

The Effect of Job Loss on Crime: Evidence from Plant Closure Events

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Preliminary

Any comments are appreciated

Abstract

We estimate the effect of job loss on crime using an exceptional individual panel data set comprising all unmarried and employed Norwegian men below the age of 40. Men originally employed in plants that subsequently closed are about 12 percent more likely to be charged of a crime than comparable men in stable plants. We perform a number of specification tests, which all support a causal interpretation of our result. There is no significant effect of plant closure on acquisitive crime, casting doubt on the conjecture that reduced licit earnings opportunities leads to engagement in illicit work. However, we find a significant effect of plant closure on violent crime, possibly suggesting a role for mental distress, frustration or anger as a mechanism through which job loss affects crime.

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 and are motivated by the assumption that crime is closely linked with labor market opportunities. However, there is limited empirical evidence supporting this assumption (Bushway and Reuter 1997, Fagan and Freeman 1999, Uggen 2000, Uggen and Wakefield 2007). The current paper utilizes a unique dataset to investigate the causal effect of job loss on criminal behavior.

Job loss could affect crime through at least two possible mechanisms. Convincing evidence shows that job loss increases the likelihood of future unemployment and welfare program participation, and negatively affects future earnings (Jacobson, Lalonde and Sullivan 1993, Stevens 1997, Huttunen, Møen and Salvanes 2006, Rege, Telle and Votruba 2009). Traditional rational choice theory of crime predicts that such a reduction in licit earnings opportunities associated with job loss should increase allocation of time towards crime for profit (Ehrlich 1973, Becker 1968). Theories within the fields of sociology and criminology predict effects on non-acquisitive crime, like violence, of changes in labor market opportunities. Felson (1998) argues that less structured routine activities, like no longer going to work, may increase exposure to criminogenic settings in which there may also be less social bounds preventing crime (Hirschi 1969). Convincing evidence suggest that plant closure imposes mental distress on affected workers (e.g. Vahtera and Kivimaki 1997, Dragano, Verde and Siegrist 2005). Agnew (1992) argues that when distress elicits anger, crime is more likely to occur, especially violent crime.

The main methodological challenge in estimating a causal effect of job loss on crime relates to unobservable individual characteristics that jointly influence involvement in criminal activities and labor market attachment. Unobserved characteristics, like an individual's capacity for self-control or attitude towards the treatment of others, could lead to a spurious correlation between unemployment and criminal behavior (Wikström and Treiber 2007, Gottfredson and Hirschi 1990). The fact that past criminal behavior can impede

employment provides another potential source of estimation bias unless past criminal behavior can be accurately measured.

It is difficult to address omitted variable problems without data on the same individual over time, and lack of such longitudinal data is a much-cited reason for the limited knowledge of relationships between labor market opportunity and crime (Freeman 1995, Cantor and Land 2001, Levitt 2001, Uggen and Wakefield 2007). Individual data on crime and employment are unavailable in most countries, because of the sensitivity of individual criminal records and because it is difficult to merge crime records with other data sources on individual characteristics.

To circumvent the most obvious forms of omitted variable bias, our empirical strategy utilizes job losses that are associated with plant closures.¹ We draw on two Norwegian data sources maintained by Statistics Norway to estimate the causal effect of job loss on crime. 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. Merging these data sources is straightforward based on a unique identifier for each person in Norway. Employment spell records in the *FD-trygd* allow us to calculate employment counts by plant and year, used to identify workers in 1995 employed in a plant that subsequently closed or was stable. In our main analytic sample, we consider all unmarried men below age 40 that were employed in 1995, yielding a sample of more than 40,000 men. We focus on unmarried young men between 18 and 40 years since women generally commit very few crimes, and 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).

Our effect estimate of plant closure on crime is based on comparisons of covariate adjusted crime rates across men originally employed in closing and stable plants. The crucial identifying assumption is that plant closure events are uncorrelated with unobserved individual determinants of crime. Importantly, if this assumption does not hold, we would expect workers in closing plants to have a higher propensity to engage in crime *even prior to the closure event*. The richness of our dataset allows us to test this, and to do additional

¹ Throughout, we use the term “plant” to refer to the establishment at which a worker is employed, which is distinct from the firm of employment (as firms can consist of multiple plants).

specification checks testing for unobserved differences across workers in closing and stable plants.

