation of our results.

Within crime categories, we find no evidence that involuntary job loss increases property crimes. Instead, we find significant evidence for an increased likelihood of being charged with serious traffic violations (19 percent increase), which is the most common crime in our sample. Our point estimates also suggest a substantial increase in the likelihood that a worker is charged with violent crime (15 percent) or crimes related to alcohol and drugs (16 percent), though these estimates are imprecise and not statistically significant. This evidence is consistent with a recent paper by Eliason and Storie (2009) showing that job loss associated with plant closure significantly increases the risk of hospitalization due to traffic accidents and alcohol-related conditions.

There exist at least two different mechanisms that are consistent with the finding that displaced workers increase non-acquisitive criminal behavior. Firstly, the finding is consistent with criminological theories that highlight the importance of frustration and mental distress in determining criminal behavior (Agnew 1992). Substantial empirical evidence indicates that exposure to plant

[3] While we also comment on results for married men, we focus on unmarried young men between 18-40 years since women generally commit very few crimes, and since the number of offenders drops substantially with age into mature adulthood (Hirschi and Gottfredson 1983). Marriage is also negatively associated with crimes (Laub et al. 1998).

closure and job loss imposes mental distress on affected workers (e.g. Vahtera and Kivimaki 1997, Dragano, Verde and Siegrist 2005). Secondly, job loss could increase rates of non-acquisitive crime through increased idleness (Felson 1998), which affords individuals greater opportunities to engage in criminal behavior.

We explore the plausibility of the idleness mechanism by utilizing data on the day-of-week that crimes were committed. If job loss increases criminal activity by increasing idleness, we expect the crime effect to be more pronounced on weekdays, with little differences on weekends. Consistent with the idleness mechanism, we find a significant effect of plant closure on crimes committed during the week, but no significant effect on crimes committed on weekends. This result is largely driven by effects on traffic violations and crimes related to alcohol and drugs. The result for crimes related to alcohol and drugs is in line with a literature that links job displacement and involvement in crime to consumption of alcohol and drugs (Dawkins 1997, Schroeder et al. 2007, Crawford et al. 2006, Eliason and Storie 2009).

Our finding that involuntary job loss has no significant effect on property crimes contrasts with the macro-level analyses linking higher unemployment to higher rates of property crime. One possible explanation is that the effect of unemployment on property crime rates operates through individuals with marginal attachment to the labor market, rather than through individuals (like those in our sample) who lose a job to which they have been closely attached. An alternative explanation is that the combination of the generosity of the Norwegian social safety net and low unemployment rates limits the extent to which displaced workers are motivated to replace licit with illicit earnings. Consistent with this, we estimate only a modest and insignificant negative effect of plant closure on a measure of workers' subsequent earnings that includes unemployment and sick leave benefits.[4]

The remainder of the paper is structured as follows. Section 2 discusses relevant contextual details about Norway which might be important in interpreting our findings. Section 3 discusses theoretical mechanisms through which plant closure could affect criminal behavior. Section 4 presents the empirical strategy. Section 5 discusses data and measurement issues that arise in studying crime, and Section 6 describes the dataset. Section 7 presents our results. Section 8 concludes by discussing our results in the context of existing findings in the literature and mechanisms consistent with our results.

[4] It is noteworthy that Oster and Agell (2007) and Edmark (2005) find that local area unemployment rates in Sweden, another Scandinavian country with extensive safety net programs, have qualitatively similar effects on crime rates as observed in US data. However, since the early 1990s the unemployment rates in Sweden have been much higher than in Norway.

2. Unemployment and Crime in a Scandinavian Welfare State

Over the last decades, Norway has been characterized by low unemployment rates, even by Scandinavian standards. In 2007, the survey-based unemployment rate was 2.5 percent, compared to 4.6 percent for the US and 7.1 percent for the European Union (OECD 2009). With strong demand for workers, the effects of job loss may not be as dramatic in Norway as in other developed countries. Moreover, the public welfare programs in Norway are generous by international standards. Virtually all Norwegian workers are covered by the state's unemployment benefit program. The size of the unemployment benefits is typically around two-thirds of the earnings in the previous calendar year, and until 2003 a typical receiver was eligible for unemployment benefits for up to three years (now up to 2 years). Persons not finding a new job when the unemployment benefits run out can get benefits of the same magnitude by participating in medical or vocational training programs or by qualifying for disability pension. Public benefit programs related to sick leave, unemployment, disability and vocational training constitute a widely-used income security net for Norwegian workers, and in recent years almost one fourth of the non-elderly adults in Norway have been receiving benefits from one of these programs at every point in time (St.meld. 2007).

As in the other Scandinavian countries, Norway has among the lowest crime rates in the western world. Norway has a murder rate of 0.71 per 100,000 inhabitants, compared with rates of 5.62 in the US and 1.41 in England and Wales (UN 2008). Similar differences are found in the International Crime Victim Surveys, where Norway is among the 15 countries with lowest victimization rates (van Dijk et al 2008). The differences in incarceration rates are also remarkable. While the US incarceration rates are about 751 per 100,000 inhabitants (BJS 2009), the UK rate is about 140 (European Sourcebook 2006), and Norway's rate is about 91 (Statistics Norway 2008). Attempts to explain these differences often refer to Norway's social safety net, as well as to less punitive policies than in the US and the UK (Christie 2000).

3. Mechanisms

Work by Gary Becker is the typical point of departure for economic studies on the causes of crime. Becker (1968) models criminal behavior under standard assumptions of rational choice theory, whereby individuals commit crime when the expected utility from doing so exceeds the expected utility of legal behavior. While Becker (1968) was primarily interested in optimal criminal enforcement, a number of economic studies have extended his model of criminal behavior (see, e.g., overview in Levitt and Miles 2007). Of particular importance are the extensions of Ehrlich (1973), who introduces a time constraint whereby individuals divide their time between licit and illicit activities.

Insights from the models of Becker (1968) and Ehrlich (1973) suggest two complementary mechanisms through which involuntary job loss might increase criminal behavior. It has been documented that exposure to plant closure and job loss reduces future earnings and employment (Jacobson et al. 1993, Stevens 1997, Huttunen et al. 2006, Rege et al. 2009). As a result, job loss could be expected to increase the marginal benefit associated with illicit earnings, while decreasing the relative time cost of engaging in illicit activities. The models of Becker (1968) and Ehrlich (1973) therefore predict that job loss should shift the allocation of time towards crime for profit. These rational choice-based models provide somewhat weaker predictions with respect to non-acquisitive crime. Job loss might reduce the time cost of engaging in non-acquisitive crime, but such crimes fail to compensate for the reduction in licit earnings.

Somewhat in contrast to rational choice-based models, criminologists and sociologists have primarily focused on self-control and opportunity as the foremost determinants of crime. In their widely cited "general theory of crime", Gottfredson and Hirschi (1990) argue that the individual-level association between unemployment and crime can be explained by low self-control. This personality trait is formed in early childhood and is associated both with criminal behavior and with difficulties succeeding in work and school. The main methodological concern in estimating effects of job loss on crime is to rule out such spurious associations (see Section 4).[5] In a meta-analysis of the relevant criminology literature, Pratt and Cullen (2000) find consistent associations between individuals' criminal behavior and measured levels of self-control.

