University of Mannheim – Master Thesis

Opportunity makes a Thief

A geospatial analysis of criminal activity*

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November 17, 2021

*Thanks to all my good friends that took the time to discuss my ideas with me and gave me great advice during the process of writing. Special thanks go out to Christian Hilscher, Dante Perelis, Jaira Pagkalinawan, Jonas Casper, and Lucas Cruz Fernandez who proofread this work and sent me valuable feedback.

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1 Introduction

The groundbreaking work of Èmile Durkheim (1938) started a new chapter in the quest to find reasons for criminal behavior. Previously dominated by a focus on predispositions, ideas about the role of society (Merton, 1938), personal and social goals, and the means to achieve said goals emerged (Agnew, 1985). Following this new interest in differing explanations for criminal behavior, Becker (1968) and Ehrlich (1973) proposed the first neo-classical models of criminal choice. Viewing criminals as rational decision-makers and the criminal act as a cost-benefit trade-off set a framework to capture different mechanisms and channels between criminal punishment, law enforcement, and gains from illegal activities and the frequency of their occurrence.

These rational choice models, as they are known today, have subsequently been used by economists to explore a variety of questions related to criminal activities. One prime example is the interaction between unemployment and crime. With the focus often being on aggregates such as unemployment rates and crime rates, the predictions made by neo-classical models are straightforward. A lack of legal income, which can be seen as a reduction in the means to achieve personal goals, or in economic jargon, to gain utility, would lead to higher shares of criminally motivated individuals within a society. Assuming all other factors remain the same, crime rates should rise when unemployment becomes more widespread.

However, the empirically observed relationship between unemployment and crime differs frequently from this prediction. For example, Cantor and Land (1985) note that in the USA during the 1960s and especially at the height of the economic crisis in 1982, the relationship was negative. Moreover, a recent review of the current literature by Chalfin and McCrary (2017) highlights the sensitivity of estimates to the studied time period. In addition, Draca and Machin (2015) document a range of possible shortcomings inherent in these simple utilitarian models that could lead to ambiguous results.

Over the years, the findings of the unemployment-crime relationship have therefore been described as a "consensus of doubt" (Chiricos, 1987) or as unable to identify and quantify a connection (Fagan & Freeman, 1999; Freeman, 1999). An early attempt to solve this apparent ambiguity was made by Cantor and Land (1985). They propose a structural model, which includes two counteracting channels that can lead to positive, negative, and insignificant results depending on the dominating channel (see also Cantor & Land, 1991).

The first channel retains the predictions from the rational choice models. Named the *motivation channel*, it describes how unemployment increases criminal motivation by depriving individuals of legal income and employment opportunities. Cantor and Land argue that an increase in the unemployment rate will lead to an overall shift in criminal motivation among the population, and hence crime rates will rise. As an opposing force, they propose that unemployment also affects criminal opportunities. The *opportunity channel*, is based on an earlier work from Cohen and Felson (1979) (see also Cohen, Kluegel, & Land, 1981; Felson, 2000), who describe the occurrence of criminal acts as convergence in space and time between three necessary elements: (1) motivated offenders, (2) suitable targets, and (3) the absence of capable guardians. Using this routine activity approach (RAA), Cantor and Land claim that individuals affected by an unemployment spell will adapt their daily routines in a way that reduces the number of criminal opportunities available for exploitation. The key arguments are that unemployed individuals will reduce their mobility and increase their time spent at home. This way, they act as capable guardians for their own property and the properties close to theirs while further reducing their likelihood of being victimized during a trip outside their home.

Following their theoretical arguments on the counteracting channels, Cantor and Land state a clear identification strategy for the structural relationship that results from them. The crucial point of this strategy is a difference in the timing between both effects. A change in the routine activity is claimed to happen immediately when one is confronted with unemployment. Any effect on criminal motivation is, on the other hand, delayed due to factors such as unemployment benefits and private savings. Therefore, including both a contemporaneous and a lagged measurement for unemployment will result in a complete structural model for the unemployment-crime relationship capable of capturing both channels.

The work by Cantor and Land (1985) has been one of the most influential studies in the literature on unemployment and crime. While we agree with Paternoster and Bushway (2001) that clearly specifying a model and making explicit empirical predictions is a valuable contribution, their argument that the subsequent literature should analyze Cantor and Land's approach *"as it was presented"* has, in our opinion, led to few new and seldom robust discoveries. Instead, research should rather evolve around such predictions and strive to verify or negate them to better our understanding of a specific relationship through the studying of underlying mechanisms. In the follow-up to Cantor and Land's series of seminal papers (Cantor & Land, 1985, 1991; Cantor, Land, & Russel, 1995), however, only few such attempts to analyze their predictions can be found, with the most taking them as given. This is in contrast to the extensive debates that have been held on the properties of their time-series approach (Britt, 2001; Hale & Sabbagh, 1991; O'Brien, 2001).

Most econometric methods proposed during this debate have already been superseded by newer ones, as is often the case when discussing the use of a specific estimator. Today, little is left from the time-series approach, and the debate around it was none that improved our understanding of the relationship. The increased availability of data gave way to the usage of panel and instrumental variable estimators, again without producing a consensus in its results. It is, therefore, our opinion that instead, a fundamental discussion of the channels themselves and their identification would yield more merit than focusing primarily on estimation techniques.

To the best of our knowledge, the only attempt to start such a discussion was made by Greenberg (2001) who questions the presented identification strategy of the two channels as a whole. While Cantor and Land (2001) were quick to answer and defend their approach, and no real debate was ever held about it, there has so far been no robust empirical evidence for their opportunity channel. In contrast, reviews of the literature by Paternoster and Bushway (2001) and also Levitt (2001) summarize the relationship between unemployment and crime as small and positive. When summing up all findings, Levitt states that a 1% change in the unemployment rate usually results in a 1-2% increase in the contemporaneous crime rate. Therefore, the direction of these findings suggests the contrary to what was proposed by Cantor and Land.

More recent estimates for the unemployment-crime relationship still result in mixed evidence for and against the opportunity channel. The papers by Gould, Weinberg, and Mustard (2002) and M. J. Lin (2008) again find positive contemporaneous effects, while Arvanites and Defina (2006) and Raphael and Winter-Ebmer (2001) reject the null and dismiss a significant effect of unemployment on crime. On the other side, evidence for the channel can be found in Rosenfeld and Fornango (2007) and Phillips and Land (2012) who both document a significant negative contemporaneous relationship.

It remains an open question if the relationship between unemployment and crime is accurately captured by the structural model proposed in Cantor and Land (1985) and especially if the opportunity channel plays a significant role in it. Without robust evidence in favor or against this channel, the literature remains unable to move away from the "consensus of doubt" and find an answer to the question of how unemployment affects criminal activity.

While several case studies show (Ayres & Levitt, 1998; Cook & MacDonald, 2011), that changes in opportunities affect criminal activity, there are no attempts to identify this relationship on an aggregate level and with measures related to the proposed behavioral changes due to unemployment. Only Kleck and Chiricos (2002) study crimes in shops and use their sales to proxy for criminal opportunities to estimate aggregate effects. However, doing so excludes residential areas from their analysis and thereby the area most likely to be impacted by behavioral changes due to unemployment. Moreover, it is not clear if sales are only linked to criminal opportunities. A decrease in sales can also entail an increase in criminal motivation due to a lack of consumption.

Because of this overall lack in consistent evidence, we propose a new approach to study the unemployment-crime relationship. Instead of estimating the structural model proposed by Cantor and Land (1985), we focus on the mechanisms underlying the two channels. These mechanisms allow for new possibilities to analyze their respective channels and hence the structural unemployment-crime relationship. In section 2 we start by providing an overview of all mechanisms included in this relationship and discuss ways in which their study can improve our understanding on how unemployment affects crime. As a consequence of the lack of empirical studies, we then single out the two mechanisms included in the opportunity channel and proceed to introduce the data necessary to analyze them in section 3. The data section further includes a brief excursion on the use of commonly used crime statistics as well as a detailed description of the process used to construct a county-level dataset. Afterward, we will analyze both mechanisms separately in sections 4 and 5. Each of these sections begins with theoretical considerations that build the foundations for empirical predictions, which we then set out to analyze empirically. Section 6 discusses our limitations and section 7 concludes.

2 Unemployment and Crime - A Tale of two Channels

As a preliminary to our empirical discussion, we start with a theoretical overview of the unemployment-crime relationship. As noted above, the majority of work on this relationship has focused on either directly estimating the unemployment-crime relationship or utilizing the structural framework built by Cantor and Land (1985). While advances in econometric methods and data availability have meant that estimated coefficients have been analyzed with more precision, a consensus in the literature has still not been reached. A possible explanation for this is that most of the underlying mechanisms of the channels have so far been neither theoretically nor empirically explored.

To improve upon this consensus of doubt, we start by partitioning the structural model into its two components, the motivation, and the opportunity channel. Each of these channels is comprised of two separate mechanisms in which we continue to partition them. The motivation channel is made up of the *unemployment-motivation* and the *motivation-crime* mechanisms, whereas the opportunity channel is the product of the *unemployment-opportunity* and the *opportunity-crime* mechanisms. The word product can here be taken quite literally. Similar to an indirect effect, the sign of the channel is the product of the signs of the two underlying mechanisms. A depiction of the two

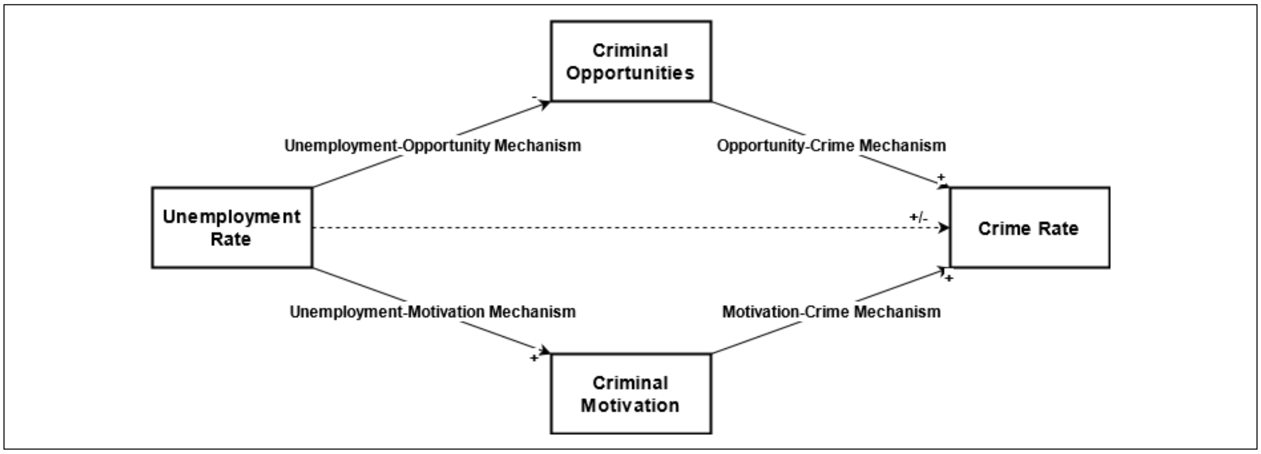


Figure 1: The Structural Unemployment and Crime Relationship

channels and their respective mechanisms can be seen in Figure 1 above.

