

A Time-Sensitive Analysis of the Work-Crime Relationship for Young People

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A.M. in Sociology, May 2018
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Abstract

While entrance into the labor market and desistance from crime both typically occur during the transition to adulthood, it is unclear whether employment causes reductions in crime for young people. Employment may reduce crime by offering routines, income, and supervision. However, selection may also occur: people may start working when they are already making positive changes in their lives and stop working when they are already making harmful decisions. To evaluate these possibilities, I model month-to-month, within-person changes in offending during the periods surrounding job transitions. Using data from Pathways to Desistance, a longitudinal study of young offenders, I find large reductions in income-related offending prior to job entry, but no further reductions after job entry. I also find that offending spikes before job exit. These patterns suggest that job transitions do not instigate changes in offending but rather occur in response to other changes in young people's lives.

Employment is often touted as a pathway out of crime. Jobs are thought to reduce crime by providing routines, supervision, and reduced economic incentives for criminal activity (Becker 1968; Osgood et al. 1996; Sampson and Laub 1993). Unemployment, on the other hand, may lead to crime by creating negative emotions and blocking opportunities for legal income (Agnew 1992; Merton 1968). However, while many studies document a negative association between employment and crime (for a review, see Uggen and Wakefield 2008), randomized control trials that evaluate whether being given a job reduces crime have yielded mixed results (e.g. Farabee et al. 2014; Uggen 2000; Visser et al. 2005).

It is particularly unclear whether employment reduces crime for young people. The prevalence of crime peaks in late adolescence and declines in the early 20s (Farrington 1986). This is the same period when people typically enter the labor market (Arnett 2004). It is thus possible that employment is associated with desistance simply because the two processes occur around the same developmental period. While some studies have found that young people offend less when employed (Hill et al. 2016; Piquero et al. 2002), a prominent randomized control trial suggests that employment does not lead to desistance for those under age 27 (Uggen 2000).

An underexplored explanation for the work-crime association is that people select into jobs when they are already making positive changes in their lives. As young people mature, they may undergo changes in identity that prompt them both to desist from crime and to look for work (Giordano et al. 2002; Paternoster and Bushway 2009). Desistance from crime may also make it easier to find a job. Similarly, people may select out of jobs when their lives are already changing for the worse. For highly changeable young people, negative developments, such as substance use or loss of motivation, may drive both increased offending and job exit.

This paper examines the timing of changes in offending relative to job transitions to evaluate two explanations for the work-crime association. The first explanation is that

employment brings benefits that lead to desistance, while unemployment creates negative circumstances that lead to crime. If this is true, reductions in offending should occur after people begin jobs, and increases in offending should occur after people stop working. The second explanation is that people select into work when they are already offending less and select out of work when they are already offending more. If this is true, reductions in offending should occur before people start working, while increases in offending should occur before job exit.

To model these possibilities, I use monthly data from Pathways to Desistance, a longitudinal study that follows justice-involved young people (primarily ages 16 to 24) in the United States from 2000 to 2010. I find that income-related offending decreases drastically in the months leading up to job entry. After job entry, there is no further decrease in offending, not even in jobs with characteristics thought to reduce crime. Offending spikes before job exit. These patterns suggest that job transitions do not instigate changes in offending but rather occur in response to changes in offending. This finding challenges the dominant narrative that being employed leads to reductions in crime, instead suggesting that young people select into and out of work based on other circumstances in their lives.

Background

The Effects of Employment and Unemployment on Crime

Dominant theories of the work-crime relationship suggest several mechanisms through which being employed may lead to desistance. First, rational choice theory suggests that the legal income from jobs disincentivizes income-generating crimes (Becker 1968; Ehrlich 1973). Employment could also deter crime because employed people have more to lose if arrested. Second, routine activity theory suggests that jobs reduce opportunities to offend by restructuring people's routines, leaving them less time to spend with deviant peers (Osgood et al., 1996).

Third, social control theory suggests that jobs lead to desistance by providing the opportunity to develop social bonds with conventional people in the workplace, who may offer supervision and positive influence (Sampson and Laub 1993). The informal social control that jobs provide strengthens with time, as people grow more attached to their jobs.

Unemployment, on the other hand, may lead to increases in crime. When people stop working, they lose the positive routines and the supervision that jobs bring. Furthermore, strain theory suggests that unemployment produces negative emotions, such as anger and frustration, which may be expressed in criminal activity (Agnew 1992). People who are unemployed may feel frustrated about the lack of legitimate opportunities to earn money, prompting them to turn to illegal income-generating activity (Merton 1968). While there is evidence of an association between unemployment and all types of crime (Fergusson et al. 1997), unemployment is more strongly associated with income-related crimes (Aaltonen et al. 2013).

Selection into and out of Jobs

While dominant theories of the work-crime relationship explain how jobs can help people desist from crime, these theories do not explain how active offenders end up in jobs. It is possible that the process of desistance begins prior to job entry and enables people to find work. There are several ways this can occur. First, people may experience changes in identity that prompt them both to seek employment opportunities and to desist from crime (Paternoster and Bushway 2009). The symbolic interactionist framework suggests that people who conceive of “the self” as delinquent will be more likely to engage in delinquent behavior (Heimer and Matsueda 1994). To desist, people must become open to change and create a new “replacement self” that is conventional (Giordano et al. 2002). For adolescents and young adults, who are at an age of identity formation, it is particularly plausible that offending and employment are both

driven by the maturation process (Massoglia and Uggen 2010). Second, along with identity change, people's desire for legal work may change. They may become more motivated to work, increasing the likelihood of finding work and leading to desistance even before they secure a job. This represents the effect of the existence of jobs on offending, which is different from the conventional view that qualities of specific jobs lead to desistance. Third, by offending less, people may be more likely to find work. People who stop offending have more time to search for jobs and may attract more help in the job search from friends and family.

The association between unemployment and crime could also be explained by selection: people may leave jobs when they are already making negative changes in their lives. This could happen in several ways. First, a confounding factor may cause both increased offending and job exit. This confounding factor could be an external issue like substance use or an internal change, like mental health problems or a change in motivation. Issues of crime, unemployment, mental health, and substance use are often interconnected for young people (Fergusson et al. 2001). Second, rational choice theory suggests that people choose legal employment over illegal activity if the returns to legal employment are large enough (Ehrlich 1973). After spending some time employed, people may decide that the returns to legal work are too low. This is particularly likely if they are in low-quality jobs that offer irregular hours and few prospects advancement. Dissatisfied with work, people may both resume offending and leave their jobs. Lastly, crime can cause job exit directly if people are incarcerated or fired because of their crimes. In these ways, criminal activity may precede and cause job exit.

The Timing of Changes in Offending

There are thus two potential explanations for the work-crime relationship. The first explanation is that being employed and being unemployed cause people to change their

offending behavior. The second explanation is that people select into work when they are already making positive changes and select out of work when they are already making harmful decisions. Most studies of work and crime only look for evidence of the first explanation. This study examines whether there is empirical evidence for each of these two possibilities by examining in detail the timing of changes in offending relative to job transitions.