Our empirical results suggest that young men's exposure to plant closure has a substantial effect on crime. The likelihood of crime of men originally employed in plants closing between 1995 and 2000 is about 12 percent higher than the likelihood of comparable men in stable plants. The specification tests support a causal interpretation of this result.

To explore plausible mechanisms, we look at sub-categories of crime. The effect of exposure to plant closure is insignificant for crime for profit, casting some doubt on a conjecture that reduced licit earnings opportunities encourages engagement in illicit work. However, we find an effect of plant closure on violent crime, suggesting a role for mental distress or idleness as a mechanism through which the effect of plant closure on crime operates.

This paper is relevant to numerous policy initiatives reflecting the idea that employment opportunities reduce the likelihood or extent of criminal activities and that well-designed educational and job assistance programs are effective tools of crime prevention. Several recent Norwegian White Papers underline that access to formal work is important to prevent crime (St. meld 2008, 2006). The preparatory work on the most recent of these White Papers states that the "overarching perspective is to reduce recidivism by improving the basic opportunities for living a lawful life: work and education" (Bakken et al. 2007, p. 5, our translation). The relevance of labor market opportunities is also emphasized in the KrAmi-program in Sweden (Jess 2005) as well as in several programs in the US (Bushway and Reuter 1997).

To date, we have limited empirical research documenting a causal effect of job loss on crime. Several studies investigate how crime is affected by unemployment rates and other measures of economic performance in state or county level data (e.g. Grogger 1998, Pudney et al. 2000, Raphael and Winter-Ebmer 2001, Oster and Angell 2007, Machin and Meghir 2004, Ihlanfeldt 2007, Lin 2008). However, these approaches are unsatisfactory as evidence of causal effects since an individual's labor market opportunities are only weakly linked to the overall performance of the economy (Freeman 1995, Levitt 2001). Some panel data studies confirm a positive association between lack of employment and crime (e.g. Thornberry and Christenson 1984, Sampson and Laub 1993, Witte and Tauchen 1994, Fergusson et al. 2001, Apel et al. 2006), but do not account for unobserved individual characteristics. One study by Uggen (2000) stands out for investigating a causal link between labor market opportunities

and crime. The study relies on data from a random assignment program in the US and finds no general effect of minimum-wage employment on recidivism.

The remainder of the paper is structured as follows. Section 2 discusses possible mechanisms through which plant closure can affect the involvement in illegal activities. Section 3 presents the empirical strategy. Section 4 discusses data and measurement issues. Section 5 describes the dataset. Section 6 presents our results, specification and robustness checks. Section 7 discusses the plausibility of different channels through which plant closure can cause crime. Section 8 concludes.

2 Mechanisms

Work by Gary Becker (1968) is the typical point of departure for studies in economics on causes of crime. The main idea is that crime is committed if the expected utility of committing crime exceeds the utility of not committing crime. While Becker (1968) was primarily occupied with optimal enforcement, a number of studies following his work extend the modeling of crime behavior (see refs in e.g. Levitt and Miles 2007, p. 459). One of the most cited and relevant for us is Ehrlich (1973), who introduces a time constraint within which the individual divides time between licit and illicit activities.

Following Ehrlich (1973) changes in labor market opportunities can result in substitution of time between licit and illicit work. Job loss is documented to reduce future labor market opportunities, including future earnings and likelihood of employment (Jacobson et al. 1993, Stevens 1997, Huttunen et al. 2006, Rege et al. 2009). This means that job loss could reduce both the expected income from licit work and the value of leisure. The theory thus predicts that job loss should shift the allocation of time towards crime for profit.²

Following Becker (1968) it is not straightforward to explain why changes in labor market opportunities may have any effect on types on crime with little or no ability to provide material gains, like violent crime or traffic violations.³ There are, however, several theories within the fields of sociology and criminology that predicts effects on such non-acquisitive crime of changes in labor market opportunities.