The emphasis on self-control suggests a mechanism through which job loss might increase criminal behavior. Convincing evidence suggests that job loss imposes mental distress on affected workers (e.g. Vahtera and Kivimaki 1997, Dragano et al. 2005, McKee-Ryan et al. 2005). Agnew (1992) argues that distress and feelings of unfair treatment can trigger frustration and anger, which lead to reduced constraints to breaking laws and social norms, as well as aggressive reactions in adverse situations. To the extent that job loss, subsequent unemployment and declining income increases mental distress and frustration, Agnew's theory predicts an increase in non-acquisitive offenses, particularly violence.

Idleness represents another mechanism through which job loss might increase criminal behavior, including non-acquisitive crimes. Without ignoring variation in individual propensities for crime, Felson (1998) argues that even individuals motivated to commit crime cannot do so unless an opportunity is present. Less structured daily routines, like no longer going to work, and increased idleness time provide greater opportunities for criminal activity. Increased idleness may also increase

[5] Crime might also have a causal effect on employment opportunities as the stigma from a criminal record restricts future access to meaningful jobs (Pager 2003, Grogger 1995, Mocan and Rees 2005). In other words, crime might be both a cause and an effect of poor labor market opportunities.

one's exposure to criminogenic settings, where alcohol and drugs may be present and where the social norms against deviant behavior are weaker (Hirschi 1969). Importantly, under this mechanism we might expect the effect of job loss on crime to be particularly pronounced during the work week, when crime opportunities among the employed are more limited.

Notably, the idleness mechanism is closely related to Ehrlich's (1973) rational choice model, since both conceive "time availability" as a critical determinant of criminal activity (which it obviously is). However, the two theories differ in their implications for the effects of job loss on different types of crimes. Rational choice-based models predict a larger increase in crimes for profit resulting from job loss, due to the increased marginal utility associated with illicit earnings. In contrast, the idleness story provides no special prediction with respect to crimes for profit, but instead emphasizes the increased opportunities to get into trouble that idleness affords, which depends on the social context idled workers inhabit. For our sample of young men, we would especially expect this to include alcohol and drug related offenses, but could extend to other types of crimes as well.

4. Empirical Strategy

We estimate the effect of exposure to plant closure on crime by comparing involvement in crime across men previously employed in closing and stable plants. The crucial identifying assumption is that plant closure events are uncorrelated with unobserved individual determinants of crime. Our dataset allows us to measure plant downsizing by looking at changes in employment levels by plant and year. We will refer to the *plant downsizing rate* (PDR) as the percentage change in employment between 1995 and 2000. More precisely, the plant downsizing rate in worker *i*'s plant is given by

$$(1) \qquad PDR^i = \frac{x_{95}^i - x_{00}^i}{x_{95}^i},$$

where x_{95}^i and x_{00}^i are point-in-time plant employment counts in 1995 and 2000, denoting number of workers (full-time equivalents) in worker *i*'s plant at the end of the year, excluding worker *i* himself. In the following, we will refer to a plant reducing employment by more than 90 percent (i.e. $PDR > .90$) as a *closing plant*, and a plant with no reduction in employment (i.e. $PDR \leq 0$) as a *stable plant*. Our main analytic sample will comprise unmarried young men whose plant of employment in 1995 either closed by 2000 (i.e. $PDR > .90$) or remained stable during this period (i.e. $PDR \leq 0$).

We estimate the following logit model for the probability that a young man employed in 1995 commits at least one crime over 2000-2004:

$$(2) \qquad \Pr\left(C_{i,04} = 1\right) = \Lambda\left(\alpha_0 + \eta W_i + \alpha_X X_i\right)$$

where

$C_{i,04}$ ~ indicator that man i commits at least one crime over 2000-2004.

W_i ~ indicator that the plant in which the man is employed in 1995 is closing (i.e. $PDR > .90$).

X_i ~ vector of 1995 characteristics of man i, including past criminal behavior and other socio-economic variables at the individual level and at the plant level.

The parameter of interest in equation (2) is η, which captures the incremental increase in a man's likelihood of committing crime due to plant closure (of his 1995 plant of employment), relative to men whose plant of employment in 1995 is stable. The crucial assumption for a consistent estimate of η is that plant closure events are determined by exogenous economic shocks and are independent of unobserved determinants of individual crime.

Estimates of η are potentially biased if men in plants that subsequently close have higher (or lower) unobserved propensities for crime. For instance, men who expect to commit crimes potentially may have lower demand for stable employment, possibly causing an over-representation of such men in plants that subsequently close. Alternatively, unobserved "third factors" (e.g. low self-control or high discount rates) could lead men with higher propensities for crime to self-select into less stable plants. If so, we would expect men in plants that subsequently close to have higher unobserved propensities for crime, biasing the estimate of η upwards. Along these same lines, it is possible that men with higher unobserved propensities for crime tend to gain employment in industries more prone to plant closures, again resulting in upwards bias.

Biases could also arise from the geographic location of closing plants and their respective workers. If plant closures are concentrated in disadvantaged neighborhoods, with poor labor market conditions and high crimes rates, this could give rise to two potential sources bias. First, if social interaction effects exist in criminal behavior (see e.g. Glaeser et al. 1996, Sampson and Raudenbush 1999, Ludwig and Kling 2007, Bayer et al. 2009), men in closing plants might disproportionately live in areas where social norms against criminal behavior are weaker. Second, areas disproportionately affected by plant closure might be those where the opportunities for criminal activity are greater.

Our empirical analysis addresses these potential sources of bias in a number of ways. Potential biases arising from the self-selection of men into certain types of plants can be addressed, at least partly, by controlling for a wide range of plant characteristics. Potential biases arising from area-specific confounders are addressed by estimating models with thousands of neighborhood fixed effects. Most importantly, however, if the propensity for crime is greater among men employed in plants that subsequently close, this should reveal itself in higher rates of crime by these men *prior* to the plant downsizing event. Our ability to control for an individual's involvement in crime prior to the

plant downsizing event addresses this source of bias. Moreover, we are able to test directly whether workers in closing plants had higher crime rates prior to plant closure. We also apply propensity score methods to assure that our estimates of η do not suffer from heterogeneity bias.

It should be noted that, absent the sources of omitted variable bias identified above, our results potentially under-estimate the impact of plant closure on crime since our plant closure measure is based on a worker's original plant of employment. Job mobility across closing and stable plants would therefore tend to attenuate our estimates.[6]

5. The Measurement of Crime

A problem for any empirical study of crime is the difficulty in measuring criminal activity. Typically, measures are constructed from either survey self-reports or registered crimes. Self-reports of criminal activity should be interpreted cautiously since they are often impossible to validate and since there are incentives to misreport (MacDonald 2002, Kirk 2006). In particular, the extent of truthful self-reporting is lower among subjects with an extensive criminal record compared to subjects with little or no criminal history (Hinderlang et al. 1981). A key advantage of register data is that "registered crimes" can cleanly be identified. Moreover, register data has the advantage that offenders cannot choose *not* to be registered, while they may decline to participate in a voluntary survey.