The first explanation suggests that employment causes reductions in crime by providing income, routines, and supervision, qualities that kick in only after the job begins. If these benefits of employment reduce crime, then we should observe decreases in offending *after* people start working. Similarly, the negative qualities of unemployment, such as lack of income, kick in after the job ends, so people should start offending more *after* they stop working. On the other hand, if people select into jobs, then we should observe decreases in offending *before* people start working, reflecting the changes people make as they get their act together and start looking for work. Similarly, if people select out of jobs, they should start offending more *before* the job ends. Increases in offending may lead to or accelerate job exit.

It is possible that both perspectives are true. Internal changes may jumpstart the process of desistance, while the structures and routines of work reinforce the internal changes and lead to further reductions in crime (Giordano et al. 2002). If this is the case, reductions in offending should be observed both before and after job entry. A related possibility is that people desist from crime before starting work, and jobs maintain lower levels of offending. If this is true, then offending should remain low for as long as people remain employed, increasing after job exit.

One prior study has modeled the timing of changes in offending surrounding job entry. Using a Norwegian sample, Skardhamar and Savolainen (2014) find that reductions in crime occur before job entry, after which there is no further decrease. Since this study defines criminal activity as felonies recorded in administrative data, it is possible that prior to employment,

people commit offenses that are less serious or undetected by the criminal justice system. I build on this study by using broader measures of offending for an American sample of young people and by modeling the periods surrounding both job entry and exit. By modeling job exit, I can test whether there is selection out of work and whether employment suppresses crime for as long as people remain employed.

Variation in the Work-Crime Relationship by Job Quality and Age

As explained above, employment is thought to reduce crime because it produces certain benefits. It is possible that only certain jobs produce enough benefits to reduce crime. Rational choice theory suggests that legal income deters illegal income-generating activity (Becker 1968; Ehrlich 1973). High-paying jobs may thus provide more incentives to desist than low-paying jobs. Routine activity theory implies that full-time jobs should reduce crime more than part-time jobs, since full-time jobs leave less time to spend on illegal activity (Osgood et al. 1996). Jobs with regular hours, which help establish routines, may also better deter crime than jobs with sporadic hours. Social control theory suggests that structure and supervision are important for desistance (Sampson and Laub 1993), so formal employment may reduce offending more than informal jobs. Social control theory also suggests that desistance occurs over time as people grow attached to their jobs, so jobs may have to last some time before they reduce offending. Past studies demonstrate that subjective measures of job satisfaction are associated with desistance (Uggen 1999; Wadworth 2006), but the evidence regarding the more objective characteristics of work—income, hours, occupation—is unclear.

Besides job quality, developmental factors may also lead to variation in the relationship between employment and crime. For adolescents, for whom employment is not a normative expectation, work may be harmful. Jobs may compete with school for adolescents' attention,

increase their autonomy by providing income, and increase their exposure to delinquent peers (Staff and Uggen 2003; Steinberg et al. 1993). As for young adults, since they are expected to adopt adult responsibilities, they should benefit from the income, routines, and relationships that jobs offer. However, young adults tend to view work as temporary sources of income rather than as long-term career paths (Arnett 2004), so they may not develop strong attachment to their jobs. It is thus unclear whether adolescents and young adults reduce offending when they start work.

Data and Methods

I use data from Pathways to Desistance, one of the largest longitudinal studies of serious offenders. It follows 1,354 people for seven years, beginning in 2000 to 2003. Respondents come from Phoenix (N = 654) and Philadelphia (N = 700), forming a racially diverse sample that is 19.2% white, 42.1% black, 34.0% Hispanic, and 4.6% another race. Prior to entering the sample, all respondents had been found guilty of a felony or serious misdemeanor that was committed between ages 14 and 17. The survey period spans ages 14 to 25, although most respondents are ages 16 to 24 . Because Pathways to Desistance only tracks young, justice-involved people, the results should not be generalized to the population at large or to older people.

This survey interviewed respondents every six months for three years and then annually for four years. At each follow-up interview, respondents filled in life history calendars to capture monthly data on key measures, including offending and employment. To improve recall, respondents first recounted salient events, like birthdays or deaths, and used these as anchors to remember the timing of other events. Compared to other data sources, life history calendars produce high-quality and accurate data (Roberts and Mulvey 2009).

I use data from 1,170 male respondents, since patterns of desistance differ by gender (Giordano et al. 2002), and there are too few female respondents to estimate female-specific

models. Of 98,640 person-months, 12.2% are missing data on employment or offending. I do not impute employment status, since I need to know with certainty the timing of jobs. I also do not impute offending, the outcome of interest. Non-random missing data, a concern for many studies of hard-to-reach populations, could lead to sample selection bias. To minimize further missing data, I use multiple imputation for missing data on the controls. I further restrict the sample to the 56,119 person-months (64.8% of non-missing observations) spent in the community. I exclude person-months spent confined (e.g. in jail or prison) because confinement severely restricts opportunities both to work and offend.

Measures

The dependent variable is offending, measured with a dummy variable indicating that the respondent self-reports any of 22 offenses each month.¹ I differentiate between income-related and violent offenses, coded as subsets of the 22 offenses, because they may have different relationships with employment. Income-related offenses may be a substitute for legal income. Income-related offenses also tend to require more time and planning to carry out, unlike violent offenses that tend to occur in the spur of the moment (Gould 2003). The most common income-related offenses are selling marijuana; selling drugs; and buying, receiving, or selling stolen property. The most common violent offenses are being in a fight, destroying or damaging property, and beating someone up (resulting in serious injury). I use binary measures of offending because 75.4% of person-months include no offending, and only 7.7% of person-

¹ The offenses are: destroyed or damaged property; set a fire; entered a building to steal; shoplifted; bought, sold, or received stolen property; used credit cards illegally; stole a car; sold marijuana; sold other illegal drugs; carjacked someone; drove drunk or high; been paid for sex; forced sex; killed someone; shot someone; shot at someone; robbed someone with a weapon; robbed someone without a weapon; beat someone so badly that they needed a doctor; been in a fight; beat someone as part of a gang; and carried a gun. Robbery with and without a weapon are counted as both income-related and violent offenses.

months include more than one type of offense. Robustness checks that use negative binomial regression to model the number of different types of offenses committed each month yield substantively similar results as the main models, which use logistic regression.

The use of self-reported offending is advantageous because it provides information on offenses that are not detected by the criminal justice system. Administrative records, the other commonly used source of crime data, only include crimes that police observe and report. Since police observe a fraction of crimes and have discretion about whether to make arrests, police and arrest records undercount crimes (Black and Reiss 1970). If offenses go undetected, it is hard to tell whether someone has desisted from crime or simply has not been caught for a while.

Self-reported offending, however, relies on people's memory. People may remember recent offenses while forgetting ones from months ago. I verify, however, that the level of reported offending does not systematically vary with how far back respondents are recalling. Another concern is that people may hesitate to admit to offending. To address this concern, respondents were asked to record their answers on a keypad, maximizing confidentiality.