² 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 et al. 2005). In other words, crime might be both a cause and an effect of poor labor market opportunities.

³ Given that non-acquisitive crime is attractive, however, the theory following Becker may straightforwardly explain that such crime increases since the disutility of imprisonment is likely to decline if the value of leisure declines.

Felson (1998) points out that even though people might be motivated for offending they cannot do so unless an opportunity is present. Less structured routine activities, like no longer going to work, increase idleness time where one might be more exposed to criminogenic settings, in which there may also be less social bounds preventing crime (Hirschi 1969). Job loss easily leads to spending more time hanging around, especially for the unmarried. Agnew (1992) argues that increased levels of frustration might lead to reduced constraints to breaking laws and social norms, and aggressive reactions in adverse situations. Convincing evidence suggest that job loss imposes mental distress on affected workers (e.g. Vahtera and Kivimaki 1997, Dragano, Verde and Siegrist 2005, McKee-Ryan et al. 2005). This distress could result from the downsizing process per se, from the job loss, or from being unemployed or adapting to a new job. If this distress is associated with more frustration and anger, the theory of Agnew (1992) predicts more non-acquisitive offences, particularly violence.

Some criminologists have influentially argued that there is *no* or only a *negligible* effect of unemployment on crime. Gottfredson and Hirschi (1990) hold that the widely observed association between unemployment and crime can be explained by a third factor formed in early childhood, such as low self-control. It is those personality traits that result in criminal activities and also make it difficult to succeed in work or school. The main methodological concern in estimating effects of job loss on crime is to rule out such spurious associations (see Sect. 3).

3 Empirical Strategy

We estimate the effect of job loss 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

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

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 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 comprises unmarried young men whose plant of employment in 1995 was either closed by 2000 (i.e. $PDR > .90$) or was 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:

$$(2) \quad Pr(C_{i,04}=1) = \Lambda(\alpha_0 + \eta W_i + \alpha_x X_i)$$

where

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

Estimates of η are potentially biased if men in plants that subsequently close have higher unobserved propensities for crime. For instance, men who expect to commit crimes potentially may have lower demand for stable employment, causing an over-representation of such men in plants that subsequently close. Alternatively, unobserved "third factors" could lead men with higher propensities for crime to self-select into less stable plants. For instance,

variation in individual risk preferences or discount rates could affect workers' criminal propensities and their selection into more/less stable plants. In either case, we might expect that men in plants that subsequently close would have higher unobserved propensities for crime, biasing 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. Moffitt 1983, Lindbeck, Nyberg and Weibull 1999, Sampson and Raudenbush 1999, Bertrand, Luttmer and Mullainathan 2000, Rege, Telle and Votruba 2007), 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, as 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, should address this source of bias. Moreover, we can evaluate the sensitivity of our estimates of η to the exclusion of covariates capturing the individual's past criminal behavior. Finally, we are able to test directly whether our measure of past plant closure is correlated with crime prior to the plant downsizing event.

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 divorce 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.

We will also apply propensity score methods to assure that our estimates of η do not suffer from heterogeneity bias. As an additional specification test, we employ difference-in-

difference estimators for individual panel data, as discussed by Imbens and Wooldridge (2008, p. 68). In addition to the notation above, let $C_{i,95}$ denote an indicator variable set to one if person i was charged over 1992-1995 and u_i an error term with mean zero. An estimate of a difference-in-difference estimator η_{did} may then be obtained running OLS on

$$(3) \quad C_{i,04} - C_{i,95} = \alpha'_0 + \eta_{did}W_i + \alpha'_X X_i + u_i$$

Here a crucial identifying assumption is that the change in the likelihood that men in closing plants commit crime (from before closure till after closure) would have been identical to the observed change in the likelihood of men in stable plants committing crimes *had plant closure not occurred*. We note that this assumption is fundamentally different from the assumptions necessary for a causal interpretation of η in eq. (2).

4 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 strong 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 registry 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 offences 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 control for previous engagement in crime should address this concern.