This partitioning allows us to discuss and analyze each of the mechanisms separately. Compared to past work that has focused only on the channels, this offers a more nuanced view of the relationship between unemployment and crime. In the remainder of this section, we will go through each of the two channels and their respective mechanisms, discussing possible findings that could arise from analyzing each mechanism separately. The end of this section will then provide an outlook on the empirical study that takes up the second part of this paper.

2.1 Unemployment and Motivation

The motivation channel depicts the positive, while lagged, relationship between unemployment and crime. It is by far the more extensively studied of the two channels and consists of the *unemployment-motivation* and the *motivation-crime* mechanisms. For the channel to yield the positive relationship that Cantor and Land have predicted, it would therefore require both to have a positive relationship between criminally motivated individuals and crime. The former is true by consturction, while the latter is a consensus in most criminologist theories.

Unemployment can reduce the ability to achieve personal or social goals (Agnew, 1985; Merton, 1938), shift the cost-benefit trade-off of criminal acts (Becker, 1968; Ehrlich, 1973) or lead to changes in perceptions of personal and social images as well as the valid social norm. Most of these theories view the change in motivation as an ad-hoc reaction to the unemployment spell. However, in a dynamic world, expectations about the future play an essential role in any decision-making process. The behavioral responses creating the mechanisms described by Cantor and Land are no exceptions, especially so since criminal tendencies peak for young adults around their early to mid-20s (Agnew, 2003; Rocque, Posick, & Hoyle, 2015).

Suppose an unemployment spell results from a transitory shock to the economy rather than a prolonged economic downturn. In this case, any individual affected by unemployment might not deem it necessary to update their expectations about (future) employment, social norms, or abilities to achieve goals. In that case, the *unemployment-motivation* mechanism is likely to be weak so that no significant increase in the share of criminally motivated individuals can be observed.

Assuming for now that the opportunity channel exists, then a weak *unemployment-motivation* mechanism during a transitory shock can explain the negative relationship between unemployment and crime usually found during an economic crisis. Similarly, this could explain the weak relationship found between 1960 and the late 1970s in the USA (Cantor & Land, 1985). A long-running, upward-trending economy sets positive future expectations so that even large shocks to the unemployment rate will have little or no potential to form criminal motivation.

2.2 Unemployment and Opportunity

The opportunity channel, on the other hand, reflects the contemporaneous effect unemployment has on crime. It again consists of two underlying mechanisms, the first of which, the *unemployment-opportunity* mechanism, depends again on the behavioral response of individuals to an unemployment shock. The second mechanism, however, reflects the behavior of criminally motivated individuals with respect to a change in criminal opportunity and is hence called the *opportunity-crime* mechanism. An increase in unemployment is, hereby, thought to reduce the mobility of affected individuals. This will increase their time spent as effective guardians of otherwise suitable targets around their home and further reduce their own likelihood of becoming the victim of a crime. In other words, changes in behavior reduce the number of criminal opportunities that could be exploited by the criminally motivated. However, neither Cantor and Land nor any following paper propose any evidence for the hypothesis that unemployment increases the stationarity of individuals around their homes. In fact, there is a wide range of possible individuals responses to an unemployment shock that makes the aggregate effect ex-ante ambiguous.

For example, in the literature on the *retirement consumption puzzle*, pensioners spend more time hunting for bargains, resulting in a sharp observable drop in consumption at the retirement age. Similar behavioral responses are likely to occur among unemployed individuals. Instead of simply increasing their time spent at home and reducing their overall mobility, spending their time in a utility-maximizing way might just entail the opposite. Some examples of such behaviors that would require more mobility would be bargain hunting, usage of welfare programs, job searching, timeintensive leisure activities, informal market activities, and, of course, criminal activities. Therefore, whether unemployment increases or decreases criminal opportunities, depends to a non-trivial degree on the spatial and temporal distribution of these activities and the aggregated behavioral response to an increase in the unemployment rate.

The second part of this channel is the *opportunity-crime* mechanism. Based on the *routine activity approach* (Cohen & Felson, 1979), this mechanism builds on criminological theory that views crime as the result of the convergence or overlap between three necessary factors: guardianship, suitable targets, and motivated offenders. Criminal opportunities encompass the first two of these. Therefore, a decrease in criminal opportunities will consecutively reduce the likelihood of their convergence with motivated offenders and hence decrease criminal activity. Figure 2 depicts such a change in convergence, where starting from an initial position, first motivation increases, and in a second step, a decrease in opportunities each affect the convergence, i.e., the overlap of these three necessary factors. Moreover, part (c) of the figure shows that the overlap is not only defined by the number of opportunities but also their temporal and spatial distribution. A change in this distribution can be inferred from a shift in the center of a circle.

So far, most evidence for this approach is limited to micro and neighborhood-level settings and case studies (Clarke & Mayhew, 1988, 1994; Mayhew, Clarke, & Elliott, 1989). It, therefore, remains to be seen if similar effects can be obtained on more aggregate levels, such as a county and whether the behaviors that generate criminal opportunities are significantly affected by unemployment.

2.3 The Tale

So how does one proceed to identify the unemployment-crime relationship? Considering both channels, each of them provides its own starting points. On the one hand, the motivation channel allows investigating differences in estimates over time through the belief and expectation updating behavior of individuals. Moreover, the role of social norms plays a significant role in forming or rather preventing, the formation of criminal motivation. While a closer look at this channel is likely to resolve some of the open questions we currently have on the unemployment-crime relationship, it can by no means answer the most crucial question at hand. Any estimation of this relationship still hinges upon the correct specification of the structural model, and hence on the existence, or non-existence, of the second, the opportunity channel.

On the other hand, this second channel offers the possibility to test the specification mentioned above directly. While the identification strategy proposed by Cantor and Land has much appeal for empirical research, neither of the two underlying mechanisms of the opportunity channel have so

(a) Initial Position

(b) Increase in Motivation

(c) Adjustment in Opportunity

Figure 2: Routine Activity Approach - Convergence of Opportunity and Motivation

far been the subject of an extensive analysis. Thus, rather than estimating the opportunity channel as a whole, we can break down the opportunity channel into two distinct mechanisms: First, how does unemployment affect criminal opportunities, and second, how will this change in criminal opportunities affect criminal activity? Each question represents one of the two mechanisms. Since the channel's existence depends on both of these mechanisms, we can use the answers to both of these questions to then debate the likelihood that the opportunity channel is a viable part of a structural unemployment-crime relationship.

In what follows, we will concentrate on this second channel, working to establish a causal relationship between both unemployment and criminal opportunities as well as between criminal opportunities and criminal activity. The remainder of this paper includes the empirical work necessary to find the answers to the questions we have just raised.

3 Data

To construct daily observations on the county-level in the USA for the entirety of 2019, we draw the data from four different sources: data on criminal offenses are taken from the FBI's *National Incidence Based Reporting System* (NIBRS) [3.1], which is part of the *Uniform Crime Report* published every year. The *Trips by Distance* dataset published by the *Bureau of Transportation Statistics* (BTS) [3.2] includes measures for guardianship and mobility. For the instrumental variable approach, we will discuss later, we use daily weather station data taken from the *National Oceanic and Atmospheric Administration* (NOAA) [3.3]. The *Bureau of Labor Statistics Local Area Unemployment Statistics* (LAUS) dataset provides us with county-level unemployment statistics [3.4]. Furthermore, the following section will also discuss any necessary matching procedure to combine the sources mentioned above. The final dataset covers 24 states in the USA throughout 2019.

3.1 NBIRS

The *Uniform Crime Report* (UCR) is a commonly used source for criminal statistics within the USA. Most literature mentioned above relies on this source and uses information collected under the *Summary Reporting System* (SRS). In our paper, we, however, make use of the newer *National Incidence Based Reporting System* (NIBRS) established in 2011. However, the ongoing transition to NIBRS from the SRS puts some restrictions on data availability (Kaplan, 2021). Therefore, to limit any bias from said transition, such as missing data due to non-transitioned law enforcement agencies (LEAs), we focus our analysis on states within the USA that have (mostly) converted to NIBRS.

As of 2019, 24 states[1] fulfill our selection criteria of a high share in NIBRS participation. This sample accounts for about one-third of the US population and has an average NIBRS participation share of 96%. The lowest share at 88% is observed in Massachusetts. The larger share of the population not covered by the NIBRS is mainly driven by populous states, such as California and Florida, which have not transitioned so far and thus are eliminated from the sample. A graphical representation of participation in the new incident reporting system can be seen in Figure 3, which shows non-participating states in grey and participating states in colors from light to dark depending on the participation rate of agencies in each state.

[1] Arkansas, Connecticut, Colorado, Idaho, Iowa, Kansas, Kentucky, Michigan, Montana, North Carolina, North Dakota, Massachusetts, Ohio, Oklahoma, Oregon, Rhode Island, New Hampshire, South Carolina, South Dakota, Tennessee, Vermont, Virginia, Washington, West Virginia

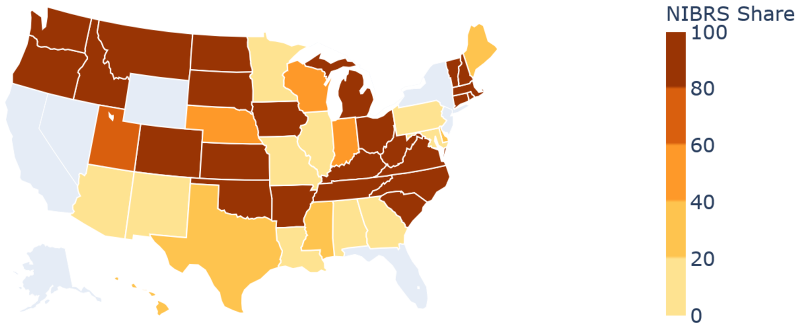


Figure 3: Share of Agencies by State Participating in NIBRS

The FBI defines an incident as a criminal act consisting of one or multiple offenses by the same offender or group of offenders without significant spatial or time intervals between the included offenses. For each incident, information is available in different categories. These include information on the offender, the arrestee, the victim, and the affected property. Furthermore, information on the circumstances of each offense are given. This allows linking offenses and analyzing patterns within an incident or disentangling and aggregating them to obtain offense-level statistics within an LEA at arbitrary time intervals. For a detailed overview of all information included in NIBRS, see Figure A1 in the appendix.