The main independent variable is constructed based on the number of months spent so far in a job spell, explained in more detail below. Job spells are defined as periods of continuous employment in legal work. Employment is considered non-continuous if respondents stop working for two or more weeks. It is possible for job spells to consist of more than one job: respondents may hold multiple jobs at a time or start a new job soon after the old one ends. Almost 90% of job spells, however, only involve one job.

One shortcoming of this dataset is that it does not distinguish between formal and informal employment until the seventh wave, so job spells may indicate any type of legal work. As a robustness check, I consider only the subset of jobs that are almost certainly formal employment: jobs in retail or as a cashier; counter help, fast food, and restaurant workers; skilled

labor; office work, clerical jobs, and telemarketing; managerial and administrative jobs, and technical and professional jobs. These models exclude jobs that may or may not be formal: babysitting, child care, caretaker, and camp counselor; manual and unskilled labor (e.g. grass cutting); and jobs categorized as other.

In all models, I control for 16 time-varying measures that may be associated both with the opportunity or motivation to offend and with the likelihood of employment. I lag the controls to prevent them from being the causal pathway from jobs to offending. Results with concurrent controls and without controls are similar to the main results. I control for school enrollment, romantic relationship status, living arrangements, contact with a probation officer, and arrest, all measured monthly. The other controls are measured at each survey wave: parenthood status, the respondents' number of close friends, the number of caring adults in their lives, the proportion of their friends who have been arrested, a measure of antisocial influence from peers, gang membership, alcohol dependence symptoms, drug dependence symptoms, symptoms of mental illness, and a measure of psychosocial maturity. I also control for age and age-squared; the results are robust to linear and dummy variable specifications of age.

Modeling Strategy

The Association between Employment and Offending

As a point of comparison, I first estimate the association between employment and offending without reference to the timing of job spells. I use fixed-effects models, which net out stable, unobserved characteristics of individuals. This model is represented by Equation 1, a logit model where Y_{it} is a binary measure of offending, X_{jit} are time-varying control variables, E_{it} is a dummy variable for whether respondents are employed in a given month, and α_i is the individual-specific intercept. The subscripts i , t , and j index respondents, months, and control

variables, respectively. This model cannot confirm or disconfirm the two perspectives presented above: if I find that on average people commit less crime when employed, it could be that starting a job led them to desist or that they were offending less before they started working. Even when employment is lagged and used to predict offending in the following month, this model still leaves unclear the timing of changes in offending relative to job transitions.

$$\text{logit}(Y_{it}) = \sum_j \beta_j X_{jit} + \delta E_{it} + \alpha_i \quad (1)$$

Models of Job Transitions

Next, I model the timing of changes in offending surrounding job entry and job exit. In these fixed-effects models, instead of a dummy variable for employment status, the main covariate is a group of dummy variables, one for each month surrounding a job transition. This allows me to track within-person changes in offending before and after job transitions.

Models of job entry are represented by Equation 2, where S_{it}^k is a set of dummy variables, with k as the number of months since the start of the job, if positive, and the number of months before the job begins, if negative. I model the period from six months before job entry to the sixth month of the job spell, since sample sizes shrink farther away from the job transition. The θ 's represent the odds of offending in each of these months compared to the reference period, which is seven to nine months before job entry. Respondents may experience more than one job transition during the survey period. In these cases, the model uses, for example, information from the first month of each of the respondent's job spells to calculate the log odds of offending during the first month of job spells for that respondent.

$$\text{logit}(Y_{it}) = \sum_j \beta_j X_{jit} + \sum_{k=-6}^6 \theta_k \cdot S_{it}^k + \alpha_i \quad (2)$$

Similarly, Equation 3 models job exit, where N_{it}^k is a set of dummy variables, with k as the number of months before or after job exit. I model the period from the third from last month of the job to six months after the job ends. I model three rather than six months before job exit because many jobs are short-lived, and I reserve the fourth from last to sixth from last months of the job to serve as the reference period. The γ 's represent the odds of offending in each month compared to the reference period.

$$\text{logit}(Y_{it}) = \sum_j \beta_j X_{jit} + \sum_{k=-3}^6 \gamma_k \cdot N_{it}^k + \alpha_i \quad (3)$$

Not every job spell contributes to every month in the periods I model. Many job spells end before six months, and some job spells do not have a full six months of non-employment before or after the job transition. I censor spells when the employment status does not match the one in the model. For example, if a job spell lasts four months, it will contribute to estimates of the first four months of the job but not the fifth or sixth.

While this strategy allows me to model job spells regardless of length, there is potential for bias. Short spells may involve more offending than longer ones because people may exit jobs quickly when they are less stable. A two-month job spell may thus inflate estimates of offending for the first and last two months in the job. As a robustness check, I deal with short spells in a different way. I include observations from the entire period regardless of employment status. For example, when modeling job entry, if a job spell lasts four months, I include the subsequent two months in estimates of the fifth and sixth months after job entry, even though the respondent is not working. Dealing with short spells this way yields similar results.

Since the models of job entry and job exit explore the transition between employment and non-employment, these models only include job spells that involve at least one month of non-employment before the job (for models of job entry) or after the job (for models of job exit).

Models of job entry thus exclude job spells that begin shortly after incarceration or after another job ends. Models of job exit exclude job spells that transition to incarceration or to another job without a full month in between. The next set of models addresses this limitation.

Variation in the Work-Crime Relationship

The next set of models focuses on different types of jobs to test for variation in the work-crime relationship. These models are identical to the previous set of models except that they only model the period during the job spell (not before or after) and thus include all job spells regardless of what they transitioned from or to. I model offending during the first six months of the job, using the first month as the reference period and including dummies for each subsequent month. I also model offending during the last six months of the job, using the last month as the reference period and including dummies for each of the earlier months of the job.

I consider the following subsets of jobs: formal employment, jobs that last at least six months, full-time jobs (35 or more hours per week), part-time jobs, jobs with regular hours, higher paying jobs, and lower paying jobs. Jobs are defined as higher paying if they pay more than the sample average for the respondent's age, adjusting for inflation. Also, testing developmental considerations, I model changes in offending for adolescents and young adults separately and for jobs that do not occur concurrently with school. I model the first and last six months of the job spell for all of these subsets to see if certain jobs are better able to reduce offending after job entry and better able to maintain low levels of offending before job exit.

Additionally, in models of the last six months of the job, I test whether offending directly causes job exit through incarceration or being fired. I restrict the sample to jobs that do not end with incarceration and then restrict the sample to jobs that end in quitting. If offending still increases prior to job exit, then factors other than incarceration and firing drive this increase.

Sample Size and Missing Data

The sample consists of 5,937 job spells. In models of job entry, 2.8% of job spells are left-censored due to the start of the survey (the job spell began before the survey), and 1.6% are left-censored due to missing data. Job spells are excluded from models of the period surrounding job entry if there is not at least a full month of non-employment before the job spell begins: this excludes the 9.7% of job spells that begin soon after incarceration and the 28.0% of job spells that begin soon after another job spell ends. Jobs that begin after incarceration or another job are included, however, in models of the first six months of the job.

