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Crime and unemployment duration

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Still unemployed, what next?
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7th June 2016

In this paper, I study the relationship between unemployment benefits, labour market conditions and crime in the light of increasing unemployment durations and temporary benefit extensions in the US. First, I find a positive reduced form effect of the benefit extensions on property crime. Second, I explore the mechanisms of the reduced form in an IV model and find that higher unemployment and longer unemployment durations are linked to higer property crime rates. These findings can rationalise the reduced form effect: Longer benefit durations are linked to longer unemployment durations which in turn contribute to increased propensities for criminal activity.

Keywords: Crime; unemployment; unemployment duration; unemployment insurance
JEL classification: J64, J65, K42

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1 Introduction

Think about two otherwise identical individuals, one of whom becomes unemployed and the other one remains employed. In the standard Becker (1968) model of crime, the unemployed faces lower returns on the labour market, and at the margin is more likely to accept illegal job opportunities and to engage in crime than the employed counterfactual. Typically there is initial support through unemployment benefits and from family or other social networks when one enters unemployment. Unemployment benefits are available for a fixed period of time only and when they expire, the individual faces a cut in income. That mechanism sets strong incentives to search for jobs and to exit unemployment before the known cutoff date. Again, think about two otherwise identical unemployed individuals, one of whom receives unemployment benefits for a longer time period. Compared to the unemployed individual who has expired unemployment benefits, that person is better off in financial terms: At the margin, the unemployed who receives benefits is *less* likely to commit crime than the counterfactual without benefits, everything else being equal. However, at the margin that person also is disincentivised, remains unemployed for longer and subsequently might become *more* likely to commit crime than the re-employed counterfactual.

In this paper, I examine recent and unprecedented structural changes in the U.S. labour market and study the relationship between unemployment benefit policies, labour market conditions and crime in that context. First, I estimate the reduced form effect of unemployment benefit durations on crime using U.S. state-level data and exploiting variation by temporary policy changes. I find that there is a positive relationship between extended unemployment benefit durations and arrests linked to property offences. Second, I argue that these results can be rationalised by the fact that the unemployment benefit extensions are associated with longer unemployment durations and higher unemployment rates. Hence, I estimate an instrumental variable model in order to quantify the effect of unemployment and in particular unemployment durations on crime. The results suggest that indeed the at first somewhat surprising effects of benefit extensions on crime can be explained by the dramatic increases in unemployment durations in the U.S. Exploiting the given variation in unemployment and unemployment benefit durations, this demonstrates a contemporaneous but yet more dynamic relationship between unemployment and crime than found in the previous empirical literature. To the best of my knowledge this is the first study of the recent structural changes on the U.S. labour market with respect to crime outcomes, and in particular the first attempt to evaluate the role of temporary unemployment benefit extensions in that context. The existing empirical literature has focused on the contemporaneous link between unemployment and crime, in particular for youth unemployment. This study contributes to that literature by explicitly evaluating the relationship between unemployment extensions as well as unemployment duration and crime, and by introducing a more dynamic dimension to the analysis.

Why should the probability to engage in crime depend on how long one has been unemployed for? A frictionless transition from entering unemployment and turning to crime seems unlikely. Again, think about two otherwise identical individuals who enter unemployment. One of them finds a job and exits unemployment shortly after, and the other one remains unemployed. At the margin, the still unemployed individual is more likely to become criminal than the re-employed counterfactual: Economic conditions become more severe the longer the individual is unemployed for and human capital depreciation further decreases expected future wages. Expected legal returns decline and higher expected relative returns to illegal action increase the propensity of crime according to the standard economic model of crime (Becker, 1968, Cantor and Land, 1985). In addition, longer unemployment spells increase the opportunity and time available for criminal activity.

In the Becker (1968) model of crime, rational decision makers choose between legal and illegal activities to maximise utility. The model is explicitly static, yet - for the reasons above - the effect of unemployment is likely to vary with the duration of unemployment. Mocan et al. (2005) suggest a dynamic model of differential human capital and criminal activity. Assume that individuals are endowed with both legal and criminal activity specific human capital. Both evolve over time depending on participation in either market, on human capital depreciation, and on investment in legal human capital acquisition. Expected gains from either legal or illegal activity thus depend on both types of human capital and respective returns. During unemployment, the legal human capital stock and returns to legal human capital fall, hence involvement in crime rises. Individuals can accumulate criminal know-how and the criminal human capital stock grows subsequently, increasing the expected returns to crime. The human capital mechanisms suggest the following hypothesis: Unemployment initially shifts the individual towards the threshold to crime, at the margin leading to a more persistent and potentially increasing propensity of crime the longer the unemployment spell lasts.[1]

In this paper, I study the effect of unemployment benefit extensions on crime. Two competing mechanisms link benefit extensions to criminal activity. On the one hand, it has been argued in the literature that benefit extensions decrease unemployment exit probabilities. In line with the model above, the person who receives the benefits is disincentivised, remains unemployed and becomes more likely to commit crime than the re-employed counterfactual. On the other hand, compared to an unemployed counterfactual wihtout a benefit extension, that person is better off in financial terms and at the margin less likely to commit crime.

[1] There are alternative models to explain the link between crime, unemployment and unemployment duration. First, the arguments above relate in a broader sense to the literature on the effect of low wages on crime (see for example Grogger (1998), Machin and Meghir (2004) or Burdett et al. (2003)): The inverse deterrence effect, here triggered by longer unemployment durations, leads to higher crime propensities. Second, the literature on the impact of education on crime discusses the role of patience (see for example Lochner (2004), Lochner and Moretti (2004) or Machin et al. (2011)): If patience decreases over the course of an unemployment spell, e.g. prompted by unsuccessful job search, the valuation of relative returns to criminal action changes. Other behavioural patterns might change and affect crime behaviour in a similar way. Third, it has been shown that social networks play an important role in determining criminal behaviour: Once unemployed, social networks are likely to change and can trigger criminal behaviour (see for example Glaeser et al. (1996) or Glaeser et al. (2003)).

The benefit extension delays the negative income shock caused by the benefit exhaustion, and thus increases returns to legal activity compared to the counterfactual situation.[2] The overall link between crime and unemployment benefit extensions is the sum of both effects. Ex ante, the sign of the overall effect is arbitrary and remains to be determined empirically.

Whilst the empirical literature on the impact of unemployment on crime has grown considerably over the last two decades, the evidence is still mixed.[3] Raphael and Winter-Ebmer (2001) find elasticities of property crime rates with respect to unemployment rates between 2.8 and 5.0 percent using U.S. data from 1971 to 1997 and. Gould et al. (2002) examine the link between crime rates of young men and unemployment in the U.S. from 1979 to 1997 and also find significant positive effects. Further, they demonstrate that low wages are a better predictor for long-term crime patterns than unemployment. Bell et al. (2015) study scarring effects of unemployment at labour market entry on crime outcomes later in life and find substantial and persistent effects both for the U.S. and the UK. Using French data from 1990 to 2000, Fougère et al. (2009) find that the youth unemployment rate, but not the overall unemployment rate, is a strong causal predictor for crime rates. They find no significant association between the long-term unemployment rate and crime, however they demonstrate a positive link between not receiving unemployment benefits and crime. In contrast, Almén and Nordin (2011) use Swedish panel data on municipality level from 1997 to 2009 to examine the relationship between long-term unemployment and crime and find strong links. Grönqvist (2011) uses Swedish register data to examine the relation between youth unemployment and crime and finds strong positive effects. Importantly for this paper, the author finds evidence that the propensity of participation in crime increases with time spent in unemployment and furthermore demonstrates that the relation between unemployment and crime is mostly predicted by an increase in available time and opportunities for crime.

This study examines recent structural changes in the U.S. labour market and temporary changes in unemployment benefit policies and studies the relationship between labour market conditions and crime in that context. Hence, the findings in the study relate not only to the literature on unemployment and crime, but also to a long-standing literature on the impact of unemployment benefits on unemployment durations as well as a more recent literature on the effects of unemployment benefits on criminal behaviour. In terms of the effect of unemployment benefits on unemployment and unemployment duration, Katz and Meyer (1990) and Meyer (1990) find a spike in exit rates from unemployment close to the benefit exhaustion date. More recently and in the context of the recent changes in the U.S. labour market, Rothstein (2011) provides evidence that unemployment benefit extensions during the Great Recession had significant negative, albeit small effects on unemployment exit probabilities. The author finds that these effects are driven by the long-term unem-

[2]These reflections directly relate to the debate in the literature about the moral hazard losses of unemployment benefits on the one hand and the insurance gains on the other hand.

[3]The interested reader might refer to the respective chapter for example in Mustard (2010), or more recently Chalfin and McCrary (2015) and Draca and Machin (2015).

ployed, and that changes in the unemployment rate due to the extension policies are mainly attributed to reduced exits from the labour force. Farber and Valletta (2015) confirm these results, and find similar effects for a milder recession in the earlier 2000s. Hagedorn et al. (2013) look at the macro effects of the unemployment benefit extensions during the Great Recession. In contrast to the other studies, the authors find that there are large effects of benefit extensions on unemployment which are driven by the response of the job creation.[4] In terms of unemployment insurance and crime, Machin and Marie (2006) empirically study the effect of a reform in the Jobseekers Allowance in 1996 on crime in England and Wales. The authors find evidence that the toughening of the unemployment benefit regime leads to higher crime rates. Grönqvist (2011) uses Swedish register data, and in contrast finds that the effect of unemployment on crime is not driven by a decrease in income but rather by an increase in time and opportunities to engage in crime. The results are in line with the findings in this paper which suggest that the unemployment benefit extensions do not offset the effect of longer unemployment durations on crime.[5]

The remainder of this study is structured as follows. In section 2, I describe the U.S. state level data from the Uniform Crime Reports and the Current Population Survey as well as the unemployment benefit extension policies. Section 3 discusses the empirical strategy and the results for the reduced form estimation. The instrumental variable estimation of the effect of unemployment on crime as well as the duration dependence of that relationship are presented in section 4. Section 5 concludes.

2 Institutional Background and Data

The theoretical notions outlined above can be tested in an empirical framework. In this study, I use U.S. state level data to estimate the link between unemployment benefit extensions and crime. In the following, I first discuss the institutional background and second, I describe the different data sources and sample specifics.

[4]Other recent studies use data from European countries evaluating different benefit policies. Schmieder et al. (2012) study the effect of unemployment insurance extensions on unemployment duration over the business cycle using German data. The authors find that the moral hazard effect of benefit extensions is smaller during recessions than in booms. Lalive et al. (2015) study spillover effects of unemployment insurance extensions in a quasi-experimental setting for Austria. The authors relate their findings to the temporary policy changes in the U.S. and argue that while existing studies find small macro elasticities of unemployment durations with respect to the benefit extensions this might be due to large search externalities and that the micro elasticities might be larger.

[5]There also is a theoretical literature studying the effect of unemployment insurance on crime. Engelhardt et al. (2008) derive a search model including crime and find that a more generous unemployment benefit system reduces the crime rate for the unemployed, but has a more ambiguous effect on the employed depending on job durations and jail sentences. Polito and Long (2014) study a job search model of unemployment, crime and social insurance with random criminal opportunities. Modeling these opportunities as a moral hazard problem, they find that under certain conditions decreasing unemployment benefits reduce the expected return to criminal action compared to job search. The model implies that it is optimal to front-load benefits and to reduce benefits over time.

