Essays on welfare benefits, employment, and crime
Stam, M.T.C.

5 Crime state dependence and employment: Modelling feedback effects to investigate crime continuity

Abstract

Substantial evidence suggests that past criminal behavior is one of the most significant predictors of future crime. However, estimating crime state dependence effects is empirically challenging due to unobserved population heterogeneity. By explicitly modelling feedback effects from past crime on current employment in a joint dynamic model, this study investigates crime state dependence and the role of employment. To control for unobserved population heterogeneity, we apply a correlated random effects bivariate probit model over individual-level data on a randomly-selected 5% subset of young adults in the Netherlands. Through this approach, we find subtantial state dependence effects across crime outcomes and sex, ranging from an increase of 47% for male non-financially-motivated crime to an increase of 100% for female financially-motivated crime. Reduced labor market opportunities following an offense appear to only minimally contribute to crime state dependence among men.

5.1 Introduction

Two-thirds of all registered yearly crime suspects in the Netherlands are repeat offenders (Statistics Netherlands 2020c). As prior criminal behavior is one of the most significant predictors of future criminal behavior (e.g. Campbell et al. 2009, Farrington 1998, Gendreau et al. 1996), a prominent question in criminological research is to what extent this relationship is causal or spurious. The substantial positive correlation between past – and future crime may be partially attributable to population heterogeneity, as individuals differ in personal characteristics that determine their latent tendency to commit crime (e.g. self-control). To the extent that these characteristics persist over time, they will also affect offending in future periods (i.e. *spurious* autocorrelation). However, past criminal behavior may also *causally* affect future criminal decision making (i.e. true state dependence). A prominent theoretical pathway through which such state dependence effects could arise is through adverse effects on employment. Past crime may reduce labor market opportunities, which in turn could incentivize individuals to commit crime, for financial gains especially. By implementing a model that controls for population heterogeneity and explicitly allows for potential feedback effects from past crime to current employment, this study investigates the role of employment in crime state dependence.

Seminal work by Heckman (1981b, 1991) first distinguished between population heterogeneity and state dependence as mechanisms underlying behavioral continuity. In following, criminological research has produced mixed results as to what extent these mechanisms explain the positive correlation between prior and future offending (see Nagin and Paternoster 2000). Some early studies find population heterogeneity to solely be the cause (e.g. Nagin and Paternoster 1991, Paternoster and Brame 1997), whereas findings from other studies support state dependence (e.g. Nagin and Farrington 1992a,b, Paternoster et al. 1997). The mixed findings from these studies may be attributeable to differences in data and sample characteristics. Population heterogeneity is supported by evidence based on administrative data on high-risk samples (i.e. ex-offenders), whereas state dependence is supported by evidence based on survey data on general population samples. One Dutch study by Blokland and Nieuwbeerta (2010a) finds support for both population heterogeneity and state dependence effects, by combining survey data on a general population sample with administrative data on a high-risk sample through hierarchical linear modeling. Quasi-experimental research using administrative data on a large general population sample, however, is scarce.

One pathway for crime state dependence is through adverse effects on employment. Prior criminal behavior may knife off opportunities for stable employment through multiple 'scarring' mechanisms.[1] Firstly, the acquirement of a criminal record may substantially reduce labor market opportunities (e.g. Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). Signalling theory argues that a criminal record may signal certain undesirable personal characteristics towards potential employers, such as low discipline and work competency (Spence 1973). To a lesser extent, this also holds true for unemployment spells (Holzer et al. 2004), which can be the result of investing in a criminal career, as well as penal intervention (such as imprisonment). These voluntary or forced absences from the labor market may additionally adversely affect the accumulation of human capital and, consequently, employment opportunities (see Becker 2009). There is, however, a paucity in research into the labor market scarring effects of criminal behavior outside of the US. Compared to the Netherlands and most other European countries, criminal records are more accessible in the US (see Corda and Lageson 2020),[2] custodial sanctions are more often imposed, and average prison terms are (much) longer (see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020). Hence, it remains unclear to what extent these findings can be generalized to other contexts.

Labor market scarring by criminal behavior may play an important role in crime state dependence, as a substantial body of micro-level evidence shows that being employed reduces criminal behavior (see Apel et al. 2008, Apel and Horney 2017, Van der Geest et al. 2011, Ramakers et al. 2020, Uggen 2000).[3] This reduction in crime may be achieved through various theoretical economic (Chalfin and Raphael 2011) and sociological (Lageson and Uggen 2013) mechanisms. From an economic perspective, the legiti-

[1]See Apel and Sweeten (2010), Bernburg and Krohn (2003), De Li (1999), Dobbie et al. (2018), Lopes et al. (2012), Pager et al. (2009), Selbin et al. (2018), Uggen et al. (2014).

[2]Criminal records are only accessible to criminal justice actors in the Netherlands. However, certain professions and employers may require a so-called 'certificate of conduct', and a criminal conviction can be ground to dismiss an application for this certificate. The aim of these regulations is to reduce labor market discrimination of ex-offenders, and evidence suggests that criminal records fulfill only a limited role in hiring decisions in the Netherlands (Van den Berg et al. 2017, Dirkzwager et al. 2015). Furthermore, a recent study by Ramakers (2020) shows that only 6% of Dutch ex-prisoners apply for a certificate of conduct in the first four years upon re-entry, of which approximately one-third is granted.

[3]There is also substantial evidence on effects of employment and and labor market prospects on aggregate crime rates (e.g. Gould et al. 2002, Lin 2008, Machin and Meghir 2004, Raphael and Winter-Ebmer 2001).

mate income from employment may reduce the relative financial benefits from financially-motivated crime (Becker 1968, Ehrlich 1973), as well as financial-strain-induced emotional stress conducive to criminal behavior in general (Agnew 1992). Employment can also have an incapacitative effect, by limiting ones time and opportunity to commit crime overall (Cohen and Felson 1979). By providing structure, responsibility and social bonds with non-deviant peers, a stable work environment may also reduce crime in general (e.g. Hirschi 1969, Laub and Sampson 1993). By reducing employment opportunities, prior criminal behavior may cause further criminal behavior through removal of such crime prevention mechanisms.