A disadvantage of register data is that the probability of capture (and thus appearance in the register) depends on the number of offenses committed, even though active offenders may be less likely to be caught for any given crime than inexperienced one-timers (Farrington et al. 2003). This may cause us to overestimate the effect of job loss on crime if, for example, unemployment tends to raise criminal activity more among subjects with a criminal record than among those without *and* there are more subjects with a criminal record in closing plants. This type of bias may also arise from enforcement behavior; for example, if the police tend to focus attention on subjects with particular personal characteristics which are correlated with unemployment (Waddington et al. 2004). Our ability to carefully control for previous engagement in crime should address such concerns.

A second problem with register data is that crimes which are not reported to (or not recorded by) the police are not captured and that crimes which are not "solved" cannot be matched to a specific individual. The extent of these problems could differ across crime types. For instance, crimes

[6] In this regard, we could improve our estimation strategy in the framework of an "event study," using plant-specific closure dates to inform our analysis. However, doing so would require us to identify with reasonable precision a "date of closure" for every closing plant that reflects the date at which individual workers became aware of their impending job loss. We have no way of definitively knowing when workers became aware of an impending job loss related to their plant's closure. In addition, identifying a single "closure date" is problematic because closing plants often close after a series of employment reductions.

at the work-place are often settled without involving the police (Nelken 2002, Ellingsen and Sky 2005), while this is less so for burglary. In this sense it is an important advantage of our dataset that we can also look at sub-categories of crime, where serious differences in registration across men in stable and closing plants may be considered less likely. Still, we should keep such limitations in mind when interpreting the results.

6. Dataset Description

We combine two register databases provided by Statistics Norway that can be merged using a unique personal identifier provided every Norwegian resident at birth or immigration. The first database contains complete records of criminal charges for every Norwegian resident over the period 1992-2005. We utilize offenses committed through 2004 due to the lag between the time offenses are committed and the charges. The database contains all serious crimes, but also misdemeanors like drunk driving, excessive speeding and shop lifting. A person is registered as "charged" if the police perform an investigation and conclude that the person did commit the recorded crime. The investigation may be initiated by the police receiving a report or by an arrest. The registration is independent of the further outcome of the case (filing of formal charges, prosecutions or convictions). Date of crime and detailed codes of "offense type" are also included on charge records. Statistics Norway has constructed sub-categories of crime and we rely on these definitions to construct crime categories that correspond to those used by the US FBI (see Appendix A).

The second database is called *FD-trygd*. It is a rich longitudinal database with records for every Norwegian resident from 1992 to 2005, containing individual demographic information (marital status, sex, age, time of marriage, number of children), socio-economic data (years of education, income, wealth), current employment status (full time, part time, minor part time, self-employed), industry of employment, indicators of participation in any of Norway's welfare programs, and geographic identifiers for about 14,000 different neighborhoods of residence.

In particular, *FD-trygd* contains records for timing of employment "events" since 1995. These events, captured by individual and date, include entry and exits into employment, changes in employment status (full time, part time, minor part time), and changes in plant and firm of employment. These employment records are constructed by data analysts at Statistics Norway from raw employment spell records submitted by employers, and verified against employee wage records (not available to us) to ensure the validity of each spell and to eliminate records pertaining to "secondary" employment spells.[7]

[7] If an individual was employed in multiple plants at a given time, primary employment was determined by employment status and recorded income (not available to us) from each source of employment.

Based on the employment records, we constructed plant-level employment counts at the end of year 1995 and 2000. The counts were constructed as measures of full-time equivalents (FTEs), with part time and minor part time employment measured as 0.67 and 0.33 FTEs, respectively. Excluded from these counts were any person identified in FD-trygd as self-employed or receiving assistance that should have precluded full time work (those receiving unemployment benefits, rehabilitation pensions or disability pensions). Plant-level FTEs were then used to construct the measure of plant downsizing from 1995 to 2000 as defined in Equation (1).

Our main analytic sample consists of unmarried men between 18 and 40 years of age in 1995. The sample selection criteria were driven by two considerations. First, sufficiently high rates of crime are required to estimate effects with any precision. We therefore focus on the demographic group responsible for most crime: young, unmarried men (Freeman 1996). Second, to study effects of job loss, these men need to have had some connection with the labor market. We therefore restrict the main analytic sample to men that were full-time employed at end of 1995, excluding a few cases where the man receive assistance that should have precluded full time work, like disability or unemployment benefits. We also require the man to have at least one year of tenure in the plant in 1995 to ensure attachment to one's current plant of employment.

To facilitate interpretations of our results, we restrict the analysis to men working either in a stable or a closing plant. As a precaution against the plant closure variable being correlated with unobserved individual determinants of crime, we exclude men working in a plant with less than 10 FTEs in 1995. The resulting dataset consists of 44,391 unmarried men, living in 9,770 different geographically defined neighborhoods.

Variables capturing individual and plant socio-economic characteristics were constructed based on *FD-trygd* records for 1995. A large number of such variables are included in all models (see Appendix B).

Summary statistics for some of these variables are presented in Table 1 (with standard deviation in parenthesis) for our main analytic sample. About 8.1 percent of our sample committed a crime from 2000 to 2004, and about 32 percent of our sample worked in a plant in 1995 that downsized by more than 90 percent from 1995 to 2000.

7. Empirical Results

7.1. Descriptive Evidence

We start by observing from Table 1 that the charge rate over 2000-2004 (*Charged 2000-2004*)[8] for the sub-sample of men employed in 1995 in plants that closed (over 1995-2000) is slightly higher than the charge rate of men employed in 1995 in plants that remained stable. The charge rate over 2000-2004 is 8.4 percent for men in closing plants compared to 8.0 percent for men in stable plants. This is what we would expect if exposure to plant closure results in more crime.

A major concern with our empirical strategy is that there are more crime-prone men in closing plants even before plants start downsizing. If so, we could not attribute the higher charge rate in closing plants over 2000-2004 to plant closure exposure. If such selection existed, we would expect higher charge rates among men in closing plants *prior to* the closure. In fact, the opposite is the case. Over 1992-1995, men in closing plants were somewhat less likely to have been charged.

7.2. Main Results

Table 2 presents the main results. The estimated plant closure coefficient (cf. η in Eq. 2) captures the incremental increase in men's likelihood of being charged with any crime due to plant closure, relative to men in stable plants. In Models 1 and 2, we report the results from logit (odds-ratios reported) and OLS regressions controlling for covariates described above and detailed in Appendix B. The estimated marginal effects are significant and very similar in magnitude (0.009) across the two models. The estimates suggest that exposure to plant closure increases the likelihood of being charged of a crime by 14 percent.

As discussed in Section 4, one concern for our empirical strategy is that plant closure events might be concentrated in disadvantaged geographic areas with poor labor market conditions and high crime rates. Models 3 and 4 therefore estimate analogous models additionally controlling for 9,770 geographically defined neighborhood fixed effects. This has no effect on the odds-ratio estimate and a modest effect on the OLS estimate, increasing the magnitude somewhat.

Even if plant closure is uncorrelated with unobserved determinants of crime, OLS and logit estimates of the effect potentially misrepresent the average effect of men's exposure to plant closure on crime if effects are heterogeneous along characteristics correlated with men's exposure to closure. To address this concern, Model 5 provides the estimate of the "average treatment effect" using propensity matching methods. Again, the estimated effect is very similar to the others (0.010).