2.1 Unemployment Benefit Extensions

The U.S. unemployment insurance system consists of a federal-state unemployment insurance programme that provides temporary financial assistance to individuals who are unemployed and meet the eligibility criteria. Guidelines for eligibility, benefit amounts and benefit periods are given by federal law, whereas the exact legislation is determined by state law. Eligibility requirements include conditions on wages and time worked during a certain period. Generally, eligibility is based on employment in covered work for a base period of 12 months. Only workers who are unemployed "through no fault on their own" meet eligibility requirements. The benefit amount is determined based on a share of an individuals's recent earnings. The maximum benefit period typically lasts for a maximum of 26 weeks, but can vary between states.[6] Note that the individual benefit period can vary substantially though and, dependent on eligibility criteria, can be as short as one week.

In times of economic downturn and high unemployment and based on state-level criteria the maximum potential benefit duration (PBD) can be extended. During the sample period (2003 to 2011) different benefit extension policies have been in place. Table 1 summarises the unemployment benefit extension policies and trigger mechanisms for the different policies. A general extension programme, the Extended Benefit (EB) programme, is activated by trigger mechanisms based on the levels of the insured unemployment rate (IUR) and the total unemployment rate (TUR) in the respective state. Depending on the level of the IUR and the TUR the maximum number of weeks of unemployment benefits may be extended by 13 or 20 weeks in cases of particularly high unemployment. EB benefits are financed to equal parts by the federal government and by the state government.

Additional extension programmes are put in place in times of economic crisis. The Temporary Extended Unemployment Compensation (TEUC) was implemented between March 2002 and March 2004. The TEUC provides additional weeks of federally-funded unemployment benefits to workers who have exhausted regular unemployment benefits. In all states, a maximum of 13 additional weeks is available. In states that trigger onto the so-called TEUC-X programme, up to another 13 weeks of unemployment benefits are made available. In June 2008, the Emergency Unemployment Compensation 2008 (EUC08) programme was signed into law. The EUC08 programme is a federally funded programme that consists of different tiers which are implemented at various points in time as detailed in table 1. The different tiers depend on the level of the IUR and the TUR in the respective state with varying requirements over time. In addition to the tiers of EUC08, the EB programme is still in place during that period.[7] Thus, if a state triggers onto all tiers and the EB programme is in effect, up to 99 weeks of unemployment benefits are available to an eligible individual. The EUC08 programme expires at the end of December 2013. My

[6]During the sample period, the benefit period amounts to 26 weeks in all states except for Massachusetts and Washington (30 weeks). See the online unemployment insurance state law information by the Bureau of Labor Statistics and the Department of Labor for further details.

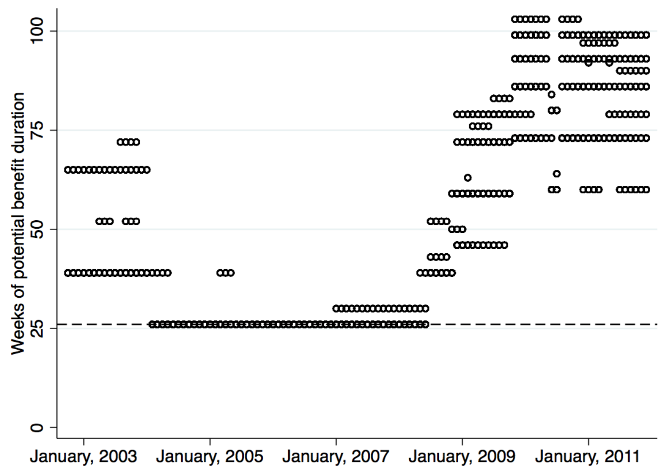
[7]States governments decide whether EB or EUC08 is paid first in the state.

Table 1: Unemployment Benefit Extensions

Programme	Time period	Tier	Extension	Trigger mechanism
EB	permanent	mandatory	13 weeks	IUR $\geq$ 5% and
				IUR $\geq$ 120% of IUR$_{year-1}$
		optional	13 weeks	IUR $\geq$ 6%
		optional	13 weeks	TUR $\geq$ 6.5% and
				TUR $\geq$ 110% of TUR$_{year-1}$ or
				TUR $\geq$ 110% of TUR$_{year-2}$
		optional	20 weeks	TUR $\geq$ 8.0% and
				TUR $\geq$ 110% of TUR$_{year-1}$ or
				TUR $\geq$ 110% of TUR$_{year-2}$
TEUC	03/'02 - 01/'04	TEUC	13 weeks	all states
		TEUC-X	13 weeks	EB triggers
EUC08	06/'08 - 09/'08	Tier 1	13 weeks	all states
	09/'08 - 09/'09	Tier 1	20 weeks	all states
		Tier 2	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 6%
	09/'09 - 02/'12	Tier 1	20 weeks	all states
		Tier 2	14 weeks	all states
		Tier 3	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 6%
		Tier 4	6 weeks	IUR $\geq$ 6% or TUR $\geq$ 8.5%
	02/'12 - 05/'12	Tier 1	20 weeks	all states
		Tier 2	14 weeks	all states
		Tier 3	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 6%
		Tier 4	6 weeks	IUR $\geq$ 6% or TUR $\geq$ 8.5%
		Tier 4	16 weeks	no EB, all conditions met
	05/'12 - 09/'12	Tier 1	20 weeks	all states
		Tier 2	14 weeks	TUR $\geq$ 6%
		Tier 3	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 7%
		Tier 4	6 weeks	IUR $\geq$ 6% or TUR $\geq$ 9%
	09/'12 - 12/'13	Tier 1	14 weeks	all states
		Tier 2	14 weeks	TUR $\geq$ 6%
		Tier 3	9 weeks	IUR $\geq$ 4% or TUR $\geq$ 7%
		Tier 4	10 weeks	IUR $\geq$ 6% or TUR $\geq$ 9%

SOURCES. - United States Department of Labor and Bureau of Labor Statistics. NOTE. - The table shows the different benefit extension policies during the sample period with the respective time period, tiers, extension and trigger mechanisms. Abbreviations: Extended Benefits (EB), Temporary Extended Unemployment Compensation (TEUC), Emergency Unemployment Compensation 2008 (EUC08).

Figure 1: Potential Benefit Durations - Sample Variation



SOURCES. - U.S. Department of Labor and own calculations. NOTE. - The figure shows the variation in the maximum potential benefit duration for the U.S. between 2003 and 2011 across states in the sample. The dashed horizontal line represents the baseline potential benefit duration of 26 weeks.

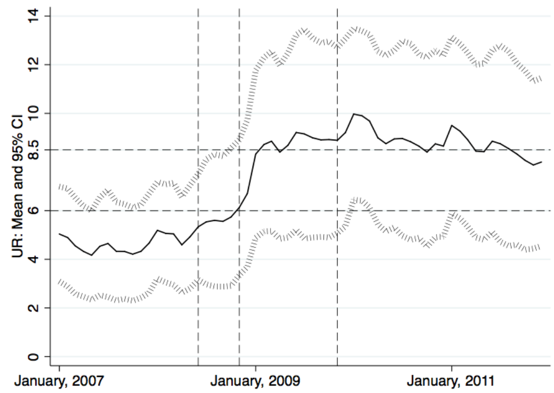
estimation sample includes data up to the end of 2011.

Information on unemployment benefit extension policies is retrieved from weekly trigger reports published by the United States Department of Labor, and the Bureau of Labor Statistics. The trigger reports indicate whether states have triggered onto an extension programme, and indicate the number of weeks of extension for the states which have triggered on. I collected the data from these reports which are published on a weekly basis. In order to match the observational level of the crime and unemployment data (see details below), I computed the median maximum potential benefit duration per state and month, measured in weeks. In the following I will refer to that variable as the potential benefit duration (PBD).

The trigger mechanisms for the policies and for the tiers within the policies are explicit, as outlined in Table 1. They are based on a combination of exact thresholds with respect to the current and sometimes also the lagged insured and total state unemployment rates, and thus not trivial to anticipate. Figure 1 shows the resulting variation in the maximum benefit durations across states and over time. The dashed horizontal line marks the baseline duration of 26 weeks. Each circle indicates that the potential benefit duration amounts to that particular number of weeks in at least one state.[8] As expected, most variation evolves during the recessions towards the beginning and the end of the sample period.

[8]Note that the circles are not weighted by the number of states in which the pontential benefit duration equals that particular number of weeks.

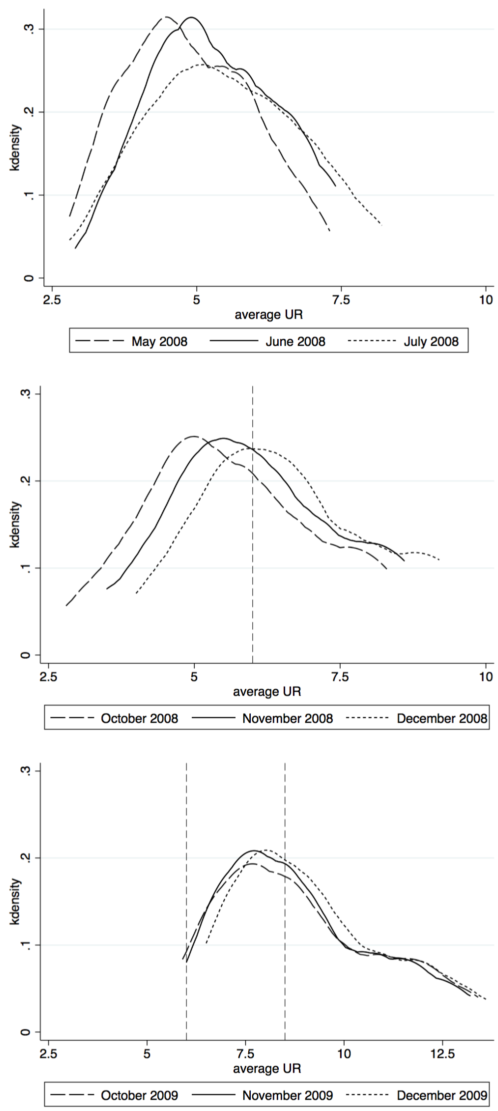
Figure 2: TUR Trigger Thresholds - EUC08



SOURCES. - U.S. Department of Labor and own calculations. NOTE. - The figure shows the mean monthly unemployment rate for the sample between 2007 and 2011 (solid line) as well as the 95% confidence interval bands (dotted lines). The dashed vertical lines mark the changes in the EUC08 benefit scheme (June 2008, November 2008 and November 2009). The dashed horizontal lines mark the trigger thresholds with respect to the total unemployment rate. See the text for further explanations.