Previous studies that dynamically model crime state dependence through employment are scarce, and are forced to make concessions due to computational challenges. Imai and Krishna (2004) estimate a dynamic discrete-choice structural model on data from the 1958 Philadelphia birth cohort study,[4] to unveil a substantial deterrent effect of anticipated adverse labor market consequences on current criminal behavior. In addition to evidence of state dependence and population heterogeneity causing continuity in criminal behavior, they find criminal history to adversely affect current labor market outcomes. Different crime types are likely to differ in underlying motivation and societal response, and the current choice to engage in legitimate labor market activities is likely to correlate with both the current choice to commit crime as well as anticipated future labor market outcomes. However, while offenses differ substantially in both motivation and response by the criminal justice system, this study only considers crime in general. Furthermore, for computational reasons, they model the current choice to commit crime, but not the current choice to engage in legitimate labor market activities. This reciprocal effect is accommodated in a closely-related study on a high-risk sample of 270 Dutch young adult male ex-offenders (Mesters et al. 2016). By simultaneously modelling crime, employment, and welfare receipt, in a dynamic discrete-choice model, they find evidence of crime state dependence through employment. Potentially due to a difference in the societal or legal response, the adverse effect of violent crime on future employment appears to be substantially

[4] The 1958 Philadelphia birth cohort study collected data on the criminal careers of all boys born in 1945 who lived in Philadelphia from the age of 10 to 18.

larger than property crime. Conversely, they find employment to only cause a statistically significant reduction in property offenses. The current study aims to assess to what extent these findings apply to a general population of young adults.

To investigate the causal pathway of crime state dependence, this study employs a joint dynamic model of crime and employment that explicitly accommodates feedback effects from past crime on current employment. Facilitated by the availability of unique individual-level administrative data (2006-2017), we apply a correlated random effects bivariate probit model over a balanced panel of a randomly-selected 5% subset of young adults in the Netherlands (aged 18 to 29 in 2006). This approach allows us to assess to what extent crime state dependence works through adverse effects of past crime on employment, which is empirically challenging as the past-crime–future-crime relationship as well as employment status are highly endogenous. To control for all time-invariant observed and unobserved heterogeneity, we model individual-specific effects in the form of including individual-level correlated random effects and initial employment – and crime conditions. By instrumenting employment status on regional unemployment rates, we furthermore exploit exogenous variation in employment caused by regional labor market conditions. To investigate heterogeneous effects, we run the analyses separately for financially-motivated offenses and other (non-financially-motivated) offenses among the men and women.

After controlling for population heterogeneity, we find substantial state dependence effects for both financially-motivated crime and other crime. Financially-motivated criminal behavior in the prior month increases the monthly probabilities of such an offense by 92% and 100%, for men and women respectively. We find the other crime state dependence effect to be almost twice as large for women (78%) as compared to men (47%). Conversely, employment only appears to limitedly function as a causal pathway for crime state dependence among men. We find prior crime to reduce current employment among men slightly (-1%), whereas employment among women is unaffected. Furthermore, we find employment to only reduce financially-motivated crime among women (23%), whereas employment reduces both financially-motivated – (25%), and other crime (9%) among men.

The contribution of this study to the literature is threefold. First, complementary to the existing literature, we investigate crime state dependence through the use of a joint dynamic model, as opposed to dynamic discrete response models with unobserved heterogeneity. Hence, this study avoids the highly-restrictive exogeneity assumption, which does not allow the outcome of dependent variables to influence future outcomes of the regressors. This assumption is unlikely to hold as employment is likely to be influenced by past crime outcomes. Second, the availability of a vast individual-level administrative dataset enables us to investigate crime state dependence among a general, young adult sample. These data additionally enable us to separately investigate young adult women, whereas the existing literature focuses on higher-risk young male (ex-offender) samples (see Imai and Krishna 2004, Mesters et al. 2016). Criminal behavior, however, differs substantially between the sexes (e.g. see Steffensmeier and Allan 1996), and evidence also suggests that crime state dependence is heterogeneous across sex (Andersson 1990, Gushue et al. 2020, Mazerolle et al. 2000). Hence, this study adresses the paucity in crime literature focused on women, and contributes to the ongoing discussion of whether the same factors influence male and female crime through similar mechanisms (see Kruttschnitt 2013, Steffensmeier and Allan 1996). Third, the Netherlands offers a different context to assess crime state dependence, whereas most of the existing research focuses on the US. The Netherlands and most other European countries have comparatively lenient criminal justice systems (see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020), and comparatively inaccessible criminal records (see Corda and Lageson 2020). As such, this study is able to assess the generalizability of US estimates for the labor market scarring effects of past crime to other contexts.

Below, Section 5.2 will first discuss the data, sample selection and relevant descriptive statistics. Section 5.3 describes the empirical model, after which we discuss the baseline estimation results in Section 5.4. We conclude and discuss the implications of our findings in Section 5.5. Appendix 5.A contains a comparison of the estimates obtained from our baseline joint dynamic model to those obtained from a standard dynamic probit approach.

Data 5.2

This study uses longitudinal individual-level administrative data from Statistics Netherlands on a randomly-selected subset of the Dutch population.[5] The monthly-level data covers the period of 2006 until 2017, which amounts to a twelve-year observation window.