As for job exit, 8.5% of job spells are right-censored due to the end of the survey (the survey ended before the job spell), and 3.7% are right-censored due to missing data. Right-censoring due to the end of the survey does not automatically introduce bias because the end of the survey is not correlated with the respondent's traits. In models of the period surrounding job exit, job spells without a full month of non-employment after the job ends are excluded: this excludes the 8.3% of job spells that transition to incarceration and the 28.0% of job spells that transition to another job. These spells are included in models of the last six months of the job.

All models use person fixed-effects, which drop respondents who lack variation on the dependent variable. Most of the dropped respondents do not offend during the period I model and are thus not at risk of desistance. Since some respondents only commit one type of offense, models of income-related and violent offending drop additional respondents due to lack of variation on the dependent variable. With these model restrictions, the analytic sample for models of job entry and exit consists of respondents who both work and offend. The results are thus most generalizable to justice-involved young people who are at some point engaged in employment and crime during the transition to adulthood.

Results

Sample Description

Table 1 displays descriptive statistics that provide evidence of an inverse relationship between employment and offending. Respondents commit income-related offenses in 10% of person-months spent employed and 18% of person-months not employed. They commit violent offenses in 8% of person-months spent employed and 12% of person-months not employed.

However, jobs in this sample may not possess the qualities that help respondents desist from crime. Most jobs are low-wage, with a median wage of \$12 per hour in 2018 dollars. The most common types of jobs are manual labor, skilled labor, and restaurant work. The jobs also tend to be short-lived. Half of job spells that begin when respondents are aged 16 to 18 end before three months. Most respondents in this age group are still in school and may not have long-term jobs. However, half of job spells end before four months for those aged 19 to 21, and half end before five months for those aged 22 to 24. Even for the oldest respondents, only 43% of job spells last six months and 26% of job spells last one year. Such shockingly short job spells may make it difficult for work to lead to desistance from crime.

Models of the Work-Crime Relationship

Figures 1 through 5 display results from the models described above. In all figures, I translate the coefficients from logistic regression into average marginal effects for interpretability. I use coefficients from the logistic models for hypothesis testing, with the results from these tests described in the text.

The Association between Employment and Offending

I first use fixed-effects models to estimate the association between employment and offending. I use logistic models with a dummy variable for employment status as the main covariate. Figure 1 displays the average marginal effect of being employed, compared to non-employment, for six models: with and without controls for each of the three dependent variables. For all types of offending, respondents are significantly less likely to offend when employed. When respondents are employed, their probability of any offending is 4.4 percentage points lower, their probability of income-related offending is 11.6 percentage points lower, and their probability of violent offending is 4.7 percentage points lower, controlling for observed confounders ($p < 0.001$).

Models of Job Transitions

To examine time trends, I turn to models of how offending changes in the period surrounding job entry. Figure 2 presents results from fixed-effects logit models that include dummy variables for each of the six months before and after job entry, with the seven to nine months before job entry as the reference period. The top panel of Figure 2 shows that respondents are significantly less likely to offending during the four months before job entry, compared to the reference period. After starting the job, there is no evidence of a further decrease in offending: compared to the month before job entry, the log odds of offending in most of the first six months of the job are not significantly different. Thus, while offending decreases before job entry, there is no evidence of further change after the job starts.

Two different patterns emerge when income-related and violent offending are considered separately. As the middle panel of Figure 2 shows, respondents are significantly less likely to commit income-related offenses in the four months before job entry, compared to the reference period. By the month before job entry, the probability of offending is 12.1 percentage points

lower than in the reference period ($p < 0.001$). After job entry, the log odds of income-related offending are either significantly higher than the month before job entry (in months 2 and 3) or not significantly different. Thus, after a steep decrease in income-related offending before job entry, there is little evidence of further change after job entry. On the other hand, the bottom panel of Figure 2 shows that there is little change in violent offending in the year surrounding job entry. Compared to the reference period, the log odds of violent offending are not significantly different in any of the months that surround job entry.

Overall, the models of job entry indicate no evidence that becoming employed leads to a reduction in offending, since neither income-related nor violent offending decrease after job entry. The decrease in income-related offending prior to job entry suggests that people are already making positive changes in their lives before starting a job. These patterns hold in models run separately for adolescents and young adults.

Next, I consider the period surrounding job exit. Figure 3 presents results from fixed-effects logit models that include dummy variables for each of the last three months in the job and the six months after job exit. The reference period is the fourth from last through sixth from last months in the job. As Figure 3 shows, all types of offending increase significantly in the two months before job exit. Compared to the reference period, the probability of income-related offending is 8.0 percentage points higher and the probability of violent offending is 10.3 percentage points higher during the last month of the job ($p < 0.01$).

After job exit, the log odds of income-related offending continue to increase. Compared to the last month of the job, the log odds of income-related offending are significantly higher in three out of the six months after job exit ($p < 0.05$). However, income-related offending appears to increase more quickly before job exit than after. In the three months before job exit, the predicted probability of income-related offending increases by 8.0 percentage points. In the six

months after job exit, the probability of income-related offending increases another 6.2 percentage points—a smaller increase in twice the amount of time. Violent offending, on the other hand, stays relatively constant after job exit, with none of the six months after job exit significantly different than the last month of the job.

Overall, the models of job exit indicate steep increases in offending in the last couple months of the job spell. After job exit, income-related offending continues to increase, but at a slower rate, while violent offending shows no evidence of a further increase.

Variation in the Work-Crime Relationship

Next, I turn to models of the first and last six months of job spells to examine variation in the work-crime relationship. Models of the first six months of job spells test whether jobs with certain characteristics can reduce offending, while models of the last six months test whether jobs can maintain persistently lower levels of offending before job exit. The models use logistic regression with fixed-effects. The reference period for models of job entry is the first month of the job, while the reference period for models of job exit is the last month. The dependent variable for models displayed in Figures 4 and 5 is any offending, but the results for income-related and violent offending are very similar.

The first purpose of these models is to test the robustness of the previous results by including in the models all job spells, not just ones that transition from or to at least one month of non-employment. As panel a of Figures 4 shows, relative to the first month of the job, there is no decrease in offending during months two through six. Panel a if Figure 5 shows that relative to the last month of the job, offending is significantly lower during the three to six months prior to job exit, affirming that offending increases prior to job exit.

Next, I turn to models that restrict the sample to jobs with certain characteristics. Panel b of Figures 4 and 5 restrict the sample to jobs that last at least six months. Panels c and d display results for full-time and part-time jobs, expecting that full-time jobs more effectively deter crime. Panels e and f display jobs that pay more and less than average, expecting that higher paying jobs more effectively deter crime. I also restrict the sample to only formal employment and to jobs with regular hours (now shown). In all of these models, I find that offending does not decrease during the first six months of job spells and significantly increases during the last six months.

I then turn to models that test developmental variation in the work-crime relationship. Panels g and h in Figures 4 and 5 show results for adolescents and young adults separately. I also restrict the sample to jobs during which respondents are not enrolled in school, since working while in school may be harmful for adolescents. These models all echo the main finding that offending does not decrease after job entry and increases prior to job exit.