The trigger thresholds in the unemployment rates are decided at the federal level. While the exact unemployment rates are not trivial to anticipate from the individual's perspective, one might be concerned about potential manipulation of the unemployment rates in order for a state to cross the threshold. In that case, one would expect a discontinuous jump in the average state unemployment rate around the changes in law or bunching of state unemployment rates in the state unemployment rate distribution just above the trigger threshold. Figure 2 shows the mean state unemployment rate between 2007 and 2011 as well as the 95% confidence interval bands. The first vertical line marks the introduction of EUC08 in June 2008 when all states triggered onto the policy independently of the unemployment rate. The second vertical line marks the introduction of a second tier with trigger thresholds in November 2008. States trigger onto the second tier of EUC08 if either the insured unemployment rate crosses the threshold of 4 percent or the total unemployment rate crosses the threshold of 6 percent (see Table 1 for more details). The latter case is represented by the lower horizontal line in Figure 2. While there is an increase in the mean unemployment rate just after the introduction of the trigger threshold, that increase appears to be smooth and due to an increasing trend in the midst of the Great Recession. The third vertical line marks the introduction of a third and fourth tier where the fourth tier is triggered by an insured unemployment rate above 6 percent or a total unemployment rate above 8.5 percent. The higher threshold is marked by the upper horizontal line in Figure 2. Again, there does not appear to be a discontinous jump in the average unemployment rate following the introduction of the trigger mechanism. The graphical evidence suggests that

Figure 3: UR Distribution and TUR Trigger Thresholds



SOURCES. - U.S. Department of Labor and own calculations. NOTE. - The figure shows the distribution of the average state unemployment rate in the months of, before and after changes in the EUC08 benefit scheme. The dashed vertical lines mark the trigger thresholds with respect to the total unemployment rate. See the text for further explanations.

there is no manipulation of the unemployment rate around the trigger thresholds.

Figure 3 shows the distribution of the average state unemployment rate in the months of, before and after changes in the EUC08 benefit scheme occurring during the sample period. If there was manipulation of the unemployment rate in order to cross the trigger threshold, one would expect the distribution of the unemployment rate to abruptly shift to the right and to change shape. The first of the three figures refers to the introduction of the EUC08 in June 2008. The distribution shifts to the right from May 2008 to June 2008 and from June 2008 to July 2008. This is in line with what one would expect during a recession period. The second of the figures shows the respective distributions for October 2008, November 2008 and December 2008. The vertical line marks the threshold in the unemployment rate in order to trigger onto tier 2. Again, the distribution shifts to the right from October 2008 to November 2008 and from November 2008 to December 2008. This is in line with what is discussed above: The shift to the right appears to be smooth and due to an increasing trend in the unemployment rate in the midst of the Great Recession, while there is no change in the shape of the distribution. The third figure shows the respective distributions for October 2009, November 2009 and December 2009. The vertical lines mark the threshold unemployment rates to trigger onto tier 3 and 4 of EUC08, respectively. There is no obvious shift in the distributions towards either of these thresholds. None of the figures show any evidence for discontinuous jumps in the unemployment rate or bunching in the distribution just above the trigger threshold. In the following, I thus assume that the threshold unemployment rate that a state has to pass in order to trigger onto the extension policy is exogenous to the state.

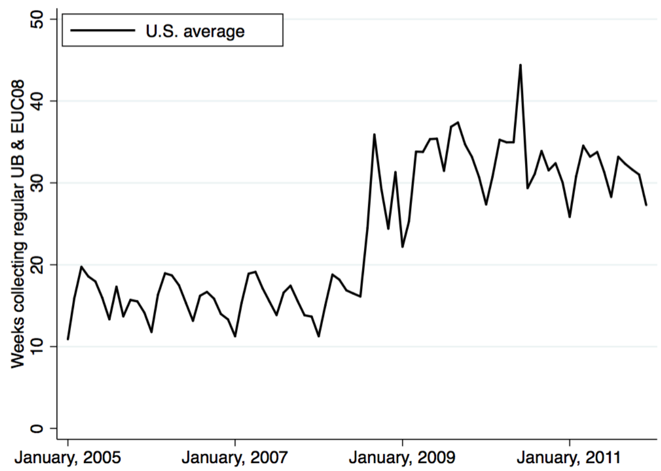
In order to be eligible for claiming EUC08 benefits, regular unemployment benefits have to be exhausted and the unemployed individual is required to have been in insured employment for at least 20 weeks or to have the equivalent in insured wages in the base period. If eligible, the EUC08 extended benefit amounts to the equivalent of the unemployed's regular weekly unemployment compensation scheme. States were not allowed to actively reduce unemployment compensation benefits through changes to benefit amounts. Alternatively, some states responded by reducing the baseline benefit period. In the data used in this study, these reductions in the baseline unemployment benefit period are taken into account.

Note that the potential benefit duration is likely to exceed the actual benefit duration in many cases. Figure 4 shows the national average duration of persons collecting unemployment insurance benefits. The benefits plotted in the graph include regular unemployment insurance as well as EUC08 extended benefits. Clearly, the *actual* benefit duration is shorter than the *potential* benefit duration. However, one can see a steep increase in the average actual benefit duration with the introduction of the extended benefits.

2.2 Crime Data

Crime data comes from the master files of the Uniform Crime Reporting programme, UCR hereafter. Since 1930, law enforcement agencies in the United States have been par-

Figure 4: Actual Benefit Durations - U.S. Average



SOURCES. - US Department of Labor and own calculations. NOTE. - The figure shows the monthly variation in the U.S. average duration of persons collecting unemployment insurance benefits between January 2005 and December 2011, where unemployment insurance here includes regular unemployment benefits as well as EUC08 benefits.

ticipating in gathering crime statistics through the Uniform Crime Reporting programme. The programme is administered by the FBI and participation is voluntary for all agencies. The UCR report the monthly number of arrests by state, age, gender and type of crime. Types of crime used in this study include the FBI categories for property crime (burglary, larceny, vehicle theft, arson) and violent crime (murder, rape, robbery, assault).

The age variable in the original data indicates single age years up to the age of 25, and five year brackets for ages above 25 (25-29, 30-34, etc.). I restrict the sample to the 16 to 39 year old population, reflecting typical crime demographics as well as potential labour market entry ages. Furthermore, I aggregate the crime data to the age groups 16 to 19-years old and 20 to 24-years old for those below the age of 25. The resulting sample is a monthly panel of the number of arrests by state, age group, gender and type of crime.

For a number of states, the arrest data is either systematically missing or not provided in every month within a year. States with systematically missing data are excluded from my sample.[9] Also excluded are states for which the arrest data covers less than 95% of the state population.[10] States with partly missing data are included for the non-missing time

[9]These states are: Georgia, Kentucky, Minnesota, New Mexico, North Dakota, Utah.

[10]There is no evidence that suggests that the excluded states differ systematically from the included states in terms of unemployment durations. These states are: Alabama, Arkansas, District of Columbia, Florida, Illinois, Indiana, Kansas, Louisiana, Mississippi, Montana, Nebraska, New Hampshire, New York, Ohio, Pennsylvania, South Dakota, Washington, West Virginia, Hawaii.

periods and excluded otherwise, leading to an unbalanced sample. Where it is appropriate, I impute single missing values using linear interpolation.

The arrest data is matched to population data in order to produce arrest rates. Population data is retrieved from the U.S. Census Bureau Population Estimates. The population is measured as the annual population by state, age and gender. I match the population data to the arrest data by year, state, age and gender, implicitly assuming that the population number is constant with respect to each month within a year.[11] Arrest rates are calculated as the number of arrests divided by the population count in the observational unit, and scaled by 100,000 for the ease of interpretation.

Here, criminality is measured by arrests. Of course, the number of arrests does not necessarily equal the number of crimes. First, not all arrestees are offenders. Yet, to my knowledge consistent monthly data at a similar observational level as the arrest data does not exist for the U.S. neither with respect to incarceration nor with respect to convictions.[12] Second, not all crimes lead to arrests. If a crime is not detected or not reported to the police, no arrest can be observed. Victimisation data is more informative in that respect, but again to my knowledge consistent monthly data at a similar observational level as the arrest data does not exist for the U.S. Furthermore, victimisation data is typically collected from surveys and hence is subject to its own type of measurement error. Nonetheless, I used data from the Bureau for Justice Statistics in order to compute correlation coefficients between annual victimisations and arrests per 1,000 U.S. population. As expected, the correlation is very high: 97% for property crime and 98% for violent crime.[13]

2.3 Unemployment Data

Unemployment data comes from the Current Population Survey, CPS hereafter. The CPS is a monthly cross-sectional survey of U.S. households conducted by the Bureau of Census for the Bureau of Labor Statistics. As such, the CPS provides information about the employment situation of surveyed individuals and households. The dataset contains information on the duration of an ongoing unemployment spell of the interviewed person as well as a large number of other labour market and demographic variables. The CPS is the basis for official unemployment statistics by the Bureau of Labor Statistics. Here, unemployment is measured according to the common definition by the International Labor

[11] To my knowledge, monthly population data is not available at the same level of observation. Thus, alternatives to this approach would be to either calculate arrest rates relative to total population numbers instead of observational cell specific population numbers, or to linearly interpolate population numbers for each month in one year.

[12] See for example Pfaff (2011).

[13] Note that if arrest rates are in constant proportion to true unobserved crime rates, the proportionality factor is an additive component of the logarithmic arrest rate. Further, if the unobserved proportionality factor is independent of the explanatory variable in the regression model, then the crime-arrest measurement problem is equivalent to the case of classical measurement error in the outcome variable which does not result in an estimation bias (see Imbens and Hyslop (2001)).

Table 2: Descriptive Statistics - Estimation Sample

Variable	Observations	Mean	S.D.
Arrest rate per 100,000, property crime	24,297	103.22	77.69
Arrest rate per 100,000, violent crime	24,297	117.22	79.81
Unemployment duration, in weeks	24,297	18.63	14.62
Potential benefit duration, in weeks	24,297	48.97	27.61
Unemployment rate:			
- state-level (BLS)	24,297	6.43	2.36
- observational unit-level(CPS)	24,297	9.97	7.58
Share of married individuals	24,297	0.37	0.27
Share of native individuals	24,297	0.86	0.10
Share of high-school graduates	24,297	0.78	0.24
Share of Black individuals	24,297	0.11	0.10

SOURCES. - UCR, CPS, BLS and own calculations. NOTE. - The table shows the number of observations, means and standard deviations of the named variables in the sample. The unit of observation is at the state, month, age group and gender level (averages).

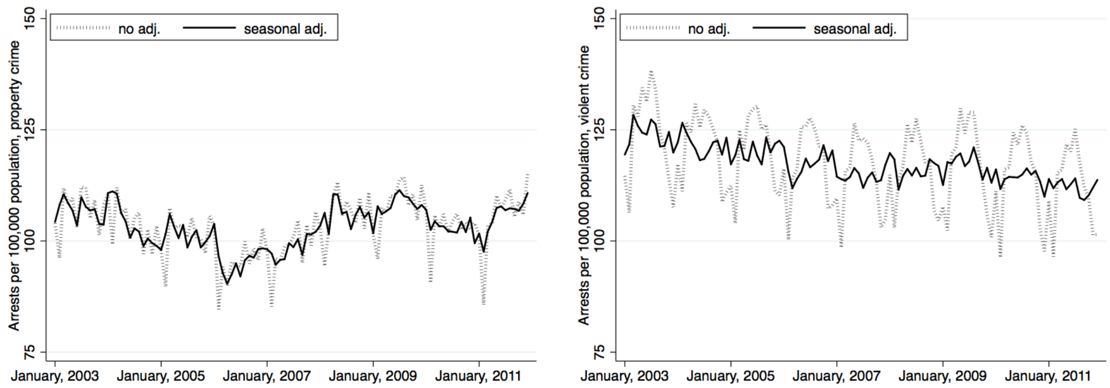
Organiszation (ILO) as being not employed, available and looking for work. Unemployment duration is measured as the elapsed duration of the ongoing unemployment spell.

In order to match the observational level of the arrest data, I compute unemployment rates and average unemployment durations as well as averages for compositional control variables by month, state, gender and age group. The unemployment rate is defined as the percentage of the labour force being unemployed. Compositional control variables include the share of married persons, the share of native born persons, the share of high-school graduates and the share of the Black population.