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[8] As noted in Section 6, our measure of crime is dated according to the time of offense. Hence, the more elaborate "charged for a crime committed 2000-2004" would be more precise.

This indicates that possible heterogeneous effects do not cause the logit and OLS result to deviate seriously from the average treatment effect.

Having a family may moderate a young man's likelihood of spending time in criminogenic settings after exposure to plant closure, for example since family commitments involve structured routine activities (Felson 1998). Also, though job loss can dissolve the social bounds that prevent crime, work related bounds may be relatively less important if the man has a family (Hirschi 1969, Laub et al. 1998). Following these arguments we would expect to see that married men are less likely to respond to exposure to plant closure by engaging in crime. Indeed, when we replicate the models over a similarly constructed sample of young *married* men, closure is associated with an insignificant reduction in the likelihood of being charge with any crime.[9]

7.3. Specification Tests

Our estimated effect of plant closure on crime would be upward biased if, at the outset in 1995, workers in closing plants have a higher propensity to engage in crime for reasons not controlled for. If true, however, we would expect workers in closing plants to have a higher propensity for crime *even prior to the closing event*. Before we formally test this assumption, recall that Table 1 suggests this is not a concern: men in subsequently closing plants had modestly lower rates of criminal activity prior to 1995 than men in plants that remained stable.

If our covariates were not capturing important determinants of crime that are correlated with our measure of plant closure, we would expect our estimated effect of exposure to plant closure on crime to change when we drop the controls for crime prior to plant closure. In Model 2 of Table 3 we report the result from the regression where we dropped the set of control variables indicating whether (and for what type of crime) the individual was charged over 1992-1995. By comparing with the logit estimate (Model 1) from Table 2 (replicated as Model 1 of Table 3) we see that excluding the pre-closure crime indicators barely moves the estimate.

In Model 3 of Table 3 we report the result from a regression of crime over 1992-1995 (*Charged 1992-1995*) on subsequent plant closure (1995-2000).[10] The results provide no evidence that exposure to plant closure is correlated with pre-existing differences in the propensity for crime. Instead, we find that the lower unadjusted crime rate observed for men in closing plants (Table 1) is entirely explained by differences in observed characteristics.

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[9] Estimating Model 1 over the sample of married young men yields an odds-ratio (s.e.) of 0.91 (.06).

[10] Here we have excluded the variables capturing pre-1995 engagement in crime from the set of included covariates.

7.4. Robustness

Table 4 presents several robustness checks. First, Models 2-5 investigate the robustness of our main result (Model 1 of Table 2) to variations in the definition of a closing and stable plant. In Models 2 and 3, we can see that letting closing plants comprise plants downsizing more than 95 and more than 80 percent, respectively, has a fairly modest impact on the estimates. In Models 4 and 5 we let stable plants be defined by plants downsizing 5 and 10 percent or less (and not by 0 or less as in our main analytic sample), which again has a modest impact on the estimated effect of plant closure on crime. In Model 6 the sample is expanded to include workers in plants with PDR between 0 and 0.9, including two additional dummies capturing downsizing events from 0 to 45 percent and from 45 to 90 percent. We see that major intermediate downsizing events also have some (though insignificant) influence on the likelihood of crime.

In Models 7 and 8 we investigate whether the estimated effect varies when the sample is restricted to workers in plants with at least 20 or 50 FTE in 1995 (and not 10 as in our main analytic sample). The estimate is unaffected by the former restriction, but increases somewhat when employees of small plants (<50 FTE) are excluded.

We have been using a dichotomous dependent variable to capture crime. This means that we are only utilizing variation in the data coming from workers not committing *or* committing crime (extensive margin). It is also possible that exposure to plant closure causes workers to intensify their engagement in crime (intensive margin). In Model 9 we have set the dependent variable to one if the worker committed two or more crimes over 2000-2004 (zero otherwise). We see that plant closure also has an effect on this measure of crime; indicating that there is not only an effect on the extensive margin.[11]

7.5. Category-of-Crime Effects

Next, we estimate the effect of plant closure on the likelihood individuals are charged with specific categories of crime. We report results for the four aggregate categories *violent* crime, *property* crime, crimes related to *alcohol and drugs,* and serious *traffic* violations (see Appendix A for details).

Table 5 reports the estimated effects of plant closure under our main specification (Table 2, Model 1). Workers exposed to plant closure have a significantly higher probability of being charged with a traffic violation (see Model 2). The estimated effect on violent crime and crimes related to

[11] This suggests that we can improve precision by also utilizing the variation in *intensity* of crime when estimating the effect of plant closure on crime. This potential improvement is, however, limited since the proportion of workers committing more than one or two crimes is small. Possibly related to the low frequency of workers with more than two crimes, estimates often failed to converge in Poisson or negative binomial specifications. In all specifications where convergence was achieved, Poisson and negative binomial estimates were consistent with the logit estimates.

alcohol and drugs, while insignificant, are roughly the same magnitude (Models 3 and 4). In contrast, we find no evidence that plant closure increases the likelihood of being charged with a property crime (Model 5).

The finding that plant closure increases criminal behavior but *not* property crime appears to contrast with the macro-level analyses linking higher unemployment to higher rates of property crime. As we mentioned in the Introduction, one possible explanation is that the effect of unemployment on property crime rates operates through individuals with marginal attachment to the labor market, rather than through individuals who lose a job to which they have been closely attached. An alternative explanation is that the combination of the generosity of the Norwegian welfare programs and low unemployment limits the extent that displaced workers are motivated to replace licit with illicit earnings. In the following we investigate these explanations, using the data to explore the plausibility of mechanisms through which job loss may increase non-acquisitive criminal behavior.

7.6. Plant Closure Effects over Time

In Table 6, we explore potential mechanisms for the estimated crime effects focusing specifically on the potential roles of earnings and idleness. To the extent that closure reduces earnings, the rational crime theory would predict an increase in the marginal benefit of illicit earnings. To the extent that closure reduces future employment, criminal behavior might increase as a result of increased idleness. Reduced earnings and/or unemployment could also be a source for mental distress, which might inhibit self-control or increase frustration, leading to more crime.

We explore the potential importance of these mechanisms by estimating the effect of closure on outcomes over three two-year time periods: 1999-2000, 2001-02 and 2003-04. Our motivation in doing so is to investigate whether the timing of the crime effects conforms with the timing of the earnings and employment effects.

We find little evidence to support the notion that reduced earnings explain the increase in criminal behavior among workers exposed to plant closure. Plant closure is associated with a reduction in subsequent earnings, which (in our data) includes payments received from work-related insurance schemes like unemployment and sick leave benefits. The estimated effects are, however, modest and insignificant across all time periods (see Panel A).[12] This presumably reflects, at least partly, the generosity of unemployment benefits and other social programs than mitigate the earnings effect of job loss. This might also explain why we find no effect of plant closure on property crimes.

[12] In US dollars, the estimates indicate a mean reduction in annual earnings of $150 to $250. Of course, the mean effect could mask large reductions in earnings suffered by some individual workers exposed to closure.