Lastly, I examine the possibility that the increase in offending prior to job exit is driven by job spells that end in incarceration or firing. I model the last six months of job spells that do not end in incarceration and also model job spells that end with quitting rather than firing. The results (not shown) reveal that even when job spells that end in incarceration and firing are excluded, offending still significantly increases prior to job exit.

Discussion

This study models temporal patterns of offending to evaluate two perspectives: the dominant perspective that being employed causes reductions in offending and the alternative perspective that criminally involved people select into and out of jobs when they have already made changes in identity, motivation to work, or behavior. This study also examines variation in patterns of offending by job characteristics and developmental factors.

I find no evidence that being employed leads to reductions in offending. There is no reduction in offending during the first six months of job spells. There is, however, an increase in offending prior to job exit. These patterns suggest that not only are jobs unable to reduce offending, but are also unable to maintain persistently low levels of offending. These patterns hold true regardless of job characteristic and age group.

Furthermore, the results provide evidence against some of the mechanisms through which being employed is thought to reduce crime. Rational choice theory suggests that the legal income from employment deters illegal income-generating activity (Becker 1968; Ehrlich 1973). However, I find that income-related offending decreases before but not after respondents start earning money. I also find little difference in income-related offending between higher paying and lower paying jobs. Routine activity theory suggests that people who spend more time employed will have fewer opportunities to offend (Osgood et al. 1996). However, patterns of offending are similar between full-time and part-time jobs. Furthermore, even in jobs with regular hours, which should help instill routines, offending does not decrease after job entry and increases prior to job exit. Lastly, social control theory suggests that supervision and structure are important for desistance (Sampson and Laub 1993). However, formal employment, which offers more supervision than informal jobs, does not appear to reduce offending. Social control theory also suggests that desistance should gradually, as job attachment grows. However, when I restrict the sample to jobs that last at least six months, I still find no evidence of reduced offending. I cannot rule out the possibility that jobs reduce crime in the long-term. In this sample, however, few job spells last more than six months.

The results support the perspective that people select into employment when they are already making positive changes in their lives. During the six months before job entry, income-related offending declines significantly, while violent offending remains stable. This suggests

that before job entry, young people act just as impulsively but begin to scale down the types of offenses that require time and planning. They may instead spend time in conventional or job-seeking activities. They may experience identity changes that make desistance and employment both desirable (Giordano et al. 2002; Paternoster and Bushway 2009). Since people start to offend less four months before job entry, it is unlikely that changes in offending are driven by the promise of a specific job. Rather, it is possible that people become motivated to find work, changing their behavior in hopes of finding a job. In sum, changes in identity, motivation to work, and behavior may drive both job entry and desistance.

Models of job exit suggest that people select out of jobs when they are already offending more. All types of offending increase sharply before job exit. I rule out the possibility that this increase is driven entirely by jobs that end in incarceration or firing, since models that exclude incarceration and firings still show increased offending prior to job exit. The increase is also not entirely driven by low-quality jobs that push discontent workers to offend, since offending increases prior to job exit regardless of job characteristics. It is thus likely that confounding factors—loss of motivation to work, changes in identity, or destructive behavior like substance use—prompt people both to resume offending and to leave their jobs. Young people are particularly changeable, and once they begin to offend more, they may subsequently leave their jobs because offending interferes with job responsibilities.

After job exit, income-related offending continues to increase but at a slower rate than before job exit, while violent offending does not increase. It thus seems unlikely that the frustration and the blocked opportunities of unemployment are the main drivers of the association between unemployment and crime, as commonly thought (Agnew 1992; Merton 1968). Rather, for young people, the association between unemployment and crime appears to be driven by changes that occur prior to job exit.

Limitations

There are several limitations regarding the sample I use. First, respondents enter the sample after committing a serious offense. Thus, respondents are selected based on a trait that is correlated with the outcome, which may lead to sample selection bias. Second, some respondents have already begun working before the start of the survey, leading to left-censoring of job spells, although this is minimal. Third, the sample comes from two locations, limiting its generalizability. When studying hard-to-reach populations, these limitations are common.

Another limitation is the lack of certain measures. The survey includes no monthly measures of changes in identity, motivation to work, and lifestyle. I thus cannot determine on what basis people select into and out of jobs. The survey also lacks subjective measures of job quality. While I show that offending does not vary by characteristics like income or hours, it remains possible that subjectively better jobs will lead to reductions in offending or at least maintain persistently lower levels of offending.

Conclusions

For young people, this study offers evidence against the dominant belief that employment leads to desistance. It is true that on average, people offend less when employed. However, time-sensitive models reveal a different story. Month-to-month, within-person patterns show that there are no reductions in offending during the first six months of job spells, which is longer than most jobs last for young offenders. Rather, job transitions occur after large changes in offending, suggesting that job transitions are a consequence of other changes in young people's lives. The results suggest that rather than simply offer jobs to young offenders, it is important to help them make the internal changes that prepare them to work. Young people today need holistic support as they navigate the instability both of the labor market and of their own behavior.