Administrative data on crime are derived from crime reports of the Dutch law enforcement agencies, which have been submitted to the public prosecutor. These reports contain information concerning crimes of which individuals are officially suspected and are strong indicators of committed offenses. When brought to trial, approximately 90 percent of cases result in a conviction (Statistics Netherlands et al. 2013). The employment data are derived from the Employee Insurance Agency (i.e. 'UWV'), which is the central Dutch administrative authority that registers all employee insurances. As we suspect employment status to be endogenous, we use the regional unemployment rate to instrument employment. The regional unemployment data cover yearly unemployment rates at the 'COROP' level, which is a commonly used geographical division of the Netherlands into 40 supra-municipal regions.

Sample and descriptive statistics 5.2.1

This study focuses on investigating crime state dependence among young adults. To this end, we generate a balanced panel by randomly selecting 5% of all registered inhabitants of the Netherlands aged 18 to 29 in the year 2006. This approach produces a full sample of 93,428 individuals (aged

[5]While Statistics Netherlands provides data on the entire registered population of the Netherlands, it is computationally infeasible to estimate our models on such a large sample. Under certain conditions, these microdata are accessible to all researchers for statistical and scientific research. For further information, contact microdata@cbs.nl. Included datasets are bijstanduitkeringtab, bijstanduitkeringint, bus, gbaadresobjectbus, gbapersoontab, integraal huishoudens inkomen, integraal persoonlijk inkomen, polisbus, spolisbus, verdtab and vslgwbtab.

18-40), and 13,453,632 monthly observations between January 2006 and December 2017. This large sample size enables us to investigate potential heterogeneity across sex, by running the analyses for men and women, separately.

Prior studies underline the importance of investigating the role of crime state dependence during early adulthood and across sex. Developmental criminological theory posits young adulthood to be an important life stage, due to a divergence that occurs in offending behavior (see Moffitt 1993). So-called 'adolescence-limited' offenders desist from criminal behavior around this age, whereas 'life-course persistent' offenders continue offending. Individuals also commonly enter the labor market around this age, which may make them particularly vulnerable to potential adverse effects of prior criminal behavior on labor market prospects. Sex differences in the developmental trajectories of criminal behavior have also become a focus of criminological research and theory (see Moffitt and Caspi 2001). Evidence suggests that men are more often repeat offenders (Moffitt and Caspi 2001), and that crime state dependence processes differ between men and women (Andersson 1990, Gushue et al. 2020, Mazerolle et al. 2000). More broadly, the underrepresentation of women in repeat offending, and offending in general (Steffensmeier and Allan 1996), has resulted in a paucity of research into crime state dependence among women. To adress this paucity, this study investigates heterogeneity across sex in the role of employment in crime state dependence during this transitionary life stage.

Table 5.1 presents descriptive statistics for the selected samples. To first explore the relationship between prior – and current crime and employment, we include monthly rates conditional on positive or negative lagged values of financially-motivated crime, other crime, and employment.[6]

While financially-motivated crime accounts for more than half of all crime committed in the Netherlands (Statistics Netherlands 2020a), we find other offenses to be more prevalent among young adults. In the full sample, we find 1.51% of individuals to commit non-financially-motivated crime in any given year within the observation window. This yearly rate

[6]Other crime is defined as any criminal offense for which no theoretical financial incentive can be identified.

is more than two times higher than the rate for financially-motivated crime (0.61%). We find much lower rates at the monthly level, with 0.06% and 0.15% for financially-motivated and other crime, respectively. If an individual has committed crime in the previous month, however, these rates increase substantially to 6.64% for financially-motivated crime and 4.03% for other crime. Conversely, the average employment rate of 78.24% is substantially lower among individuals who have committed financially-motivated crime (42.63%) or other crime (58.13%) in the previous month. Compared to unemployed individuals, those who are employed show substantially lower rates for other crime (0.11% vs 0.28%), and financially-motivated crime especially (0.03% vs 0.16%). We find a *gross* average personal primary income of €31,411 and a *net* average standardized household income of €25,779.

A comparison of the sexes shows that the gap in crime rates is especially large for other crime, as the monthly rate is around six times higher for men (0.25%) than for women (0.04%). This is likely mainly attributable to the overrepresentation of men in violent crime (Statistics Netherlands 2020b). While the gap narrows when we consider financially-motivated crime, we still find that men commit these offenses approximately three times as often as women, with monthly rates of 0.09% and 0.03%, respectively. The relative difference between men and women decreases when we consider rates conditional on having committed crime the previous month, both for financially-motivated crime (7.30% vs 4.62%) and other crime (4.29% vs 2.48%). This is in line with evidence that shows that although repeat offenders are much more prevalent among men, offending frequency among male and female repeat offenders is more comparable (Moffitt and Caspi 2001). Apart from repeat offenders, we find the highest crime rates among unemployed men, and the lowest crime rates among employed women. However, women are less often employed than men (77.09% vs 79.42%), and earn much less on average (€37,184 vs €25,749).

In line with our expectations, we find substantially higher crime – and lower employment rates if individuals have committed crime in the previous month, for both men and women. The differences in conditional crime rates are especially large, as crime in the prior month increases the current monthly rate by around factors of 20 up to 100. The size of these differences emphasize the importance of investigating as to what extent this is attributable to population heterogeneity, or true state dependence.