For this study, I calculate the average unemployment duration per observational unit where unemployment duration is measured as the length of the ongoing unemployment spell at the time of the interview. There are two main concerns with that type of data. On the one hand, reported unemployment spells are uncompleted, i.e. right-censored. Right-censoring implies that the average unemployment duration is underestimated. On the other hand, unemployment spells that occur between two interview dates are not observed. Thus, short unemployment durations are underrepresented which leads to length-biased sampling and to an overestimation of the average unemployment duration.[14] The empirical results are tested using the median instead of the average unemployment duration, but otherwise

[14]For a more detailed discussion of problems with the measurement of unemployment durations in the CPS see for example Kiefer et al. (1985) or Kiefer (1988).

Figure 5: Arrest Rate - Property and Violent Crime



SOURCES. - UCR and own calculations. NOTE. - The figures show the average arrest rate for property crime (left) and violent crime (right) per 100,000 population in the U.S. between 2003 and 2011 with (solid line) and without (dotted line) seasonal adjustment.

rely on the validity of the duration measurement.

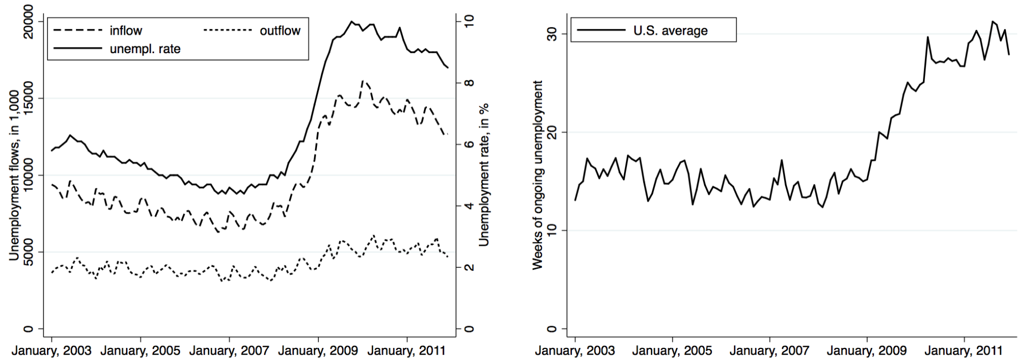
2.4 Sample Descriptives

The sample ranges from January 2003 to December 2011, and is restricted to the 16 to 39 year old population. That results in a sample of 24,297 data points at the state, month, age group and gender level. Table 2 provides sample summary statistics. The average number of arrests per 100,000 population amounts to 103 for property crime and 117 for violent crime. The average unemployment duration amounts to 18.6 weeks with a relatively high variation, compared to an average potential benefit duration of 48.97 weeks. The mean unemployment rate, averaged over the observational cells, is 9.97%. That unemployment rate is higher than the general BLS state unemployment rate, as the sample is restricted to younger age groups. In terms of compositional control variables, the average share of married persons in the sample is about 37%, the average share of native born persons is about 86%, the average share of high-school graduates is about 78% and the share of the Black population is about 11%.

Figure 5 shows the time trends of the U.S. average arrest rates with respect to property and violent crime. The time trends suggest an upwards trend in the property crime arrest rate from 2007 onwards.[15] For the violent crime arrest rates, no trend is evident from the graphical analysis. Note that the arrest rates shown in the graphs are averages over the

[15]Note that the sample period follows a longer period of a sharp decline in crime in the U.S. The trends seen in the graphs can be understood as a snapshot of long term trends, and do not offset the overall decline in crime over the last decades.

Figure 6: Unemployment Rate, Flows and Duration - U.S. Average



SOURCES. - BLS, CPS and own calculations. NOTE. - The left figure shows the average unemployment rate as well as the raw in- and outflows from unemployment in the U.S. between 2003 and 2011. The right figure shows the average state unemployment duration in weeks in the U.S. between 2003 and 2011.

states in the sample and do not necessarily reflect the state specific crime trends that are used in the regression analysis.

Figure 6 (left) shows the unemployment rate as well as the unemployment in- and outflows for the U.S. from 2003 to 2011. The graph shows a steep increase in the unemployment rate from 2008 onwards that coincides with an increase in the inflows into unemployment and with constant outflows from unemployment. Figure 6 (right) shows the average state unemployment duration as measured in weeks within the sample. Most strikingly, there is a very large increase during the Great Recession: While the average duration amounts to about 15 weeks at the beginning of the sample period, the graph shows an almost 15 week increase in the *average* unemployment duration. Two observations are particularly interesting here: First, the increase in average unemployment rates starts around January 2008 whereas the increase in average unemployment durations seems to start about one year later in January 2009. Second, while unemployment rates are elevated during an earlier recession at the beginning of the sample period, average unemployment durations do not seem to be similarly affected then as they are later during the Great Recession. These observations highlight the dramatic changes in the U.S. labour market during the last recession: While European labour markets have experienced long-term unemployment for much longer, this is a more recent phenomenon for the U.S.

3 Reduced Form: Unemployment Benefit Extensions

The reduced form estimates the link between the potential unemployment benefit duration and the crime rate, where the potential benefit durations vary over space and time according to the extension programmes. As discussed in section **??**, two competing mechanisms connect benefit extensions and criminal activity. On the one hand, benefit extensions decrease unemployment exit probabilities and at the margin increase the propensity to commit crime. On the other hand, the benefit extensions delay the negative income shock caused by benefit exhaustion and at the margin decrease the propensity to commit crime. The reduced form link between crime and unemployment benefit extensions is the sum of both effects and the sign of the total effect is to be determined empirically. In the following section, I first outline the empirical strategy and then discuss the results from the reduced form estimation.

3.1 Empirical Strategy

In order to estimate the reduced form effect of the unemployment benefit extensions on crime, I exploit variation in the benefit duration that stems from the variation across states with respect to the implementation of the extension programmes at different points in time. As described in more detail in section 2, the benefit extension trigger mechanisms are based on statewide total and insured unemployment rates: If there is a large inflow into unemployment, unemployment rates increase and eventually exceed pre-defined thresholds triggering extension policies. Moreover, benefit extensions are implemented at the state level. Local labour markets within a state are differently affected by economic conditions: The individual labour market experience is in general independent from the policy which averages over all local labour markets within that state. While the type of data I have does not allow me to allocate individuals to local labour markets other than at the state level, a similar - but due to autocorrelations somewhat weaker - argument can be made with respect to age and gender specific unemployment rates. Based on these arguments, I base the empirical strategy on the assumption that the policy is as good as randomly assigned *conditional* on the previous unemployment situation and hence can be treated as quasi-exogenous.[16]

I follow two approaches in order to estimate the reduced form. First, I estimate the effect of the benefit extension using policy dummies. Second, I estimate the effect of the maximum potential benefit extension using a continuous variable instead of policy dummies. Both approaches exploit variation in the benefit duration that stems from the variation across states with respect to the implementation of the extension programmes at different points in time.

In the following, let $\ln(\mathrm{CR})_{tsag}$ denote the logarithmic arrest rate for period t, state s, age group a and gender g. Further, let X_{tsag} be a matrix of observable characteristics to account for compositional differences between the data cells including the lagged unemployment rate $UR_{t-1,sag}$. Lastly, let α_s, α_g and α_a denote fixed effects for state, gender and age group, let $f(t_s)$ be a (monthly) state-time trend and ε_{tsag} the error term. The standard

[16] Note that while the framework might suggest a regression discontinuity design, such an empirical design is not feasible here due to data limitations.

errors of all models discussed below are clustered at the state level, and regressions are weighted by the population of the observational unit.

First, let E_{ts} be a dummy variable equal to 1 if a benefit extension is in place in state s at time t, and equal to 0 otherwise. In other words, $\mathrm{E}_{ts} = 0$ whenever the maximum potential benefit duration equals the baseline duration of 26 weeks. The variation in the policy dummy stems from the variation in the timing of the benefit extensions within and across states. The estimating equation can be written as:

$$\ln(\mathrm{CR})_{tsag} = b_0 + b_1 \mathrm{E}_{ts} + b_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \tag{1}$$

Second, let PBD_{ts} denote the maximum potential benefit duration in state s at time t. That measure includes both the baseline plus the extension benefit duration, and hence varies from 26 up to 99 weeks. The variation in the benefit durations stems from variation not only in the timing but also in the magnitude of the benefit extensions within and across states. Analogously to equation (1) the estimating equation can be written as:

$$\ln(\mathrm{CR})_{tsag} = \beta_0 + \beta_1 \mathrm{PBD}_{ts} + \beta_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \tag{2}$$

Both models (1) and (2) include fixed effects to take unobserved heterogeneity problems into account. These occur when unobserved, time-invariant differences between data cells are correlated with the variables of interest. In addition, I include a state-time trend to control for crime trends over time. I prefer that parametric specification over a more flexible approach using state-time fixed effects as the fixed effects approach removes much of the variation needed for identification. I test the parametric specification alternatively using no trend, a linear trend and a quadratic trend.

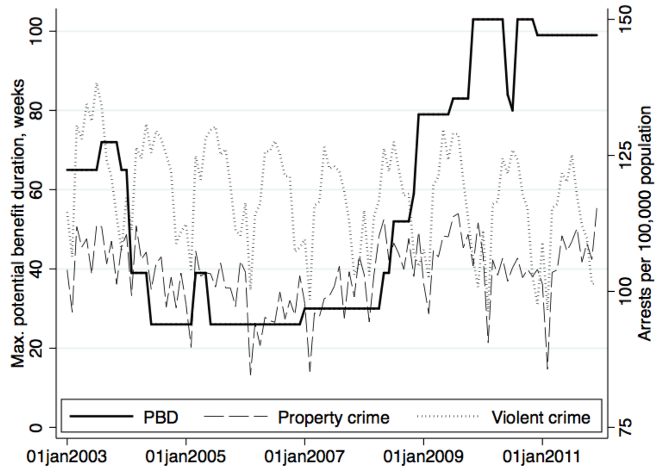
3.2 Baseline Results

Figure 7 illustrates the reduced form effect of potential benefit durations on the arrest rates for property and violent crime, respectively. The solid line shows the maximum potential benefit duration in the U.S. in the respective month while the dashed and the dotted lines show the trends in the U.S. property and violent crime arrest rates. The figure suggests that the increase in the property crime arrest rate in 2007 coincides with an increase in the potential benefit durations while there is no evident link for violent crime. Note, however, that the figure is based on U.S. averages and thus does not reflect variation across states which on the contrary is taken into account in the regression analysis.

The estimation results corresponding to equation (1) and (2) are shown in Table 3. Columns (1) to (4) show the results for total crime, columns (5) and (6) for property crime and columns (7) to (8) for violent crime. My preferred specification includes quadratic state-time trends and control variables as shown in columns (4), (6) and (8).