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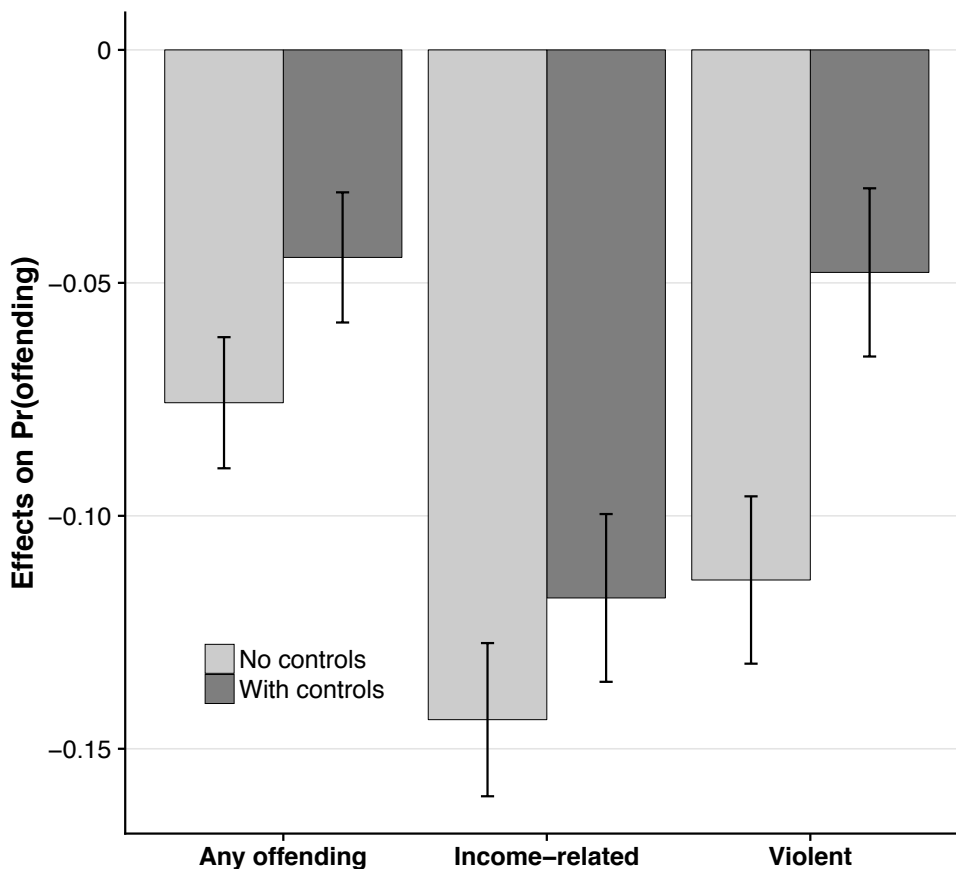
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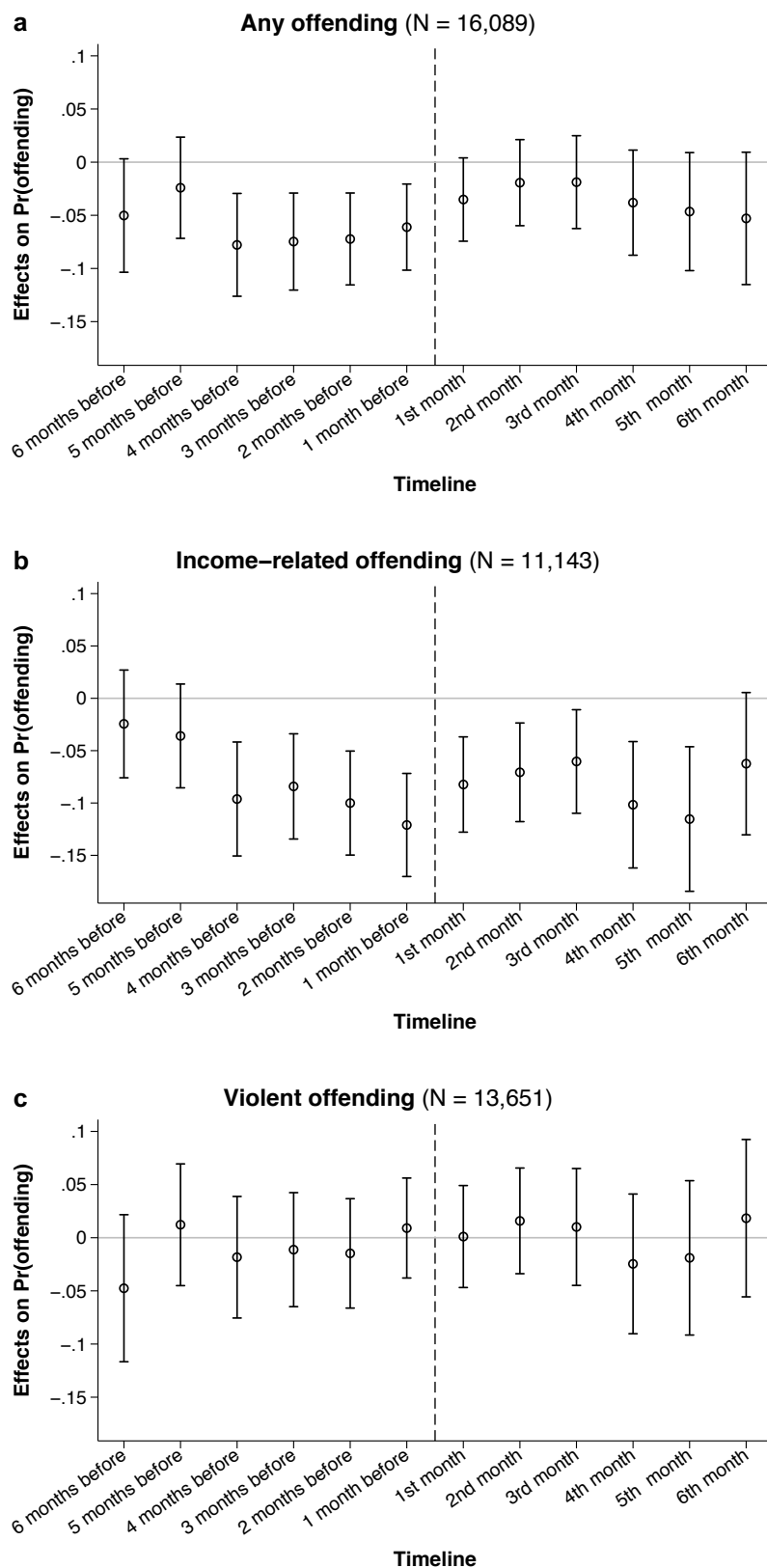
Table 1. Descriptive Statistics by Employment Status

Variable	Employed Mean (SD)	Not employed Mean (SD)
Dependent Variables		
Any offense	0.22	0.27
Income-related offense	0.10	0.18
Violent offense	0.08	0.12
Control Variables		
School enrollment		
Enrolled	0.15	0.26
Not enrolled	0.81	0.69
School not in session	0.04	0.05
Romantic relationship status		
No relationship	0.38	0.48
Steady relationship	0.48	0.40
Steady relationship and seeing others	0.04	0.05
Several relationships	0.03	0.03
Engaged	0.04	0.02
Married	0.04	0.02
Parenthood status		
No children	0.63	0.67
Has non-resident child(ren)	0.14	0.18
Lives with child(ren)	0.24	0.16
Living arrangement		
Living with family	0.54	0.68
Living in own place	0.31	0.13
Living with relatives	0.09	0.11
Living with friends	0.06	0.06
Homeless	0.00	0.01
Number of close friends	2.34 (1.82)	2.28 (2.05)
Number of caring adults	1.70 (1.18)	1.67 (1.22)
Proportion of four closest friends arrested during recall period	0.35 (0.39)	0.37 (0.40)
Peer antisocial influence	1.47 (0.62)	1.51 (0.73)
Belongs to a gang	0.05	0.08
Contact with probation officer	0.23	0.30
Self-reported arrest	0.03	0.05
Number of alcohol dependence symptoms	0.33 (1.13)	0.30 (1.05)
Number of drug dependence symptoms	0.39 (1.33)	0.54 (1.57)
Brief Symptom Inventory (mental health)	0.35 (0.43)	0.39 (0.48)
Psychosocial maturity	0.18 (0.62)	0.01 (0.60)
Age (ranges from 14 to 25)	20.1 (2.12)	19.0 (2.29)
N (person-months)	27,774	28,345