Table 5.1: Descriptive statistics, 2006-2017

	Full sample	Men	Women
Financially-motivated crime (yearly, %)	0.61	0.90	0.31
Financially-motivated crime (monthly, %)	0.06	0.09	0.03
if financially-motivated crime$_{t-1}$ = 1	6.64	7.30	4.62
if financially-motivated crime$_{t-1}$ = 0	0.06	0.08	0.03
if employment status$_t$ = 1	0.03	0.05	0.02
if employment status$_t$ = 0	0.16	0.25	0.08
Other crime (yearly, %)	1.51	2.59	0.46
Other crime (monthly, %)	0.15	0.25	0.04
if other crime$_{t-1}$ = 1	4.03	4.29	2.48
if other crime$_{t-1}$ = 0	0.14	0.24	0.04
if employment status$_t$ = 1	0.11	0.19	0.03
if employment status$_t$ = 0	0.28	0.49	0.08
Employment (monthly, %)	78.24	79.42	77.09
if employment status$_{t-1}$ = 1	98.33	98.39	98.28
if employment status$_{t-1}$ = 0	6.03	6.29	5.79
if financially-motivated crime$_{t-1}$ = 1	42.63	43.43	40.19
if financially-motivated crime$_{t-1}$ = 0	78.28	79.47	77.12
if other crime$_{t-1}$ = 1	58.13	59.12	52.18
if other crime$_{t-1}$ = 0	78.29	79.49	77.12
Native (%)	79.70	81.26	78.16
Welfare dependency rate (monthly, %)	2.94	2.21	3.65
Annual personal primary income	31,411	37,184	25,749
Annual standardized household income	25,779	26,387	25,183
Number of individuals	93,428	46,266	47,162
Number of observations	13,453,632	6,662,304	6,791,328

Empirical methodology 5.3

To estimate true crime state dependence, this study employs a joint dynamic model following the framework by Wooldridge (2000) and Biewen (2009), in the form of a correlated random effects bivariate probit. As we expect employment to have an important reciprocal relationship with crime, we explicitly accommodate feedback effects from past crime to contemporaneous employment. However, time-invariant unobserved heterogeneity affecting criminal behavior across time, makes estimating the effect of past crime on current crime particularly empirically challenging. Additionally, unobserved heterogeneity in particular individual characteristics may affect both criminal behavior, as well as employment status. This study adresses these endogeneity problems by instrumenting employment status using regional unemployment rates, and by estimating individual-specific effects. The individual-specific terms accommodate unobserved time-invariant determinants of crime, such as self-control, morality and intelligence. We account for correlation between individual-specific effects and the initial crime condition following Wooldridge (2005).[7]

Let y_{it} indicate individual crime status, and let w_{it} denote whether individual i is employed. Then the joint density of $y_{i1},..., y_{iT}, w_{i1},..., w_{iT}$ given exogenous variables z_{it}, initial values y_{i0}, w_{i0}, and individual-specific effects c_{ij}, can be written as

$$f(y_{i1},...,y_{iT},w_{i1},...,w_{iT}|z_{it},y_{i0},w_{i0},c_{ij},\theta,\gamma,\beta) \tag{5.1}$$

$$= \prod_{t=1}^{T}\Phi_2[(2y_{it}-1)(\theta_1 z_{it}+\theta_2 w_{it}+\theta_3 y_{it-1}+c_{i1}), \tag{5.2}$$

$$(2w_{it}-1)(\gamma_1 z_{it}+\gamma_2 y_{it-1}+\gamma_3 w_{it-1}+c_{i2}), \tag{5.3}$$

$$(2y_{it}-1)(2w_{it}-1)\rho]$$

In addition to initial crime condition values y_{i0}, the individual-specific effects c_{ij} include initial values for employment status w_{i0}. Through this approach, we avoid bias from unobserved personal characteristics that

[7] The implemented approach proposed by Wooldridge (2005) is a simplified alternative to the commonly implemented solution to the initial conditions problem by Heckman (1981a).

are correlated to z_{it}, as well as unobserved factors correlated to the initial states of crime y_{i0} and employment w_{i0}. The individual-specific effects are specified as follows:

$$c_{ij} = \alpha_{0j} + \alpha_{1j}y_{i0} + \alpha_{2j}w_{i0} + \alpha_{3j}\bar{z}_i + \alpha_{4j}y_{i0}\bar{z}_i + \alpha_{5j}w_{i0}\bar{z}_i + \alpha_{ij} \tag{5.4}$$
$$\text{For } j = 1, 2$$

Where it is assumed that the joint density of the random effects error terms follows a bivariate normal distribution.

Equation (5.3) includes lagged crime y_{it-1}, capturing potential feedback effects from crime on employment. Combined with the inclusion of contemporary employment status w_{it} in equation (5.2), this approach captures potentially adverse effects of crime on labor market opportunities, and vice versa. Equation (5.3) also accounts for state dependence effects in employment, and for possible correlations between employment status and unobserved time-invariant determinants of crime status c_{i2}. To avoid logical inconsistency, we exclude current crime from this equation (see Maddala 1986). We run the analyses separately for financially-motivated crime and other crime.

To further control for endogeneity, we instrument employment status on regional unemployment rates. We find the regional unemployment rate to be a suitable instrumental variable, as we consider both of the assumptions of instrument relevance and instrument exogeneity to hold. First, we find the regional unemployment rate to cause a sufficient amount of variation in employment (as will be discussed in Section 5.4). Second, we are unable to conceive of mechanisms through which the regional unemployment rate might affect criminal behavior other than through its effect on employment. Multiple prior studies have also used unemployment rates to instrument for various endogenous variables of interest. For example, local unemployment rates have previously been used as an instrumental variable to study the effect of youth unemployment on future wages and employment (Gregg 2001, Gregg and Tominey 2005), industry-age-year unemployment rates have been used to investigate the effect of becoming unemployed on psychological health (Gathergood 2013), and state unemployment rates have been used to estimate returns to schooling (Arkes 2010).

The correlated random effects bivariate probit model endogenizes employment status w_{it} upon which crime status is conditioned. Simulation evidence shows that when the average probability of the dependent variable is close to 1 or 0 (which clearly applies to crime outcomes in this study), this approach is preferable to (fixed effects) two-step or linear probability estimators (Bhattacharya et al. 2006, Chiburis et al. 2012).