Our primary goal is to construct a county-level dataset with daily observations on criminal offenses. As a first step, we limit our observations to *Part I* offenses, namely murder, rape, aggravated assault, robbery, larceny/theft, burglary, and motor vehicle theft (MVT). These seven offenses are commonly analyzed in the literature and can further be aggregated into two categories based on their motivation, as seen, for example, in Cantor and Land (1985). The group of violent crimes, defined by the intention to harm the victim, include murder, rape, and aggravated assault. In contrast, property crimes, where the intention is monetary gain, consist of robbery, larceny/theft, burglary, and motor vehicle theft (MVT). Following the literature, we slightly deviate from the FBI's definition by allocating robberies into property instead of violent crimes and exclude other offenses, such as gambling, drug, or financial offenses.

Taking advantage of the more granular information contained in NIBRS provides the possibility to extract the day and the time of day an offense was committed. For the latter, we follow Felson and Poulsen (2003) and distinguish between daytime (5 am-5 pm) and nighttime crimes. Additionally, we generate a location dummy to differentiate between crimes committed in residential and non-residential areas. This dummy is constructed from a categorical variable, which includes one category for residential and numerous sub-categories for non-residential locations. For a full list, see appendix A1.

Up until this point, all our data remained at the law enforcement agency level. The next step will be to infer the counties where these agencies are located and, therefore, effectively create county-level observations. This process is, however, not as straightforward as one might think at first. In an interesting piece, Maltz and Targonski (2002) discuss the complexity and drawbacks of using officially provided county-level criminal data. Especially imputations of missing data and the allocation of criminal offenses from LEAs that serve multiple counties introduce unwanted biases into the data.

One way to minimize this bias mentioned by Maltz and Targonski (2002) is to focus on counties with larger populations. As a consequence, Phillips and Land (2012) only analyze the 400 most populous counties within the USA using county-level observations from the National Archive of Criminal Justice Data (NACJD). However, this dataset is not available for 2019 and does not include the additional information contained in NIBRS. Also, as shown by Sameem and Sylwester (2018), differences between urban and rural areas play a relevant role in the unemployment crime relationship. Analyzing only densely populated areas might therefore preclude estimated coefficients from obtaining external validity.

Therefore we now set out to build a more complete dataset, which reduces any bias from non-reporting and aggregation to a minimum by focusing on the 24 states named above. Furthermore, their large share in NIBRS participation makes it possible to build an allocation mechanism for the offenses included in our dataset so far and distribute them to the counties served by each LEA.

First, we distinguish between LEAs active in one county, 90.7% of observations, and those active in multiple counties. Allocation for those only active in one county is straightforward, as their offenses can be attributed to this one county. Matching offenses of multi-county LEAs requires some mechanism that accounts for the number of counties an agency is active in and county-level information that helps us distribute the offenses proportionately. Most of these multi-county LEAs are part of a *Metropolitan Statistical Area* (MSA) which they serve as a whole. As such, it allows us to assume a certain degree of similarity between the contained counties.

The starting point of our mechanism is, therefore, the population living within each county. The assumption is that a county with a larger population will also report more offenses, though not necessarily have a higher crime rate. Therefore, if an LEA is active in two counties, the county with the larger population will also make up the larger share of offenses reported by this agency. To improve upon this naive estimate, we obtain information on the populations covered by each

LEA, i.e., the population within the agency's jurisdiction. For one-county agencies, this value can only be as large as the county they are located in. On the other hand, for multi-county LEAs, their covered population may be above or below the population living within every single county they serve. However, it has to be smaller or equal to the sum of these populations.

Based on this, we begin our mechanism by calculating the uncovered population in each county. This is the population living there, minus the population already covered by one-county agencies. As an example, assume we have two counties, A and B with 10,000 inhabitants each. We further have 2 law enforcement agencies *LEA-1* serves 5,000 people and is only active in county A and *LEA-2* is active in A and B and covers 15,000 people. The first step now is to calculate the uncovered population in each county, Net_Pop_c. Since county A has an active one-county LEA, we start by subtracting the covered population of this LEA from the county's total population. Afterward, the remaining uncovered population in county A equals 5,000. On the other hand, in county B there is no such LEA active, and whole population is counted as uncovered.

$$Net\_Pop_c = Pop_c - \sum_i (LEA\_Pop_i | c(i) \stackrel{!}{=} c),$$

where c(i) is the set of counties covered by LEA i, and c denotes a specific county in the set of all counties C.

For each multi-county LEA, we can now sum up the uncovered populations within all counties in which they are active. In our example, that means that since only *LEA-2* is a multi-county agency, the uncovered population is equal to the 10,000 inhabitants of county B and the 5,000 remaining from county A. The total uncovered population of *LEA-2* is then 15,000.

$$LEA\_Net\_Pop_i = \sum_c (Net\_Pop_c | c \in c(i))$$

As a final step, we calculate the share of the uncovered population within each county of the total uncovered population within an LEA. In our example, this means that the share of county A equals $\frac{5{,}000}{15{,}000} = \frac{1}{3}$ and similarly the share for county B equals $\frac{2}{3}$. In general we have,

$$Share_{i,c} = \frac{Net\_Pop_c}{Total\_Net\_Pop_i}.$$

Offenses of multi-county LEAs are then distributed to all counties in which they are active by using their respective shares of the uncovered population. For example, this means that one-third of the offenses of *LEA-2* will be distributed to county A and the remaining two-thirds to county B.

Generally, if an LEA is active in more than two counties, the offenses are distributed so that the sum of shares always equals 1.

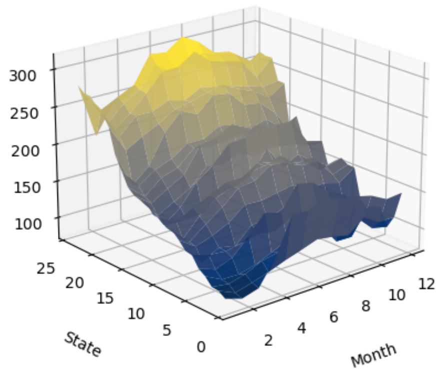
An underlying assumption of our distribution algorithm is, of course, that each unit of population is only covered by one LEA. In reality, jurisdictions can overlap, which will bias this algorithm to distribute more crime to counties in which no other LEA is active if this is the case. Nonetheless, given the information provided by the *UCR*, we perceive this as the most straightforward algorithm that does not require any other assumptions than the two we have stated so far.

Table 1: Summary Statistics: County-Level Offenses

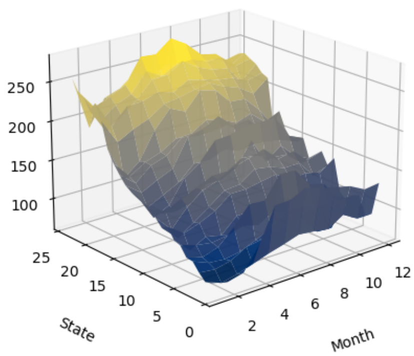
	Obs	Mean	SD	Min	Max
Offenses (Total)	475583	4.88	14.77	0.0	334
Offenses (Daytime)	475583	2.78	8.13	0.0	168
Offenses (Residential)	475583	2.04	5.87	0.0	127
Violent Offenses	475583	4.30	12.93	0.0	297
Larceny	475583	3.17	9.43	0.0	220
Burglary	475583	0.70	2.21	0.0	65
MVT	475583	0.42	1.77	0.0	45
Robbery	475583	0.12	0.64	0.0	20
Violent Offenses	475583	0.58	2.17	0.0	69
Aggravated Assault (AA)	475583	0.39	1.47	0.0	56
Rape	475583	0.07	0.32	0.0	23
Murder	475583	0.01	0.10	0.0	5

Following this procedure, we obtain a dataset containing county-level observations on criminal offenses. The summary statistics for these variables are shown in Table 1. Moreover, Figure 4 shows monthly crime rates for all 24 states in 2019 in a contour plot. Based on this figure, differences in crime rates across states are visible, as are differences within each state across the year. Highlighting these differences is an essential part of our analysis, as it shows the heterogeneity that is neglected when data is analyzed on national and yearly aggregates. Moreover, in the appendix, Figure A2 shows the same plots for the seven offenses included in our dataset.

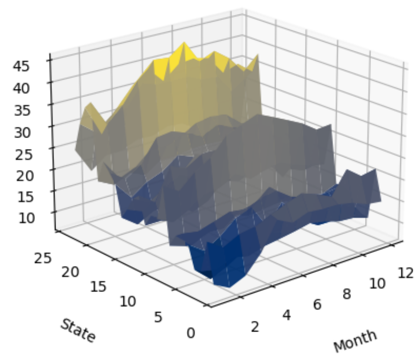
Before we proceed to describe the remainder of our data, we want to leave a cautionary note, which would warrant its own study. Since law enforcement agencies report their offenses voluntarily to the FBI, it is not unreasonable to assume that the published data contains a measurement error of some form. If this error is uncorrelated with any dependent variable included in our specifications, this will lead to larger standard errors and diminished power of our tests. However, if this assumption is violated and some correlation between the error and any dependent variable exists, our estimated



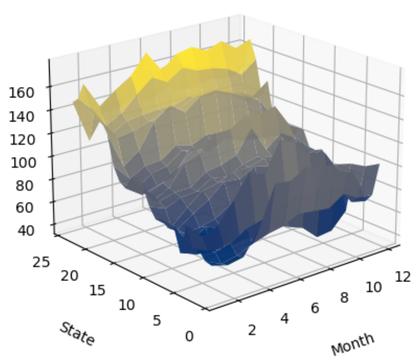
Total Crimes



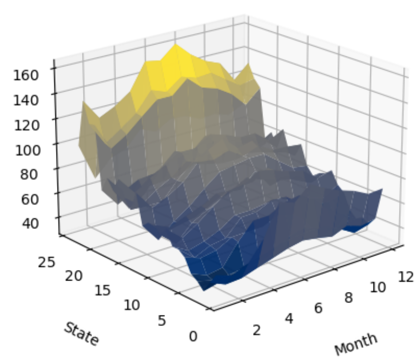
Property Crimes



Violent Crimes



Daytime Crimes



Residential Crimes

Figure 4: Crime Rates per 100.000 Inhabitants for different Offense-Level Subgroups

Z-axis is scaled according to the lowest and highest values of computed crime rates.The state index starting from 1 to 24 corresponds to the following states: Oklahoma, Massachusetts, New Hampshire, Connecticut, Vermont, West Virginia, Idaho, Virginia, Michigan, Iowa, Ohio, Rhode Island, South Dakota, Kansas, North Dakota, Kentucky, North Carolina, Montana, Colorado, Oregon, Tennessee, Arkansas, South Carolina, Washington.

coefficients are no longer consistent. While out of the scope of this work, future research in the field would benefit from a thorough analysis of the measurement error contained in LEA reported offenses.