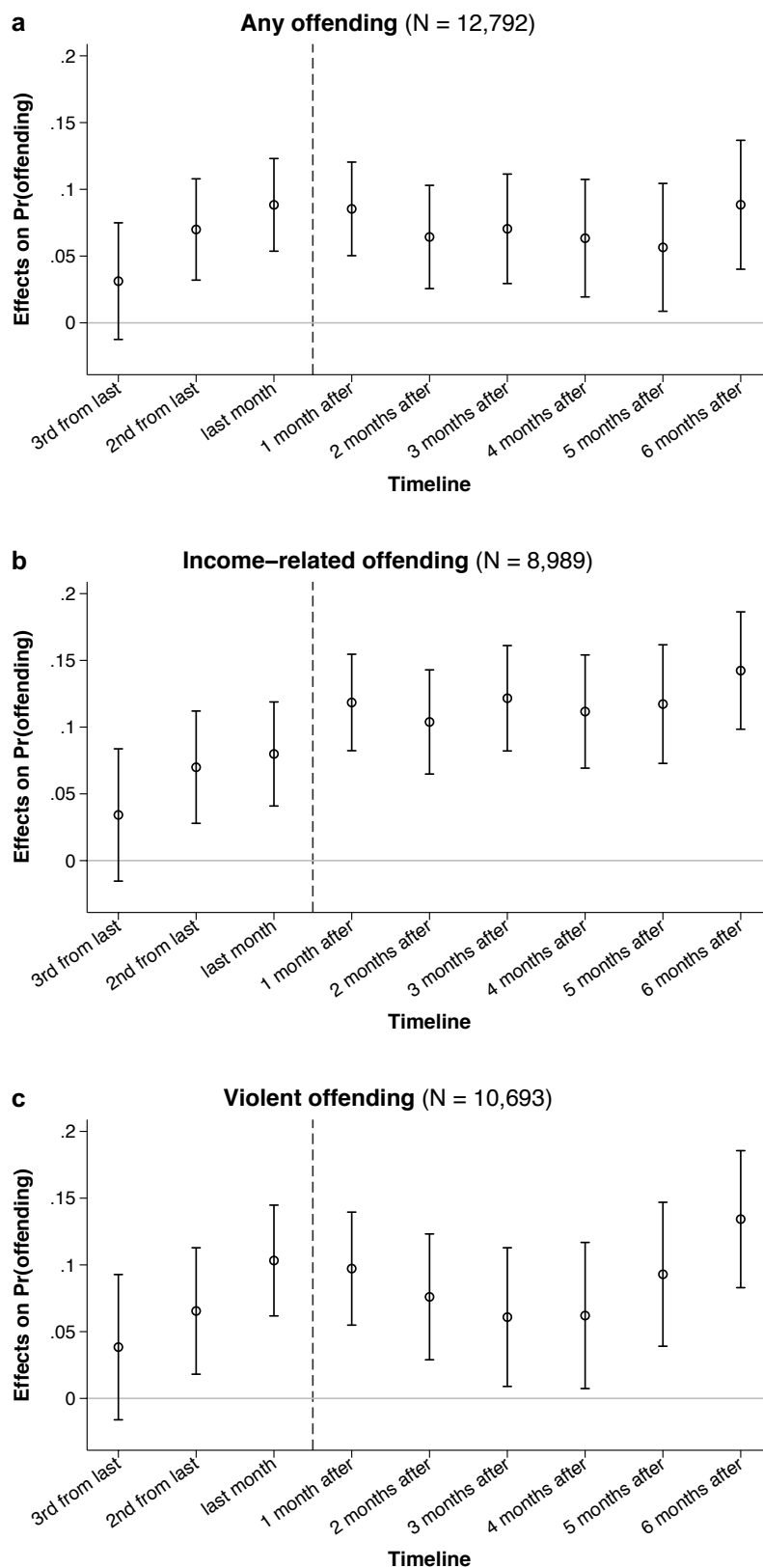
Note: School enrollment, romantic relationship status, living situation, contact with a probation officer, arrest, and age are measured monthly. The rest are measured by the survey wave (every six months for the first six waves and every twelve months for the next four waves). Descriptive statistics for the control variables are based on non-imputed values. Standard deviations are not presented for discrete variables.

Figure 1. Predicted probability of offending when employed, relative to non-employment

Note: The figure displays the average marginal effects calculated from logistic regression models with person fixed-effects. The bars show the difference in the probability of offending between employment and non-employment. Negative values indicate the people offend less when they are employed. There are two models, with and without controls, for each of three dependent variables: any offending, income-related offending, and violent offending. The models with controls include 16 time-varying confounders: school enrollment, romantic relationship status, parenthood status, number of close friends, number of caring adults, living arrangement, proportion of close friends arrested, peer antisocial influence, belonging in a gang, contact with a probation officer, arrests, number of alcohol dependence symptoms, number of drug dependence symptoms, Brief Symptom Inventory (measuring mental health), psychosocial maturity, age, and age-squared. Error bars are 95% confidence intervals. The sample is restricted to person-months in which respondents are not institutionalized (e.g. not in prison/jail). Fixed-effects models drop all people without variation on the dependent variable, resulting in sample sizes of 48,096 person-months in models of any offending, 37,236 person-months in models of income-related offending, and 43,334 person-months in models of violent offending.

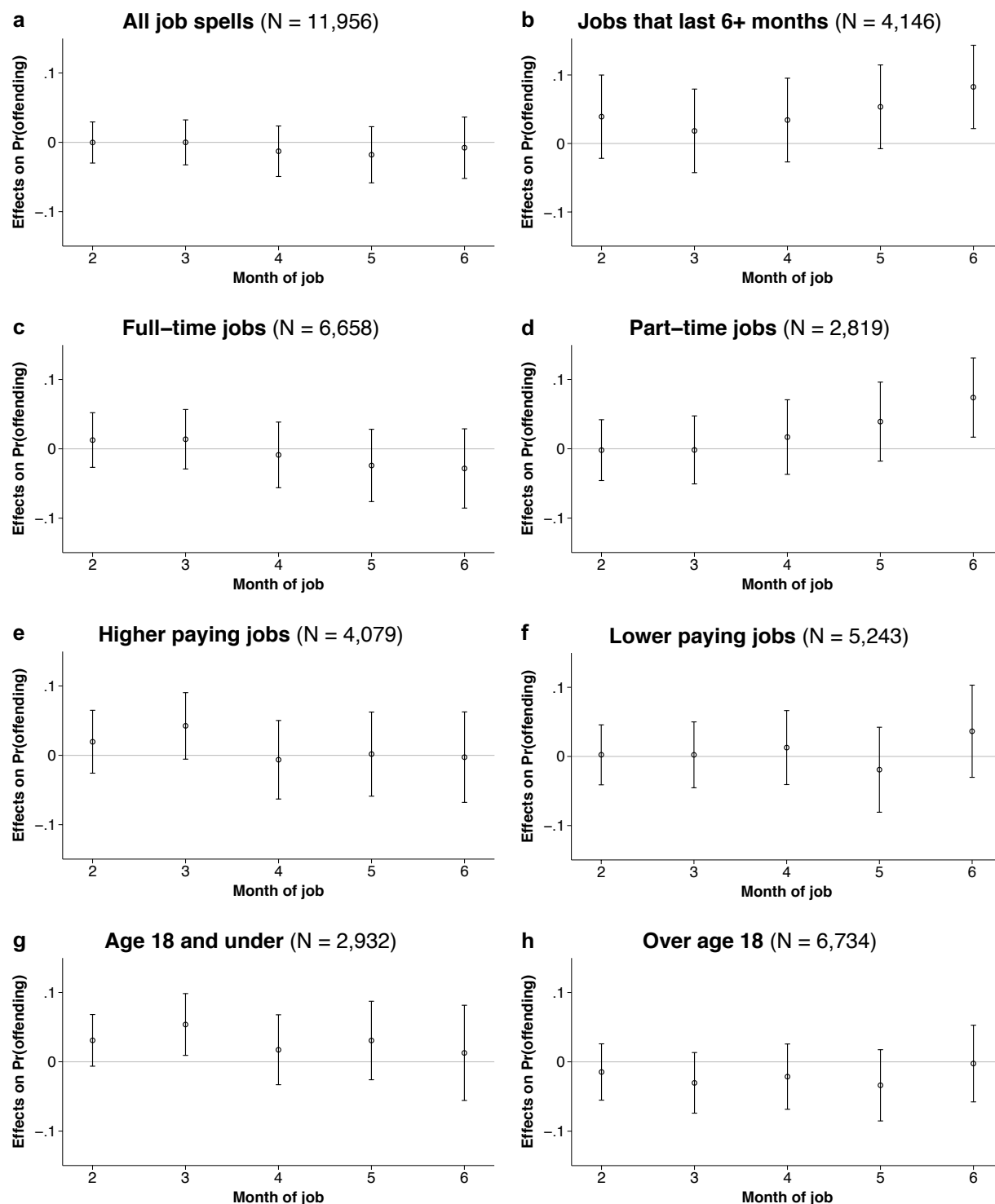
Figure 2. Changes in the probability of offending surrounding job entry