Panel A shows the results that correspond to equation (1) using the policy dummy. The results suggests a statistically significant increase in the total arrest rate by 4.4% when

Figure 7: Reduced Form - Potential Benefit Durations and Arrests



SOURCES. - U.S. Department of Labor, UCR and own calculations. NOTE. - The figure shows the maximum potential benefit duration in the U.S. between 2003 and 2011 (solid line) as well as the average arrest rate per 100,000 population for property crime (dashed line) and violent crime (dotted line).

the policy dummy is triggered on (column (4)). For property crime, the increase amounts to 3.9% and is statistically significant (column (6)); for violent crime to 3.7% and is only marginally statistically significant (column (8)). Panel B shows the results that correspond to equation (2) using the continuous measure of the maximum potential benefit duration. Note that for ease of interpretation, potential benefit duration is measured in months rather than in weeks here.[17] The results suggest that a one month increase in the potential benefit duration increases the total crime arrest rate by 0.3%, the property crime arrest rate by 0.4% and has no significant impact on the violent crime arrest rate. For property crime, that translates into an increase in the arrest rate by 1.24 arrests per 100,000 population at the mean (103 arrests) if the potential benefit duration was extended by 12 weeks. The results for both specifications using the two different policy variables are robust to the inclusion or exclusion of the control variables as well as the different trend specifications.

3.3 Placebo Test

The reduced form estimation suggests that the benefit extensions are associated with an increase in crime. Although this can be rationalised by the notions above, one might be concerned that the effect of the unemployment benefit extensions on crime is confounded with underlying and unobserved factors that correlate with the recession period. Hence, I

[17] A month is defined as four weeks.

Table 3: Reduced Form and Placebo Test

Y =	(1) ln(CR)	(2) ln(CR)	(3) ln(CR)	(4) ln(CR)	(5) ln(PCR)	(6) ln(PCR)	(7) ln(VCR)	(8) ln(VCR)
		Total crime rate.			Property crime rate.		Violent crime rate.	
	Panel A: Full sample, extension dummy.							
Extension (dummy)	0.049*** (0.013)	0.043*** (0.008)	0.045*** (0.014)	0.044*** (0.015)	0.039** (0.015)	0.039** (0.015)	0.038** (0.018)	0.037* (0.019)
	Panel B: Full sample, months of extension.							
PBD (in months)	0.003* (0.002)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.001 (0.001)	0.001 (0.001)
	Panel C: 1980s sample, placebo test.							
PBD^{GR} (in months)			-0.000 (0.005)		-0.003 (0.002)		0.000 (0.009)	
State, age, gender F.E.	yes	yes	yes	yes	yes	yes	yes	yes
Linear state trend t	no	yes	no	no	no	no	no	no
Quadr. state trend t, t^2	no	no	yes	yes	yes	yes	yes	yes
Lagged U Rate	yes	yes	yes	yes	yes	yes	yes	yes
Control var.	no	no	no	yes	no	yes	no	yes
Sample size A	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037
Sample size B	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037
Sample size C			3,790		3,790		3,790	

SOURCES. - CPS, UCR and own calculations. NOTE. - Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The dependent variables are the logarithmic arrest rates for total, property and violent crime, where the arrest rate is computed as the number of arrests per 100,000 population. The independent variable E is an extension dummy and PBD is the potential benefit duration in months. Panel A and B: January 2003 - December 2011. Panel C: May 1981 - November 1982. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1.

conduct a placebo analysis in order to assess the validity of the above findings. In particular, I match the benefit extensions during the Great Recession to an earlier, and in terms of unemployment rates comparable recession at the beginning of the 1980s. The matching is based on months into the recession. That allows me to estimate the effect of "fake" benefit extensions during a recession that is similar to the Great Recession but without such extension policies taking place at the time. The estimating equation for the placebo test can then be written as follows, where ER designates the earlier recession in the 1980s and GR the Great Recession:

$$\ln(\text{CR})_{tsag}^{ER} = \lambda_0 + \lambda_1 \text{PBD}_{ts}^{GR} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \tag{3}$$

If the reduced form effect of the benefit extensions on crime, β_1 was driven by underlying factors related to the business cycle, one would expect the "fake" benefit extension to have a similar impact λ_1 on crime during the 1980s recession, too. In other words, one would expect $\hat{\beta}_1$ and $\hat{\lambda}_1$ to be not statistically different one from the other.

Panel C in Table 3 shows the results for the placebo analysis. The placebo coefficient is not significantly different from zero for either of the crime types. The estimated placebo coefficient is not within the 95% confidence interval of the full sample estimated coefficient neither for total nor for property crime. The findings of the reduced form as well as the placebo analysis suggest that indeed there is a reduced form effect of the potential unemployment benefit duration on property crime. For violent crime, the results are more ambiguous. In the next section, I suggest and analyse potential labour market mechanisms that can rationalise these results.

4 Mechanisms: Unemployment (IV)

As discussed above, the overall reduced form effect is the sum of competing disincentive (duration) and income effects. Finding that positive reduced form coefficients, $\hat{b}_1 > 0$ and $\hat{\beta}_1 > 0$ suggests that the disincentive is the predominant effect and outweighs the income effect. That implies that, at the margin, individuals are disincentivised by longer benefit durations with respect to job search, remain unemployed and become more likely to commit crime than they would have been otherwise. In the following, I study that mechanism by analysing the impact of unemployment and unemployment duration on crime in an instrumental variable framework using the potential benefit durations as an instrument. First, I outline the empirical strategy and discuss the IV framework. Second, I discuss the results from the first stage and two stage least square estimation.

4.1 IV Strategy

Same as in the reduced form framework, let $\ln(\text{CR})_{tsag}$ denote the logarithmic arrest rate for period t, state s, age group a and gender g. Further, let UR_{tsag} denote the unemployment rate for period t, state s, age group a and gender g, defined as the percentage of the labour force who are unemployed per observational cell. Similarly, let UD_{tsag} be the average unemployment duration for period t, state s, age group a and gender g, measured in weeks. In the following, let $\mathrm{U}_{tsag} \in \{\mathrm{UR}_{tsag}, \mathrm{UD}_{tsag}\}$ denote either of these two unemployment variables. As above, X_{tsag} is a matrix of observable characteristics to account for compositional differences between the data cells which includes the lagged unemployment rate $\mathrm{UR}_{t-1,sag}$. Again, let α_s, α_g and α_a denote fixed effects for the state, gender and age group, let $f(t_s)$ be a quadratic state-time trend, and ε_{tsag} the error term. Standard errors are clustered at the state level, and regressions are weighted by the population of the observational unit. A simple fixed effects model estimating the correlation between unemployment and crime can be written as:

$$\ln(\mathrm{CR})_{tsag} = \theta_0 + \theta_1 \mathrm{U}_{tsag} + \theta_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \tag{4}$$

The fixed effects take into account that unobserved heterogeneity between the data cells might be correlated with the variables of interest. For example, if differences between the states systematically affect crime and labour market attitudes, one state may display higher crime rates and worse labour market conditions independent of the effect of interest for this study. Still, the simple fixed effects model suffers from reversed causalities. In particular, one might be concerned about a recession bias (Cook and Zarkin, 1985, Raphael and Winter-Ebmer, 2001). Recessions not only affect labour markets but also the quality and quantity of criminal opportunities: Potential victims for example have less income and consume less during recessions than they would do during more buoyant times. If that is the case, then there is procyclical variation in criminal opportunities which creates a downward bias in the estimates of the impact of the (tautologically procyclical) unemployment rate and crime.[18] Exploiting the variation in the timing and the magnitude of unemployment benefit durations (as discussed in greater detail in the previous sections) in an instrumental variable framework not only allows me to study unemployment as a mechanism of the reduced form effect but also addresses the identification problem of the fixed effect model.

The first stage of the IV model links the observational cell unemployment rate and unemployment duration, respectively, to the benefit extension in state s at time t. In order to estimate the first stage effect of the benefit extensions, I follow a similar approach to the reduced form analysis: First, I estimate the effect of the benefit extensions being in place using policy dummies and second, I exploit the magnitude of the extensions by using a continuous variable indicating the maximum potential benefit duration. The corresponding

[18] In general, there might be concerns over and above these reflections. First, individuals might choose to participate in crime based on unobservable characteristics which correlate with equally unobservable characteristics determining unemployment. If the unobservable characteristics are positively correlated with unemployment and are also positively correlated with participation in criminal activity, the fixed effects model is upwards biased. Second, firms choose locations. If crime rates in a local labour market are high, firms may not enter that market but choose different local labour markets with lower crime rates. In that case, the existing crime rate impacts on the current and future local labour market conditions.

first stage equations can be written as:

$$\mathrm{U}_{tsag} = c_0 + c_1\mathrm{E}_{ts} + c_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \quad (5)$$
$$\mathrm{U}_{tsag} = \gamma_0 + \gamma_1\mathrm{PBD}_{ts} + \gamma_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \quad (6)$$

As outlined before, benefit extensions decrease unemployment exit probabilities. While the benefit extensions are enacted as a response to an increasing inflow into unemployment, that implies that the relative outflow from unemployment decreases and both the unemployment rate and the unemployment duration increase as a consequence. Indeed, that is what can be observed in the data as discussed above and illustrated in figure 6.

Using the same notation as above, the second stage of the two-stage least squares model can be written as follows:

$$\ln(\mathrm{CR})_{tsag} = \delta_0 + \delta_1\hat{\mathrm{U}}_{tsag} + \delta_2 X_{tsag} + \alpha_s + \alpha_g + \alpha_a + f(t_s) + \varepsilon_{tsag} \quad (7)$$

The instrumental variable model relies on the same set of assumptions concerning the exogeneity of the benefit extension trigger mechanisms as the reduced form model (see discussion above). In addition, the exclusion restriction requires potential benefit durations to be exogenous with respect to crime rates, i.e. unemployment to be the only mechanism through which the unemployment benefit extensions affect crime. The main threat to identification here is the concern that potential benefit durations and crime both directly correlate with recessions, in which case the exogeneity assumption would be violated and one would still expect a recession bias.[19] The empirical model captures the crime response to a change in legitimate opportunities. Yet, criminal opportunities may increase during recessions if the government decreases spending on the criminal justice system. The placebo test in the reduced form analysis suggests that these concerns do not hold. Nonetheless, I discuss these potential threats to identification later in the paper and provide empirical tests where possible.

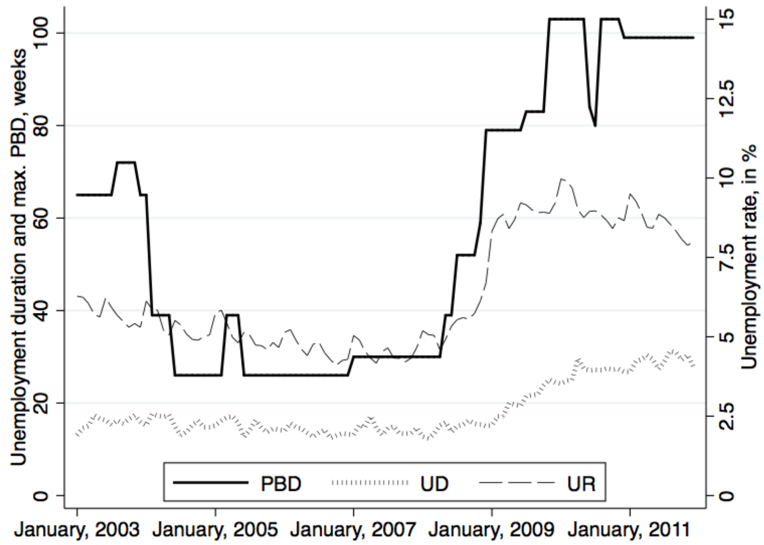
In the exactly identified case, the two-stage least square estimator is equivalent to the indirect least square estimator. Hence, it equals the ratio of the reduced form coefficient on the instrument to the first stage coefficient:

$$\delta_1 = \frac{\beta_1}{\gamma_1} \quad (8)$$

The sign of δ_1 hence depends both on the sign of the reduced form and the first stage coefficients β_1 and γ_1. Given that $\hat{\beta}_1 > 0$, one expects that $\hat{\delta}_1 > 0$ if $\hat{\gamma}_1 > 0$ and $\hat{\delta}_1 < 0$ if $\hat{\gamma}_1 < 0$.