To determine the absolute size of the crime state dependence and feedback effects, we calculate average partial effects (APEs). APEs respresent the change in the dependent variable from a one unit increase in the explanatory variable averaged over the distribution of other population characteristics. For the baseline model, the APE of the crime state dependence effect is given by:

$$\begin{aligned} APE = E[&P(y_{it} = 1 | z_{it}, w_{it}, y_{it-1} = 1, w_{it-1}, y_{i0}, w_{i0}) \\ &-P(y_{it} = 1 | z_{it}, w_{it}, y_{it-1} = 0, w_{it-1}, y_{i0}, w_{i0})] \end{aligned} \tag{5.5}$$

While the inclusion of separate individual-specific effects c_{ij} for the crime and employment equations avoids the imposition of severe restrictions on cross-equation unobserved correlations, it does impose a serious computational burden. On the available system, it is consequently not computationally feasible to estimate the model over the full 5% subset of the population at once. To adress this issue, we employ a minimum distance approach to combine estimates obtained from five separate regressions over 1% subsets. Another limitation related to computation feasiblity is that the model does not accommodate error term serial correlation. Evidence from a related study to estimate welfare benefits receipt state dependence, however, suggests that controlling for serial correlation has little effect (Chay and Hyslop 2014). Standard errors are clustered at the individual level, to account for the group structure induced by potential specification errors and prevent overstatement of the significance of the estimated effects. To avoid potential bias from non-exogenous panel attrition, we fit the models on balanced panel data (see Biewen 2009). Finally, to assess the extent to which feedback of prior crime on employment influences our estimates, we compare the results from the baseline model to those obtained from a dynamic correlated random effects probit model in Appendix 5.A.1 (e.g. see Wooldridge 2010).

5.4 Estimation results

Tables 5.2 and 5.3 present the estimation results produced by the baseline joint dynamic model, for financially-motivated crime and other crime among men and women separately. To enhance the interpretability of the estimation results, we compute the average partial effects (APEs) for the variables of interest. The APEs indicate how much the dependent variable changes due to a one unit increase in the variable of interest averaged over the distribution of other population characteristics. The statistically significant estimates for the unemployment rate ($p<.001$) suggest that the regional unemployment rate is a sufficiently strong instrument for employment. The APEs show that a one percentage point increase in the regional unemployment rate reduces employment by approximately 0.09 percentage points.

Table 5.2 shows statistically significant and sizeable positive estimates for the financially-motivated crime state dependence effects, for both men (0.5449) and women (0.5721). This indicates that past criminal behavior substantially increases future criminal behavior, even after controlling for observed and unobserved population heterogeneity. The crime equation estimates furthermore show that employment significantly reduces financially-motivated crime (-0.1727 and -0.1631 for men and women, respectively). However, when we consider the employment equation estimates, we find prior financially-motivated crime to significantly reduce employment among men only (-0.1332). Employment therefore does not appear to be a pathway for financially-motivated crime state dependence among women.

The APEs show that prior financially-motivated crime increases current financially-motivated crime by 92% and 100%, for men and women respectively. In other words, committing a financially-motivated offense in the prior month approximately doubles the probability of committing such an offense in the current month. This relative increase is slightly larger for women as compared to men. In absolute terms, however, the increase is more than twice as large for men (0.0877 vs 0.0357 percentage points). This is attributable to the substantially higher baseline rate for men. While the state dependence effects are very large, they are dwarfed by the differences in the corresponding conditional crime probabilities, shown in Table 5.1. This implies that population heterogeneity explains a substantial proportion of the positive correlation between past – and future crime. The relative reduction in financially-motivated crime by employment is also comparably-sized across sex, whereas the absolute reduction is around three times as large for men (-0.0289 percentage points or 25%) as compared to women (-0.0095 percentage points or -23%). In addition to the small and statistically non-significant effect for women, we find the reduction by prior crime on employment among men to be very limited in size (-0.94%). Despite the substantial inverse effects of employment on financially-motivated crime, this leads to the conclusion that the role of employment in crime state dependence among men is limited.

Table 5.3 presents the estimates for other (non-financially-motivated) crime among men and women. In line with financially-motivated crime, the other crime state dependence estimates are statistically significant and sizeable for both men (0.2165) and women (0.3385). Employment, however, only significantly reduces other crime among men (-0.0441). Furthermore, the feedback effect of prior other offenses on current employment is only statistically significant for men (-0.0836). In line with the findings for financially-motivated crime, employment therefore does not appear to be a pathway for other crime state dependence among women. Conversely, relative to their baseline rates, the APEs show that the other crime state dependence effect is almost twice as large for women (78.19% or 0.0333 percentage points) as compared to men (46.67% or 0.1194 percentage points). As opposed to the relatively small and statistically non-significant effect for women, we find employment to noteably reduce other crime among men (-9%). The small size of the feedback effect of prior other crime on employment (-0.57%), however, also limits the role of employment in state dependence among men.

To summarize, the estimation results show that crime state dependence effects are substantial across sex and crime outcomes. Across sex, we find comparably-sized relative state dependence effects for financially-motivated crime, while for other crime the effect is almost twice as large for women. Interestingly, employment appears to only reduce financially-motivated crime among women, while it reduces all crime among men. As we furthermore do not find statistically significant feedback effects of any prior crime on current employment for women, employment does not appear to be a pathway for crime state dependence among women. While statistically significant, we find the feedback effects of prior crime on current employment among men to be limited in size. Despite the noteable reductions by employment in male crime, we therefore conclude that employment fulfills only a limited role in male crime state dependence.