3.2 Trips by Distance

To analyze the opportunity channel directly and infer the effects from changes in guardianship and system activity on crime, a precise measure for these mechanisms must be found. While the literature usually relies on assumptions on an induced change in behavior due to changes in the unemployment rate or other variables, the *Trips by Distance* dataset allows us to observe this behavior directly. Additionally, compared to variables often used in criminology, such as the proportion of the population living alone (Felson & Cohen, 1980), the percentage of single-parent households (Smith & Jaroura, 1988), or the dispersion of activities away from the family and household (Cohen, Felson, & Land, 1980), our variables introduce much more temporal variation and again do not rely on underlying assumptions on changes in behavior.

The *Maryland Transportation Institute and Center for Advanced Transportation Technology Laboratory* compiles an anonymized national panel, constructed from data from millions of mobile devices, which is published by the *Bureau of Transportation Statistics*. Different sources for data on mobile devices and a multi-level weighting procedure address geographic variation and ensure that the final data is representative of the population within each geographic level.

The data offers us two main statistics to measure criminal opportunity. First, we observe the share of the population that did not leave its home during the course of a day. This is taken as an indicator for guardianship. At this point, it is worth mentioning that our measure is likely to capture only personal (home occupancy) and social (informal control) levels of guardianship. Moreover, Wilcox, Madensen, and Tillyer (2007) also distinguish between physical (target hardening) and natural (surveillance through environmental design) dimensions of guardianship. While we are not able to observe these in our data, the short time frame we observe makes it unlikely that substantial changes in either one of these dimensions are occurring on a macro level. Additionally, we can measure system activity by the number of trips taken per person within the county. These trips are grouped by their spatial length so that we can distinguish between shorter and longer movements. As soon as a person remains for more than 10 minutes at one distinct location, the trip counts as finished, and a new trip is started if the person moves again.

3.3 NOOA

The trips data, as described above, has a very direct limitation, namely endogeneity. Since the data is silent on actual locations of trips and offers no other means of distinguishing a trip by its purpose, trips taken to commit criminal offenses are included side by side with trips meant for leisure or legal employment. This creates an endogeneity issue through reverse causality, as higher crime rates will, ceteris paribus, increase the number of trips we observe. On the other hand, one could also draw a picture where non-criminals react to crime rates by changing their respective movement patterns. For example, if one is afraid of being robbed in the street, then the best response might be to stay at home or make shorter, more direct trips instead of wandering around the city.

To avoid these endogeneity issues, we turn to NOOA's *Global Historical Climatology Network* (GHCN). Studies on the impact of weather on movement and activity patterns have been especially prevalent in the psychological literature. A recent review Turrisi et al. (2021) documents many of these findings. Their paper suggests that one of the clearest impacts of weather on human activity can be found through precipitation. Compared to other weather indicators, which have more mixed findings, a solid majority of the studies reviewed (80%) on the effect of precipitation find a strong negative relationship between rain and human activity. The F-statistic provided in the later sections confirms their findings (see Table 5).

Within the 24 states in our sample, the GHCN provides us with 28,061 stations for which we can access daily weather data. Each station also provides us with its exact geographic coordinates so that we can apply a reverse-geocoding algorithm that matches each station with the county it is located in. This procedure allows for precise matching of nearly all stations except 4, which are not located on land but of the coast or within the great lakes. These stations are excluded. The matched stations each report on average for 317 out of 365 days in 2019. While different reasons lead to the inactivity of stations on certain days, nearly all counties contain more than one station. This allows us to compute the mean value of precipitation in mm per day, within each county among all reporting stations. This considerably reduces the number of missing entries and increases the reporting to 356.6/365 days with more than 75% of counties having no missing entry in 2019.

3.4 Unemployment and County-Level Dataset

Finally, the *Local Area Unemployment Statistics* dataset includes monthly unemployment statistics on the county level. This dataset is published by the *Bureau of Labor Statistics* and covers all counties in the USA.

As described in this section, we obtain four different sources of data, which we bring on a common geographical level. While our Trips data and the unemployment statistics included in LAUS are already on this level, the other two sources required data-specific matching algorithms to obtain one cohesive dataset. While the 24 states used in our analysis contain 1485 distinct counties, the combination of all four data sets only allows us to analyze 1377, or 93.1% of these counties. Of the 102 counties not included in our sample, 39 (2.6%) lack data on criminal offenses, most likely due to the ongoing transitioning to NIBRS. Additionally, 63 counties (4.2%) had no weather station with data on precipitation located within them.

The summary statistics for non-crime related variables in the final dataset can be seen in Table 2

Table 2: Summary Statistics: Non-Crime Variables

	Obs	Mean	SD	Min	Max
Unemployment Rate (month)	502605	3.75	1.50	1.0	21
% of Population at Home	495664	19.05	4.79	4.5	74
Trips per Person	495664	4.23	0.93	0.4	21
Precipitation (mm)	491537	3.21	8.03	0.0	263

4 The Unemployment-Opportunity Mechanism

The main feature of the data we presented in the last section is that it provides us with observable macro-level measurements for guardianship and the mobility behavior within a county. Together, they allow us to analyze the relationship between unemployment and criminal opportunities. Moreover, our daily data on crimes will later allow us to link these changes in criminal opportunities to a county's criminal activity. Therefore, starting in this section with the *unemployment-opportunity* mechanisms, we will first discuss theoretical predictions before moving on to the empirical results.

4.1 Predictions

Starting with the predictions set up by Cantor and Land (1985) and depicted in Figure 1 above, this mechanism is thought to result in a negative relationship. An increase in unemployment should subsequently reduce the number of criminal opportunities available for exploitation.

$$Unemployment \uparrow \quad \Rightarrow \quad Opportunity \downarrow$$

This prediction relies on the simple assumption that unemployment will decrease the mobility of affected individuals and hence increase the share of their day they spend at home. In section 2.2 above, we have already outlined a few examples for possible responses to an unemployment spell that might lead to the opposite, or at least reduce any such effect if it should exist. Moreover, from the routine activity approach (Cohen & Felson, 1979), we also know that it is not only the number of capable guardians or suitable targets that ultimately determine if a crime occurs but also their overlap due to their respective spatial and temporal distribution. Consequently, the relationship between unemployment and criminal opportunities is likely to be more complex than previously stated.

As a first step, we move away from the concept of criminal opportunities and rather predict a relationship between unemployment and the two variables we use as proxies for the behavioral change mentioned in Cantor and Land (1985). An increase in guardianship is reflected through the share of the population staying at home, whereas the overall system activity or the mobility behavior is proxied through the number of per person trips taken within a county on a given day. Thus, the first prediction we offer is that any effect we observe should generally be stronger regarding the number of trips compared to the population at home.

This has two reasons. The first is a mechanical one, as staying at home implies, that an individual has reduced his or her trips on a given day to zero. However, a more intuitive argument is that compared to staying at home, trips generate direct monetary costs during the trip (bus ticket, fuel) or at arrival (admission, purchase). Therefore, as unemployment results in an inward shift of the budget constraint, reducing the number of trips should be more prominent than the (relative) cost-free alternative of staying at home.

$$
\begin{aligned}
Unemployment \uparrow \quad &\Rightarrow \quad \text{\% of Pop at Home} \uparrow \\
Unemployment \uparrow \quad &\Rightarrow \quad \text{\# of Trips p.P.} \downarrow\downarrow
\end{aligned}
$$

Furthermore, some responses to unemployment we have mentioned, such as visiting government offices or the reliance on social services, also make a case for a possible non-linear relationship. High levels of unemployment will, among others, increase waiting times at public services and competition for bargains. Furthermore, a large unemployment rate is likely to produce network effects (Gallie, Paugam, & Jacobs, 2003; Rözer, Hofstra, Brashears, & Volker, 2020). Unemployed relatives and friends increase the likelihood of shared activities and create incentives to undertake trips and leave one's home. Thus, taken together, we think it is plausible to predict a reversed relationship between a quadratic unemployment term and both our proxies.

$$\begin{aligned} Unemployment^2 \uparrow &\quad \Rightarrow \quad \text{\% of Pop at Home} \downarrow \\ Unemployment^2 \uparrow &\quad \Rightarrow \quad \text{\# of Trips p.P.} \uparrow \end{aligned}$$

4.2 Results

Leaving the realm of theory and predictions, it is now time to put our data to work and investigate the empirical evidence for the *unemployment-opportunity* mechanism. In the following two tables, we analyze the effect of unemployment and its square on each of our two proxies. Table 3, shows the effect on the share of the population staying at home, and Table 4 the effect on the number of trips taken per person. The coefficients are calculated using a fixed-effects estimator, which we apply to control for unobserved county-level heterogeneities that do not vary over time.

To consistently estimate our coefficients, it is necessary that our regressors, namely the unemployment rates and its square, are strictly exogenous. This means that the unemployment rate needs to be exogenous with respect to the error term of all time periods. This comes from the demeaning procedure inherent to the fixed effects estimator. However, this assumption is only slightly stricter than estimating the coefficients using OLS due to the fact that unemployment data is only available on a monthly basis. Therefore, any endogeneity concerns would apply to both estimators. This, of course, is no argument against a possible bias. Hence, other solutions, such an instrumental variables approach, are needed to identify the causal effect that unemployment has on criminal opportunities.

In the unemployment and crime literature it is common to use local industry compositions (Gould et al., 2002; Sieger, 2013), oil prices (Raphael & Winter-Ebmer, 2001), union membership (M. J. Lin, 2008) or a combination of those as an instrument for the unemployment rate. However, from our work, it becomes apparent that these instruments neglect the underlying mechanisms of the structural unemployment model. Oil prices will directly affect any movement patterns and might even increase the share of people staying at home if they cannot afford to take their car somewhere. High oil prices might also be an incentive to start working from home on at least a few days during the week—both times affecting criminal opportunities. Therefore, the IVs are still correlated with the error term and remain unable to solve the endogeneity problem.

Similarly, industry compositions create movement patterns that can alter criminal opportunities making it related to the error term of the criminal opportunity mechanism. For example, the share of the manufacturing industry, which is often used, will not only be correlated with a typical commuting behavior, as this is no work that can be done from home, it will also more likely entail night shifts or commuting to outskirts of the city where such industries are located rather than the city center. Clearly, these variables cannot be valid and exogenous instruments while simultaneously assuming that the opportunity channel is an essential part of the *unemployment-crime* relationship.