The second problem with register data is that crimes which are not reported to (or not recorded by) the police are omitted from the dataset and that crimes which are not “solved” cannot be matched to a specific criminal. The extent of these problems could differ across crime types. For instance, crimes at the work-place may often be 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 narrowly defined 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 of any crime data in mind when interpreting the results.

5 Dataset Description

We combine two register databases provided by Statistics Norway 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, but we use date of offence committed only through 2004 due to time lag between offence and charge. A person is registered as charged if the police perform an investigation and conclude that the person did commit a crime. The registration is independent of the further outcome of the case (filing of formal charges or convictions). Date of crime and detailed codes of type of crime are included. Statistics Norway has constructed sub-categories of crime and we rely on these definitions.⁴ Importantly for our mechanism investigation, we can identify crime for profit and violent crime. Moreover, the database does not only contain serious crimes, but also misdemeanors like drunk driving, excessive over-speeding and shop lifting.

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 on any of

⁴ See: http://www.ssb.no/emner/03/05/nos_kriminal/vedlegg.pdf

Norway's welfare programs, and geographic identifiers for about 14,000 different neighborhoods of residence.

In particular, the database 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.⁵

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 dataset comprises the overall native population of Norwegian residents, and we will look at several sub-samples. The construction of our *main analytic sample*, however, is based on two main considerations. First, we need some occurrence of crime to be able to estimate any effects with any precision. Since women, as well as older or married men, commit relative few crimes, we focus on unmarried men between 18 and 40 years of age in 1995. 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 minimal 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

⁵ 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.

plant with less than 10 FTEs in 1995. The resulting dataset consists of more than 44,000 unmarried men, living in about 9,700 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).

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

[Table 1 about here]

6 Empirical Results

6.1 Descriptive Evidence

We start by observing from Table 1 that the crime rate over 2000-2004 (*Charged 2000 - 2004*)⁶ for the sub-sample of men employed in 1995 in closing plants is slightly higher than the crime rate of men employed in 1995 in stable plants. The crime 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 job loss 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 crime rate in closing plants over 2000 - 2004 (8.4 compared to 8.0) to downsizing events - it could simply reflect more crime prone subjects working in closing plants. If such selection existed, we would expect the incidence of crime in 1995 among our sampled men in plants about to downsize to be higher than the incidence among our men in plants staying stable. We see from Table 1, however, that the 1995-crime rate (*Charged 1995*) of our sampled men working in plants that subsequently closed (2.6 percent), is *lower* than the crime rate of our men working in stable plants (3.0 percent).

⁶ As noted in the Dataset Description section, our measure of crime is dated according to the time of offence. Hence, the more extensive "charged for a crime committed 2000-2004" would be more precise.

6.2 Main Results

Table 2 presents the main results. The estimated plant closure coefficient (cf. Eq. 2) captures the incremental increase in our men’s likelihood of committing crime due to plant closure, relative to our men working in stable plants.

Models 1 and 2 are the logit and the OLS regressions without controlling for neighborhood fixed effects; while Model 3 includes 9,770 geographically defined neighborhood fixed effects in the OLS regression. The estimated marginal effect is similar in magnitude (0.01) across all three models. In Model 4 we include the logit model with neighborhood fixed effects. We note that this conditional logit model causes us to drop half of the observations, while the estimated odds-ratio is very similar to the one estimated in the logit model without neighborhood fixed effects (Model 1). In the following we will be referring to Model 1 as our main result. Together these models suggest that plant closure significantly increases the likelihood of being charged for crime.

As discussed in Section 3, 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. If so, including dummies for a number of geographically defined areas would be expected to reduce the estimated effect. The modest effect of including 9,770 neighborhood fixed effects (compare Models 2 and 3) indicates that such geographic differences are not important. The result suggests that men originally employed in plants that closed between 1995 and 2000 were about 0.1 percentage points more likely to commit crime over 2000-2004 than comparable men in stable plants, an increase of about 12 percent.

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.⁷ The estimate is very similar to the ones in Models 1 - 3 (0.01). This indicates that possible heterogeneous effects do not cause the logit and OLS result to deviate seriously from the average treatment effect.