In Panel B, we report estimates showing that exposure to closure reduces the subsequent time spent in full time employment by roughly a week per year. The estimated effect is largest in the period most contemporaneous with the measured closures (1999-2000), but remains substantial and significant across all periods. This provides some support for the notion that idleness or mental distress due to unemployment plays a role in explaining the effect of closure on crime.

The time dimension of crime effects (Panels C-G) are somewhat difficult to evaluate given the imprecision of the estimates. Panel C indicates that the estimated effect of closure on the probability of being charged with any crime is largest in the final years of our analysis (2003-2004). The estimated effects of closure on violent crime (Panel D) and traffic violations (Panel G) also appear driven by higher crime rates of exposed workers in the latter years of our analysis. These results potentially pose a challenge to the idleness and mental distress mechanisms. If, for example, plant closure-induced reductions in full-time employment afford exposed workers more opportunities to commit crime, we might have expected crime effects to be largest when measured over 1999-2000, when the negative employment effect was largest. On the other hand, if prolonged unemployment is necessary in order to trigger criminal behavior associated with idleness and/or mental distress, this potentially explains the later timing of crime effects. Though imprecisely estimated, the timing of effects on crimes related to alcohol and drugs (Panel F) closely conforms with the timing of employment effects. This suggests that the time afforded from reduced employment plays an immediate role in alcohol and drug-related crimes.

7.7. Crime Effects by Day-of-Week

To further investigate the plausibility of the idleness mechanism, we also estimate the effect of plant closure on crimes committed on different days of the week. Our motivation for this analysis is straightforward. If job loss increases criminal activity by increasing idleness, we expect the crime effect to be more pronounced on weekdays, with little differences on weekends.

In Figure 1 we report odds-ratios (with 95 percent confidence intervals) from logit regressions of exposure to plant closure on *Charged 2000-2004* on the given day of the week, applying our main specification (Model 1, Table 2). The figure also includes results where weekdays are grouped together and where Saturdays and Sundays are grouped together. The estimated positive effect of closure on crime holds only for crimes committed on work days, consistent with a role for idleness.

We replicate this analysis by individual categories of crime in Figure 2. These results also offer some support for the idleness mechanism, though they should be interpreted cautiously as the confidence intervals are wide. For crimes related to alcohol and drugs and for traffic violations, the

positive closure effect is driven by an increase in crimes committed during the work week. The result regarding crimes related to alcohol and drugs suggests a relationship between idleness and consumption of alcohol and drugs. One might also conjecture that this is indication of a mental health effect (e.g. Vahtera and Kivimaki 1997, Dragano et al. 2005), of which consumption of alcohol and drugs could be a symptom. However, if our result was solely related to mental distress, we might expect an effect on this type of crime on weekends as well, of which there is little indication. For property crimes, we find weak support that closure induces a shift from weekend to weekday crimes.

In contrast, for violent crimes there is no support for an idleness mechanism, as closure significantly increases the likelihood of committing violent crime on Saturdays but otherwise has little effect. While a potential challenge to the idleness mechanism, this finding is complicated by the nature of violent crime. Violent crime requires victims, who are perhaps most available on Saturdays.[13] Assuming that plant closure does not affect the opportunity to commit violent crimes, the significant increase in violent crimes committed on Saturdays could reflect the impact of job loss on mental distress.

8. Conclusion

In this paper we estimate the impact of plant closure on crime using a panel data set comprising more than 44,000 unmarried Norwegian men below the age of 40. Our results suggest that plant closure in the man's plant of employment significantly increases the likelihood of being charged of a crime. The men originally employed in plants that closed from 1995 to 2000 were about 14 percent more likely to be charged with at least one crime over 2000-2004 than comparable men in stable plants. We find no evidence of differences in charge rates prior to plant closure and estimates are robust to neighborhood fixed effects, lending support to a causal interpretation of the result.

Contrary to the predictions of traditional rational crime theory, as well as the existing literature analyzing the effect of area unemployment on crime rates, we find no evidence that exposure to plant closure increases the likelihood of property crime. Instead, closure is associated with significantly higher charge rates for traffic violations, with insignificant effects of similar magnitude estimated for violent crime and crimes related to alcohol and drugs.

Our analysis of plausible mechanisms potentially helps reconcile our findings for property crime with those of the macro-based literature. We find that plant closure has only a small and insignificant effect on a measure of subsequent earnings that includes employment-related insurance benefits. We attribute this result to the generosity of employment-related insurance benefits

[13] Card and Dahl (2009) employ the idea of "victim availability" in testing rational choice models of violent behavior.

and the low unemployment rates in Norway, which reduce the income loss associated with job displacement.

Our findings suggest that idleness is likely an important mechanism through which plant closure affects crime. Plant closure has a substantial negative effect on the subsequent full-time employment that persists over time. Consistent with an idleness story, we find in our day-of-week analysis that the effect of plant closure on crime is driven almost entirely by an increase in weekday crimes. Plant closure appears to have little or no effect on crimes committed on the weekend. While this does not rule out a potential role for mental distress in explaining the crime effects, it does indicate that mental distress cannot be the sole mechanism at work. If it were, there is no reason why the crime effects would be limited to weekdays. On the other hand, some role for mental distress is suggested by the significant increase in violent crimes committed on Saturdays.

Support for the idleness mechanism is particularly strong for alcohol and drug-related offenses, where the positive crime effects over time conform closely with the negative employment effects. This also suggests a relationship between idleness and consumption of alcohol and drugs, as indicated by recent studies (e.g. Crawford et al. 2006, Deb et al. 2009, Eliason and Storie 2009).

In contrast, for our aggregate measure of crime ("committed any crime"), as well as in the categories of violent crime and traffic violations, the crime effects appear largest in the later years of our study when the employment effects are smaller. This potentially represents a challenge to the idleness mechanism. However, it seems plausible that prolonged unemployment is necessary to trigger (some) criminal behaviors associated with idleness or, for that matter, mental distress. This could explain why larger crimes effects are observed later in time.

Additional research can hopefully further our understanding of the relationships between job displacement, abuse of alcohol and drugs, mental health, and crime.

Table 1: Summary Statistics. Main Analytic Sample

Variable	All	Men in closing plants	Men in stable plants
Plant closure	0.3167		
Charged 2000-2004	0.0813	0.0843	0.0800
Age	29.88 (4.914)	30.39** (4.919)	29.640 (4.894)
Income	250 530 (167 232)	251 668 (115 013)	250 002 (186 543)
Number of children	0.412 (0.721)	0.430** (0.739)	0.403 (0.712)
Years of education	12.81 (2.64)	12.83 (2.77)	12.79 (2.57)
Years of working experience	10.50 (5.02)	10.97** (5.06)	10.28 (4.99)
Net wealth	-102 174 (458 991)	-106 109 (432 658)	-100 350 (470 694)
FTE of plant	172.8 (340.7)	194.9** (286.7)	162.6 (362.6)
Mean age of workers in plant	38.62 (4.62)	39.50** (4.75)	38.22 (4.49)
Mean years of education of workers in plant	12.38 (1.23)	12.36 (1.24)	12.39 (1.23)
Mean income of workers in plant	258 020 (91 783)	257 848 (96 313)	258 100 (89 607)
Rate of females in plant	0.251 (0.216)	0.262** (0.220)	0.245 (0.214)
Rate of all workers in plant committing a crime 1995	0.017 (0.028)	0.016** (0.029)	0.018 (0.028)
Charged 1992-1995	0.109	0.102**	0.112
Charged 1992	0.0386	0.0383	0.0387
Charged 1993	0.0352	0.0315**	0.0369
Charged 1994	0.0294	0.0284	0.0299
Charged 1995	0.0287	0.0262*	0.0298
Charged 1996	0.0270	0.0248*	0.0280
Charged 1997	0.0283	0.0270	0.0289
Charged 1998	0.0233	0.0233	0.0233
Charged 1999	0.0242	0.0238	0.0244
Charged 2000	0.0231	0.0231	0.0231
Charged 2001	0.0229	0.0246	0.0221
Charged 2002	0.0212	0.0211	0.0212
Charged 2003	0.0205	0.0223	0.0196
Charged 2004	0.0193	0.0203	0.0189
# observations	44 391	14 060	30 331