In our view, the best means to identify the causal link between unemployment and opportunity would therefore be through a natural experiment that would allow for a difference in difference or regression discontinuity setting. Such natural experiments can, for example, be found in policy changes or, as studied by Greenstone, Hornbeck, and Moretti (2010), through the study of winner and runner-up counties at bids for million-dollar plants. Unfortunately, our data only spans the year 2019, and hence we will have to postpone such identification to future research. Nonetheless, the following estimates show an interesting pattern, which sheds light on the *unemployment-opportunity* mechanism.

Table 3: Unemployment and Population At Home

	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment	-0.204***	-0.437***	0.0309	0.130**	0.0113	0.0737
	(-11.18)	(-7.99)	(1.16)	(2.77)	(0.45)	(1.58)
Unemployment squared		0.0191***		-0.00700**		-0.00443
		(3.91)		(-2.66)		(-1.59)
Month			x	x		
Week					x	x
N	495664	495664	495664	495664	495664	495664
Cluster	1377	1377	1377	1377	1377	1377

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method.

In contrast to the original predictions by Cantor and Land, Table 3 column (1) estimates a negative relationship between the unemployment rate and guardianship. The coefficient translates into a decrease of the population staying at home on a given day by 0.2 p.p. for every percentage point increase in the unemployment rate. Therefore, if this is translated into criminal opportunities, then our results would indicate that an increase in unemployment also leads to an increase in said opportunities rather than decreasing them. However, the following specifications highlight that this effect is not robust and highly sensitive to the addition of month and week fixed effects.

Adding a squared term of the unemployment rate in column (2) retains both the sign and significance of the original unemployment rate. Due to the opposing direction of the squared term, we can further calculate the level of unemployment needed at which the overall effects become positive, such that unemployment would lead to a decrease in criminal opportunities. This level is 22.9%, and therefore about 12.7 standard deviations above the mean of the unemployment rate in our sample and even higher than our maximum value. Therefore columns (1) and (2) indicate that a non-linear relationship exists but in the opposite direction as assumed in prediction 1.

As we already noted, these results are sensitive to the addition of time-fixed effects. Moreover, all but one of the following three specifications are entirely insignificant, and the only one retaining this statistical property does so by alternating the signs of the two coefficients. Thus, while these estimations do not allow for a conclusive statement on the effect of unemployment on guardianship, the effect seems rather complicated and likely to be driven by other factors than unemployment itself.

Table 4: Unemployment and Trips per Person

	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment	-0.255***	-0.420***	-0.0672***	-0.100***	-0.0677***	-0.102***
	(-12.08)	(-24.23)	(-5.10)	(-4.68)	(-5.19)	(-4.85)
Unemployment squared		0.0136***		0.00234		0.00243
		(8.61)		(1.54)		(1.62)
Month			x	x		
Week					x	x
N	495664	495664	495664	495664	495664	495664
Cluster	1377	1377	1377	1377	1377	1377

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method.

Table 4 on the other hand, estimates very robust coefficients w.r.t to the effect of unemployment on trips per person. Even though the magnitude of the effect drops sharply when time-fixed effects are added, the significance of the unemployment coefficient remains constantly above the 0.1% level indicating that an increase in unemployment reduced the number of trips, and therefore criminal opportunities. Moreover, the squared unemployment term added in every even column has the sign we predicted above, indicating that the effect slows down when unemployment reaches very high levels. However, the significance of this squared term fades when either month or week fixed effects are added.

As we have discussed above, there are many different ways in which unemployment can affect the mobility of individuals. While our predictions concerning a non-linear specification have not materialized, at robust significance levels of 5% and below after time fixed effects were added, the notion that per person trips will be impacted more strongly turned out to be true. Whereas the effect on the population staying at home depended much more strongly on the month and week of the year, unemployment consistently decreases the number of trips an individual is willing to undertake.

So what do these results mean for the *unemployment-opportunity* mechanism? Table 4 indicates that individuals become increasingly stationary as a result of unemployment. However, it is quiet as to where the time between trips is spent and therefore the temporal and spatial distribution of the population. Unemployed individuals might choose to stay at home or outside, pursuing activities that mitigate some of the adverse effects of the unemployment spell. However, if a large fraction of their time would be allocated towards guarding their property or activities done from home that might entail a guardianship effect, then we would have expected the effects in Table 3 on the share of the population at home to be similar to the ones found for trips per person.

This means that the effect of unemployment on criminal opportunities is only established for trips per person. Therefore, the opportunity channel as a whole is dependent on how individuals spend their time between trips, or in other words, their spatial and temporal distribution during the day. If significant proportions of their time are spent at home, a strong guardianship effect could still emerge that decreases criminal opportunities. However, suppose most of the time is spent outside, looking for jobs or bargain hunting. In that case, the proposed opportunity channel is likely to play only a minor role in the structural unemployment crime relationship.

5 The Opportunity-Crime Mechanism

Continuing with the second mechanism included in the opportunity channel, we will analyze the proposed link between criminal opportunities and criminal activity on a macro level in this section. With their 1979 work, Cohen and Felson (1979) set the foundations for a strain in criminological research focused on the role of opportunities in creating crimes. This has been a substantial deviation from the usually offender-based explanations. As such, it has not been widely accepted for most of the past century and is up to today a topic of debate. However, most of the empirical evidence for their theory is based on case studies or micro-level data. While, nonetheless, Cantor and Land use it to draw their predictions of the *opportunity-crime* mechanism, the validity on aggregate levels such as a county or larger still remains to be proven. Therefore, in the following section, we set out to find evidence in favor of or against this mechanism.

5.1 Predictions

Again, the natural starting point for our predictions are those set up by Cantor and Land. The relationship proposed in their work predicts that an increase in criminal opportunities also increases criminal activity. This can again be seen above in Figure 1.

$$Opportunity \uparrow \quad \Rightarrow \quad Crime \uparrow$$

As simple as this prediction is, it has one critical caveat. It neglects any possible heterogeneity between different offenses. Similar to most studies in the field, we focus on the seven *Part I* offenses defined by the FBI: murder, rape, aggravated assault, robbery, burglary, larceny/theft, and motor vehicle theft (MVT). Each of these offenses is characterized by a specific target. For violent offenses, these targets are all other individuals, whereas, for property offenses, a target can either be an individual, a motor vehicle, or an apartment, among other things. Similarly, motives for different crimes can vary substantially. While emotions and personal motives often cause violent offenses, property offenses can be seen by an individual's way to earn a living. Therefore, a situation that might provide an opportunity for one crime might equally preclude another crime from happening. (Felson & Clarke, 1998)

For example, an empty house will be a good target for any burglar, but it cannot facilitate a murder or, for that matter, any other violent crime due to the absence of a victim. Therefore, our first and most straightforward augmentation to the predictions is distinguishing between the two crime categories. On the one hand we assume, that property offenses have, as argued by Cantor and Land, a clear and positive relationship with criminal opportunities as we observe them through our two variables. At this point, it is worth reiterating that the two variables we use as proxies for criminal opportunities reflect the presumed change in behavior due to unemployment as stated in the original paper by Cantor and Land (1985). On the other hand, the relationship between criminal opportunities and violent offenses is ex-ante ambiguous since these offenses might not be directly driven by the presence of opportunities or their lack of.

$$
\begin{aligned}
\textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Property Offenses} \uparrow \\
\textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Violent Offenses} \uparrow\downarrow
\end{aligned}
$$

Moreover, if we not only differentiate between the two categories of crimes but rather their included offenses, then the reason for this apparent ambiguity becomes even more evident. The group of violent crimes consists of murder, rape, and aggravated assault, all crimes that require at least one more person to be present to be committed. However, compared to property offenses such as larceny, most violent crimes are very serious offenses that will likely be not very sensitive to small changes in criminal opportunities. Additionally, violent crimes are also more often committed by individuals known to the victim, such as friends or family. Hence, it is unclear if an increase in guardianship or a decrease in trips might facilitate more opportunities for these crimes as individuals spend more time around the persons who are most likely to commit these crimes.

Another way of analyzing this prediction is to look at residential and non-residential crimes. For example, if staying at home increases the exposure to motivated violent offenders, the effects of the opportunity proxies might differ between the two crime categories. While the effect for opportunity on residential property crimes is positive and likely stronger than on non-residential property crimes, the effect on violent crimes might then become negative.

$$
\begin{aligned}
\textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Residential Property Offenses} \uparrow \\
\textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Residential Violent Offenses} \downarrow
\end{aligned}
$$

Adding onto the previous results, we can use data on the time of day an offense was committed to differentiate between daytime and nighttime crimes. Both our variables that proxy the change in mobility behavior due to unemployment, as described by Cantor and Land, will likely impact the criminal opportunity structure more directly during the day than during the night. Most individuals will, irrespective of their employment status, spend most nights at home. However, spending a whole day at home also means that a guardian is present during the daytime. Similarly, most trips individuals undertake will be around daylight hours, either to commute or because of the opening hours of places they intend to visit. Therefore, if opportunity impacts crime on the aggregate level, this effect should be more pronounced for crimes happening during the day than those during the night.

$$\begin{aligned} \textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Crime} \uparrow \\ \textit{Opportunity} \uparrow \quad &\Rightarrow \quad \text{Daytime Crime} \uparrow\uparrow \end{aligned}$$

5.2 Results

Again, we now turn towards the empirical estimation of this mechanism using the data we introduced in section 3. However, in contrast to the continuously distributed opportunity data, which served as our dependent variables in the last section, the criminal data is strongly skewed to the right and contains a non-trivial number of zero observations. This makes directly applying the fixed effects estimator from section 4 problematic. The right-skewness of the data, as evidenced in the summary statistics in Table 1 above, can cause an upward bias. At the same time, the large probability mass with zero values can bias the estimates towards zero.

A common approach to deal with such skewed non-negative data is to apply a log transformation. However, zero entries require that an arbitrarily small number be added to at least every zero observation such that the natural logarithm is defined for all data points. This procedure, no matter how small the added number, does impact our coefficients and ultimately could affect the implications we draw from them (N'guessan, Featherstone, Odeh, & Upendram, 2017).

To circumvent this issue, it is often proposed to apply an inverse hyperbolic (arc-sine) transformation, as it is defined even for zero entries. Even though this solves the issue of adding an arbitrary amount to our data, the arc-sine transformation is sensitive to the number of zeros contained in the data (Bellemare & Wichman, 2020) and the size of the non-zero entries (Aihounton & Henningsen, 2021). Since our data contains roughly 40% of zero entries for the total number of offenses reported on a given day and the mean of this variable is found to be around 4.9, it is unlikely that this transformation will produce unbiased estimates. Therefore, we make use of the count structure of our crime data, which restricts it to the natural number space $\mathbb{N}_0$, to estimate the coefficients with a Poisson estimator. This non-linear quasi maximum likelihood estimator is helpful because the Poisson distribution, from which it is built, allows dealing with skewed data when it is in count form while not setting any restrictions on the zero entries.