⁷ The average treatment effect estimate shown relies on the nearest neighbor with caliper (0.01) matching method available in the Stata® 9 command *psmatch2* by Leuven and Sianesi (2003). That is, each “treated” observation was matched to the nearest “control” observation within 1 percentage point in the estimated probability of men’s plant closure. Reducing the magnitude of the caliper had little impact on the estimate. For computational reasons, we were unable to accommodate neighborhood fixed effects.

[Table 2 about here]

6.3 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 further, we recall that Table 1 suggests this is not a concern: men in stable plants had modestly higher rates of criminal activity prior to 1995 than men in plants that subsequently closed.

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 control for crime prior to plant closure. In Model 2 of Table 3 we report the result from the regression where we dropped the control variable indicating whether the individual was charged over 1992-1995. By comparing with the logit estimate from Table 2 (reported in Model 1 of Table 3) we see that excluding the pre-closure crime indicator 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).⁸ We see that this model yields no significant covariate-adjusted correlation between subsequent plant closure and crime, suggesting that our main result is not seriously biased by men in plants that subsequently downsize initially being more or less crime prone for unobserved reasons.

Finally, in Table 4 we apply difference-in-difference estimators for individual panel data discussed by Imbens and Wooldridge (2008, p. 68), cf. eq. (3). While the crime rate declined by 3.2 percentage points for men in stable plants from 1992/1995 to 2000/2004 (cf. Table 1), it only declined by 1.8 percentage points for men in closing plants. That is, the crime rate declined by 1.4 percentage points more for men in stable plants compared to men in closing plants (see estimated treatment effect in Model 1 of Table 4). This may be taken to indicate that the crime rate of men in closing plants had been 7.0 percent (instead of 8.4

⁸ Here we have excluded the *Charged 1992-1995* variable from the set of included covariates.

percent, cf. Table 1) had these plants not downsized over the period. Or put in relative terms, the crime rate is 20 percent (1.4/7.0) higher than it would have been had the closing plants not downsized. In Models 2 and 3 of Table 4 we add control variables, and we see that this has a modest influence on the estimated effect.

Overall, these specification tests strongly suggest that our estimated effect of exposure to plant closure on crime is not upward biased. There may, however, be some indication that it is downward biased.

[Table 3 and 4 about here]

6.4 Robustness

Table 5 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 all other models), 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 and 50 FTE in 1995 (and not 10 as in our main analytic sample). We see that these restrictions only have fairly modest impacts on our estimate.

Table 6 addresses how long it takes from plant closure events occur till they materialize in criminal engagement (Model 1 replicates our main result). The estimated odds-changes modestly as crime in years just after 2000 are dropped from the dependent variable, though there is some indication that the odds-ratio continues to increase as we move away

from the closure event. This may indicate that the effect of plant closure over 1995-2000 on subsequent crime is not fully exhausted by the end of our data window (2000-2004).

[Tables 5 and 6 about here]

7 Discussion of Mechanisms

As discussed in Section 2 there are several mechanisms through which plant closure can affect the likelihood of engaging in crime. The richness of our panel data enables us to empirically explore the plausibility of some of these mechanisms.

7.1 Substituting licit earnings with illicit income?

Within the traditional rational crime theory an individual would reallocate time from licit earnings activities to illicit income activities if the relative payoff from formal work declined. It is well-documented that exposure to plant closure reduces future earnings as well as the likelihood of future employment (Jacobson et al. 1993, Stevens 1997, Huttunen et al. 2006, Rege et al. 2009). If such a shift in the time allocation explains our estimated effect of plant closure on crime, we should observe an effect of plant closure on sub-categories of crime that entail income opportunities. Crime for profit, like e.g. larceny or burglary, is a sub-category of crime in our dataset that entail income opportunities.

In Table 7 we present logit⁹ estimates (odds-ratios) for the probability of being charged for a given sub-category of crime over 2000-2004. From Model 2 we see that the estimated effect of plant closure on crime for profit is low and statistically insignificant; casting some doubt on the conjecture that reduced licit earnings opportunities leads to engagement in illicit work.