Notes: Standard deviations are in parentheses. Variables are measured in 1995 unless otherwise specified. * and** indicate that the variable is significantly different across the group of men in closing and stable plants at the 5 and 1 percent level (t-test).

Table 2: Main results: Effect on Crime (2000-2004) of Young Unmarried Men Being Exposed to Plant Closure (1995-2000)

	Model 1	Model 2	Model 3	Model 4	Model 5
Dependent variable: Charged over 2000 – 2004					
Plant closure	1.14** (0.047) [0.0090]	0.0087** (0.0029)	1.14** (0.046)	0.0099** (0.0034)	0.0097** (0.0036)
Neigborhood FE included			X	X	
Mean of dependent variable	0.08	0.08	0.08	0.08	0.08
R-squared		0.07		0.29	
N	44 391	44 391	44 391	44 391	43 836

*Note:*Model 1 is logit estimate (odds-ratio) for the effect on crime (2000-2004) of closure of the plant of employment in 1995; with implied mean marginal effect in brackets. Model 2 is OLS estimate. Model 3 and Model 4 replicate the logit and OLS models (respectively), augmented with neighborhood fixed effects. Model 5 is propensity-matched estimate of the average treatment effect using the nearest neighbor matching method (with caliper 0.01, and bootstrapped standard errors), cf. *psmatch2* written for *Stata9* by Leuven and Sianesi (2003). * and ** denote significance at the 5 and 1 percent levels. For Models 1-3, robust standard errors in parentheses corrected for non-independent observations within plant. All models include covariates described in text, and 9 770 neighborhood fixed effects if indicated.

Table 3: Specification Tests: Plant Closure (1995-2000) Uncorrelated with Preceding Crime

	Model 1	Model 2	Model 3
Dependent variable:	Charged 2000-2004	Charged 2000-2004	Charged 1992-1995
Plant Closure	1.14** (0.047)	1.13** (0,047)	1.00 (0.036)
Covariates for *Charged 1992-1995* by category dropped		X	X
Mean of dependent variable	0.08	0.08	0.11
N	44 391	44 391	44 391

Note: Odds-ratios from logit estimation. * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations within plant. All models include covariates described in text.

Table 4: Robustness Checks: Effect of Plant Closure (1995-2000) on Crime (2000-2004) by Varying Definitions and Samples

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Dependent variable:	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged 2000-2004	Charged at least twice 2000-2004
Plant Closure (PDR>.9)	1.14** (0.047)			1.15** (0.046)	1.16** (0.045)	1.13** (0.045)	1.13* (0.054)	1.18** (0.070)	1.21** (0.083)
PDR > .95		1.11* (0.048)							
PDR > .8			1.12** (0.045)						
PDR in (0.45,0.9]						1.07 (0.051)			
PDR in (0,.0.45]						0.97 (0.033)			
Sample redefinition		Obs. with PDR in (0.9,0.95] excluded	Obs. with PDR in (0.8,0.9] also included	Obs. with PDR in (0,0.05) also included	Obs. with PDR in (0,0.1) also included	Obs. with PDR in (0,0.9] also included	Obs. with FTE < 20 excluded	Obs. with FTE < 50 excluded	
Mean of dependent variable	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.03
N	44 391	42 850	46 407	49 884	56 238	92 039	34 832	23 055	44 376

Note: Odds-ratios from logit estimation. * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations within plant. All models include covariates described in text.

Table 5: Effect of Plant Closure (1995-2000) on Crime (2000-2004) by Category of Crime

	Model 1	Model 2	Model 3	Model 4	Model 5
Dependent variable: Charged (2000-04) for:	Any Crime	Traffic violation	Violent crime	Alcohol and Drugs	Property crime
Plant Closure	1.14** (0.047)	1.19** (0.064)	1.15 (0.13)	1.16^{+} (0.097)	0.91 (0.13)
Mean of dependent variable	0.08	0.04	0.01	0.02	0.01
N	44 391	44 391	44 376	44 376	44 376

Note: Odds-ratios from logit estimation. +, * and ** denote significance at the 10, 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations within plant. All models include covariates described in text. Crime categories follow closely the US FBI definitions and are defined in the Appendix. Models 3-5 estimated with 15 fewer observations due to no variation of the outcome variable within the group of persons with missing on education.

Table 6: Effect of Plant Closure (1995-2000) on Earnings, Employment and Categories of Crime over Time

	1999-2000	2001-2002	2003-2004
A. Dependent variable: Mean earnings over given period			
Plant Closure	-1 122 (2 327)	-1 303 (2 981)	-1 493 (2 954)
Mean of dependent variable	330 732	368 872	396 538
B. Dependent variable: Days full-time employed in given period			
Plant Closure	-16.8** (2.76)	-11.0** (2.76)	-12.4** (3.05)
Mean of dependent variable	644.4	647.3	620.6
C. Dependent variable: Charged for any crime over given period			
Plant Closure	1.06 (0.057)	1.08 (0.061)	1.18** (0.070)
Mean of dependent variable	0.04	0.04	0.04
D. Dependent variable: Charged for property crime over given period			
Plant Closure	0.99 (0.17)	0.88 (0.17)	0.95 (0.21)
Mean of dependent variable	0.00	0.00	0.00
E. Dependent variable: Charged for violent crime over given period			
Plant Closure	0.88 (0.15)	1.15 (0.19)	1.10 (0.19)
Mean of dependent variable	0.01	0.00	0.00
F. Dependent variable: Charged for alcohol and drugs related crimes over given period			
Plant Closure	1.24* (0.13)	1.17 (0.13)	1.18 (0.14)
Mean of dependent variable	0.01	0.01	0.01
G. Dependent variable: Charged for traffic violation over given period			
Plant Closure	1.05 (0.080)	1.17^{+} (0.093)	1.17* (0.089)
Mean of dependent variable	0.02	0.02	0.02

Note: N=44 391. Panels A and B report OLS estimates. Panels C-G reports odds-ratios from logit estimation. $^{+}$, * and ** denote significance at the 10, 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations within plant. All models include covariates described in text. Some crime models estimated with 15 fewer observations (N=44 376) due to no variation of the outcome variable within the group of persons with missing education.