As we mentioned earlier, our data suffers from reverse causality issues because we cannot distinguish the purpose of trips or for leaving one's house. This creates an endogeneity problem that we deal with using an instrumental variables approach. For this reason, we use the weather data described in the data section to construct an IV for the mobility behavior. While the classical fixed effects estimator, including an IV, can be obtained through the *2SLS* procedure, this is not possible for the quasi-maximum likelihood estimator used in the Poisson fixed effects regression. As a result, we follow Lin and Wooldridge (2019), who propose a two-step fixed effects Poisson estimator, that includes an instrumental variables approach, and prove its consistency.

Similar to the classical *2SLS* estimator, a first stage is computed using the endogenous variable as the dependent variable and all exogenous variables in addition to the instrument as controls. However, the first stage, as it has no count data as its dependent variable, does not require the Poisson estimator and rather uses the standard fixed effects estimator instead. Moreover, while the *2SLS* estimator uses the predicted values of the dependent variable in the first stage regression as a predictor in the second stage, the two-step fixed effects Poisson estimator uses the residuals from the first stage (ε_{FE}) and adds them as an additional control variable to the second stage. Doing so allows not only to estimate the exogenous effect of the previously endogenous variable, but also to test if the variable was endogenous to begin with. The latter can be inferred through the statistical inference of the added residual term to the second-stage regression. To obtain consistent estimates for the standard errors and account for the two-step procedure, the delta method needs to be used to compute them (Lin & Wooldridge, 2019).

Table 5: First Stage Regressions with Fixed Effects Estimator

	Population at Home		Trips per Person	
	(1)	(2)	(3)	(4)
Precipitation	0.0144***	0.0168***	-0.00589***	-0.00717***
	(29.39)	(36.43)	(-30.37)	(-36.56)
Month		x		x
F-Statistic	864.04	1327.23	922.58	1336.60
N	484734	484734	484729	484729
Cluster	1377	1377	1377	1377

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. F-statistics are calculated and reported for the excluded instrument Precipitation.

The results for the first stage are presented in Table 5 and coefficients for both of our opportunity variables are in line with those described in the review conducted by Turrisi et al. (2021). Rainy days will lead to fewer trips per person and an increase in the share of the population that stays at home altogether. These findings are furthermore robust to the addition of month-fixed effects, indicating that this is not driven by differences in mobility behavior throughout the seasons. Most important to us are the reported F-statistics, which allow us to assess the strength of our instrument. Seeing as this statistic is very large, we can generally rule out any weak instrumental variables bias as indicated by Stock and Yogo (2005).

Moving on to estimate the second stages, the remainder of this section will discuss the results of estimations regarding the effect of a change in criminal opportunity to criminal activity. First of all, Table 6 depicts the relationship between the total offenses reported on a given day within a county and the two opportunity variables. The coefficients with respect to the share of the population at home and the number of trips per person are shown in panels A and B, respectively. For each panel, there are two groups of three coefficients each. The first three columns represent the estimates from a classical fixed effects regression, whereas columns (4) through (6) use the same data but instead apply the Poisson fixed effects estimator. For each of the two estimators, three specifications are presented. Starting with a basic model that only includes the opportunity variable, we subsequently use our instrumental variables approach and finally add month fixed effects to the equation to control for the time dynamics that have been shown to be of great importance in Figure 4 above.

Throughout all specifications, the estimated coefficients with respect to to both opportunity indicators remain significant at and below the 1% level. Moreover, the predicted sign in both panels mirrors the initial predictions set up by Cantor and Land (1985). An increase in guardianship, as proxied by the share of the population at home, reduced the number of crimes reported, whereas an increase in the trips per person, i.e., an increase in mobility, produces the opposite. Therefore it seems as if an increase in opportunities also entails an increase in criminal activity.

Further, there are two additional findings we can take from Table 6. First, the included residual term from the first stage shown in columns (5) and (6) rejects the null hypothesis in both panels. According to Lin and Wooldridge (2019), this signals that our opportunity variables are, in fact, exogenous and that the instrumental variables approach might not be necessary. Due to our reverse causality issue, we will, however, retain this approach throughout this section. Secondly, the coefficients estimated using the Poisson estimator are relatively small compared to their linear counterparts. This drop in magnitude results from the change in how we treat the distribution of our dependent variable and indicates that in a linear model, the right-skewness of our crime data exerted an upward bias onto our coefficients. Based on this finding and the fact that, the estimated coefficient vary less among the Poisson specifications, our following estimations utilize the specification shown in column (6).

Table 6: The Effect of Opportunity on Total Crimes

Panel A: Population at Home

	Fixed Effects			Poisson FE		
	(1)	(2)	(3)	(4)	(5)	(6)
% at Home	-5.794***	-14.76**	-18.05***	-1.906***	-3.003**	-2.639**
	(-9.99)	(-2.81)	(-4.03)	(-14.88)	(-2.97)	(-3.21)
ε_{FE}					1.109	0.812
					(1.09)	(0.99)
IV		x	x		x	x
Month			x			x
N	471978	461413	461413	471978	461413	461413
Cluster	1377	1377	1377	1377	1377	1377

Panel B: Trips per Person

	Fixed Effects			Poisson FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Trips p.P.	0.24***	0.347**	0.42***	0.095***	0.085***	0.075***
	(10.61)	(2.82)	(4.02)	(17.34)	(3.71)	(3.76)
ε_{FE}					0.01	-0.025
					(0.41)	(-1.35)
IV		x	x		x	x
Month			x			x
N	471978	461413	461413	471978	461413	461413
Cluster	1377	1377	1377	1377	1377	1377

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method.

The link we have just established between criminal opportunity and criminal activity does not reflect the heterogeneity between different offenses. It does not even account for heterogeneity between the two more broadly defined groups of crimes that are often analyzed. Since property offenses take up an overproportional share in total crimes, the effects observed in Table 6 could very well be driven by property offenses alone. To analyze our second prediction and to disentangle the *opportunity-crime* mechanism, Tables 7 and 8 show estimated coefficients for both aggregate and individual property and violent offenses respectively. A common feature of both tables is that one offense drives the total effect. Namely these are larceny for property offenses and aggravated assault

Table 7: The Effect of Opportunity on Property Offenses

Panel A: Population at Home

	Property	Robbery	Larceny	Burglary	MVT
% at Home	-2.168*	-5.731	-4.049***	2.742	3.185
	(-2.52)	(-1.31)	(-4.59)	(1.74)	(1.67)
ε_{FE}	x	x	x	x	x
Month	x	x	x	x	x
N	461413	312063	460683	458159	449285
Cluster	1377	872	1365	1352	1302

Panel B: Trips per Person

	Property	Robbery	Larceny	Burglary	MVT
Trips p.P.	0.0658**	0.142	0.113***	-0.0585	-0.0726
	(3.16)	(1.38)	(5.35)	(-1.54)	(-1.60)
ε_{FE}	x	x	x	x	x
Month	x	x	x	x	x
N	461413	312063	460683	458159	449285
Cluster	1377	872	1365	1352	1302

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.

for violent offenses. The remaining offenses are not significantly affected by a change in criminal opportunities.

Starting with property offenses shown in Table 7, the first column indicates the estimated coefficients for the aggregate group of all property offenses. Even though significance and magnitude are somewhat smaller than the effect on total crimes we estimated earlier, the null hypothesis is not rejected, and a significant effect of both opportunity variables on property crimes is established at the 5% and 1% levels. However, from the following four columns, which show the results for each of the offenses included in property crimes separately, we can only find similar results for larceny. Reasons for the apparent insignificance among the remaining three offenses could be manifold and are likely to be different for each of the offenses. Beginning from the left, what has been characterized as opportunity by Cantor and Land, and is mirrored by our variables, might have a counteracting effect on robberies. While fewer trips and more people at home reduce the number of possible victims to be exploited by a robber, they also reduce the number of guardians on the street that could prevent victimization. The concept of guardianship adopted by Cantor and Land focuses solely

Table 8: The Effect of Opportunity on Violent Offenses

Panel A: Population at Home				
	Violent	AA	Murder	Rape
% at Home	-6.253***	-7.552***	-0.772	0.768
	(-3.35)	(-3.92)	(-0.06)	(0.19)
ε_{FE}	x	x	x	x
Month	x	x	x	x
N	454635	449974	226901	399866
Cluster	1328	1308	627	1134

Panel B: Trips per Person				
	Violent	AA	Murder	Rape
Trips p.P.	0.145**	0.173***	-0.00149	-0.0141
	(3.25)	(3.79)	(-0.00)	(-0.15)
ε_{FE}	x	x	x	x
Month	x	x	x	x
N	454635	449974	226901	399866
Cluster	1328	1308	627	1134

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.

on protecting oneself and one's property. However, guardianship usually has multiple dimensions, with the presence of other individuals around being one of them (Wilcox et al., 2007). Therefore, a decrease in our opportunity measures might reduce the number of social guardians that could protect an individual from being robbed.

A similar argument can be made for motor vehicle theft. On the one hand, fewer trips and an increased share of individuals staying at home, reduces the number of vehicles suitable for theft, because they are, for example, parked in a driveway or a garage. On the other hand, it also reduces social guardianship, i.e., through pedestrians or cars passing by a parked car, increasing its attractiveness as a criminal target.

For burglaries, we assume that a different mechanism is at play. Compared to larceny, which is a highly opportunistic offense, burglaries are often planned prior and then executed accordingly. This makes them less dependent on the availability of opportunities, as a burglar will spend time and effort to search and find his opportunity before committing the actual offense rather than reacting to short-run changes in the opportunity structure.

Moving on to violent offenses, for which results are shown in Table 8, a similar picture emerges. Again, we first find a significant relationship between criminal opportunities and the aggregate group of violent offenses. However, if we differentiate between the included offenses, this relationship disappears for all but one offense category, aggravated assault. Moreover, both of our measurements for criminal opportunity have no significant effect on either one of the other two offenses, murder and rape. As we have discussed earlier, a possible reason for these results is that most offenders for these crimes are found to be known to the victim. An increase in the share of people at home or a decrease in trips undertaken per person could, therefore, result in an increase in exposure towards motivated offenders. However, as we see by our t-statistics for these estimates, the relationship between criminal opportunities and these two offenses is insignificant to any common significance level. Therefore, it might be suitable to explain these findings through the motivation of an offender and the severeness of the crime. It seems conceivable that an individual motivated to commit such a crime will do so independently of the opportunities available to them.

In contrast to the results for the overall effect of opportunities on crime, analyzing the offenses and group separately paints a rather bleak picture for the future of the *opportunity-crime* mechanism. Even though it seems, as the sum of crimes committed in a county is affected by changes in the share of the population staying at home and the number of per person trips, the significance of this finding disappears for five out of seven offenses. However, before we rule out this mechanism as the main driver for crime in general, we investigate the possibilities of temporal and geographic displacements among crimes.