7.2 Frustration, Anger and Family

Following theories in sociology and criminology, changes in labor market opportunities may also affect types of crime with little or no ability to provide material gains, like violent crime or traffic violations. Latent frustration and anger might make persons act

⁹ We have also estimated the models in Table 7 using OLS with municipality fixed effects and negative binomial regression (without neighborhood effects). The results were very similar.

irrational (Agnew 1992), and convincing evidence suggests that plant closure imposes mental distress on affected workers (e.g. Vahtera and Kivimaki 1997, Dragano et al. 2005). If job loss raises levels of frustration and anger, we would expect to see more non-acquisitive offences - in particular violence. Sub-categories of crime available in our dataset that fits this theory include violence, traffic offence and damage of property.

We look at such non-acquisitive sub-categories of crime in Models 3-6 of Table 7. In Model 3 we have created a variable comprising the three sub-groups violent crime, traffic violations and damage of property (“frustration crime”). We see that the estimated coefficient is significant. Though clear conclusions cannot be based on these admittedly rather loose empirical observations, they are at least consistent with a role for frustration or strain as a mechanism through which the effect of plant closure on crime operates.

[Table 7 about here]

Having a family may moderate a young man’s likelihood of committing crime after exposure to plant closure, for example since family activities retain some 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). Following these arguments we would expect to see men with family or children being less likely to respond to exposure to plant closure by engaging in crime.

In Table 8 we look at men defined in the same way as in our main analytic sample *except* with respect to the requirement that they are unmarried. Model 1 replicates our main result. In Models 2-6 we change the requirement in our main analytic sample that men are unmarried to i) that they are not unmarried (Model 2), including the married, the divorced and the widowed; ii) that they are currently married (Model 3); and iii) that they have one or more children (Model 4), regardless of marital status. We see that there is *no* effect of plant closure on crime for these men. Overall this indicates that unmarried men comprise a selected group whose likelihood of crime is more affected by changes in labor market opportunities.

[Table 8 about here]

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 close down from 1995 to 2000 were about 12 percent more likely to be charged for at least one crime over 2000-2004 than comparable men in stable plants. The dataset enables us to perform a number of robustness and specification tests, which all support a causal interpretation of this result.

Breaking down on the type of crime, we find little evidence that the increase in crime is due to substitution of licit with illicit sources of income. Crime for profit is far less affected by plant closure than violent crime and violations of traffic regulations. Overall, this may indicate that it is frustration, rather than reallocation of time from licit to illicit work, that increases the crime propensity of young and unmarried men who loses their job.

Appendix

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

- age: 22 categories (18, 19, ..., 38, 39)
- age of youngest child: 6 categories (≤ 1 , 1-3, 3-7, 7-13, >13 , missing)
- number of kids: 5 categories (0, 1, 2, 3-4, ≥ 5)
- number of kids in household: 5 categories (0, 1, 2, 3-4, ≥ 5)
- years of education: 5 categories (≤ 10 , 10-13, 13-16, ≥ 16 , missing)
- committed crime over 1992-1995: 2 categories
- years of labor market experience: 5 categories (≤ 5 , 5-10, 10-15, 15-20, >20)
- income: third order polynomial
- net wealth: third order polynomial
- earnings: third order polynomial
- received sick money in year: 2 categories
- years of tenure in 1995-plant: 6 categories ($=1$, 1-3, 3-5, 5-10, 10-15, >15)
- industry of 1995-plant: 10 categories
- number of FTEs in 1995-plant: 9 categories
- mean age of all employees in 1995-plant: 9 categories
- mean years of education of all employees in 1995-plant: 9 categories
- mean income of all employees in 1995-plant: 9 categories
- rate of all employees in 1995-plant female: 9 categories
- rate of all employees in 1995-plant committing crime in 1995: 9 categories

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Table 1: Summary Statistics Main Analytic Sample

Variable	All	Men in closing plants	Men in stable plants
Plant closure	0.3172		
Charged 2000-2004	0.0812	0.0841	0.0799
Age	29.88 (4.914)	30.39 (4.920)	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)
Yrs of education	12.81 (2.64)	12.83 (2.77)	12.79 (2.57)
Yrs 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)
Tenure	4.33 (3.77)	4.49 (3.98)	4.26 (3.66)
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 female 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.