Figure 1: Effect of Plant Closure on *Charged 2000-2004* by Day of Week

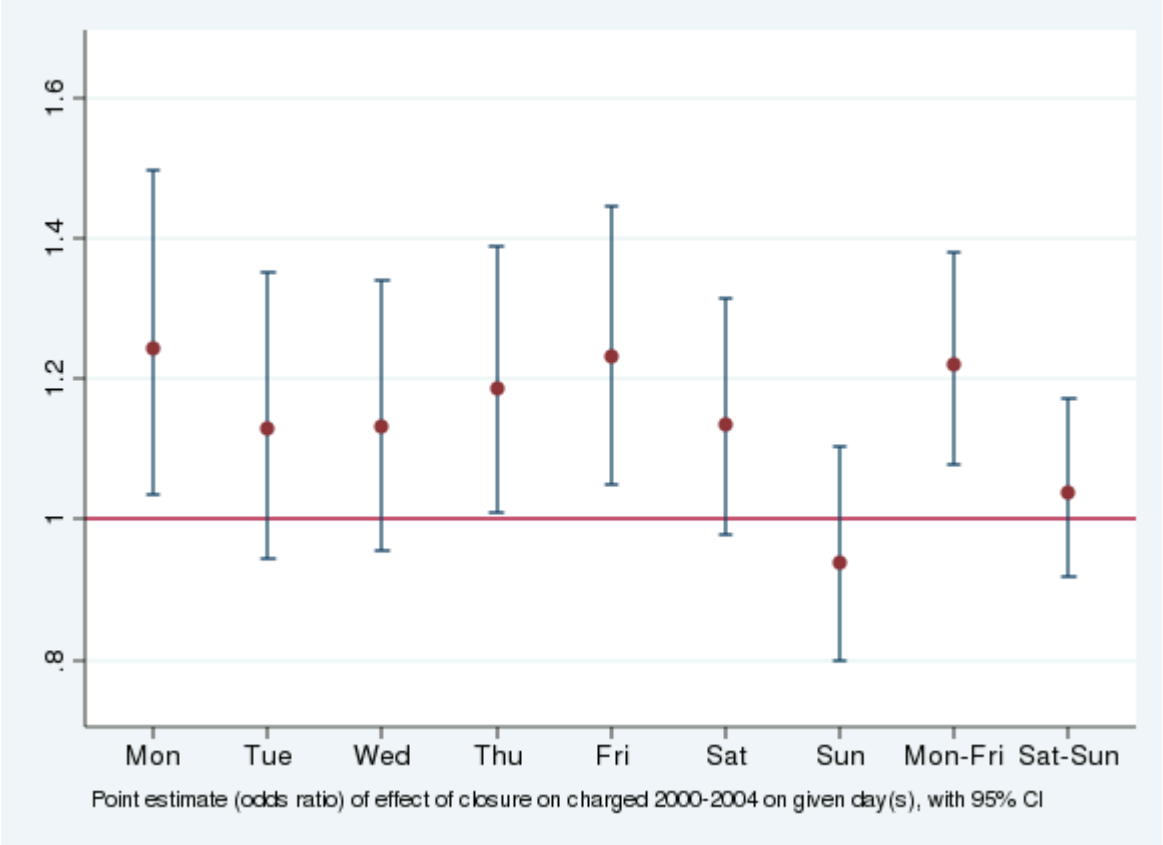
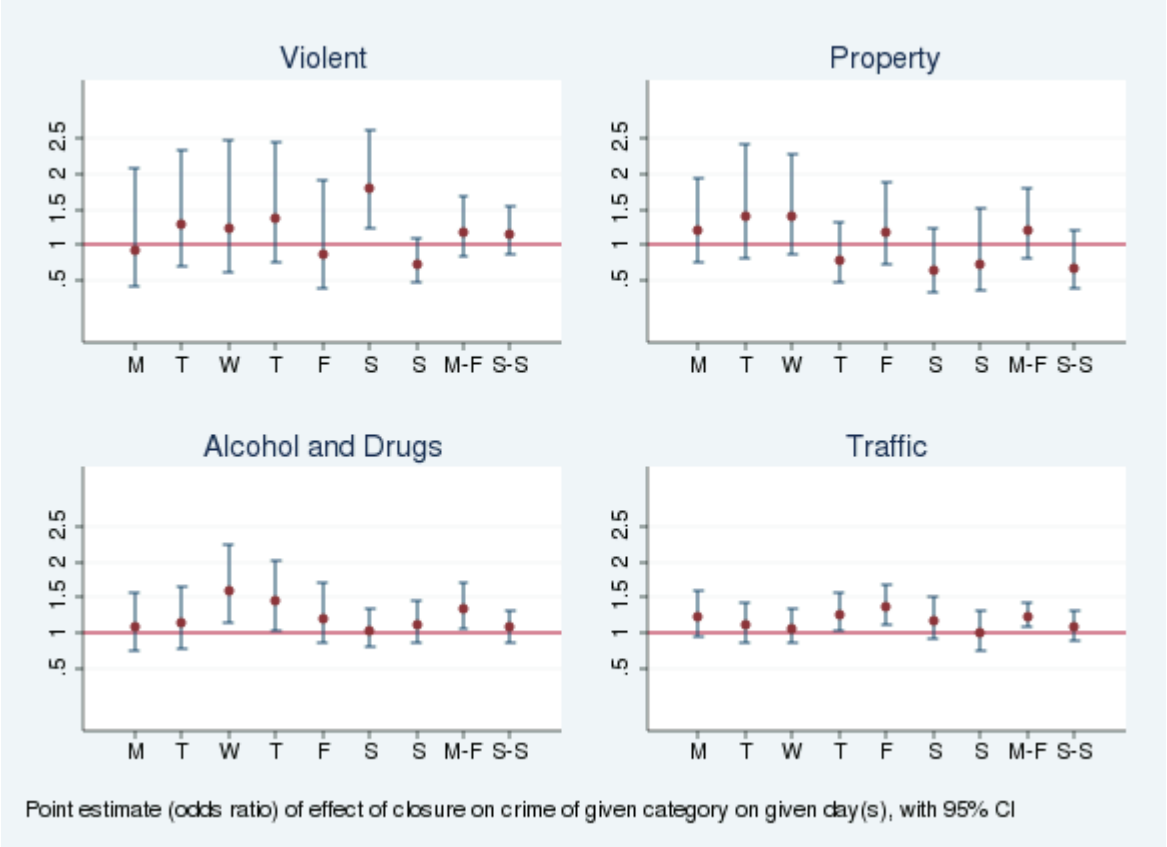


Figure 2: Effect of Plant Closure on Given Crime Category by Day of Week



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Appendix A

Definition of crime categories

In this paper we have created crime categories that resemble the categories most widely used in the literature, i.e. the categories defined by the Uniform Crime Reporting (UCR) Program of the US FBI (see http://www.fbi.gov/ucr/cius2007/). These categories deviate somewhat from the standards in the official Norwegian crime statistics. There are some important reasons why it is difficult to make comparisons of crime categories across countries. First, there are differences in legislations so that the specific contents may differ. Second, there are differences in registration routines and coding schemes used by the police. Third, the priority of the police, and thus also detection rates, are influenced by public concerns. For further examples of the difficulties that apply, see e.g. the appendixes in The European Sourcebook of Crime and Criminal Justice Statistics (http://www.europeansourcebook.org/).