Table 5.2: Correlated random effects bivariate probit estimates for financially-motivated crime

	MEN		WOMEN	
Financially-motivated crime				
Crime_{t-1}	0.5449***	(0.0323)	0.5721***	(0.0559)
Employment	-0.1727***	(0.0174)	-0.1631***	(0.0277)
Crime_{i0}	-5.8558	(6.5650)	-14.1145	(11.5638)
Employment_{i0}	0.3901*	(0.1516)	-0.1360	(0.2129)
Crime_{i0}*employment_{i0}	43.8266***	(1.8378)	53.7407***	(3.9681)
Crime_{i0}*$\overline{\text{age}}$	0.7053**	(0.2258)	1.5161***	(0.3842)
Employment_{i0}*$\overline{\text{age}}$	-0.0263***	(0.0051)	-0.0110	(0.0071)
Age	-0.0300***	(0.0017)	-0.0130***	(0.0024)
$\overline{\text{Age}}$	0.0322***	(0.0044)	-0.0045	(0.0054)
Native	-0.2144***	(0.0124)	-0.1056***	(0.0189)
Constant	-3.1829***	(0.1224)	-2.9465***	(0.1501)
Employment				
Unemployment rate	-0.0169***	(0.0012)	-0.0156***	(0.0012)
Crime_{t-1}	-0.1332***	(0.0350)	-0.0989	(0.0600)
Employment_{t-1}	3.0628***	(0.0056)	3.0822***	(0.0052)
Crime_{i0}	-0.9759	(1.9494)	-4.9841	(4.1490)
Employment_{i0}	-0.3198***	(0.0750)	0.1651*	(0.0701)
Crime_{i0}*employment_{i0}	-10.5999***	(0.7739)	-7.8417***	(1.4165)
Crime_{i0}*$\overline{\text{age}}$	0.0597	(0.0668)	0.1180	(0.1479)
Employment_{i0}*$\overline{\text{age}}$	0.0780***	(0.0026)	0.0584***	(0.0024)
Age	0.0020**	(0.0006)	-0.0037***	(0.0006)
$\overline{\text{Age}}$	-0.0580***	(0.0022)	-0.0359***	(0.0020)
Native	0.0725***	(0.0052)	0.0515***	(0.0049)
Constant	-0.8061***	(0.0607)	-1.2598***	(0.0559)
Random effects parameters				
Crime	0.4117	(0.0290)	0.4210	(0.0284)
Employment	0.3494	(0.0043)	0.3434	(0.0041)
Correlation	-0.3170	(0.0361)	-0.2129	(0.0567)
Average partial effects				
Financially-motivated crime				
Crime_{t-1} (%point)	0.0877***	(0.0061)	0.0357***	(0.0041)
Crime_{t-1} (%)	91.96		100.35	
Employment (%point)	-0.0289***	(0.0032)	-0.0095***	(0.0017)
Employment (%)	-24.92		-23.03	
Employment				
Unemployment rate (%point)	-0.0909***	(0.0067)	-0.0864***	(0.0066)
Unemployment rate (%)	-0.11		-0.11	
Crime_{t-1} (%point)	-0.7510***	(0.1910)	-0.6673	(0.3487)
Crime_{t-1} (%)	-0.94		-0.86	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 5.3: Correlated random effects bivariate probit estimates for other crime

	MEN		WOMEN	
Other crime				
Crime_{t-1}	0.2165***	(0.0212)	0.3385***	(0.0613)
Employment	-0.0441***	(0.0111)	-0.0316	(0.0231)
Crime_{i0}	1.0354	(3.6953)	-9.2386	(8.4454)
Employment_{i0}	0.5767***	(0.1001)	-0.3246*	(0.1646)
Crime_{i0}*employment_{i0}	17.6316***	(0.8746)	35.6382***	(2.6887)
Crime_{i0}*$\overline{\text{age}}$	0.4745***	(0.1312)	1.4958***	(0.3023)
Employment_{i0}*$\overline{\text{age}}$	-0.0304***	(0.0033)	-0.0016	(0.0054)
Age	-0.0388***	(0.0010)	-0.0136***	(0.0019)
$\overline{\text{Age}}$	0.0463***	(0.0032)	-0.0025	(0.0046)
Native	-0.1538***	(0.0082)	-0.1305***	(0.0149)
Constant	-3.0775***	(0.0904)	-2.8569***	(0.1253)
Employment				
Unemployment rate	-0.0171***	(0.0012)	-0.0156***	(0.0012)
Crime_{t-1}	-0.0836***	(0.0221)	-0.0382	(0.0526)
Employment_{t-1}	3.0633***	(0.0056)	3.0826***	(0.0052)
Crime_{i0}	-0.7550	(1.4198)	2.2607	(4.6646)
Employment_{i0}	-0.2385**	(0.0748)	0.1670*	(0.0700)
Crime_{i0}*employment_{i0}	-7.7478***	(0.5106)	-6.9261***	(1.2969)
Crime_{i0}*$\overline{\text{age}}$	0.0499	(0.0485)	-0.1209	(0.1664)
Employment_{i0}*$\overline{\text{age}}$	0.0756***	(0.0026)	0.0584***	(0.0024)
Age	0.0021***	(0.0006)	-0.0038***	(0.0006)
$\overline{\text{Age}}$	-0.0573***	(0.0022)	-0.0358***	(0.0020)
Native	0.0688***	(0.0052)	0.0516***	(0.0049)
Constant	-0.8232***	(0.0610)	-1.2626***	(0.0560)
Random effects parameters				
Crime	0.2891	(0.0126)	0.3072	(0.0280)
Employment	0.3464	(0.0043)	0.3433	(0.0041)
Correlation	-0.2385	(0.0371)	-0.2919	(0.0605)
Average partial effects				
Other crime				
Crime_{t-1} (%point)	0.1194***	(0.0117)	0.0333***	(0.0061)
Crime_{t-1} (%)	46.67		78.19	
Employment (%point)	-0.0238***	(0.0061)	-0.0036	(0.0022)
Employment (%)	-8.70		-7.98	
Employment				
Unemployment rate (%point)	-0.0909***	(0.0066)	-0.0863***	(0.0066)
Unemployment rate (%)	-0.11		-0.11	
Crime_{t-1} (%point)	-0.4557***	(0.1192)	-0.2900	(0.2990)
Crime_{t-1} (%)	-0.57		-0.37	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Conclusion 5.5

This study investigates crime state dependence by explicitly modelling feedback effects from past crime on current employment in a joint dynamic model. We apply a correlated random effects bivariate probit model over a balanced panel of a randomly-selected 5% subset of all Dutch citizens aged 18 to 29 in 2006, covering a twelve year observation window (2006-2017). Through this approach, we assess to what extent adverse effects of past crime on employment is a causal pathway for crime state dependence. This is empirically challenging as evidence indicates that both employment status as well as the past-crime–future-crime relationship are highly endogenous. Following Wooldridge (2005), we control for all time-invariant observed and unobserved heterogeneity by modelling individual-specific effects, including individual-level correlated random effects and initial crime – and employment conditions. We additionally exploit exogenous variation in employment caused by regional labor market conditions, by instrumenting employment status on regional unemployment rates. To investigate heterogeneous effects, we analyze financially-motivated crime and other (non-financially-motivated) crime for men and women, separately.