Note: The figure displays average marginal effects calculated from logistic regression models of (a) any offending, (b) income-related offending, and (c) violent offending. The models include person fixed-effects and 16 time-varying controls. The estimates show the change in the probability of offending in each month surrounding job entry, relative to the seven to nine months before job entry. Lines that surround the point estimate indicate 95% confidence intervals. Estimates are significantly different than the reference period when the confidence interval does not intersect with 0. Sample sizes are in person-months.

Figure 3. Changes in the probability of offending surrounding job exit

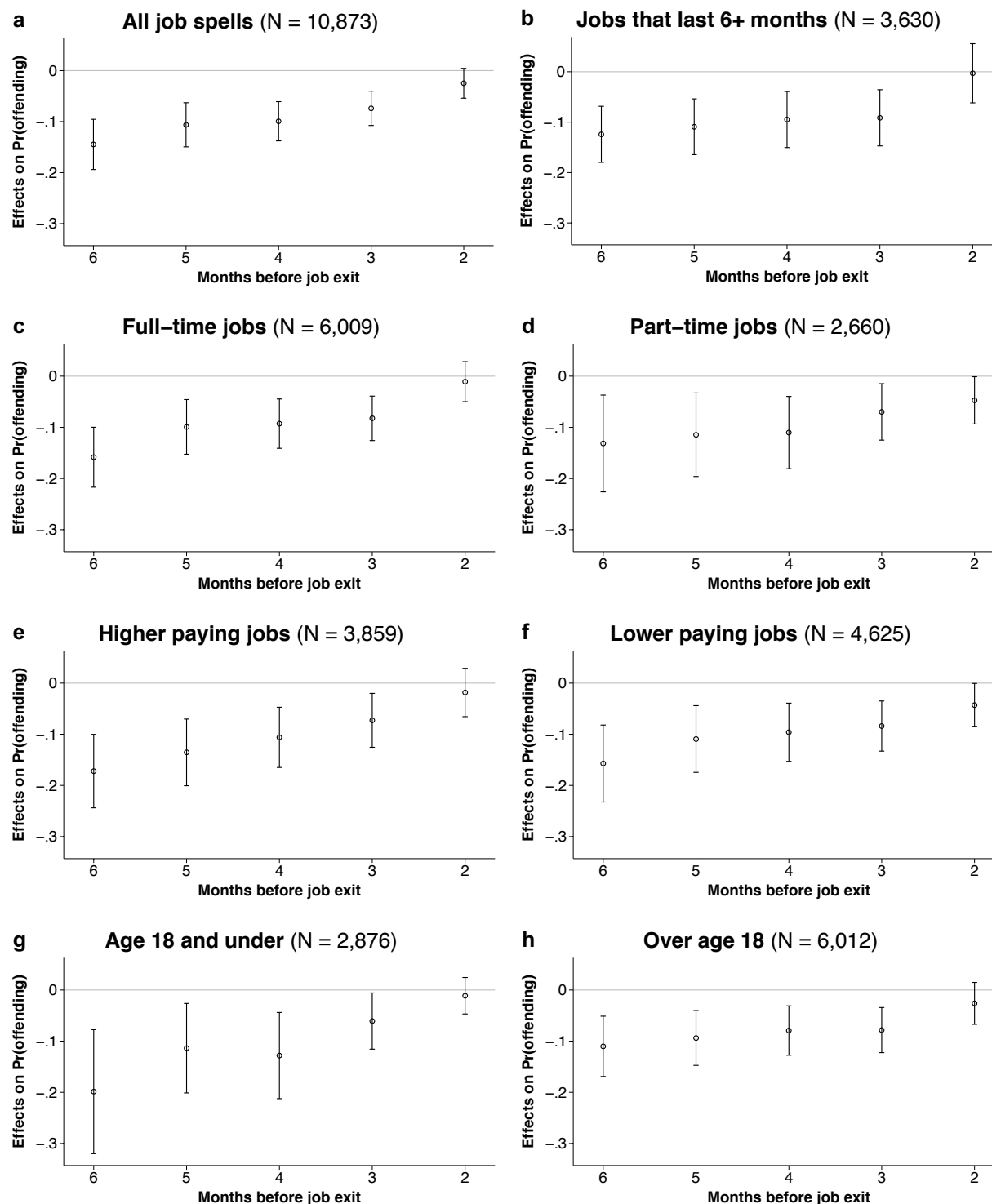
Note: The figure displays average marginal effects calculated from logistic regression models of (a) any offending, (b) income-related offending, and (c) violent offending. The models include person fixed-effects and 16 time-varying controls. The estimates show the change in the probability of offending in each month surrounding job exit, relative to the sixth from last to the fourth from last months of the job. Lines that surround the point estimate indicate 95% confidence intervals. Estimates are significantly different than the reference period when the confidence interval does not intersect with 0. Sample sizes are in person-months.

Figure 4. Changes in the probability of offending during the first six months of job spells, relative to the first month



Note: The figure displays average marginal effects calculated from logistic regression models with person fixed-effects and 16 time-varying controls. The estimates show the change in the probability of any offending during months 2 to 6 of job spells, relative to the first month. Lines that surround the point estimate are 95% confidence intervals. The models are run first for all job spells and then for various subsets: jobs that last at least six months, full-time and part-time jobs, jobs that pay higher and lower than average, and by age. Sample sizes are in person-months.

Figure 5. Changes in the probability of offending during the last six months of job spells, relative to the last month of the job



Note: The figure displays average marginal effects calculated from logistic regression models with person fixed-effects and 16 time-varying controls. The estimates show the change in the probability of any offending during sixth from last through second from last months of job spells, relative to the last month. Lines that surround the point estimate are 95% confidence intervals. The models are run first for all job spells and then for various subsets: jobs that last at least six months, full-time and part-time jobs, jobs that pay higher and lower than average, and by age. Sample sizes are in person-months.