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	1.13** (0.045) [0.01]	0.0082** (0.0029)	0.0097** (0.0030)	1.15** (0.065)	0.011** (0.0033)
Neighborhood FE included			X	X	
Mean	0.08	0.08	0.08	0.16	0.08
R-squared		0.06	0.28		
N	44 391	44 391	44 391	20 577	44 380

Note: Model 1 is logit estimate for the effect on crime (2000-2004) of closure of the plant of employment in 1995; with implied mean marginal effect in brackets. Models 2-3 are the OLS estimates of the effect, without and with neighborhood fixed effects. Model 4 is the conditional logit model 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 residuals within plant. All estimates adjust for 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	1.13** (0.045)	1.12** (0.045)	1.01 (0.037)
Covariate <i>Charged 1992-1995</i> included	X		
Mean	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 residuals within plant. All estimates adjust for covariates described in text.

Table 4: Specification Tests: Difference-in-Difference Estimates of the Effect of Plant Closure (1995-2000) on Crime (2000-2004)

	Model 1	Model 2	Model 3
Dependent variable:	Charged 2000-2004	Charged 2000-2004	Charged 1992-1995
Plant Closure (treatment effect)	0.0144** (0.00376)	0.0143** (0.00376)	0.0131** (0.00376)
Individual covariates included	No	Yes	Yes
Neighborhood fixed effects included	No	No	Yes
Mean	0.08	0.08	0.08
R-squared	0.00	0.00	0.23
N	44 391	44 391	44 391

Note: OLS regression of treatment effect. * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent residuals within plant. Covariates and neighborhood fixed effects included as described in text.

Table 5: 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
Dependent variable: Charged 2000-2004								
Plant Closure (PDR>.9)	1.13** (0.045)			1.13** (0.045)	1.14** (0.044)	1.12** (0.044)	1.11* (0.052)	1.15* (0.068)
PDR > .95		1.10* (0.047)						
PDR > .8			1.11** (0.0030)					
PDR in (0.45,0.9]						1.08 (0.050)		
PDR in (0,.0.45]						0.97 (0.032)		
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	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.07
N	44 391	42 850	46 407	49 891	56 285	92 042	34 832	23 055

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 residuals within plant. All estimates adjust for covariates described in text.

Table 6: Impact over time: Effect of Plant Closure (1995-2000) on Crime Measured over Given Periods

	2000-2004	2001-2004	2002-2004	2003-2004
Dependent variable: Charged over given period				
Plant Closure	1.13** (0.045)	1.13** (0.049)	1.14** (0.056)	1.16 (1.87)
Mean	0.08	0.07	0.05	0.03
N	44 391	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 residuals within plant. All estimates adjust for covariates described in text.

Table 7: Sub-categories of Crime: Effect of Plant Closure (1995-2000) on Given Sub-Category of Crime (2000-2004)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
			"Frustration crime"			
Dependent variable: Charged (2000-2004) for:	Any Crime	Crime for profit	Overall	Traffic Violation	Violent Crime	Damage of property
Plant Closure	1.13** (0.045)	1.05 (0.105)	1.13** (0.053)	1.13* (0.056)	1.16 (0.132)	1.22 (0.289)
Mean dep. var.	0.08	0.01	0.06	0.05	0.01	0.00
R-squared						
N	44 391	44 352	44 391	44 391	44 064	34 027

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 residuals within plant. All estimates adjust for covariates described in text.

Table 8: Other Samples: Effect of Plant Closure (1995-2000) on Crime (2000-2004)

	Model 1	Model 2	Model 3	Model 4
Dependent variable: Charged 2000-2004				
Plant Closure	1.13** (0.045)	0.92 (0.048)	0.92 (0.057)	1.01 (0.043)
Sample redefinition	Main analytic sample (unmarried men)	Non-unmarried men	Married men	Men that are fathers
Mean	0.08	0.05	0.04	0.06
N	44 391	43 844	38 480	51 847

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 residuals within plant. All estimates adjust for covariates described in text.