We find substantial state dependence effects for both financially-motivated crime and other crime, after controlling for observed and unobserved population heterogeneity. Regardless of sex, financially-motivated criminal behavior in the prior month approximately doubles the probability of committing financially-motivated crime in the current month (92% and 100% for men and women, respectively). Relative to their baseline rates, however, we find the other crime state dependence effect to be almost twice as large for women (78.19% or 0.0333 percentage points) as compared to men (46.67% or 0.1194 percentage points).

Conversely, employment appears to only fulfill a limited role in male crime state dependence. We find a statistically significant reduction in current employment among men by prior criminal behavior ($p<.001$). While this feedback effect is limited in size (-1%), we find employment to substantially reduce both financially-motivated crime (25%) and other offenses (9%) among men. For women however, we do not find feedback effects of prior criminal behavior on current employment status. Despite a notable inverse effect of employment on financially-motivated crime among women (-23%), we therefore conclude that employment does not function as a causal pathway for female crime state dependence.

The state dependence effects unveiled in this study are in line with the most closely-related study on a male-only sample (e.g. Imai and Krishna 2004, Mesters et al. 2016), and expand upon this by unveiling substantial crime state dependence among women. The estimation results for the contemporaneous effects of employment on crime mainly support Becker's rational choice theory (Becker 1968, Ehrlich 1973), over routine activity – (Cohen and Felson 1979) and social control theory (Hirschi 1969, Laub and Sampson 1993). The much larger inverse effects of employment on financially-motivated crime as compared to other offenses (in line with Mesters et al. 2016), suggest that employment mainly affects criminal behavior through an income effect (Becker 1968, Ehrlich 1973). Nonetheless, while to a lesser extent, our findings for men also support the routine activity – (Cohen and Felson 1979) and social control perspective (Hirschi 1969, Laub and Sampson 1993), from which follows that employment reduces all crime through incapacitative and social control mechanisms.

The limited effects of prior crime on future employment deviate from the substantively reduced labor market opportunities found in other studies (e.g. Apel and Sweeten 2010, Bernburg and Krohn 2003, De Li 1999, Dobbie et al. 2018, Lopes et al. 2012, Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). This may be attributable to the focus of these studies on the US, where the criminal justice system is comparatively punitive, and criminal records are more accessible to parties outside of criminal justice actors (see Aebi and Tiago 2020, Corda and Lageson 2020, Kaeble 2018, Motivans 2020). Compared to the US, custodial sanctions are less often imposed, and the average duration of imprisonment is around ten times shorter in the Netherlands (2.6 and 4.5 years for US state and federal prisoners versus 3.8 months for Dutch prisoners, see Aebi and Tiago 2020, Kaeble 2018, Motivans 2020). Furthermore, only criminal justice actors can directly access criminal records in the Netherlands (in line with most other European countries, see Corda and Lageson 2020). Both factors may reduce adverse effects of criminal history on human capital and future labor market prospects. Hence, the prior crime–employment estimates of this study are likely more representative of the EU context.

The application of a novel joint dynamic approach to unique administrative data on a general population sample enables this study to disentangle crime state dependence through employment from crime state dependence via alternative mechanisms. However, a limitation of this study is that we are unable to further investigate heterogeneity in the prior crime–employment relationship across penal interventions. Theoretically, labor market scarring may materialize through the obtainment of a criminal record and unemployment induced by penal intervention or investment in a criminal career (see Dobbie et al. 2018, Pager et al. 2009, Selbin et al. 2018, Uggen et al. 2014). While the available data allows us to control for prior employment, we do not possess data on sentencing decisions (e.g. sanction type and severity). As evidence suggests that employment prospects worsen as crime severity (Uggen et al. 2014) and sentence length (Ramakers et al. 2014) increases, further research is warranted as to what extent the role of scarring effects in crime state dependence is heterogeneous across crime – and sanction characteristics.

Further research is also warranted to address the paucity in evidence on alternative pathways for crime state dependence. From an economic perspective, the choice to commit crime is oftenly approached as a purely objective, economic process through which individuals maximize utility defined in monetary terms (e.g. Becker 1968). As such, a rational assessment of risk-return trade-off determines the decision to pursue legal or 'illegal employment', i.e. a criminal career (Ehrlich 1973). However, this decision-making process could be influenced by various subjective, psychological factors. An extensive literature review by Piquero et al. (2011) finds the deterrent effect of potential punishment on criminal behavior to be influenced by heterogeneity in personal traits, such as moral inhibitions, impulsivity, heuristic biases, and hyperbolic discounting. Direct experience with the criminal justice system may influence an individual's perception of the celerity, certainty, and severity of punishment, and potentially reduce the deterrent effect of possible penal intervention (Stafford and Warr 1993). While multiple studies show psychological and cognitive personal traits to mediate crime state dependence (e.g. Walters 2016a,b), causal evidence of state dependence through psychological and cognitive mechanisms is scarce. This may be attributable to the considerable empirical challenges that this question poses, which ostensibly require comprehensive individual-level longitudinal data on crime as well as relevant psychological and cognitive measures.