Our creation of UCR-like categories is based on the fine-graded standard categorizations used to produce official crime statistics in Norway (Statistics Norway 2008). In the overview below, we also refer to Norwegian legislation (of which some is available in English translations online at URL: http://www.ub.uio.no/ujur/ulov/english.html). Most of the legislations refer to the General Civil Penal Code, but there are also offences regulated elsewhere.

In the paper we have aggregated the UCR (2004) categories as follows:

Violent crime comprises the UCR categories *criminal homicide*, *forcible rape*, *robbery, aggravated assault, other assaults, sex offences,* and *offences against the family and children.*

Property crime comprises the UCR categories *burglary, larceny-theft, motor vehicle theft,* and *stolen property: buying, receiving, possessing.*

Alcohol and Drugs comprises the UCR categories *drug abuse violations, driving under the influence,* and *drunkenness.*

Traffic violation follows the Norwegian definition, but driving under the influence is excluded (since it is included in the "Alcohol and Drugs" category above). Traffic violations include only more serious violations like *excessive* speed driving.

Overview of categories

UCR Categories	Translation of Norwegian Category
Criminal homicide	
Drap (§ 233)	Murder
Uaktsomt drap (§ 239)	Manslaughter
Forcible rape	
Voldtekt (§ 192)	Rape
Seksuell omgang m/trusler, underfundig adferd o.l. (§ 192, 200)	Sexual intercourse by threats, cunning behaviour etc.
Seksuell omgang med bevisstløs (§192, 200)	Sexual intercourse with unconscious person
Voldtektsforsøk (§ 192, jf. § 49)	Attempted rape
Robbery	
Utpressing og ran (kapittel 25)	Blackmail and robbery
Aggravated assault	
Legemsbeskadigelse (§ 229)	Wounding or inflicting bodily harm
Grov legemsbeskadigelse (§ 231)	Inflicting grievous bodily harm
Drapsforsøk (§ 233 jf. § 49)	Attempted murder
Burglary	
Innbrudd (§ 147)	Housebreaking and burglary
Larceny-theft	
Simpelt tyveri (§ 257, 261-262)	Simple and minor larceny
Grovt tyveri (§ 258)	Aggravated larceny
Nasking	Petty larceny
Motor veichle theft	
Brukstyveri av motorkjøretøy (§ 260)	Theft of motor vehicle
Arson	
Forsettelig forvoldelse av ildebrann (§ 148)	Arson
Uaktsom forvoldelse av ildebrann (§ 151)	Negligently causing fire
Other assault	
Legemsfornærmelse (§ 228)	Assault
Uaktsom legemsbeskadigelse (§ 237, 238)	Negligently inflicting bodily harm
Annet (§ 234, 240-245)	Other assault
Forgery and counterfeiting	
Dokumentfalsk (kapittel 18)	Forgery
Pengefalsk (kapittel 17)	Counterfeiting
Fraud	
Bedrageri og utroskap (kapittel 26)	Fraud and betrayal
Embezzelment	
Underslag (kapittel 24, § 255-256)	Embezzlement
Stolen property: buying, receiving, posessing	
Heleri og etterfølgende bistand (kapittel 31)	Receiving stolen goods, assistance to the offender

UCR Categories	Translation of Norwegian Category
Vandalism	
Simpelt skadeverk (§ 291)	Inflicting ordinary damage
Grovt skadeverk (§ 292)	Inflicting serious damage
Skadeverk, forseelse (§324-436)	Inflicting damage to property, misdemeanour
Weapons: carrying, possessing	
None apply	
Prostitution and commercialized vice	
None apply	
Sex offences (except rape, prostitution)	
Incest (§ 197, 198) Incest	
Seksuell omgang med barn (§195-196, §200)	Sexual intercourse with children
Annen seksuell omgang (§ 193, 199)	Other sexual intercourse
Seksuelt krenkende eller annen uanstendig atferd (§ 201)	Sexual violating or other indecent behavior
Pornografi (§ 204)	Pornography
Annet (§ 200, 202, 203)	Other
Drug abuse violations	
Narkotikaforbrytelse (§ 162 1. og 4. ledd)	Crime of narcotics
Grov narkotikaforbrytelse (§ 162 2. og 3. ledd)	Serious crime of narcotics
Narkotika, bruk (lov om legemidler)	Use of narcotics
Narkotika, besittelse (lov om legemidler)	Possession of narcotics
Narkotika, diverse (lov om legemidler)	Other crime in connection with narcotics
Gambling	
Åger og lykkespill (kapittel 29)	Usury and gaming offenses
Offenses against the family and children	
Misligholdt forsørgelsesplikt mv. (§ 219)	Defaulting obligation to support dependents
Inngåelse av ugyldig eller omstøtelig ekteskap (§ 220)	Contracting non-valid or annulable marriage
Bortføring av barn (§ 216)	Kidnapping children
Annet (§ 215-219)	Other
Driving under the influence	
Promillekjøring (veitrafikkloven)	Driving under influence of alcohol etc.
Liquor law	
Forseelse og forbrytelser mot alkoholloven	Violations of the Alcohol legislation
Drunkenness	
Drukkenskap (løsgjengerloven)	Drunkenness
Disorderly conduct	
Ordensforstyrrelse (løsgjengerloven)	Disorderly conduct
Forseelse mot politivedtektene	Violations of the Police regulations
Vagrancy	
Annet (løsgjengerloven)	Other, vagrancy
All other offenses	
Everything else	

Covariates included in all regressions

The following variables are included in all models (unless explicitly specified otherwise):

- age: third order polynomial
- age of youngest child: 6 categories (≤1, 1-3, 3-7, 7-13, >13, missing)
- number of kids: third order polynomial
- years of education: 5 categories (≤ 10, 10-13, 13-16, ≥16, missing)
- charged once for property crime over 1992-1995: 2 categories
- charged twice or more for property crimes over 1992-1995: 2 categories
- charged once for violent crime over 1992-1995: 2 categories
- charged twice or more for violent crimes over 1992-1995: 2 categories
- charged once for crime related to alcohol and drugs over 1992-1995: 2 categories
- charged twice or more for crimes related to alcohol and drugs over 1992-1995: 2 categories
- charged once for other (not violent, property and alcohol/drugs) crime over 1992-1995: 2 categories
- charged twice or more for other (not violent, property and alcohol/drugs) crimes over 1992-1995: 2 categories
- years of labor market experience: third order polynomial
- income: third order polynomial
- net wealth: third order polynomial
- earnings: third order polynomial
- received sick money in year: 2 categories
- industry of 1995-plant: 10 categories
- number of FTEs in 1995-plant: third order polynomial
- mean age of all employees in 1995-plant: third order polynomial
- mean years of education of all employees in 1995-plant: third order polynomial
- mean income of all employees in 1995-plant: third order polynomial
- rate of all employees in 1995-plant female: third order polynomial
- rate of all employees in 1995-plant committing crime in 1995: third order polynomial
- mean of annual male unemployment rate 1995-2000 in municipality of residence in 1995: linear