[19] See for example Cook and Zarkin (1985) for a discussion of four possible linkages between crime and business cycles: Legitimate opportunities, criminal opportunities, use of criminogenic commodities and the criminal justice system response to crime.

Figure 8: First Stage - Potential Benefit Durations and Unemployment



SOURCES. - U.S. Department of Labor, BLS, CPS and own calculations. NOTE. - The figure shows the maximum potential benefit duration in the U.S. between 2003 and 2011 (solid line) as well as the average unemployment duration in weeks (dotted line) and the average state unemployment rate in percent (dashed line).

4.2 First Stage Results

The first stage of the model estimates the link between unemployment and the extension policy and the potential benefit duration, respectively, as described in equations (5) and (6). To reiterate, the variation in the benefit extension stems from the variation in the timing and the magnitude of the benefit extensions within and across states. Increases in the state unemployment rate trigger the benefit extension, while the trigger thresholds as well as the magnitude of the extension are determined by federal law. Changes in federal legislation lead to non-smooth changes in potential benefit durations over time.

Figure 8 illustrates the first stage mechanism: An inflow into unemployment increases the unemployment rate. If the unemployment rate exceeds the pre-defined trigger threshold, the unemployment benefit period in that state is extended based on federal legislation. Yet, the benefit extensions decrease unemployment exit probabilities and relative outflows from unemployment. That is reflected by an increase in the average unemployment duration, and a subsequent increase in the unemployment rates. Conditional on the previous unemployment rate, the current unemployment rate thus depends - inter alia - on the benefit extension and the benefit duration.

Panel A of Table 4 shows the results corresponding to the first stage estimating equa-

Table 4: First Stage

Y =	(1) OLS U Rate	(2) OLS U Rate	(3) OLS U Duration	(4) OLS U Duration
	Panel A: Extension dummy.			
Extension	1.405***	1.452***	-0.510	-0.491
(dummy)	(0.122)	(0.127)	(0.513)	(0.512)
	Panel B: Weeks of extension.			
PBD	0.047***	0.047***	0.095***	0.095***
(in weeks)	(0.005)	(0.004)	(0.014)	(0.014)
State, age, gender F.E.	yes	yes	yes	yes
Quadr. state trend t, t^2	yes	yes	yes	yes
Lagged UR	yes	yes	yes	yes
Control var.	no	yes	no	yes
Sample size	24,037	24,037	24,037	24,037

SOURCES. - CPS and own calculations. NOTE. - Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, and the share of black population. Fixed effects for the state, age group and gender are included. The dependent variable is the unemployment rate computed as the number of unemployed individuals as a percentage of the labour force in the observational unit or the average ongoing unemployment duration in the observational unit. The instrument PBD is the potential benefit duration, measured in weeks. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1.

tion (5) using the extension dummy. Columns (1) and (2) report the results with respect to the current unemployment rate, columns (3) to (4) with respect to the average unemployment duration. Columns (2) and (4) are the preferred specifications including the full set of control variables.[20] I find large F-Statistics above 100 for all specifications. The results suggest that the observational cell unemployment rate significantly increases by 1.5 percentage points when a benefit extension is in place. That compares to a typical increase in the unemployment rate by 5 percentage points during a recession. No statistically significant effect is found for the average unemployment duration.

As discussed before, the potential benefit duration is the more interesting measure of the benefit extensions as it allows for additional variation within and across states. The results

[20]Following the discussion in the reduced form analysis, I use the state-trend specification in the IV framework. The fixed effects estimation results are available upon request.

corresponding to the first stage estimating equation (6) are shown in Panel B of Table 4. The results suggest that the observational cell unemployment rate significantly increases by 0.05 percentage points with a one week increase in the potential benefit duration. In terms of the unemployment duration, the results suggest that the average unemployment duration significantly increases by 0.1 weeks with a one week increase in the potential benefit duration. If, as in a previous example, the potential benefit duration is extended by 12 weeks, that translates into a 1.2 week increase in the average (ongoing) unemployment duration.[21]

4.3 IV Baseline Results

Based on the results both from the reduced form and from the first stage estimation, I estimate a two-stage least square model as described by estimating equation (7) and restrict the analysis to using the potential benefit duration as an instrument. The results both for the unemployment rate and the average unemployment duration are discussed in the following.

Unemployment Rate

Table 5 shows the results corresponding to the estimating equation (7) when U_{tsag} is the observational cell unemployment rate. Panel A shows the results for the total crime rate, panel B for the property crime rate and panel C for the violent crime rate. The results for the preferred specification, the two stage least square model with the full set of control variables, are shown in columns (3), (6) and (9), respectively.

The OLS estimations yield consistent results across the three different crime types: A one percentage point higher unemployment rate is associated with a 0.2 per cent lower arrest rate for total, property and violent crime. Whilst statistically significant, these associations do not have a causal interpretation due to the potential biases outlined above. Indeed, confirming the arguments above, the results reveal a substantial downward bias of the OLS compared to the 2SLS estimations.

The 2SLS estimations yield significant positive estimated coefficients for total crime and property crime, but not for violent crime. A one percentage point increase in the current cell unemployment rate increases the arrest rate for property crime by 2 per cent. That translates into an increase of about 2 arrests at the mean monthly arrest rate for property crime for a one percentage point increase in the unemployment rate. The estimate is statistically significant at the 1% level. The results are in line with the arguments above: Higher unemployment rates are linked to an increase in property crime while the corresponding semi-elasticities for violent crime are not significantly different from zero.

[21] Note that these results are within the range of estimates found in the literature.

Table 5: Unemployment Rate - Baseline

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) 2SLS
	Panel A. Total crime rate.			Panel B. Property crime rate.			Panel C. Violent crime rate.		
Y =	ln(CR)	ln(CR)	ln(CR)	ln(PCR)	ln(PCR)	ln(PCR)	ln(VCR)	ln(VCR)	ln(VCR)
U Rate	-0.002***	0.016***	0.016***	-0.002**	0.020***	0.020***	-0.002***	0.008	0.007
	(0.001)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)
Share married	-0.252***		-0.256***	-0.237***		-0.232***	-0.245***		-0.258***
	(0.065)		(0.080)	(0.074)		(0.078)	(0.078)		(0.087)
Share native	0.106		-0.002	0.087		-0.033	0.182**		0.118
	(0.081)		(0.088)	(0.133)		(0.132)	(0.066)		(0.075)
Share high-school	0.007		0.092	-0.062		0.028	0.009		0.069
	(0.081)		(0.080)	(0.118)		(0.110)	(0.071)		(0.066)
Share Black	-0.191*		-0.317***	-0.228		-0.376**	-0.027		-0.097
	(0.096)		(0.106)	(0.163)		(0.169)	(0.088)		(0.099)
Lagged U Rate	-0.003***	-0.010***	-0.010***	-0.002***	-0.011***	-0.011***	-0.003***	-0.006*	-0.006*
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)
State, age, gender F.E.	yes	yes	yes	yes	yes	yes	yes	yes	yes
Quadr. state trend t, t^2	yes	yes	yes	yes	yes	yes	yes	yes	yes
Lagged U Rate	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control var.	yes	no	yes	yes	no	yes	yes	no	yes
Sample size	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037

SOURCES. - CPS, UCR and own calculations. NOTE. - Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, and the share of black population. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for total crime (panel A), property crime (panel B) and violent crime (panel C). The arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Unemployment Duration - Baseline

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) 2SLS
	Panel A. Total crime rate.			Panel B. Property crime rate.			Panel C. Violent crime rate.		
Y =	ln(CR)	ln(CR)	ln(CR)	ln(PCR)	ln(PCR)	ln(PCR)	ln(VCR)	ln(VCR)	ln(VCR)
U Duration	0.0004***	0.008***	0.008***	0.001***	0.010***	0.010***	0.0003**	0.004	0.004
(in weeks)	(0.0001)	(0.002)	(0.002)	(0.0002)	(0.003)	(0.003)	(0.0001)	(0.003)	(0.003)
Share married	-0.251***		-0.218***	-0.235***		-0.184**	-0.244***		-0.240***
	(0.067)		(0.076)	(0.074)		(0.088)	(0.080)		(0.079)
Share native	0.095		0.092	0.078		0.083	0.171**		0.162**
	(0.082)		(0.083)	(0.134)		(0.127)	(0.068)		(0.070)
Share high-school	0.015		0.071	-0.054		0.003	0.016		0.060
	(0.082)		(0.073)	(0.117)		(0.109)	(0.073)		(0.064)
Share Black	-0.209**		-0.264***	-0.243		-0.311*	-0.0420		-0.072
	(0.098)		(0.102)	(0.165)		(0.165)	(0.091)		(0.094)
Lagged U Rate	-0.004***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.004***	-0.003**	-0.003***
	(0.001)	(0.001)	(0.001)	(0.0008)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State, age, gender F.E.	yes	yes	yes	yes	yes	yes	yes	yes	yes
Quadr. state trend t, t^2	yes	yes	yes	yes	yes	yes	yes	yes	yes
Lagged U Rate	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control var.	yes	no	yes	yes	no	yes	yes	no	yes
Sample size	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037	24,037

SOURCES. - CPS, UCR and own calculations. NOTE. - Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, and the share of black population. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for total crime (panel A), property crime (panel B) and violent crime (panel C). The arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1.

Unemployment Duration

The observations and arguments made above naturally lead to the question how the increase in unemployment durations in the U.S. labour market is linked to crime, or in other words to what extend the link between unemployment and crime is duration dependent. Table 6 shows the results corresponding to the estimating equation (7) when U_{tsag} is the observational cell average ongoing unemployment duration. Panel A shows the results for the total crime rate, panel B for the property crime rate and panel C for the violent crime rate. The results for the preferred specification, the two stage least square model with the full set of control variables, are shown in columns (3), (6) and (9), respectively.

The OLS specification yields significant positive, but small associatons between the average unemployment duration and the arrest rates for all three different crime types. As for the previous case, the OLS estimates are substantially downward biased compared to the 2SLS estimations. Similar to the case of unemployment rates, the 2SLS estimations yield significant positive estimated coefficients for total as well as for property crime, but not for violent crime. A one week increase in the average ongoing unemployment duration increases the arrest rate for property crime by 1 per cent. That translates into an increase of about 1 arrest at the mean monthly arrest rate for property crime for a one week increase in the average ongoing unemployment duration. The estimate is statistically significant at the 1% level. The results are in line with the previous findings: Longer unemployment durations same as higher unemployment rates are linked to an increase in the arrest rate for property crime but not for violent crime.

The baseline results refer to the average ongoing unemployment duration. Table 7 shows the results when I use the median instead of the average ongoing unemployment duration (Panel A). Using the median instead of the average duration avoids some concerns that occur with censored unemployment durations. The 2SLS results are robust to the baseline specification in terms of magnitude and statistical precision across all three crime types.