This study furthers the insight into crime state dependence across sex, and its causal pathway. Secondary punishment by the labor market does not uniformly appear to be a factor of importance in crime state dependence, as the unveiled adverse effects of prior crime on employment are not substantive. Nonetheless, the notable reductions in financially-motivated crime from being employed emphasize the importance of labor market activation efforts. Furthermore, the substantial crime state dependence effects through alternative mechanisms also carry major policy implications. As criminal behavior causes a sizeable increase in future criminal behavior, efforts to prevent the commission of a first offense are pertinent. Such crime prevention strategies may be more cost-effective than initially apparent, as the benefits of such efforts stretch beyond the prevention of a singular offense.

5.A Standard dynamic probit model

To investigate the impact of modelling feedback of prior crime on employment on the state dependence estimates, we present estimates in this appendix obtained from standard dynamic correlated random effects probit analyses (e.g. see Wooldridge 2010). This model is specified as follows:

$$f(y_{i1}, ..., y_{iT} | z_{it}, w_{it}, c_i, \theta) = \prod_{t=1}^{T} f(y_{it} | z_{it}, w_{it}, y_{it-1}, c_i, \theta) \tag{5.6}$$

Equation (5.6) is effectively specified analogously to the crime equation of the baseline model, including the specification of an individual-specific effect c_i (as shown in equation (5.4)). Note that, in contrast to the baseline model, this approach is subject to the strict exogeneity assumption. This means that conditional on the individual-specific characteristics c_i and the crime status in the previous period y_{it-1}, the contemporaneous crime status y_t must not be correlated to past or future values of other variables. In other words, current crime status may not affect future employment (among others). As such, the estimates produced by this approach need to be interpreted with caution, as the baseline estimates indicate that the strict exogeneity assumption does not hold. More specifically, the joint dynamic model estimates show prior crime to limitedly affect current employment, conditional on the individual-specific characteristics (as shown in Section 5.4). Furthermore, it is not possible to instrument employment and to control for employment state dependence through this approach. Nevertheless, we relax these conditions, to compare the results from the baseline joint dynamic model to those obtained from a standard dynamic approach.

Standard dynamic probit estimation results 5.A.1

A comparison of the estimates shown in Tables 5.4 and 5.5 to the baseline estimates (Tables 5.2 and 5.3), reveals that standard dynamic correlated random effects probit analysis severely overstates crime state dependence. Modelling feedback effects of prior crime on employment substantially reduces the size of the state dependence estimates, ranging from almost threefold for male other crime (0.3234 to 0.1194 percentage points) to more than fivefold for female financially-motivated crime (0.1934 to 0.0357 percentage points). Furthermore, this single-equation approach also produces estimates for employment status which are multitudes larger than the baseline estimates.

Table 5.4: Correlated random effects probit estimates for financially-motivated crime

	MEN		WOMEN	
Financially-motivated crime				
Crime_{t-1}	0.6525***	(0.0343)	0.7410***	(0.0619)
Employment	-0.3136***	(0.0132)	-0.3058***	(0.0200)
Crime_{i0}	2.2775**	(0.6985)	0.3926	(1.4291)
Employment_{i0}	0.8085***	(0.1384)	0.1104	(0.1786)
Crime_{i0}*employment_{i0}	0.0620	(0.1580)	-0.4383	(0.4532)
Crime_{i0}*$\overline{\text{age}}$	-0.0455†	(0.0238)	0.0267	(0.0455)
Employment_{i0}*$\overline{\text{age}}$	-0.0325***	(0.0048)	-0.0109†	(0.0062)
Age	-0.0339***	(0.0017)	-0.0179***	(0.0024)
$\overline{\text{Age}}$	0.0295***	(0.0042)	0.0034	(0.0052)
Native	-0.3158***	(0.0158)	-0.2000***	(0.0216)
Constant	-3.1276***	(0.1148)	-3.2024***	(0.1398)
Average treatment effects				
Crime_{t-1} (%point)	0.3555***	(0.0359)	0.1934***	(0.0358)
Crime_{t-1} (%)	493.19		801.24	
Employment (%point)	-0.0844***	(0.0044)	-0.0292***	(0.0023)
Employment (%)	-59.31		-61.83	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.

Table 5.5: Correlated random effects probit estimates for other crime

	MEN		WOMEN	
Other crime				
Crime$_{t-1}$	0.3356***	(0.0208)	0.5585***	(0.0601)
Employment	-0.1508***	(0.0085)	-0.1954***	(0.0159)
Crime$_{i0}$	0.6310	(0.4510)	1.8484	(1.2861)
Employment$_{i0}$	1.0959***	(0.0962)	0.5215***	(0.1431)
Crime$_{i0}$*employment$_{i0}$	-0.1198	(0.0923)	0.1144	(0.3136)
Crime$_{i0}$*$\overline{\text{age}}$	0.0090	(0.0155)	-0.0311	(0.0414)
Employment$_{i0}$*$\overline{\text{age}}$	-0.0410***	(0.0033)	-0.0233***	(0.0049)
Age	-0.0426***	(0.0010)	-0.0203***	(0.0019)
$\overline{\text{Age}}$	0.0423***	(0.0030)	0.0175***	(0.0043)
Native	-0.2769***	(0.0108)	-0.1744***	(0.0172)
Constant	-2.8790***	(0.0826)	-3.3970***	(0.1158)
Average treatment effects				
Crime$_{t-1}$ (%point)	0.3234***	(0.0284)	0.1618***	(0.0331)
Crime$_{t-1}$ (%)	142.78		437.39	
Employment (%point)	-0.1058***	(0.0066)	-0.0267***	(0.0025)
Employment (%)	-33.66		-46.13	
Observations	6,662,304		6,791,328	
Individuals	46,266		47,162	

Notes. Standard errors are clustered by individual, *** indicates p<.001, ** p<.01, * p<.05 and † p<.10.