5.3 Spatial and Temporal Heterogeneity

Both of the two opportunity variables we analyze, based on the theoretical predictions of Cantor and Land, have disproportional effects on the criminal opportunity structure during the day and within residential areas. An increase in the share of the population at home will most certainly have a more considerable impact on the share of the population at home during daylight hours compared to the share at home during the night. Moreover, any trip made by a person will result in them being away from their home and most likely even outside of a residential area if that trip leads to a place of employment or consumption. It is therefore possible that our estimations do not facilitate an overall change in criminal opportunities but rather a redistribution of their temporal and spatial occurrences.

Based on the location coding included in NIBRS, we can compare estimates for crimes reported inside and outside of residential areas. If a decrease in criminal opportunities in residential areas

Table 9: The Effect of Opportunity and the Location of Offenses

Panel A: Population at Home

	Property Crimes		Violent Crimes	
	Residential	Non-Residential	Residential	Non-Residential
% at Home	-4.507^{***}	-0.559	-3.711	-8.426^{***}
	(-3.96)	(-0.57)	(-1.63)	(-3.52)
ε_{FS}	x	x	x	x
Month	x	x	x	x
N	460452	461052	446943	436397
Cluster	1364	1371	1295	1254

Panel B: Trips per Person

	Property Crimes		Violent Crimes	
	Residential	Non-Residential	Residential	Non-Residential
Trips p.P.	0.114^{***}	0.0326	0.0785	0.204^{***}
	(4.18)	(1.36)	(1.45)	(3.58)
ε_{FS}	x	x	x	x
Month	x	x	x	x
N	460452	461052	446943	436397
Cluster	1364	1371	1295	1254

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.

leads to the displacement of criminal activities into other areas, we would expect that the estimated coefficients for the corresponding locations have opposing signs. However, this is not the case for either one of the two offense groups, as shown in Table 9. In fact, as can be seen in the appendix A2 where the same specifications are shown for all offenses, this is not the case for any of them.

Additionally, we can see that while property offenses are only affected within residential areas, the opposite is true for violent offenses. This is, however, by no means an argument for the displacement of crimes. It rather again highlights the nature of our criminal opportunity variables and the nature of these crimes. For both variables we analyze, a decrease in criminal opportunities leads to a concentration of individuals within residential areas. This means that they act both as guardians for themselves but also other properties and therefore prevent property offenses from happening in the places they are located in. However, by increasing the concentration of the population within residential areas, possible victims of violent offenses outside these areas are removed. Hence criminal opportunities for violent offenses are more directly affected outside these residential areas

Table 10: The Effect of Opportunity and the Time of Day of Offenses

Panel A: Population at Home

	Property Crimes		Violent Crimes	
	Daytime	Nighttime	Daytime	Nighttime
% at Home	-3.831***	0.365	-7.956**	-5.400*
	(-3.88)	(0.32)	(-2.97)	(-2.34)
ε_{FS}	x	x	x	x
Month	x	x	x	x
N	457773	460463	437721	449353
Cluster	1352	1363	1258	1302

Panel B: Trips per Person

	Property Crimes		Violent Crimes	
	Daytime	Nighttime	Daytime	Nighttime
Trips p.P.	0.106***	0.00233	0.190**	0.121*
	(4.38)	(0.09)	(2.98)	(2.18)
ε_{FS}	x	x	x	x
Month	x	x	x	x
N	457773	460463	437721	449353
Cluster	1352	1363	1258	1302

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.

than within. This again highlights the complex and heterogeneous relationship between criminal opportunities and offenses, without altering our previous results. Even though these findings do not validate our earlier prediction completely, the insignificant coefficients for residential violent offenses suggest that there are some effects that offset the *opportunity-crime* mechanism for these offenses.

While our last results rule out the spatial displacement of crimes, differential effects on the opportunity structure throughout the day could still lead to temporal displacements of crimes that create overall insignificant results. Using the definition of daytime and nighttime crimes proposed by Felson and Poulsen (2003), Table 10 differentiates between crimes that happen during the day, from 5 am to 5 pm, and those that happen during the night. Similar to our previous results for spatial displacement, we do not observe any differential effects between daytime and nighttime crimes for either one of the two variables. These findings again hold for all offenses, which are depicted in the appendix A3. As with our spatial analysis of crimes, the temporal differentiation highlights the heterogeneity between the two aggregate groups. While only daytime property offenses react to a change in criminal opportunities, violent offenses show a negative relationship with both variables during the night as well. However, compared to the effect on daytime violent offenses, which is significant to the 1% level, nighttime violent offenses are only significant to the 5% level. We view this at least as a slight confirmation of the earlier prediction that daytime crime will be affected more strongly.

Taken together, neither Table 9 nor 10 provide evidence for temporal or spatial displacement effects of criminal activity. This rules out that the estimated coefficients for offense level data reported in Tables 7 and 8 are biased towards zero due to a reallocation of criminal activity across these two dimensions. Moreover, without such a bias to suggest otherwise, the only offenses for which we can identify an overall significant effect of the opportunity structure on their occurrence are larceny and aggravated assault. With heterogeneities between offenses being further highlighted through our spatial and temporal decompositions, the *opportunity-crime* mechanism, even though significant at the aggregate level for all crimes, is driven purely by these two offenses. Hence, the analysis of this mechanism again sets limitations to the applicability of the opportunity channel.

6 Limitations

Most limitations of our analysis stem from data availability. Obtaining daily county-level criminal statistics demands a matching and allocation procedure. While only a small share of our observations (9.3%) stem from multi-county LEAs, we can still only proxy the true spatial distribution of actual criminal occurrences. Moreover, the transition to the national incident-based reporting system (NIBRS) only allows us to estimate the relationship for a subset of 24 already transitioned states. While these cover roughly half of all law enforcement agencies in the USA, especially populous states, have so far not adapted NIBRS, which could limit the external validity of our estimates.

To construct proxies for the change in criminal opportunities that represent the effects stated by Cantor and Land, we use data on the share of the population staying at home and the number of trips per person on a given day within a county. Even though this data is computed from multiple sources using millions of mobile devices, it could still underrepresent poor individuals and the unemployed if low income corresponds with a lack of access to such devices. Another restriction to our dataset comes from the availability of the trips data, which is only published as far back as 2019. Together with the start of the Covid-19 pandemic in early 2020, we limited the data to 2019 to reduce the risk of introducing unwanted confounders. However, fixed estimators make it impossible to include many macroeconomic variables, as they have no within-year variation, or to analyze the sensitivity of our estimates over time.

The latter restriction also precludes us from obtaining a better identification strategy through means of applying a difference in difference or regression discontinuity estimator for the *unemployment-opportunity* mechanism. Our estimated coefficients regarding these mechanisms might therefore be seen as naive, and their confidence can surely be improved with greater data availability.

7 Conclusion

After nearly 50 years of research on the unemployment-crime relationship, the literature has still not reached a consensus. A reliance on the structural model by Cantor and Land (1985) and the notion from Paternoster and Bushway (2001) to analyze this model "as it was presented" have failed to produce robust estimates for the effect of unemployment on criminal activity. While one reason for the differences in estimates might be found in the sensitivity of the estimates regarding the analyzed time period (Chalfin & McCrary, 2017), the two channels making up the structural model and especially their underlying mechanisms have so far been unexplored. In this work, we started by discussing these channels and highlighting the potential values that would come from analyzing them.

Thereby we argue, that a closer look at the *motivation channel*, could resolve some of the sensitivity with respect to the studied time periods. Differences in the perception of economic conditions can affect expectations and the updating behavior of beliefs for individuals, both factors that will likely influence the formation of criminal motivation. Moreover, the *opportunity channel* offers ways to directly test the specification of the structural unemployment-crime model on a macroeconomic level. Since correctly specifying this model should be of focus for future research before analyzing differences over time, we use the empirical part of this paper to analyze the two mechanisms underlying the *opportunity channel*.

Our results and prediction for the *unemployment-opportunity* mechanism indicate a less clear relationship than previously thought. On the one hand, we find a robust and negative effect on the number of per person trips, an activity that is likely costly. On the other hand, the effects with respect to the share of population at home are neither robust nor significant as soon as we add time-fixed effects.

For the *opportunity-crime* mechanism, our findings indicate a robust and significant effect of opportunities on criminal activity. However, when we allow for heterogeneity between offenses, we find that only two of the seven offenses, larceny, and aggravated assault, drive this effect. The other five offenses all reject the null hypothesis and hence remain unaffected by a change in both the population at home and the number of trips per person. Allowing for spatial and temporal displacement does not alter these findings so that this mechanism can only be established for the two previously named offenses.

In conclusion, the *opportunity channel* that Cantor and Land have proposed seems unlikely to play an essential role in the unemployment-crime relationship. The first of its two underlying mechanisms can only be established for the number of trips per person and hence might not entail the large effects on guardianship it is thought to facilitate. Moreover, the second mechanism rejects the null for five out of seven offenses that have been commonly analyzed in the literature. This means that even if the opportunity channel is to play a role, it will likely only do so for larceny and aggravated assault. While our results have not resolved the lack of conclusive findings on the unemployment-crime relationship, they have shown the importance of understanding the underlying mechanisms of the proposed structural model. For future research, we propose to continue this work, such that an empirically tested structural model for this relationship may emerge, which can be used to solve the consensus of doubt.

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Affidavit

“I, Florian Fickler, affirm that this review was written by myself without any unauthorized third-party support. All used references and resources are clearly indicated. All quotes and citations are properly referenced. This thesis was never presented in the past in the same or similar form to any examination board. I agree that my thesis may be subject to electronic plagiarism check. For this purpose, an anonymous copy may be distributed and uploaded to servers within and outside the University of Mannheim.”