The estimation results so far refer to the *average* effect of the unemployment duration on the arrest rates for the different crime types. Compared to the estimation results of the OLS model, the magnitudes of the coefficients in the 2SLS estimation are substantially larger. The direction of the bias is consistent with the arguments made before as well as with the direction of the bias found in the literature on unemployment and crime. In terms of the magnitude of the bias, it is likely that the instrument captures a local effect: The instrument particularly affects individuals with longer unemployment spells which may in return lead to substantially higher results than found in the OLS model. That is in line with the notion that the effect may be larger for more vulnerable population groups and in particular for those who have been unemployed for longer.

In order to better understand that type of duration dependence, one is interested in the effect of unemployment duration on crime at different points along the duration distribution. Panel B of Table 7 shows the results when the sample is split into subsamples depending on whether the average unemployment duration is shorter than the baseline benefit duration,

Table 7: Unemployment Duration - Specification Tests

Y =	(1) 2SLS ln(CR)	(2) 2SLS ln(PCR)	(3) 2SLS ln(VCR)	Sample Size		(4) 2SLS ln(CR)	(5) 2SLS ln(PCR)	(6) 2SLS ln(VCR)	Sample Size
	Panel A. Specification test - median duration.					Panel B. Duration dependence - intervals.			
U Dur.	0.008***	0.010***	0.004	24,037	U Dur. < baseline PBD	0.013***	0.015***	0.008***	18,386
	(0.003)	(0.003)	(0.003)			(0.003)	(0.003)	(0.003)	
					U Dur. > baseline PBD	-0.040	0.002	-0.072	3,940
						(0.026)	(0.010)	(0.047)	
					U Dur. > max. PBD	-0.0009	-0.002	0.000	1,711
						(0.001)	(0.002)	(0.001)	
	Panel C. Duration dependence - quintiles.					Panel D. Type of Unemployment.			
U Dur.: Q1	0.051	0.069	0.053	4,791	U Dur.: job losers	0.007**	0.010***	0.003	19,492
	(0.090)	(0.113)	(0.150)			(0.003)	(0.003)	(0.003)	
U Dur.: Q2	0.088	0.082	0.060	4,788	U Dur.: job leavers	0.008**	0.010**	0.004	9,577
	(0.073)	(0.080)	(0.076)			(0.003)	(0.004)	(0.003)	
U Dur.: Q3	0.132***	0.192***	0.044	4,793	U Dur.: new entrants	0.009**	0.011**	0.004***	7,280
	(0.046)	(0.063)	(0.034)			(0.003)	(0.004)	(0.002)	
U Dur.: Q4	0.058***	0.065**	0.029**	4,820	U Dur.: re-entrants	0.006***	0.008***	0.002	17,536
	(0.020)	(0.027)	(0.014)			(0.002)	(0.002)	(0.002)	
U Dur.: Q5	0.013*	-0.001	0.022*	4,845					
	(0.007)	(0.006)	(0.012)						
State, age, gender F.E.	yes	yes	yes			yes	yes	yes	
Quadr. state trend t, t^2	yes	yes	yes			yes	yes	yes	
Lagged U Rate	yes	yes	yes			yes	yes	yes	
Control var.	yes	yes	yes			yes	yes	yes	

SOURCES. - CPS, UCR and own calculations. NOTE. - See table notes in Table 6 and further explanations in the text. *** p<0.01, ** p<0.05, * p<0.1.

longer than the baseline benefit duration but shorter than the extended benefit duration, or longer than the extended benefit duration. For all three types of crime, an increase in the average unemployment duration is linked to a significant increase in the arrest rate for the first subsample, i.e. when the average duration is shorter than the baseline benefit duration. The magnitude of the estimated semi-elasticity for property crime is not significantly different from the whole sample estimation result, whereas for violent crime it is larger and more precisely estimated. For the other two subsamples, the estimated coefficients are not significiantly different from zero. The first group includes individuals who are at the margin of exhausting regular benefits and hence are the most likely to be affected. Sample sizes decrease drastically for the last two categories compared to that group which is likely to affect the statistical precision of the estimates.

An alternative to study the effect on crime along the unemployment duration distribution is to split the sample into subsamples according to the quintiles of the distribution.[22] The corresponding results are shown in Panel C of Table 7. The quintiles range from 0 to 7 weeks average unemployment duration, from 7 to 12 weeks, from 12 to 18 weeks, from 18 to 28 weeks, and from 28 weeks and more. The results suggest that the semi-elasticities of the arrest rate for property crime with respect to the average unemployment duration are positive and significant for the third and the fourth quintile, but not significantly different from zero neither for the two lowest quintiles nor for the highest quintile. A one week increase in the average ongoing unemployment duration is linked to an increase in the arrest rate for property crime by 19 per cent in the third quintile, and by 6.5 per cent in the fourth quintile. For violent crime, I find that the semi-elasticities of the arrest rate with respect to the average unemployment duration are positive and significant for the two highest quintiles, but not significantly different from zero for the lower three quintiles. Here, a one week increase in the average ongoing unemployment duration is linked to an increase in the arrest rate for violent crime by about 3 per cent in the fourth quintile and by about 2 per cent in the highest quintile. The results suggest that the initial effect of unemployment on crime occurs only after a some time spent in unemployment. For property crime, the effect seems to be persistent for longer unemployment durations but fades away with very long-term unemployment. For violent crime, the initial effect is delayed compared to property crime but is more persistent for the very long-term unemployed. The differences between the different crime types are in line with the potential mechanisms: While property crime is likely to be driven by the income shock related to unemployment as well as opportunities and time availability, violent crime might be driven by behavioural responses to unemployment.[23]

[22]The main advantage of that approach is that the subsamples are of similar size which addresses some of the statistical power issues of the previous approach. The main disadvantage is that splitting the sample into subsamples does not take compositional effects into account: While one state might be in the lowest quintile in one month, the same state might be in another quintile in another month. When interpreting the results, that has to be kept in mind.

[23]See for example the discussion on the income argument in Becker (1968), the idleness time argument

Are the differences between the crime types related to the type of unemployment? Panel D of Table 7 shows the estimation results separately by type of unemployment. The average unemployment duration here is the average unemployment duration by type of unemployment: Job losers, job leavers, re-entrants and new entrants. The semi-elasticities of the arrest rate for property crime with respect to the average unemployment duration are positive, statistically significant and similar in magnitude across all types of unemployment. Job losers are directly affected by the instrument, they fulfill at least one of the eligibility conditions to receive extended unemployment compensation. Other groups of unemployed are affected by externalities of the longer benefit durations: Higher unemployment rates and longer unemployment durations increase the competition for jobs. That lowers the relative returns to legal activity, and at the margin pushes individuals into the crime market instead. The semi-elasticity of the arrest rate for violent crime with respect to the average unemployment duration is positive and statistically significant for new entrants but not for any other group of unemployment. That is in line with typical crime demographics and the results by age group as discussed further below.

4.4 Further Results

So far, I have discussed the baseline results and specification tests for the IV strategy. In the following, I present additional tests for potential confounders that could invalidate the IV strategy as well as a discussion of heterogeneity results across gender and age groups.

Potential Counfounders

As briefly discussed above, one might be concerned about potential confounding factors that could invalidate the instrumental variable strategy. The two main threats to identification concern changes in the law enforcement system that confound the effect of unemployment and unemployment duration on crime, as well as a direct effect of the unemployment benefits on crime. Governments could react to increased expenditure for the unemployment insurance system caused by higher unemployment rates by changes in the law enforcement system. If they decided to cut down expenditure on the police force as a response to the unemployment benefit extensions, the potential benefit durations would impact on crime other than through the labour market mechanisms discussed above.

I empirically test that concern using two approaches. First, I estimate the reduced form effect of the potential benefit duration on the number of police employees per capita.[24] The IV strategy might be invalid if one found a negative reduced form effect between the size of the police force and the potential benefit durations. Second, I estimate the IV model including a control variable for the ratio of police employees per capita. While that approach

in Grönqvist (2011) or the frustration and anger argument in Angew (1992).

[24]The number of police employees per capita is calculated as the ratio of police employees per 1,000 population per state and year.

Table 8: Robustness Checks - Police and Benefit Income

Y =	(1) OLS Police per cap.	(2) 2SLS ln(CR)	(3) 2SLS ln(CR)	(4) 2SLS ln(PCR)	(5) 2SLS ln(PCR)	(6) 2SLS ln(VCR)	(7) 2SLS ln(VCR)
	Panel A. Police per capita.						
PBD	0.0009**						
	(0.0004)						
U Rate		0.015***		0.019***		0.007	
		(0.006)		(0.006)		(0.006)	
U Duration			0.007***		0.009***		0.003
			(0.002)		(0.003)		(0.002)
Police per cap.		0.129***	0.187***	0.120**	0.191***	0.127***	0.152***
		(0.030)	(0.051)	(0.058)	(0.063)	(0.046)	(0.056)
	Panel B. Income control - U Rate.						
U Rate		0.023***	0.025***	0.027***	0.030***	0.016*	0.017*
		(0.007)	(0.008)	(0.007)	(0.009)	(0.010)	(0.010)
Income			0.000***		0.000**		0.000
			(0.000)		(0.000)		(0.000)
Welfare			-0.0002***		0.0004***		-0.0004***
			(0.000)		(0.0001)		(0.0001)
U Benefits			0.000		0.000		0.000
			(0.000)		(0.000)		(0.000)
	Panel C. Income control - U Duration.						
U Duration		0.015***	0.029**	0.017***	0.034**	0.010*	0.019*
		(0.005)	(0.011)	(0.004)	(0.014)	(0.006)	(0.011)
Income			0.000***		0.000**		0.000*
			(0.000)		(0.000)		(0.000)
Welfare			-0.0006***		-0.0001		-0.0007***
			(0.0002)		(0.0003)		(0.0002)
U Benefits			-0.0004**		-0.0004**		-0.0002
			(0.0002)		(0.0002)		(0.0001)
State, age, gender F.E.	yes	yes	yes	yes	yes	yes	yes
Quadr. state trend t, t^2	yes	yes	yes	yes	yes	yes	yes
Lagged U Rate	yes	yes	yes	yes	yes	yes	yes
Control var.	yes	yes	yes	yes	yes	yes	yes
Sample size A	24,037	24,037	24,037	24,037	24,037	24,037	24,037
Sample size B		2,080	2,080	2,080	2,080	2,080	2,080
Sample size C		2,080	2,080	2,080	2,080	2,080	2,080

SOURCES. - CPS, UCR and own calculations. NOTE. - Panel A: Police per capita measures the ratio of police employees to the population per state and year. Panel B and panel C: The income control variables include average income, welfare income and unemployment benefit income. The sample is reduced to the CPS March sample only due to data availability limitations. For all other variables, see table notes in Table 5 and Table 6 as well as further explanations in the text. *** p<0.01, ** p<0.05, * p<0.1.

allows me to test the robustness of the baseline IV results to the inclusion of that control variable, the ratio of police employees per population is arguably an endogenous control variable and thus the results should not be interpreted over and above that robustness test.

The estimation results for both approaches are shown in Panel A of Table 8. Column (1) corresponds to the reduced form estimation. The results display a significant positive association between the potential benefit duration and the police per capita ratio, however ever so small in magnitude and very close to zero. If governments did respond to the policy with changes in the law enforcement system, one would expect to find a negative coefficient in that specification. The results suggest that one can rule out that concern. Finding a significant positive effect suggests - if anything - that governments might respond to increased crime rates. Yet, the usual endogeneity concerns about policing and crime apply and no further conclusion can be made from that empirical test. Columns (2) to (7) of Panel A of Table 8 show the results of the 2SLS estimation for the different crime types both for the unemployment rate and the unemployment duration when controlling for the police per capita ratio. For all specifications, the results are robust: The estimated coefficients are minimally attenuated compared to the baseline IV specifications, however the differences are very small and not statistically significant.

The overall reduced form effect of the benefit extentsions on crime is the sum of competing disincentive and income effects, as discussed in more detail above. The baseline IV strategy identifies the combined effect of the disincentive effect that impacts on crime through unemployment mechanisms and the income shock that is experienced through unemployment. If unemployment benefits and in particular potential benefit durations directly affected crime over and above the unemployment mechanisms, that would cause concerns for the IV strategy. In order to empirically test for that and to disentangle the two effects, I include income and benefit income control variables from the CPS March samples. Unfortunately, the income and benefit information is available from the CPS March sample only which leads to a substantial reduction in the sample size and more importantly in the variation over time. Similar to the police employment case, the income control variables are arguably endogenous control variables and hence the results should be interpreted as a robustness test only.

Panel B and C of Table 8 show the estimation results for the 2SLS specification for all three crime types and regarding the unemployment rate and the unemployment duration, respectively. Columns (2), (4) and (6) show the estimated semi-elasticities for the arrest rates for total, property and violent crime with respect to unemployment based on the reduced (March only) sample without controlling for any benefit income. Columns (3), (5) and (7) show the estimation results based on the reduced sample and controlling for the average income, welfare income and unemployment benefit income. The estimated coefficients on the unemployment rate (panel B) are robust to the inclusion of the income variables compared to the reduced sample estimation. The estimated semi-elaticities are larger than in the full sample IV baseline specification for all three crime types, but lie within the 95 per cent confidence intervals around the baseline estimates. The estimated coefficients on the unemployment duration (panel C) are significantly larger than the full sample baseline estimates both for the reduced sample estimation without and with the income control variables. Not controlling for the income variables attenuates the estimated coefficients for the reduced sample, yet they lie within the 95 per cent confidence intervals around the full specification on the reduced sample. Despite the mentioned problems with the approach, the results illustrate that the semi-elasticities of the arrest rates with respect to the unemployment variables tend to be larger once the income effect of the benefit extensions is taken into account, albeit compared to the reduced sample not significantly larger.

Demographic Heterogeneity

Typically, crime rates for younger age groups are higher than for older age groups, peaking in the late teens or early twenties. Here, heterogeneity across the age groups is particularly interesting as the different groups also differ in their vulnerability with respect to weak labour markets. The specifications so far are based on all age groups from 16 to 39 and include age group fixed effects. Alternatively, I estimate the 2SLS model on the subsamples for each age group (16-19, 20-24, 25-29, 30-34 and 35-39). The results for all three crime types are shown in Panel A (unemployment rate) and Panel B (unemployment duration) of Table 9. The semi-elasticities of the arrest rate for property crime with respect to the unemployment rate (Panel A) are positive and statistically significant for the younger age groups, strongest for the age group 20-24 and fading out from the 30 to 34 year-old group onwards. For violent crime, the results suggest a significant and positive, albeit small effect for the youngest age group only. The semi-elasticities with respect to unemployment durations reveal a similar pattern, although fading out slightly earlier for the property crime arrest rate. The results, both for the unemployment rate and the unemployment duration, are consistent with typical crime demographics.

The empirical literature on crime typically looks at gender specific samples, and in particular at males only. Similar to the case of the different age groups, the analysis so far is based on males and females and includes gender fixed effects. Again, I alternatively estimate the 2SLS model on the gender subsamples. The results are shown in Panel C (unemployment rate) and Panel D (unemployment duration) of Table 9. The estimated semi-elasticities for the male sample are estimated imprecisely, and are not significantly different from zero for any of the three crime types. For females, the semi-elasticities of the property crime rate both with respect to the unemployment rate and the unemployment duration are positive and statistically significant.

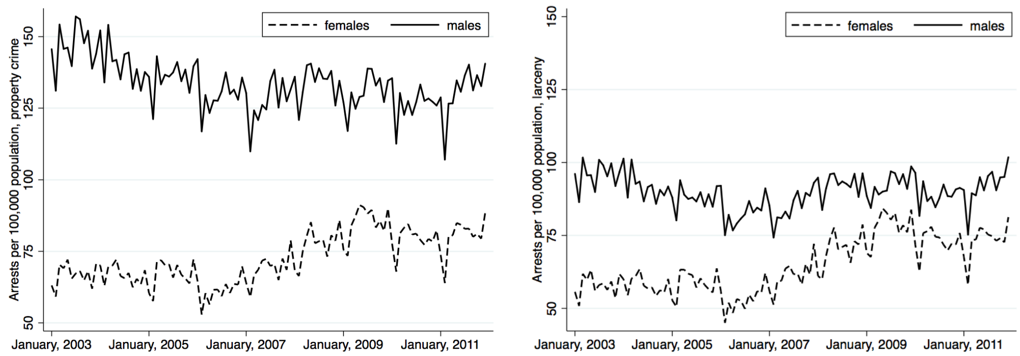
These results might be surprising at first, but can be rationalised by two observations. First, the results by gender and offence as shown in Panel E and Panel F of Table 9 suggest that the positive effect for the female sample is driven by a strong and significant positive effect for larceny crime. For the male sample, the results are more mixed: While there is a positive effect for larceny, too, there is a negative effect for arson. When these offences

Table 9: Robustness Check - Demographic Heterogeneity

		(1)	(2)	(3)		(4)	(5)	(6)
		2SLS	2SLS	2SLS		2SLS	2SLS	2SLS
		Panel A. U Rate by age group.				Panel B. U Duration by age group.		
Subsample	Y =	ln(CR)	ln(PCR)	ln(VCR)		ln(CR)	ln(PCR)	ln(VCR)
Age 16-19:	U Rate	0.019***	0.024***	0.009*	U Dur.	0.014***	0.018***	0.006***
(4,936 obs.)		(0.005)	(0.006)	(0.005)		(0.003)	(0.003)	(0.002)
Age 20-24:	U Rate	0.015***	0.028***	0.002	U Dur.	0.011**	0.020***	0.001
(4,941 obs.)		(0.005)	(0.006)	(0.006)		(0.005)	(0.006)	(0.004)
Age 25-29:	U Rate	0.011**	0.018***	0.003	U Dur.	0.008*	0.013	0.002
(4,804 obs.)		(0.005)	(0.006)	(0.005)		(0.005)	(0.008)	(0.004)
Age 30-34:	U Rate	0.005	0.005	0.005	U Dur.	0.002	0.002	0.002
(4,658 obs.)		(0.007)	(0.007)	(0.009)		(0.003)	(0.003)	(0.004)
Age 35-39:	U Rate	-0.003	-0.004	-0.006	U Dur.	-0.003	-0.004	-0.005
(4,698 obs.)		(0.008)	(0.010)	(0.009)		(0.007)	(0.010)	(0.009)
		Panel C. By gender, U Rate.				Panel D. By gender, U Duration.		
Subsample	Y =	ln(CR)	ln(PCR)	ln(VCR)		ln(CR)	ln(PCR)	ln(VCR)
Males	U Rate	0.003	0.003	0.001	U Dur.	0.001	0.002	0.0008
(12,069 obs.)		(0.005)	(0.006)	(0.004)		(0.002)	(0.003)	(0.002)
Females	U Rate	0.026***	0.041***	0.007	U Dur.	0.012***	0.018***	0.003
(11,968 obs.)		(0.006)	(0.008)	(0.008)		(0.004)	(0.005)	(0.004)
		Panel E. By gender and off., U Rate.				Panel F. By gender and off., U Duration.		
Subsample	Y =	ln(PCR) Burglary	ln(PCR) Larceny	ln(PCR) Arson		ln(PCR) Burglary	ln(PCR) Larceny	ln(PCR) Arson
Males	U Rate	-0.004	0.011	-0.163**	U Dur.	-0.002	0.006*	-0.089*
(12,069 obs.)		(0.008)	(0.007)	(0.080)		(0.005)	(0.003)	(0.057)
Females	U Rate	-0.032	0.056***	-0.108	U Dur.	-0.014	0.025***	-0.048
(11,968 obs.)		(0.034)	(0.009)	(0.092)		(0.016)	(0.006)	(0.040)
State, age, gender F.E.		yes	yes	yes		yes	yes	yes
Quadr. state trend t, t^2		yes	yes	yes		yes	yes	yes
Lagged UR		yes	yes	yes		yes	yes	yes
Control var.		yes	yes	yes		yes	yes	yes

SOURCES. - CPS, UCR and own calculations. NOTE. - Panel A and B: The fixed effects do not include age fixed effects. Panel C to panel F: The fixed effects do ont include gender fixed effects. For all other variables, see table notes in Table 5 and Table 6 as well as further explanations in the text.

Figure 9: Arrest Rate by Gender - Property Crime and Larceny



SOURCES. - UCR and own calculations. NOTE. - The figure shows the average arrest rate for property crime(left) and larceny (right) per 100,000 population in the U.S. between 2003 and 2011 for males (solid line) and females (dashed line).

are combined to the property crime category, the coefficient becomes noisier. Second, there appears to be a decrease in the overall gender gap in property crimes during the sample period as shown in figure 9, and in particular for larceny. Note that larceny is by far the largest offence category for female arrests, while that does not necessarily hold to the same extent for males.

5 Conclusion

In this paper, I study the relationship between labour market conditions and crime in the context of temporary unemployment benefit extensions and increasing unemployment durations in the United States. First, I estimate the reduced form effect of the unemployment benefit extensions on crime. Second, I exploit the variation in the timing and magnitude of these unemployment benefit extensions within and across states in order to identify the crime elasticities with respect to unemployment rates and durations. Based on that variation in unemployment and unemployment benefit durations, I provide new evidence on the causal effect of unemployment on crime.

The reduced form results suggest a positive relationship between unemployment benefit extensions and crime. The results can be rationalised by the labour market mechanisms: The effect appears to be driven by underlying increases in unemployment rates and durations linked to the benefit extensions. In line with previous findings, I find that higher unemployment is linked to higher criminality. Linking that to the recent structural changes in the U.S. labour market, I find that the link between unemployment and crime is duration dependent. The main results are consistent with expectations from theoretical models on labour markets and crime.

There are two main limitations to the analysis. The first concerns the external validity of the quasi-experiment and is thus common to the literature using a similar methodology. The increases in unemployment durations in the United States, unlike in European countries, have been unprecedented and one might argue that they are particular to the Great Recession. Here, I identify the impact of unemployment on crime from these changes and thus the analysis is likely to pick up a local effect. The second limitation refers to concerns about compositional changes in unemployment. It has been argued that the unemployment benefit extensions have led to reduced exits from the labour force. That means that the composition of the unemployed population changes with the policy compared to the pre-policy period. The implications for this analysis are not obvious ex-ante, and unfortunately the type of data which I have access to does not allow me to analyse that concern in more detail.

Overall, the study adds to the literature on labour markets and crime, and yields new insights into the causal relationship between unemployment and crime which can be important for well-targeted policy decisions. Moreover, the results suggest that there are unintended effects of benefit extensions on property crime rates. In terms of welfare considerations, it would be extremely interesting, but beyond the scope of this study, to conduct a cost-benefit analysis of the benefit extensions taking these effects into account.

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