Mannheim, November 17, 2021

Florian Fickler

Appendix

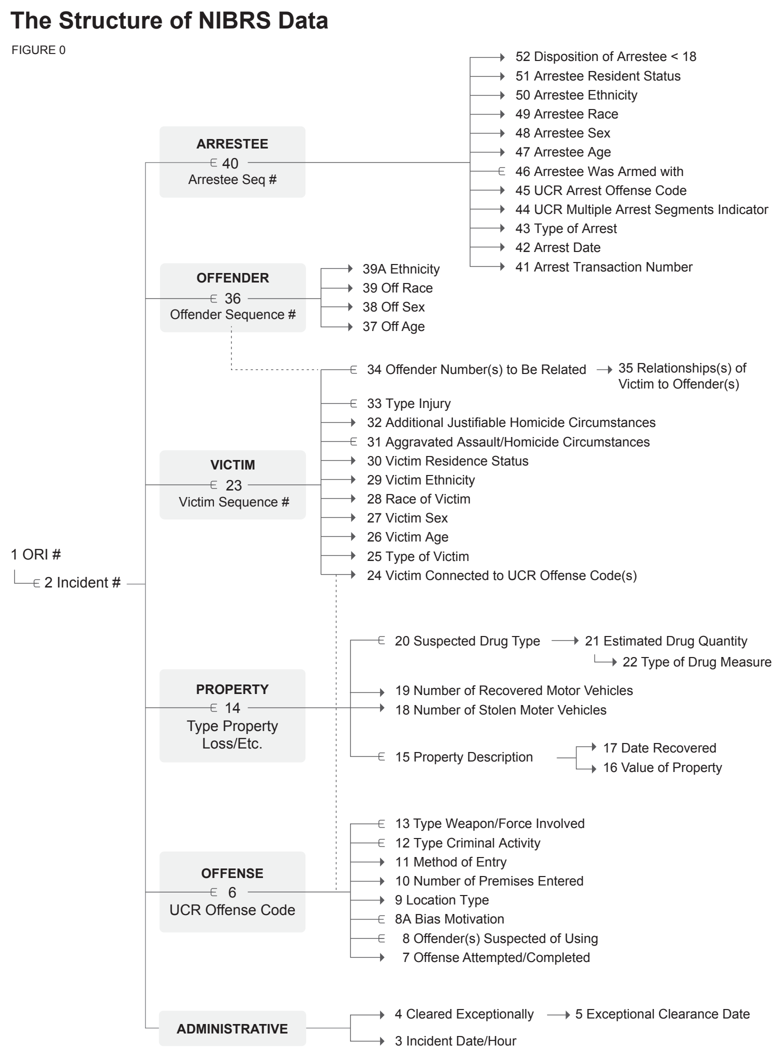

Figure A1: NIBRS Data Structure (taken from FBI's UCR)

Table A1: Location Codes contained in NIBRS

CODE	LOCATION	CODE	LOCATION
1	Air/Bus/Train Terminal	37	Abandoned/Condemned Structure
2	Bank/Savings and Loan	38	Amusement Park
3	Bar/Nightclub	39	Arena/Stadium/Fairgrounds/Coliseum
4	Church/Synagogue/Temple/Mosque	40	ATM Separate from Bank
5	Commercial/Office Building	41	Auto Dealership New/Used
6	Construction Site	42	Camp/Campground
7	Convenience Store	44	Daycare Facility
8	Department/Discount Store	45	Dock/Wharf/Freight/Modal Terminal
9	Drug Store/Doctor's Office/Hospital	46	Farm Facility
10	Field/Woods	47	Gambling Facility/Casino/Race Track
11	Government/Public Building	48	Industrial Site
12	Grocery/Supermarket	49	Military Installation
13	Highway/Road/Alley/Street/Sidewalk	50	Park/Playground
14	Hotel/Motel/Etc.	51	Rest Area
15	Jail/Prison/Penitentiary/Corrections Facility	52	School-College/University
16	Lake/Waterway/Beach	53	School-Elementary/Secondary
17	Liquor Store	54	Shelter-Mission/Homeless
18	Parking/Drop Lot/Garage	55	Shopping Mall
19	Rental Storage Facility	56	Tribal Lands
20	Residence/Home	57	Community Center
21	Restaurant	58	Cyberspace
22	School/College	0	Not Specified
23	Service/Gas Station		
24	Specialty Store		
25	Other/Unknown		

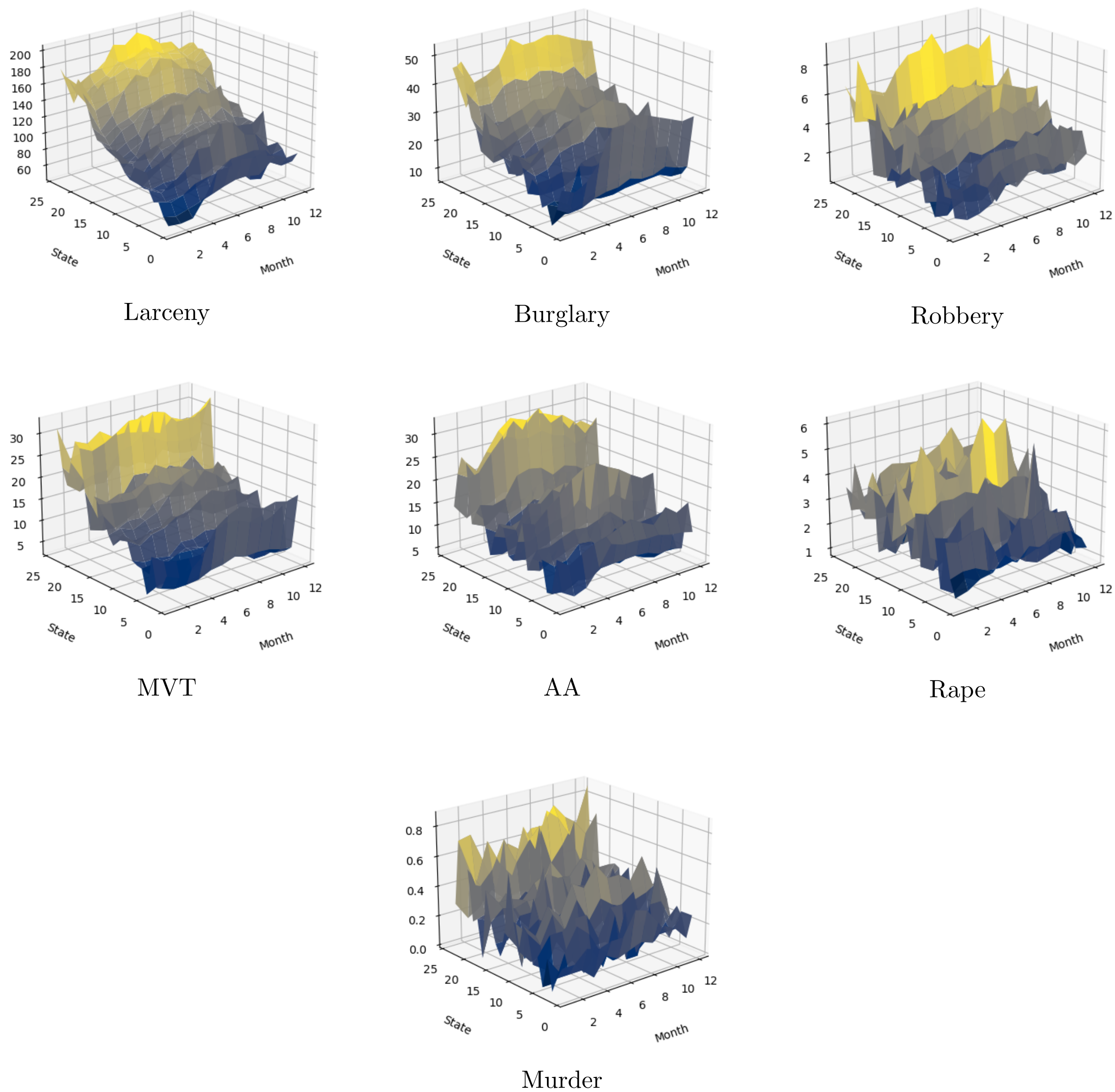

Figure A2: Crime Rates per 100.000 Inhabitants for different Offense-Level Subgroups

Z-axis is scaled according to the lowest and highest values of computed crime rates.The state index starting from 1 to 24 corresponds to the following states: Oklahoma, Massachusetts, New Hampshire, Connecticut, Vermont, West Virginia, Idaho, Virginia, Michigan, Iowa, Ohio, Rhode Island, South Dakota, Kansas, North Dakota, Kentucky, North Carolina, Montana, Colorado, Oregon, Tennessee, Arkansas, South Carolina, Washington.

Table A2: The Effect of Opportunity on *Part I* Offenses in Residential and Non-Residential Areas

Panel A: Residential

	% at Home	t	Trips p.P.	t	N	Cluster
Robbery	-9.621	(-1.39)	0.220	(1.33)	241952	674
Larceny	-6.687***	(-4.92)	0.169***	(5.23)	457768	1341
Burglary	-2.729	(-1.63)	0.0709	(1.78)	454110	1328
MVT	3.823	(1.58)	-0.0992	(-1.72)	429089	1231
AA	-4.633	(-1.91)	0.0985	(1.71)	441777	1273
Murder	-0.843	(-0.04)	-0.00370	(-0.01)	192452	531
Rape	1.004	(0.20)	-0.0233	(-0.20)	386208	1091

Panel B: Non-Residential

	% at Home	t	Trips p.P.	t	N	Cluster
Robbery	-4.459	(-0.93)	0.116	(1.03)	285756	795
Larceny	-2.772**	(-2.85)	0.0868***	(3.70)	459275	1354
Burglary	15.23***	(5.31)	-0.354***	(-5.19)	444456	1290
MVT	2.586	(0.98)	-0.0488	(-0.78)	429839	1230
AA	-11.43***	(-4.28)	0.274***	(4.36)	427076	1223
Murder	-0.657	(-0.04)	-0.00323	(-0.01)	133872	369
Rape	0.698	(0.09)	-0.00272	(-0.01)	308098	860

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.

Table A3: The Effect of Opportunity on Part I Offenses during Day and Nighttime

Panel A: Daytime

	% at Home	t	Trips p.P.	t	N	Cluster
Robbery	-0.404	(-0.07)	0.0288	(0.20)	262189	727
Larceny	-5.439***	(-5.18)	0.147***	(5.78)	456490	1342
Burglary	-0.496	(-0.24)	0.0190	(0.39)	450258	1313
MVT	3.109	(1.28)	-0.0690	(-1.18)	432345	1239
AA	-7.811*	(-2.56)	0.184*	(2.54)	428921	1229
Murder	7.055	(0.42)	-0.181	(-0.46)	159016	439
Rape	-10.71	(-1.68)	0.265	(1.75)	352208	987

Panel B: Nighttime

	% at Home	t	Trips p.P.	t	N	Cluster
Robbery	-9.379	(-1.83)	0.220	(1.81)	278569	777
Larceny	-1.898	(-1.58)	0.0599*	(2.12)	458245	1342
Burglary	7.316**	(3.05)	-0.169**	(-2.99)	448345	1304
MVT	3.289	(1.24)	-0.0804	(-1.29)	432222	1239
AA	-7.476**	(-3.14)	0.167**	(2.97)	441677	1273
Murder	-6.007	(-0.35)	0.116	(0.29)	173021	477
Rape	10.59	(1.80)	-0.264	(-1.90)	362859	1022

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; t statistics in parentheses. Clustered SE computed with delta-method. Number of observations vary due to the exclusion of counties that do not report any of the analyzed offenses during the year of 2019. This exclusion is a result of the Poisson distribution used in the PQML estimator.